PHYSICS IN PERSPECTIVE

VOLUME II
PART A

The Core Subfields of Physics

NATIONAL
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733

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IN
PERSPECTIVE

VOLUME II
PART A

The Core Subfields of Physics

Physics Survey Committee • National Research Council

NOTICE: The study reported herein was undertaken under the aegis of the Committee on Science and Public Policy (COSPUP) of the National Academy of Sciences–National Research Council, with the express approval of the Governing Board of the National Research Council.

Responsibility for all aspects of this report rests with the Physics Survey Committee, to whom sincere appreciation is here expressed.

The report has not been submitted for approval to the Academy membership or to the Council but, in accordance with Academy procedures, has been reviewed and approved by the Committee on Science and Public Policy. It is being distributed, following this review, with the approval of the President of the Academy.

ii

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BRUCE N. GREGORY
CHARLES K. REED

*In general, no agency was represented on the Committee or any of its Panels by more than one person at a time.

Preface

The Physics Survey Committee was appointed by the President of the National Academy of Sciences in mid-1969 to survey the status, opportunities, and problems of physics in the United States. Volume 1 of *Physics in Perspective* constitutes the full report of the Committee. This, and the companion parts of Volume 11, complete the report of the survey.

The Survey Committee early concluded that it was essential that it obtain detailed information from experts in each of a number of physics subfields and interface areas. For each of these subfields and interface areas a chairman was appointed by the Chairman of the Survey Committee, and groups of recognized experts were brought together to survey and report on their respective subject areas.

Several of the subfields have relatively well-defined and traditional boundaries in physics. Included are the core subfields of acoustics, optics, condensed matter, plasmas and fluids, atomic, molecular, and electron physics, nuclear physics and elementary-particle physics. The reports of these panels constitute Part A of Volume II. In addition, there are several important interface areas between physics and other sciences. In the case of astronomy, where activity is particularly vigorous at the interface and is overlapping, the Physics and Astronomy Survey Committees agreed to form a joint panel that would report on astrophysics and relativity, an area of special interest to both. The broad area in which physics overlaps geology, oceanography, terrestrial and planetary atmospheric studies, and other environmental sciences was defined as earth and planetary physics, and a panel was established to survey it. In covering the physics–chemistry and physics–biology interfaces, the broader designations "physics in

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chemistry" and "physics in biology" were chosen to avoid restricting the work of the panels to the already traditional boundaries of these inter-disciplinary fields.

Although each panel—and particularly those responsible for the core subfields—was asked to consider the interaction of its subfield with tech-nology, the Committee anticipated that the emphasis would be on recent developments that advanced the state of the art and on what is generally described as *high technology*. Therefore, to include more specifically the active instrumentation interface between physics and the more traditional manufacturing sectors of the economy—steel, drugs, chemicals and con-sumer goods, to name only a few, in which many old parameters are being measured and controlled in new and ingenious ways—a separate panel was established.

Panels were also appointed to centralize the statistical data-collection activities of the survey and to address the questions of physics in educa-tion and education in physics. Each of these panels prepared a report, and, in addition, an extended report on the dissemination and use of the in-formation of physics was prepared by a member of the Committee. With the exception of the one on statistical data, all these reports are included in Part B of Volume II; the Statistical Data Panel report constitutes Part C of Volume II.

The Nuclear Physics Panel was commissioned to carry out its survey on an accelerated time scale, and in greater depth than the other panels, in response to a specific request from the President's Science Advisory Com-mittee (PSAC) for findings and recommendations that could be used in policy and planning discussions at an early date. The final report of that panel, which appears in Part A, is a revised and updated version of the one transmitted to PSAC in 1971.

A number of subjects in classical physics, such as mechanics, heat, thermodynamics, and some elements of statistical physics, were not con-sidered explicitly in the survey. This omission is in no sense intended to imply any lack of importance of these fields but merely indicates that they are mature fields in which relatively little research *per se* is currently being conducted.

In the very nature of the survey, the Committee and its panels have explored many alternatives and options in developing their reports. It should thus be emphasized that the lack of explicit mention of any one of these does not imply that it has not been considered or examined.

Early in the survey, the Committee developed and addressed to each panel a lengthy charge, which appears as Appendix A. This charge was broad-ranging and dealt with the structure and activity of a subfield, viewed not only internally but also in terms of its past, present, and po-tential contributions to other physics subfields, other sciences, technol-

ogy, and society generally. Consonant with the overall survey objectives, each panel was asked to develop several detailed budgetary projections ranging from one that would permit exploitation of all currently identified opportunities in a subfield to one that continued to decrease during the period under consideration.

Clearly, the charge was most directly relevant to the more traditional subfields; in the case of the interface panels, some questions were inevitably unanswerable without a survey of equivalent scope of the field or fields on the other side of the interface. In astronomy, such a survey was available. Nevertheless, from the reports included in this and in the companion parts of Volume II, it is plain that the panels have responded in depth to the questions asked.

Initial draft responses to the charge were presented to the Survey Committee by the panel chairmen during an extended working session in June 1970, and, following subsequent discussions and reviews, preliminary panel reports were submitted to the Committee during the summer of 1971. Whenever possible, each of these preliminary reports was forwarded for comment to a group of some ten readers, selected jointly in each case by the panel chairman and the chairman of the appropriate division of the American Physical Society or other Member Society of the American Institute of Physics. These readers were chosen, insofar as possible, from among the most active scientists in each subfield, with particular emphasis on younger scientists who had not been involved previously in the survey. The Committee received excellent cooperation from all of them. They provided fresh insight and new viewpoints on many aspects of the panel reports. Their comments and those of the Survey Committee and other reviewers were carefully considered by the panels in the preparation of the final reports that appear herein.

It must be emphasized that the panel reports and their recommendations and conclusions were addressed specifically to the Survey Committee. The many instances in which the Committee concurred with and supported these findings are reflected in the Committee's report, Volume I. On occasion, however, the Committee, from its broader viewpoint covering not only all of physics but also its broader external interaction, not unexpectedly reached somewhat different conclusions.

The panel reports are being made available here in the form submitted to the Survey Committee, not only to provide the detailed technical background and documentation for many of the Committee's findings, but also because they provide, to a unique degree, a measure of the vitality and strength of the different subfields of physics. Repeatedly in its activity, the Survey Committee has been reminded of the unity of physics and, indeed, of all science. This intellectual thread is interwoven through all the panel reports.

The Survey Committee is profoundly grateful to the members of its panels and most particularly to their chairmen, for their effective and thoughtful responses to the often difficult questions posed to them. Perhaps the most difficult have been those relating to the future style, direction, and thrust of physics under conditions in which not even all those projects and groups judged excellent by peer and support agency reviews can hope to find support. These questions are much more directly answerable in some subfields than in others—in those dependent upon very large facilities rather than on more modest instrumental requirements—but they are very significant in all subfields.

The panel chairmen responded frequently and effectively to Committee requests for additional information and assistance; they participated fully in a number of the major Committee working sessions and they gave most generously of their time and effort throughout the survey.

Support for the survey activity has been provided equally by the Atomic Energy Commission, the Department of Defense, the National Aeronautics and Space Administration, and the National Science Foundation. Additional assistance has been provided through grants from the American Physical Society and from the American Institute of Physics.

Staff of all the federal agencies engaged in the support of physics have given generously of their time and effort in searching out and providing answers to innumerable questions. Liaison representatives of these agencies participated in many long days of discussion as the Committee and panel reports developed. The Committee is deeply grateful to all of them.

The Committee and its panels cannot hope to acknowledge in detail all the assistance that they have received from many persons and organizations throughout the country. Over and above their major contributions to the activity of the Survey Committee itself, George W. Wood, Charles K. Reed, Bruce N. Gregory, and Bertita E. Compton have worked directly with the different panels in many, many ways and have provided an overall coherence that otherwise would have been quite impossible. They deserve our particular gratitude. Jacqueline Boraks has accomplished the often overwhelming task of copy editing the entire survey report with remarkable effectiveness and taste. In these panel reports she has been ably assisted by Jeannette W. Lindsay. And finally, Beatrice Bretzfield, the Secretary to the Physics Survey at the Academy, and Mary Anne Thomson, my administrative assistant at Yale, have been of tremendous help to the Committee and to me throughout the survey. To all of these, I would express both my personal thanks and that of the Committee and its panels.

D. ALLAN BROMLEY, *Chairman*
Physics Survey Committee

Contents

I
Elementary-Particle Physics

PANEL MEMBERS

ROBERT G. SACHS, University of Chicago, *Chairman*
JAMES D. BJORKEN, Stanford Linear Accelerator Center
JAMES W. CRONIN, University of Chicago
THOMAS FIELDS, Argonne National Laboratory
LOUIS N. HAND, Cornell University
DONALD H. MILLER, Northwestern University
WILLIAM J. WILLIS, Yale University

LIAISON REPRESENTATIVES*

MARCEL BARDON, National Science Foundation
ALBERT OPP, National Aeronautics and Space Administration
ERWIN R. SCHMERLING, National Aeronautics and Space Administration
WILLIAM A. WALLENMEYER, Atomic Energy Commission

READERS

HANS A. BETHE, Cornell University
ROBERT HOFSTADTER, Stanford University
J. DAVID JACKSON, University of California, Berkeley
W. K. H. PANOFSKY, Stanford University
ORESTE PICCIONI, University of California, San Diego
D. D. REEDER, University of Wisconsin
KARL STRAUCH, Harvard University
ROBERT L. WALKER, California Institute of Technology

*In general, no agency was represented on the panel by more than one person at a given time.

Preface

Elementary-particle physicists in the United States find themselves today at the threshold of a wonderful opportunity for another explosive penetration into a new level of scientific discovery. The National Accelerator Laboratory is on the verge of producing a proton beam of energy ten to fifteen times greater than that of any other accelerator in this country and three to seven times greater than that of the highest energy accelerator in the world—the Russian 70-GeV machine at Serpukhov.

This step occurs when the field is in a period of unusual activity and promise. New exciting results, new ideas, and new methods appear on many fronts. The opportunities they open up are the culmination of intense and productive work extending over a period of some 20 years.

That period has been marked by the discovery of parity violation in weak interactions; the discovery that there are two different kinds of neutrino; confirmation of the idea that the vector part of the weak interaction is generated in a manner remarkably similar to the generation of electromagnetic interactions; the discovery that there is a difference between the world of particles and the world of antiparticles, even when the latter is viewed in a mirror (i.e., violation of CP invariance), and the associated discovery that there is some aspect of the weak interactions that depends on the direction of flow of time; the elucidation of the structure of neutron and proton in terms of their internal charge and current densities; the discovery that protons appear to have a particulate structure; the realization that electromagnetic properties of particles not only relate to the usual massless photons but also involve very massive vector mesons that are subject to strong (nuclear) interaction; the exploration of the

3

limits of relativistic quantum electrodynamics down to a distance of the order of 10^{-14} cm; the opening up of the field of hadron spectroscopy and the discovery of the underlying SU(3) symmetry relating the hadrons; the realization of phenomenological theories for dealing with relativistic reactions between hadrons and interpreting them, especially at very high energy; the resulting experiments that keep pressing the question of: When will the energy be high enough?; the development of the bubble-chamber technique and the automatic data-handling machines that have made much of the spectroscopy possible; the development of wire chambers, and more recently proportional wire chambers, connected on-line to computers making possible data rates capable of producing millions of events in a reasonable time; the conception, design, and construction of entirely new kinds of accelerators; the development of practical large superconducting magnets to keep the power costs within reason and to produce higher magnetic fields; the development of ac and rf superconducting systems to the point that it will soon be possible to use them to obtain higher energy or better duty cycle without increasing the size or power requirements of accelerators; and the development of such ideas as the electron ring accelerator, which may make acceleration to much higher energy feasible.

The field has been marked by one splendid experience after another in research into the fundamental properties of matter, in training of scientific and technical manpower, and in technological results. The achievements may be measured by the discovery of new properties of matter or of new aspects of the fundamental laws of physics, and it may be judged by the number of Nobel Prizes awarded in the field. It may be gauged by the quality of the people who have been attracted to the field and by their training not only for this but for other activities (about 50 percent of the PhD graduates in high-energy physics have gone into other fields). It may be measured by the technological innovations that have been needed for the work and have had a widespread influence on other fields, or it may be judged by the completion of large projects requiring large extrapolations of old technologies and the invention of new ones, both being at least as challenging as any in modern engineering. And, in spite of the need for these extrapolations and innovations, huge construction projects have on the whole been completed on schedule and within authorized budgets. The success of the U.S. research in this field may also be inferred from the efforts of the Western European countries and the Soviet Union to emulate the methods and style of the U.S. program, an effort that has been so successful that the Western Europeans have now surpassed us both in certain technical areas and in the financial support provided by their governments, while the Soviet Union has succeeded in building and operating the highest energy proton accelerator in the world.

The history of achievement for the U.S. program is now being capped by the opportunity to explore an entirely new domain of high-energy physics at the National Accelerator Laboratory.

This inspiring story has been possible only because of the enthusiastic support that the work has had from many sources, most particularly from the Joint Committee on Atomic Energy of the Congress, from the Atomic Energy Commission, and, more recently, from the National Science Foundation. However, the support situation has developed serious inconsistencies in the past few years resulting in bleak funding prospects that stand in stark contrast to the state of the field and its record of past accomplishment. The United States still has the greatest potentiality for research in this field of any country, but the capability is being rapidly dissipated for lack of adequate funding. One accelerator (the Princeton–Pennsylvania Accelerator) has been shut down, and all other accelerators are being utilized at less than 75 percent of their capability and are facing even further cuts. Many hundreds of highly skilled technicians and engineers have lost their jobs, and there is also a shortage of jobs for PhD's trained in the field. University groups are finding it more and more difficult to obtain the funds needed to mount experiments that will take advantage of the opportunities.

The total funding of operations and equipment of the Atomic Energy Commission (AEC) accelerator laboratories has been decreasing in absolute dollars during a period of severe inflation and at a time when one new major accelerator (SLAC) has come into operation, when a major improvement program increasing the capability of another (the AGS at Brookhaven) is being completed, and when university groups from all parts of the country are trying to prepare for experiments at the National Accelerator Laboratory (NAL).

One source of this paradoxical situation is the absence of any procedure for establishing a commitment to a long-range plan. The time required to plan, instrument, carry out, and analyze a single experiment in high-energy physics may run from two to five years. The time required to plan, design, propose, and construct an accelerator is even longer. With such time elements built into the work, long-range planning and firm advance commitments to agreed-upon plans are a necessity. Several attempts have been made to establish such long-range plans. At the request of the Joint Committee on Atomic Energy, the AEC prepared a program for high-energy physics which was transmitted to the Committee by President Johnson on January 26, 1965. The plan was well received and became the basis for further later actions authorizing the construction of accelerators. However, subsequent funding fell short of that proposed in the plan; and construction of several important items contained in the plan, such as the electron–positron storage rings, were delayed by years.

A revised and less optimistic plan was presented in the 1969 Report of the High Energy Physics Advisory Panel (HEPAP), but the subsequent funding for operations and equipment has also fallen far short of that plan. Thus, there has been an enormous disparity between the actual funding and the planning assumptions that appeared to have been accepted at the time the laboratories were created.

Meaningful planning for the future is essential in this field, but the planning without some degree of commitment to implementation is worse than useless. Future prospects for this and other fields of research involving long time delays would be far brighter if some means could be found to obtain a reasonably firm commitment of funds over a period commensurate with the natural lead times built into the program.

The purpose of this report of the Elementary-Particle Physics Panel is to respond to its charge from the Physics Survey Committee to set forth the nature and accomplishments of the field and to indicate its scientific prospects and resource needs for the future. The report includes a general statement on the nature of the field (Chapter 1), followed by a rather detailed description of its status (Chapter 2). The statement on status is intended for an audience of physicists who have not been working in elementary-particle physics and for those physics students who may be interested in learning something of the nature of this field. Therefore, an attempt is made to present the substance of the physics in the hope that the intended audience will gain an appreciation of the work described. This detailed description of physics can be avoided by omitting the sections of Chapter 2 that follow the summary Section 2.3.

The scientific accomplishments and prospects are the principal justification for support of this work, but there are other less direct justifications too. These are presented in terms of interactions with technology (Chapter 3), the interaction with other research fields (Chapter 4), and the interaction with society (Chapter 5).

Justification for research in elementary-particle physics by reference to its impact on technology or society is necessarily on tenuous ground. Even the interaction with all but a few other research fields is indirect. The discussion must make use of historical examples, spin-off arguments, or speculative suggestions bordering on fantasy. Nevertheless, there is good reason to believe that important consequences along these lines can be expected in the long term. In the past, the actual achievements of this field have not followed very closely the prior justifications but have often surpassed them in importance and interest.

Because the more practical benefits of this research can be expected to be realized only long after its completion, and the beneficiaries of the research are largely unknown, virtually the only source of support for the field is the federal government. Research that leads to short-term and

easily foreseeable practical benefits will be, and should be, supported by industry, but it is only society as a whole that can realize the profit on an investment whose payoff is so diffuse and spread over so many years. This is certainly in keeping with the traditions of the federal government, which has even provided the support for many shorter-term research and development efforts because of uncertainty about the outcome or because the required initial investment was too large in relation to its resources to be risked by any one private institution. It is essential that the federal government continue to show the same foresight as in the past in regard to basic research even when the payoffs are many years away, because virtually all sectors of society will share in these benefits in the long run.

A projection of the funding required to make full use of the capability for elementary-particle physics that exists in the country is presented in Chapter 9. The consequences of other, less optimistic, funding patterns are also discussed there, with particular emphasis on the nature of the physics research that is likely to be omitted in each case.

Chapter 9, which is the culmination of this panel report, is preceded by chapters covering basic information on the field: Chapter 6 on Institutions and Their Relationships, Chapter 7 on Training and Manpower, and Chapter 8 on Facilities for the Future.

We must emphasize that none of the past achievements in elementary-particle physics would have been possible without the tremendous contributions of other areas of physics and engineering. Because this subfield is a natural descendant of nuclear physics and its predecessors, it has the benefit of the best traditions and style of those subfields in the exploration of the properties of matter and the natural laws. But it depends both for its concepts and techniques on many other fields too. And the severe demands for highly refined instrumentation have led to the use of methods and results taken from virtually every subfield of physics. Finally, the magnitude of the accelerator engineering problems, the engineering of ancillary equipment, and the magnitude of the data-analysis problem have called upon the most sophisticated engineering for their solution.

This dependence of elementary-particle physics on the other subfields illustrates the organic character of physics as a whole. Each part of the organism depends on the functioning of the whole, possibly particle physics more than most. But particle physics also plays an important role in keeping the organism alive, both by making demands on other subfields and by stimulating exchanges of ideas and people. All of physics is the beneficiary of discovery in any subfield of physics, and the opportunities for discovery are great in high-energy physics today because the successes of the past have raised so many cogent questions to which unexpected answers are the thing to be expected.

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Contents

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I

ELEMENTARY-PARTICLE PHYSICS

1 The Nature of Elementary-Particle Physics

1.1 UNANSWERED QUESTIONS AND UNKNOWN PHENOMENA

The nature and purposes of elementary-particle physics concern both the
discovery of new phenomena exhibited by matter (and other forms of
energy) under extreme conditions and the understanding of known
phenomena. The understanding that is wanting has to do with facts
ranging from some that are well known, even to the point of weighing
heavily in our everyday lives, to esoteric phenomena whose existence has
been discovered in the exploration of the subfield. The investigation of
the behavior of matter under extreme conditions is not only intended to
add to the agglomeration of information requiring explanation but also
becomes part of the larger body of knowledge, available to be called
upon when needed whether or not it has a bearing on any existing theory.
It may be needed simply as it stands, for example, in astrophysics where
matter is studied under extraordinary conditions, or in future technology,
which points to the handling of matter under more and more unusual
circumstances. It may be needed to determine the systematics of the data,
to provide for interpolations, extrapolations, and standardizations. And of
course, it is always needed to plan experiments providing the underpin-
nings for further progress toward understanding.
    The progress that has been made in this subfield has furnished remark-

able insights and baffling questions. Some of the questions concern facts that are so familiar that they are often taken for granted. For example, is there some underlying reason for the invariable quantization of electric charge in an elementary unit? The existence of such an elementary charge is essential to chemistry—and therefore to life—and its magnitude governs the quantitative chemical properties of matter. Or, why is the proton so much more massive than the electron? There would be no specific molecular structure, hence again no life, if this mass ratio were not so large. Someday our knowledge of the strong interactions of nuclear particles may make it possible for us to answer the latter question, and, of course, no one can guess what the ramifications of that success would be.

Our understanding of the elements making up the universe requires knowledge of the stability of atomic nuclei, knowledge that can be explained, at least qualitatively, in terms of our comprehension of the forces in nuclei and their structure. However, the existence of hydrogen, one of the most important of all elements, depends on the stability of its nucleus, the proton, which is not understood, and this is the more baffling since it appears to be a soluble problem. The closely related neutron is unstable against beta decay into a proton because the neutron mass is slightly greater than the proton mass; whereas if the mass difference had the opposite sign, the proton would be unstable against positron decay into the neutron. Yet almost any simple explanation that one can give of the difference between the neutron and proton masses would suggest this opposite sign. Such explanations depend on the difference in electromagnetic properties, and the electrostatic energy of the proton associated with its charge would make it the more massive. Even those simple magnetic effects that can be easily identified act in the same way. Apparently the explanation requires an insight into the electromagnetic structure of neutrons and protons at very small distances. This is just starting to be studied by using the scattering of very-high-energy electrons and muons (μ-mesons) from the nuclear particles.

The recent discovery of new phenomena and their tentative explanations have left a trail of additional tantalizing questions, which serve as foci for much of the activity in this field. Such questions are

Is there a single fundamental building block of matter such as the quark in terms of which the observed particles can be understood?

Do elementary particles of enormous mass exist in relatively stable states?

Are there unforeseen reactions between particles capable of a much larger energy release than any seen so far?

What are the connections between the three known classes of interaction between particles: strong, electromagnetic, and weak?

How does gravitation relate to the elementary particles?

Will our present conceptions of space–time break down when applied to regions of smaller and smaller dimension?

Why is the weak interaction apparently dependent on the direction of flow of time? Are there other interactions that have this property?

Do heavy electrons besides the muon exist?

Why does the muon exist?

There are many more such questions, and Chapter 2 describes how some of them have arisen, some of the clues that have been discovered recently, and some of the unexpected properties of matter that have been revealed by the extreme treatment of the particles required for this exploration.

1.2 THE SEARCH FOR SIMPLE LAWS AND FOR BASIC CONSTITUENTS OF MATTER

It is the simplicity of natural laws that provides them with their great power. From an overall point of view, the laws of classical physics—those dealing with macroscopic phenomena—are simple. The fact that they can be described in terms of so few categories, such as mechanics, thermodynamics, electrodynamics, optics, is an indication of this. The simplicity of complex systems is usually revealed by taking them apart and reducing them to their basic constituents. Thus, in the study of matter, one attempts to isolate the smallest constituent, the atom, and study its properties and its constituents. In this way, a certain simplicity shows up in terms of a category of laws requiring the concepts of quantum mechanics and describing the motions of the parts of the atom, the electrons, and the nucleus. Deeper investigation involves the reduction of the nucleus to its constituent particles, the proton and neutron, and so on.

Thus the laws of nature at the molecular and atomic level have been found to have a beautiful mathematical form; they have opened up an entire realm of understanding of nature and have, at the same time, justified the physicist's faith in elegant simplicity. Similarly, penetration into the nucleus of the atom has led to considerable understanding of nuclear structure, which is of immense value for both scientific and technical purposes. But it has also led to deeper questions concerning the nature of the nuclear particles, the neutron and proton, and the forces acting between them. The search for the answer to these questions has required a further penetration into the structure of neutron and proton, but the ultimate simplicity that we seek in regard to the laws governing these subnuclear phenomena still eludes us.

Each generation of physicists has hoped to discover ultimate laws, and each has been surprised to find that more questions were raised with every new penetration. This experience leads us to be cautious in our expectations. Will the next penetration yield more answers than questions? We simply do not know. Will it be necessary to take one more step, penetrating more deeply? We surmise that it will. Will there ever be an end? If so, how will we know when we have reached it? We *might* arrive at a closed form of the laws of physics. Or at the other extreme, deeper penetration *might* reveal fewer and fewer phenomena suggestive of ultimate simplification, leaving the subject open-ended. But as long as the field continues to be rich in new phenomena, physicists will bend all their ingenuity to finding methods to make still further penetration possible.

The effort required for elementary-particle research is large when measured in terms of intellectual difficulty, manpower, and cost. The magnitude of the effort tends to slow the pace; but we must be careful not to let the pace be greatly slowed, since the rate to maintain progress must be such that ideas can be generated and tested within the interval of scientific productivity of a single man's career. Otherwise, the sense of direction and inspiration of the work may be lost.

1.3 THE NEED FOR HIGH ENERGY

The search for simple laws and the basic constituents of matter has always required the development of new tools and techniques in addition to those that were readily available at the time. In particular, methods for subjecting matter to more and more severe conditions had to be developed. The severe conditions might refer to high electric fields, high pressures, high temperatures, and so on; but ultimately, for the investigation of the constituents of matter the most important parameter is the relative energies of the particles.

The breakup of an atom of small nuclear charge requires an energy of a few electron volts (eV), while many thousands of electron volts (keV) are required to remove inner electrons from atoms with large atomic numbers. The breakup of a nucleus requires millions of electron volts (MeV), and the investigation of the structure of nuclear particles requires billions of electron volts (GeV).

Since the middle of the nineteenth century, exploration of the properties of matter and the unknown laws of physics has depended on the use of the highest available particle energies in an essential way. The first direct investigations of beams of elementary particles, the study of electrons in cathode rays, required energies of the order of several keV. The

quantitative understanding of atomic structure involved arc and spark discharges from a few hundred eV to several keV to produce detailed atomic spectra of outer electron shells, x rays up to 100 keV for the spectra of inner shells of heavy atoms, and alpha particles at energies of several MeV (from radioactive sources) to establish the Rutherford model of the atom. The study of nuclear structure required controlled sources of particles of energy in the range of several MeV, a need that started the development of the accelerator art. Information concerning the unstable nuclear particles that are produced in very-high-energy collisions of protons, neutrons, and electrons was first obtained from cosmic rays, an uncontrolled source of particles of energies ranging from some GeV up to extremely high energies (with rapidly decreasing intensity, however). Quantitative work on these phenomena has required the extension of the accelerator art into the multi-GeV region in order to provide properly controlled sources.

Thus, we see that present-day high-energy physics is the descendant of earlier generations of work that could properly have been called high-energy physics in their time. It carries on the tradition of direct attack on our ignorance of the laws of nature, a tradition that has been persistently successful and one that serves the interests and aspirations of all of physics. Of course, the outcome of such basic investigations is necessarily the least predictable aspect of physics. It should be kept in mind that this ignorance is two-sided; although one cannot predict what *will* be forthcoming, neither can one predict that an exciting phenomenon *will not* be forthcoming. If one judges by all of our past experience, important new discoveries certainly will be made.

1.4 THE NEED FOR HIGH PRECISION

It has frequently occurred in physics that a new discovery has been made when the only significant parameter that has been changed is the precision of the experiment (the increased precision usually being made possible by the introduction of a new method or technique). This general statement certainly holds true for elementary-particle physics, as well as for other fields, and it is important to realize that the fundamental properties of elementary particles must be studied by means of high-precision experiments at lower energies, as well as by going to very high energy. And every high-energy phenomenon must also be followed up by experiments of higher and higher precision, as the means become available.

There are many examples of contributions that have been made to the understanding of elementary-particle physics resulting from the precision

of the experiments. One is the observation of the Lamb shift, which made use of the measurement of very-low-energy phenomena with extreme precision. It provided the basis for confidence in relativistic quantum electrodynamics and lent credence to the "renormalization" of calculations involving unbounded (infinite) integrals. This and related high-precision experiments at low energy served both to extend the theory to relativistic energies and to place some limitations on modifications of the theory that are to be expected at very high energy. Related experiments of remarkably high precision have been carried out on muonium, the atom of ultimate simplicity made up of a positive muon and an electron. These yield very accurate values of some of the fundamental atomic constants, as well as additional tests of quantum electrodynamics. The production of the muons for such experiments requires intermediate energy accelerators, in the range of several hundred MeV.—accelerators that are the forerunners of the high-energy machines.

Another example in the domain of modern high-energy physics is the unexpected discovery of CP violation, which was the result of careful quantitative work and has opened entirely new vistas in physics. Although this work did not need the highest available energy, it did require an accelerator of high enough energy and intensity to produce a sufficiently intense beam of neutral K mesons so that their decay could be observed with reasonable statistical accuracy.

Careful quantitative experiments over the entire width of the high-energy spectrum have been and continue to be required for the discovery of the multitude of resonant states of hadrons (with masses in the range of 1 to 2 GeV/c^2). These resonant states, which also may be interpreted as single (unstable) particle states, provide most of the available information about the symmetries underlying the structure of strong interactions. Of course, none of the quantitative experiments at high energy would have been possible without the earlier decision to provide accelerators with high enough energy and intensity to produce strange particles and resonances in abundance. A concomitant expectation is that the new accelerator at the National Accelerator Laboratory, and other devices for getting to very high energies, will make possible quantitative experiments that will play an increasing role in physics, and that these experiments will include measurements on unforeseen phenomena as well as the foreseeable measurements of known parameters.

It seems safe to assume that both the new phenomena that will be found and the new insights into the natural laws that will follow from the exploratory and quantitative work will be important to physics at the deepest levels of understanding and, ultimately, in the realm of application.

2 Status of Elementary-Particle Physics

2.1 EMERGENCE OF THE FIELD

The outlines of the field of particle physics as it exists today had already emerged in the 1930's. The burden of activity was in the field of nuclear physics, which produced a wealth of phenomenological information having not only its own intrinsic value but also calling for interpretation, and further experimentation, in terms of the constituent particles. At this stage, the lepton family (limited to the electron, positron, and undetected neutrino) and the hadron* family (limited to the neutron and proton) were recognized as distinct in their properties. The electrical force that bound electrons together with atomic nuclei to form atoms and molecules was reasonably understood, as was the beautiful and subtle quantum mechanics, which determined particle motions and stability of atoms. The existence of the powerful forces between hadrons, the strong interactions responsible for the existence of nuclei, was comprehended in general terms, and even its rudimentary properties, especially its high degree of symmetry (isotopic spin invariance), were discerned. Besides electro- magnetism (and, of course, gravitation) the other known link between lepton and hadron families came from the instability of nuclei against beta decay, the very slow decay by emission of electron or positron, and the mysterious, somewhat speculative neutrino. This phenomenon was evi- dence for the existence of weak interaction of much more feeble strength and clearly distinct from the strong and electromagnetic forces.

The picture outlined here led the physicist to ask fundamental and rather obvious questions concerning the ultimate relationship of the four families of particles (lepton, hadron, photon, and graviton) and of the four kinds of interaction (strong, weak, electromagnetic, and gravita- tional). And it led to further experimentation at higher energies aimed at understanding the small distance behavior of nuclear forces, penetrating more deeply into nuclear structure, and revealing whatever interesting phenomena might be lurking behind the barrier of available energy.

Since 1930, much has been learned about the lepton and hadron fami- lies and of the properties of the strong, weak, and especially the electro- magnetic interactions. But on the fundamental issues of the ultimate relationships between lepton and hadron and between strong and weak interaction, we remain profoundly ignorant. Our ignorance has been further deepened by the recent discovery of the phenomenon of CP viola-

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*Hadron is a general term introduced later (in the 1960's) for particles subject to interactions of nuclear strength.

19

tion. The CP puzzle in fact may signal the existence of a fifth interaction whose relationship to the other four is at present utterly incomprehensible. Or it may signal the failure of our space–time concepts at very small distances.

2.2 SOME PROBLEMS AND SPECULATIONS

The 40 years of progress in understanding, while not answering the basic questions to which we referred, have led to a much stronger conviction that such questions are not mere metaphysics, but have ultimate operational consequences. The lepton family and the hadron family possess strikingly similar properties, despite the major distinction that hadrons interact through the strong force. Perhaps the most accurately measured number in physics is the ratio of electron charge to proton charge, which is 1, to within one part in 10^{20}. Not only is the structure of the electromagnetic interactions of hadron and lepton extremely similar, but the same is true of the weak interactions. The present successful description of the weak interactions was in fact motivated by a lepton–hadron analogy. The ratio of the beta decay rate for the muon to the beta decay rate for the neutron is accounted for by interactions having the same strength to within a few percent; this strikingly demonstrates the existence of a deep relationship in weak interaction properties between lepton and hadron.

There are hints that the lepton–hadron similarities go even further. After the discovery of the "strange" hadrons called K, Λ, Σ, Ξ, there eventually followed the generalization of isotopic-spin symmetry to that described by the Lie group SU(3); the SU(3) symmetry, however, is not exact. In the lepton world* e, ν_e, ν_μ have approximately the same mass, but not exactly; the muon breaks the symmetry by about 100 MeV. This is the same order of magnitude as the SU(3) breaking; e.g., the difference in mass of Λ and nucleon is 170 MeV. The interaction that breaks the SU(3) symmetry of hadrons also breaks a more subtle strong symmetry—chiral symmetry. In the absence of muon mass, the leptons also would possess a chiral symmetry. Thus muon mass and strong SU(3) symmetry-breaking may well have a common origin. But there are difficulties with improving the analogy further. At present, we must regard the similarities as suggestive but far from conclusive.

However, the similarity of weak and electromagnetic properties of lepton and hadron families are themselves sufficient to lead the physicist

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*ν_e and ν_μ are the neutrinos associated with electron and muon, respectively.

to expect their ultimate unification. This expectation is quite like the conviction that Einstein held that electromagnetism and gravitation are manifestations of a single unified phenomenon. He labored many years to establish such a unified theory but failed, having insufficient clues, from experiment or elsewhere, to find a solution. Is it the same for the question of unification of lepton and hadron concepts? Must we mount a laborious and costly climb to yet higher and higher energy, only to find the answer still out of reach?

There is strong reason to believe the contrary. The basis for such a hope lies in the shortcomings of present weak-interaction theory. Conventional theory predicts that antineutrino–electron elastic scattering increases with energy, reaching resonance-like values at center-of-mass energies of the order of 1000 GeV. Thus, if no drastically new leptonic phenomenon occurs below that energy, the leptons themselves will interact strongly with each other through the "weak" force; at such energies there could be a strong lepton–hadron coupling as well.

There are several rather technical theoretical reasons why this option is unattractive. The most appealing alternative lies in the possible existence of the W-boson, a charged particle of mass small compared to 1000 GeV, exchanged in weak processes between the leptons or hadrons much as photons are exchanged in electromagnetic processes. For a W-mass small compared to 1000 GeV, the $\bar{\nu}_e$–e elastic scattering remains small at high energy. However, the amplitude for the process

$$e^+ + e^- \rightarrow W^+ + W^-$$

now grows too rapidly with energy (as calculated on the basis of either electromagnetic or weak production). A strong high-energy W^+-W^- interaction is the most reasonable way out of the dilemma. Thus, the W, were it to exist, could very possibly be one of a big family of strongly interacting hadron-like objects. It even has been proposed that W *is* a hadron, i.e., that it interacts strongly with the ordinary hadrons.

But it is clear that we have entered far into a speculative world. The example is only meant to hint at how rich such a new world of phenomena can be, and how it may be a link between the worlds of hadron and lepton. And some such link *must* exist at energies bounded above by 1000 GeV in the center of mass, an energy perhaps attainable in the next decade by intersecting storage rings. However, there are rather good arguments that the upper limit is below 100 GeV and perhaps as low as 30 GeV, the latter value of the order attainable at the National Accelerator Laboratory (NAL) without storage rings.

Thus, while the last decade has witnessed a great deal of elucidation of

the properties of known interactions and discovery of new excited hadron
levels (quite like the activity in nuclear or atomic physics), there is a fair
chance that the next decade will be one characterized by the breakdown
of our present picture of weak interactions. This would return the thrust
of particle physics toward revolution in our conception of the funda-
mental laws, instead of further refining the existing picture.

2.3   RECENT PROGRESS IN THE FIELD–SUMMARY

High-energy physics stands with a sense of great expectations at the
threshold of new phenomena and new opportunities to understand the
relationships and interactions among the elementary particles. It has
arrived at this point by observation of the phenomena exhibited by par-
ticles at high, intermediate, and low energies, and by accurate measure-
ments of these phenomena and of the known interactions among the
particles.

   This activity has been to a great extent organized according to the three
classes of interaction–strong, electromagnetic, and weak–because the
techniques used both to investigate the interactions and to use the inter-
actions to probe into structures are likely to be quite different for each
class. Therefore, it is appropriate to organize the description of the status
of the field in the same way, although we shall not overlook the existence
of important relationships between the phenomena falling into the dif-
ferent categories, relationships that, as we have seen, run beyond the
depth of our understanding.

2.3.1   *Electromagnetic Phenomena*

Since classical (macroscopic) electrodynamics is so well understood, it is
not too surprising that great progress has been made in its microscopic
application to particles in the form of relativistic quantum electro-
dynamics. However, since the invention of the subject in the late 1920's,
it has been known that many straightforward calculations lead to un-
pleasant infinite answers for the values of well-defined physical quantities.
In more recent years, it has been found possible to modify the calcula-
tions systematically by means of renormalization procedures that elimi-
nate the divergences, yielding very precise values of the physical quantities
that are in good agreement with experiment. It has been surmised that the
success of these procedures is due to the fact that the original theory is
somehow modified or "cut off" at high energies or very small distances in
such a way as to reduce the importance of the infinite terms. In the past

decade, intensive efforts have been made by means of high-precision experiments at low or intermediate energy and by means of experiments at high energy using charged leptons (to avoid the effects of strong interactions) to determine this point of breakdown of quantum electrodynamics, but without success. Present indications are that our concept of space as applied to quantum electrodynamics is valid down to distances of the order of 10^{-14} cm.

Both the validity of quantum electrodynamics and the small size of the electromagnetic interactions lead to the conclusion that electromagnetism provides an ideal means for probing the structure of hadrons. In particular, it has been possible to study the charge structure of the proton, and of the neutron to a lesser extent, by scattering very-high-energy-charged leptons (especially electrons) from them. This has led to some clear ideas about the electric charge and current distributions in the proton, implying that the proton is so highly structured that it probably should not be thought of as an elementary particle. Very recent results on the inelastic scattering of electrons (meaning that the electron looses a substantial amount of its energy on scattering) suggests that the proton may be made up of subunits. Furthermore, it has been found that there exist hadrons (ρ^0, ω, and ϕ mesons) of large mass that otherwise are very similar to the electromagnetic quanta (photons), and that energetic photons may be converted into these hadrons and then proceed to act as strongly interacting particles.

These are some of the more striking results that have been obtained in recent work with electromagnetic phenomena. There exists, in addition, a great mass of data, resulting from the enormous effort that has gone into this field. Some of it has been correlated or fitted into theoretical models, some has proved to be extremely useful as a tool for study of the strong interactions, and some simply has to be added to our store of information to be used as a basis for further work.

2.3.2 *Weak-Interaction Phenomena*

The study of weak interactions has been greatly enriched by the discovery of the strange particles. All of these particles appear to decay by means of a weak interaction similar to that responsible for the beta decay of nuclei and the muon (see Table I.1). This implies a much broader application of the weak interaction, because these strange particles not only decay into leptons but they also have important decay modes consisting of hadrons only. Nevertheless, it has been possible to account for much of the information and make successful predictions on the basis of a "universal" weak interaction, applicable to all particles if one takes into account the SU(3)

TABLE I.1   Elementary Weak Decay Processes

Particle	Charge State	Decay Mode	Particle	Charge State	Decay Mode	Particle	Charge State	Decay Mode
muon[a]	$\mu^\pm$	$e^\pm \nu_\mu \nu_e$	K-mesons (cont.)	$K^\pm$	$\pi^\pm \pi^+ \pi^- \gamma$	sigma (cont.)	Σ^+	$n\pi^+$
pion	$\pi^\pm$	$\mu^\pm \nu_\mu$		K_S^0	$\pi^+ \pi^-$			$p\gamma$
		$e^\pm \nu_e$			$\pi^0 \pi^0$			$n\pi^+\gamma$
		$\mu^\pm \nu_\mu \gamma$			$\pi^+ \pi^- \gamma$			$\Lambda e^+ \nu_e$
		$\pi^0 e^\pm \nu_e$			$\pi^+ \pi^- \pi^0$ (?)		$\Sigma^{0\,b}$	$\Lambda\gamma$
		$e^\pm \nu_e \gamma$		K_L^0	$3\pi^0$			$\Lambda e^+ e^-$
	$\pi^{0\,b}$	2γ			$\pi^+ \pi^- \pi^0$		Σ^-	$n\pi^-$
		$\gamma e^+ e^-$			$\pi^\pm \mu^\pm \nu_\mu$			$n e^- \nu_e$
		$e^+ e^- e^+ e^-$			$\pi^\pm e^\pm \nu_e$			$n\mu^- \nu_\mu$
K-mesons[c]	$K^\pm$	$\mu^\pm \nu_\mu$			$\pi^+ \pi^-$			$\Lambda e^- \nu$
		$\pi^\pm \pi^0$			$2\pi^0$			$n\pi^- \gamma$
		$\pi^\pm \pi^+ \pi^-$	neutron	n^0	$p e^- \nu_e$	cascade	Ξ^0	$\Lambda\pi^0$
		$\pi^\pm \pi^0 \pi^0$	lambda	Λ^0	$p\pi^-$		Ξ^-	$\Lambda\pi^-$
		$\mu^\pm \pi^0 \nu_\mu$			$n\pi^0$			$\Lambda e^- \nu_e$
		$e^\pm \pi^0 \nu_e$	sigma	Σ^+	$p e^- \nu_e$	omega minus	Ω^-	$\Xi^0 \pi^-$
		$\pi^\pm \pi^\pm \mu^\pm \nu_\mu$			$p \mu^- \nu_\mu$			$\Xi^- \pi^0$
		$e^\pm \nu_e$			$p\pi^0$			ΛK^-

[a] It is assumed throughout the table that ν_e is associated with electron decay and ν_μ with muon decay whether or not there is direct evidence. The symbol ν stands for neutrino or antineutrino, as required.
[b] The π^0 and Σ^0 modes are included for the sake of completeness, although they are due to electromagnetic rather than weak interactions.
[c] K_S^0 is the short-lived and K_L^0 the long-lived state of the K^0, $\bar{K}^0$ system.

symmetry of the hadrons suggested by strong interaction phenomena. This has suggested the sort of connection between leptons and hadrons discussed in Section 2.2.

A remarkable relationship between the weak interactions and the electromagnetic interactions also has been established. The weak interaction appears to be generated by a current, similar to the electric current, and this current is composed of two terms, a vector term and an axial vector term. That the former is essentially proportional to the electromagnetic current is now rather firmly established, at least for the nonstrange particles, and has important implications concerning the connections between the particles. Many interesting and important relationships concerning the axial vector current have also been discovered and more will be said about this later (see Section 2.9).

The weak interactions of the strange particles known as K-mesons were the origin of the notion that parity might not be conserved, a revolutionary notion that proved correct when tested in nuclear beta decay. Now the same particles have revealed that there are weak-interaction phenomena in which the symmetry known as CP is broken. The CP symmetry refers to the simultaneous interchange of every particle with its antiparticle and application of the parity operation, and it had been widely assumed to be the universal symmetry replacing the broken mirror symmetry denoted by P. A tremendous effort has gone into the study of this effect, and it is now rather well established that it implies a failure of time-reversal symmetry, the symmetry of the physical laws of motion with regard to the sense of flow of time. As we have already noted at the end of Section 2.2, it may imply a failure of our concepts at a very deep level.

The study of weak interactions by means of neutrino beams has made it possible to vary the parameters in a way that is impossible when experiments are limited to the decay phenomena. Experiments carried out so far have led to the remarkable fact that there exist two different neutrinos, and a lower limit has been placed on the mass of the W-boson, but otherwise they have only indicated that our general notions about low-energy neutrino dynamics are correct. More intense and more energetic beams of neutrinos shot into large vats of liquid hydrogen are still needed to obtain more quantitative information.

2.3.3 Strong-Interaction Phenomena

The earliest recognition of strong interactions came with the realization that strong forces or potentials between nucleons (neutrons and protons) were required to hold the atomic nucleus together against the repulsion exerted by electrostatic forces. Although, as in the case of electromagnetism and gravitation, the earliest manifestation was in terms of a force or potential between two particles, the underlying, primitive interaction is not a force but rather an interaction (as a term in the Hamiltonian), capable of causing emission and absorption of hadrons, either singly, as for mesons, or in particle–antiparticle pairs, as for baryons (nucleons and related particles). The same is true for electromagnetism, where the primitive interaction is responsible for the emission and absorption of photons, and for the weak interactions, where only the emission and absorption processes have been observed, because the corresponding forces between particles are so very weak.

Because of this possibility of emission and absorption, and because the strength is great, the strong interactions lead to myriad phenomena of

great complexity requiring an enormous amount of data for their analysis. Particles (hadrons) of many different kinds are produced in their inter- action at high energy. The role of virtual particles, i.e., those that are in the field surrounding a particle and can be converted to real particles if the necessary energy is supplied,* becomes very important. Methods have been developed to study the particles in their virtual state, as well as to produce real particles singly and multiply. These methods require high energy, and analysis of the results requires an understanding of the param- eters in terms compatible with the special theory of relativity (relativistic kinematics). Tremendous progress has been made in regard to producing great quantities of information, finding methods for treating the rela- tivistic kinematics and other underlying theoretical problems of a general nature, and correlating the information in such a way as to guide this very complex field toward some understanding in relatively simple terms. As we shall see, many questions remain unanswered, but the observed phe- nomena have already revealed many remarkably systematic and suggestive features.

The many reactions that occur at high energy between the elementary particles have been found to yield surprisingly well to analysis in terms of resonances between the particles, similar in some ways to the resonances of nuclear physics. The existence of a very large number of resonances (see Figures I.8 and I.9) may be interpreted as the existence of a large number of excited states of a given hadron, or, equivalently, each such state may be interpreted as another (unstable) particle. One may in fact question whether the lowest states of this system are in any way more fundamental than the excited states. The existence of such a large number of "elementary" particles then raises the question as to whether they are indeed elementary or whether they are different manifestations of some complex structure just as in the case of the chemical elements.

The classification of particles produced in strong reactions into the cate- gories of "strange" and "nonstrange" particles belonging to isotopic spin multiplets of a type familiar in nuclear physics has been followed with a further classification in terms of representations of the SU(3) group. This classification has made it possible to fit many of the particles and reso- nances into a pattern similar in some respects to the periodic table of chemical elements. It has also made it possible to make quantitative pre- dictions concerning masses and instabilities of hadrons and to guide the

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*Another way of expressing this is to note that, in accordance with the usual statement of the Heisenberg uncertainity principle, the energy of a system that lives for a very short time is very poorly determined. Therefore, a particle can live for a very short time in apparent violation of energy conservation, and it is then called "virtual."

development of the theory of weak interactions in the direction of universality.

The introduction of SU(3) symmetry has made it possible to correlate many phenomena and has led to suggestions for many new experiments. Its success has also led to the suggestion that the threefold symmetry implied by SU(3) may indicate that there are just three fundamental particles underlying the structure of hadrons. These proposed particles, usually called quarks, would then be *the* elementary particles, and the known hadrons would be composed from them. Although this suggestion may be a too literal interpretation of the SU(3) symmetry, it has been fairly successful in its most simpleminded form, based on the assumption that the hadrons are made up of a minimum number of quarks.

If the quarks exist in their simplest form, they would have a number of peculiar properties: their electric charges would be integral multiples of $e/3$, where e is the usual unit, the charge of the electron. They would be spin one-half particles but would appear to violate the Pauli principle. Their masses would be larger than the nucleon mass, in fact, possibly much larger, and the forces between them would be enormous, much greater than the strong interactions between hadrons.

If they exist, they should be stable in matter and detectable; in particular, the charge of one-third would provide a good signature and would lead to very interesting chemical properties. Very high energy may be needed to produce them, and cosmic rays might be the only existing source with high enough energy. However, over the lifetime of the earth, many could have been produced by the cosmic rays, and a variety of experiments have been carried out to search for them. No generally accepted evidence for the existence of quarks has resulted, but experiments continue, and a special effort can be expected whenever a new accelerator makes a new energy domain accessible.

In order to try to understand and predict the behavior of hadrons, the mathematical properties of reaction amplitudes have been studied in great detail, resulting in many new insights into relativistic reaction theory. Connections (crossing relations) have been found between the behavior of particle and antiparticle that lead to substantial results, such as making it possible to write dispersion relations relating the forward scattering amplitude at one energy to an integral in energy of measurable cross sections. And some of these relationships have been verified by experimental tests.

The behavior of the amplitudes and cross sections at very high energies are interesting because there is always the hope that the theory will simplify when the kinetic energy is so large compared to the potential that the effect of the latter can be treated as a small correction to the motion. This has led to theories of the asymptotic (very-high-energy) behavior of

amplitudes, to such ideas as the Pomeranchuk theorem, stating that the total cross section for a process involving a particle should become asymptotically equal to that of the antiparticle undergoing the same process.

A very rich source of ideas for both theories and experiments has arisen from the examination of the behavior of the amplitudes in their dependence on the orbital angular momentum treated as a continuous complex (i.e., having real and imaginary parts) variable (Regge pole theory). In particular, the simplest form of the Regge pole model led to very specific predictions about the asymptotic behavior of cross sections, especially in the diffraction (small-angle) region of scattering. This model has been confronted with many experiments with only moderate success, but the failures have led to broadening of the model and the generation of further ideas. The model in its various modifications still proves to be an important basis for the analysis of various phenomena and for further experimental ideas.

The sections to follow discuss in more detail all the physics outlined briefly in this section, but even the remainder of this chapter provides too little space to cover all aspects of the status of this field today.

2.4 LIMITS OF QUANTUM ELECTRODYNAMICS

Studies of electromagnetic interactions and their use as a probe are a natural extension of the earliest work in particle physics because they are of overriding importance in atomic and molecular physics. The theory of the interactions between electrons, μ-mesons (muons), and photons, namely quantum electrodynamics, is found to be valid even in the interpretation of the highest precision and highest energy measurements made to date. As noted earlier, in order to apply the theory, renormalization techniques must be used to eliminate divergences arising from certain unbounded integrals over an energy variable. The need for infinite renormalization implies an inadequacy of the theory, which should make itself apparent at high enough energies. Recent attempts to probe for this breakdown of the theory take on two forms. One is to measure such quantities as the g-factor of the electron or muon to very high precision. Here, one is looking for very small deviations from the theory arising from deviations in the very-high-energy contributions to the divergent integrals that have been eliminated in the renormalization procedures. These are low-energy experiments in the same general category as the measurement of the Lamb shift. The other method is the more direct measurement of cross sections for processes involving photons, electrons, positrons, and muons at high momentum and high momentum transfer.

Because there is the quantum-mechanical complementary relationship between momentum and distance (large momentum or momentum transfer corresponding to small distance), the results of tests of the theory can be expressed in terms of a length that may be construed as a measure of the structure of electron, muon, or photon, depending on the particular experiment. Alternatively, the length may be interpreted as a measure of the limits of validity of our space–time concepts, at least as they apply to electromagnetic phenomena. The most recent work has shown no deviation from quantum electrodynamics, indicating that our use of space–time is valid down to distances of the order of 10^{-14} cm and times of the order of 10^{-24} sec!

2.5 ELECTRODYNAMIC BEHAVIOR OF HADRONS

Photons, electrons, and muons interact with the other particles (hadrons) primarily through electromagnetism and therefore may be used to study the distributions of electric charges and currents associated with the particle structure. This offers a particularly attractive way to study the structure of hadrons both because the part of the theory concerned with the probe (quantum electrodynamics) can be trusted to a good level of precision and because the interaction with the hadronic system is relatively mild ($e^2/\hbar c = 1/137$) so that the structure of the system is not appreciably disturbed by the probe.

Experiments of this kind are usually performed with machines that accelerate electrons to high energy. The electrons themselves may be used as probes, or they may be converted to energetic photons, positrons, muons, or other particles. The highest energy machine of this type is the Stanford Linear Accelerator (20 GeV), but the other electron accelerators have also contributed enormously to progress in this area.

Since muons seem to behave in every respect like heavy electrons, they may also be used as electromagnetic probes, and some work of this kind has been carried out with the muon beams available at proton accelerators. It is expected that it will soon be possible to perform the muon experiments at much higher values of the momentum transfer than those available with electrons, because very high energy, and rather intense, beams of muons should be produced at the National Accelerator Laboratory. Such experiments may not only yield information on electromagnetic effects, they also will offer an opportunity to search again for a difference between muon and electron interactions.

Elastic scattering of electrons by protons, shown schematically in Figure I.1, is the prototype of experiments of this kind and the experi-

PROTON PROTON

$(\vec{0}, Mc^2)$ $(\vec{q}, q_0 + Mc^2)$

PHOTON

$(\vec{q}, q_0)$

$(\vec{p}, E)$ $(\vec{p}', E')$

ELECTRON ELECTRON

FIGURE I.1 Elastic scattering of an electron by a proton produced by exchange of a virtual photon of momentum $\vec{q} = \vec{p}' - \vec{p}$ and energy $q_0 = E' - E$ when the initial proton is at rest.
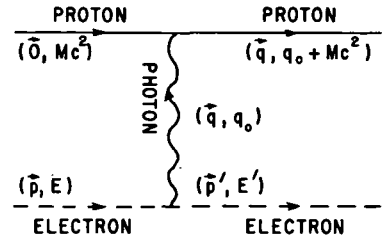
ment subject to the most straightforward analysis. The results of these experiments can be interpreted in terms of the charge and current densities within the proton since those are the sources of the electromagnetic field producing the scattering. However, the distributions are given most directly as functions of the momentum transferred to the proton (or, equivalently, that lost by the electron) rather than as functions in space. These functions of momentum transfer, known as form factors, are then the Fourier transforms of the spatial distributions.

Because of the highly relativistic character of the process at high energy, it is most convenient to measure the momentum transfer in a relativistically invariant way by using the square of the length q of the four-vector of transferred energy and momentum as the parameter. This is $q^2 = (E' - E)^2 c^{-2} - (\vec{p}' - \vec{p})^2$, where (E,p) and $(E',\vec{p}')$ are energy (relativistic, including the mass) and momentum of the electron before and after collision, respectively. The cross section for elastic scattering is measured at various energies as a function of q^2. The result can be expressed in terms of known factors depending on E and q, which describe the scattering by a point charge (Mott scattering), and two form factors depending only on q^2. These form factors are then the direct measure of the charge and electric current density in the proton giving a gauge of its internal structure.

Experiments have now been carried to quite large values of $|q^2|$ [up to $|q^2| = 25$ (GeV/c)^2], and the form factor measuring the charge density is found to be very small at large $|q^2|$, decreasing roughly as $|q^2|^{-2}$. This was a surprising result, because a truly elementary particle consisting of a single point charge would have a constant form factor at large $|q^2|$, the magnitude of the constant being a measure of the magnitude of the charge. Evidently the proton has so complex a structure that the original "bare" elementary proton serving as the focal point for generating the physical particle occurs in the physical system with a vanishing probability. Or possibly there is no such focal point.

A tantalizing extension of this insight into the structure of the proton has been given by recent experiments on inelastic scattering of electrons

by protons and neutrons. These experiments are analogous to the Franck-Hertz experiment of atomic physics; electrons are scattered with a loss of energy, leaving the hadron system consisting of one or more hadrons in a state of excitation, as indicated in Figure I.2. Clearly, there is a parameter in addition to q^2 involved here, namely, the energy of excitation of the hadronic system. Again, it is salutary to use relativistically invariant parameters, and the additional parameter is chosen to be the apparent mass of the hadronic system (rather than its energy). This is called the "missing" mass, since it is deduced from observation of the momentum and energy of the scattered electron without looking at the other particles. The cross sections measured as a function of the missing mass show maxima at the positions of known excited states of the nucleon, as one would expect. At high values of the momentum transfer and values of the missing mass above the resonance region, the ratio of the cross section to the Mott cross section, predicted for elastic scattering of an electron from a point charge, is nearly constant. Furthermore, the dependence on the missing mass is such that the cross-section ratio, or, more exactly, the generalized form factors, depends to a good approximation on only one parameter—the ratio of q^2 to the electron's energy loss.

A possible interpretation (but not necessarily the correct one) of this "deep" inelastic scattering is that the proton is made up of a collection of point charges, each one acting as an elastic scattering center, which, by recoiling elastically, leaves the whole system in an excited state. Although this interpretation is controversial and can only be verified by extensive measurements on the details of the inelastic process, it is possible that we are seeing a new level of the structure of matter.

2.6 PHOTONS AND VECTOR MESONS

A wealth of quantitative information has been gained recently on the production of mesons from nucleons by incident gamma rays: photopro-
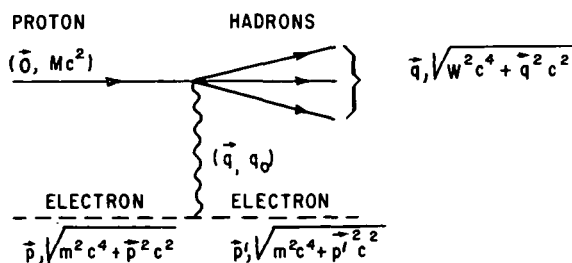
FIGURE I.2 *Inelastic scattering of an electron by a proton producing a hadron state of mass W.*

duction. The most characteristic result of these experiments is the simi-
larity of photon-induced reactions to those induced by incident π-mesons.
A striking example occurs in comparing the behavior of the total γp cross
section with the average pion cross section, $\frac{1}{2}(\sigma_{\pi^+ p} + \sigma_{\pi^- p})$. At almost all
energies they are in the ratio of $1/250$. This similarity appears to go much
deeper and has led to the concept of vector dominance.

Somewhat oversimplified, the idea is that the photon as it passes
through the vacuum spends some time dissociated into virtual particle–
antiparticle pairs. While the predominant such fluctuation is that of an
$e^+ e^-$ pair (because of its small mass), there is a small chance of finding the
fluctuation to be a hadron system, say a $\pi^+ \pi^-$ pair in a $J = 1$ state (since
the photon has unit angular momentum). If at the instant of collision the
photon is in such a fluctuation, the collision will be that of two hadron
systems; hence the qualitative similarity of the data on such processes
with those of pion-induced processes. Quantitatively $\sigma_\gamma =$ (probability of
finding hadron fluctuation in γ) $\times$ (cross section for the fluctuation).
This mechanism seems to be especially important at high energies, ac-
counting for at least 50 percent of the total cross section, if not more. The
reason is connected to the fact that the apparent lifetime (viewed from
the laboratory) of a given fluctuation increases with energy because of the
relativistic time dilation, thereby increasing the probability factor.

A most interesting example is the behavior of the total photon absorp-
tion cross section on nuclei as a function of atomic number A. Because of
the small electromagnetic coupling strength, the mean free path of a
photon in nuclear matter is hundreds of nuclear radii. Naively, then, one
would expect the cross section $\sigma_{\gamma A}$ to be directly proportional to A. How-
ever, if the reaction is dominated by the two-step quantum process

 (1) $\gamma \rightarrow$ hadron fluctuation
 (2) hadron fluctuation absorbed on nucleus

then the absorption cross section would be roughly proportional to the
nuclear surface area because of the small hadron mean free path in nu-
clear matter. Experimentally, the A-dependence is roughly $A^{0.9}$ about
halfway between $A^{1.0}$ and $A^{0.8}$, the latter being the dependence of the
high-energy neutron cross section on nuclei.

The hadronic fluctuation often scatters elastically from the nucleon or
nucleus (shadow scattering) picking up the necessary energy to convert
the virtual state into a real hadron state, that is, it is "liberated." The
most prominent such fluctuation is the $\pi\pi$ resonance called the ρ-meson,
which has the same quantum numbers as the photon. The photoproduc-

FIGURE I.3 Annihilation of a positron
by an electron to produce a photon that
is converted into a vector meson.
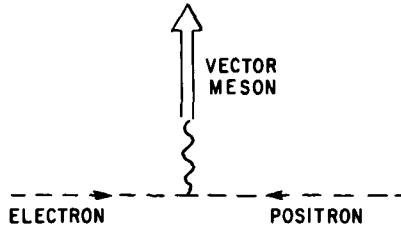
tion of the ρ from nucleons and nuclei shows all the features character-
istic of elastic scattering of a π-meson under similar conditions when the
energy of the photon is great enough that the difference in mass between
the photon (zero mass) and ρ (mass $= 765$ MeV/c^2) is not important.

In actuality, the conversion of a photon into any one of the vector
mesons, including not only the ρ but also the ω (mass $= 784$ MeV/c^2) and
the ϕ (mass $= 1019$ MeV/c^2), can take place only through the interme-
diate step of a virtual process because the mass of the photon is zero. For
example, in the photoproduction process, the photon may be scattered
from the nucleus into a virtual state having any value of the square of its
four-momentum, q^2, rather than* $q^2 = 0$, as required for a real photon.
When $q = m_\rho c$, the virtual photon may then transform into a real ρ-meson
with the same momentum and energy. This freedom to assign a mass to
the virtual photon is another way to express the well-known consequence
of the Heisenberg uncertainty principle that the energy is undetermined
for the short duration of the virtual process.

Another way in which virtual photons of finite mass may be produced
is in the process of electron–positron annihilation illustrated in Figure I.3.
Although electron–positron annihilation produces nonzero values of q^2,
it is not easy to attain large enough q^2 to produce a vector meson. The
electron–positron energy in the center-of-mass system must be equal to
$m_v c^2$; but, at the relativistic energies required here, most of the energy of
an incident positron goes into kinetic energy of the center of mass if the
target electron is at rest, and the remaining energy is much too small to
produce the meson unless the incident energy is enormous. This difficulty
is overcome if the electron and positron collide when both have high
momenta in opposite directions. In fact, if the momenta are equal and op-
posite, the center of mass is at rest and all the energy goes into the pro-
duction process.

*Note that if the momentum is $\vec{q}$ and the energy cq_0, the mass of a particle is
given by $m^2 c^2 = q_0{}^2 - \vec{q}^2 = q^2$ according to the usual Einstein relation.

The required condition can be attained by making use of electron–positron storage rings wherein energetic electrons and positrons are orbited (after injection into the ring from an accelerator) in opposite directions and steered onto a collision course. In the ensuing collision, a variety of particles may be produced. Figure I.4 shows the results for production of pairs of π-mesons as a function of q^2 in the neighborhood of $q = m_\rho c$. Since the ρ-meson is known to decay strongly into two pions, the peak at the ρ-mass is indicative of ρ-production as shown in Figure I.3, followed by decay of the ρ. The shape of the curve is determined by the natural width of the ρ-state, which is quite unstable and therefore has a large width.

2.7 ELECTRON–POSITRON STORAGE RINGS

The successful production of ρ-mesons is an indication of the power of the electron–positron storage ring for quantitative study of production of the vector mesons and other particles as well. In fact, very recent results with the electron–positron storage ring at Frascati indicate that substantial numbers (as many as six) of charged hadrons are produced in e^+e^- collisions at a total energy of 2 GeV, and that the rate is greater than anticipated. This pleasant surprise is exciting, both because it must be explained and because it provides a better-than-expected opportunity to
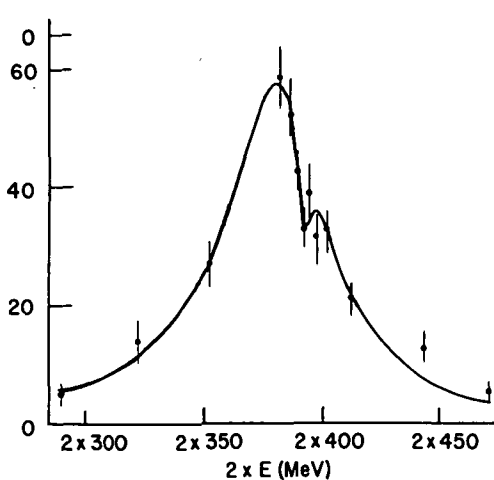
FIGURE I.4 Spectrum of events for electron–positron collision in Orsay storage ring producing two pions as a function of the square of the effective mass of the pion pair. The anomaly on the right-hand side is associated with interference between ρ° and ω° mesons.

study the electromagnetic interactions of hadrons. The most baffling aspect of these experiments concerns the reaction $e^+ + e^- \rightarrow \pi^+ + \pi^-$, which would appear to be the simplest hadronic process and, therefore, the best understood. The rate is determined by the electromagnetic form factor of the pion, which is related to the form factor of the nucleon. If the preliminary indication that the cross section is an order of magnitude larger than expected is verified, it means that we do not understand the behavior of the form factors for positive values of q^2.

The tremendous advantage of the storage ring is its use of all the available energy in a collision. Under more usual laboratory conditions, one of the colliding particles is nearly at rest; and it is then easy to show from the relativistic relationship between energy and momentum that the total energy available in the center-of-mass system is $E_{cm} = (2mE_{lab})^{1/2}$ for equal mass particles. Thus, the useful energy increases only as the square root of the laboratory energy, the kinetic energy of the center-of-mass motion accounting for the balance. To produce a vector meson by a positron collision with an electron at rest would require a positron energy of more than 500 GeV!

The storage rings also provide a powerful tool for studying quantum electrodynamics and many of the related phenomena for virtual photons of very high mass. Indeed, as we shall see in the next section, the process of annihilation is analogous and complementary to the deep inelastic electron scattering.

Perhaps this device will offer the only opportunity to discover directly the physical meaning of the infinite renormalization of the theory.

2.8 SOME FUNDAMENTAL CONNECTIONS BETWEEN ELECTRO-MAGNETIC PHENOMENA

One of the important advances in our understanding of physics has been the recognition that there are close connections between processes like electron–positron annihilation and elastic electron scattering and also between photoproduction and the inelastic electron scattering. In either elastic or inelastic scattering one is dealing with virtual photons, as indicated in Figures 1.1 and 1.2. However, it is not difficult to see (by looking just at the electron momentum transfer), that q^2 is negative (i.e., spacelike in the language of special relativity) in either of these figures. Therefore, the corresponding mass would be imaginary, implying that no real vector mesons could intervene in the process, although virtual mesons can.

It should be clear that when the final-state hadrons shown schematically

in Figure I.2 consist of a nucleon and a single vector meson (electroproduction of the meson) the process corresponds to photoproduction of the meson by a virtual photon. It can also be viewed as the scattering of the Lorentz contracted Coulomb field of the fast-moving lepton, since this field contains many (virtual) photons which spend part of their time as virtual hadronic fluctuations. How the character of such electroproduction processes changes with increasing imaginary mass is as yet unexplored, but it will be an important guide to the correctness and detailed nature of these "vector-dominant" processes. At very large mass, they become part of the deep inelastic phenomenon discussed in Section 2.5, a phenomenon apparently very sensitive to what goes on in small regions of space–time.

The connection between electron scattering and electron–positron annihilation insofar as the dependence on the virtual photon four momentum is concerned can be seen, for example, by examination of Figure I.3. Here, we find $q^2 = (E' + E)^2 c^2 - (\vec{p}' + \vec{p})^2$, which differs from the value of q^2 for electron scattering (Figure I.1 or I.2) simply in that the sign of the momentum *and* energy of an electron have been changed in converting it to a positron. Thus, an outgoing electron has been converted to an incoming positron, and the corresponding values of q^2 have changed from negative (spacelike) to positive (timelike). Another way to express this is to say that we have interchanged the "energy transfer" channel and the "momentum transfer" channel, a procedure about which more will be said in connection with strong interactions.

One consequence of these considerations is that we may define a form factor in such a way that, for positive values of q^2, one physical process (such as annihilation) is described, while for negative values another, apparently quite distinct, process is described by the *same* function. This makes it possible to bring to bear the powerful mathematical tools of analytic continuation of a function from one domain to another in order to relate these physical phenomena (see Section 2.20).

2.9 THE NATURE OF THE WEAK INTERACTIONS

The determination of the nature of the weak interactions depends on observations of their emission and absorption effects, the most common such effect being the beta-decay of radioactive nuclei. This radioactivity is produced by the interaction between a single nucleon and the electron–neutrino system. Although all such nuclear processes seem to be understandable in terms of a single interaction between elementary particles, there exist a great variety of elementary decay processes involving other

particles and therefore, apparently due to different weak interactions. These are indicated in Table I.1, a listing of the weak decays of elementary particles as they are now known (see also Table I.2 in Section 2.14). The neutron beta-decay is the one elementary process in the list relating directly to the interaction responsible for nuclear decay.

This apparent variety of weak interactions offers an opportunity to test our notions of simplicity in physics; will Nature admit of an anarchy of weak interactions, or is there some universal unifying principle? The order of magnitude of the interaction, some 10^{-9} times weaker than the electromagnetic interaction measured by $e^2/\hbar c = 1/137$, seems to be about the same for all processes, which speaks in favor of some degree of universality. Other evidence available to date also gives quantitative support to the concept of universality between the weak interactions of different elementary particles. However, the evidence is far from complete, and some of the most definitive tests have yet to be made. Also, there exist unifying relationships between certain aspects of the weak, electromagnetic, and strong interactions. Unfortunately, all of this impending elegance suffers from the fact that no natural place has been found so far in this unified picture (or outside it!) to account for the recently discovered CP violation.

Clearly the rich variety of phenomena associated with the many processes listed in Table I.1 offer many opportunities for study of weak interactions. The simplest process of all is the one involving only leptons (electrons, neutrinos, muons), namely, the decay of the muon. In this case, there are no extraneous influences due to strong interactions modifying the primitive weak interaction, as might be expected for hadron decay. Recent precise measurements of the muon decay spectrum seem to confirm the notion that the interaction has the same simple form that has been ascribed to the weak coupling of nucleons.

This form is based on an interaction generated by a current in a manner similar to the generation of electromagnetism by electric currents. The weak current is made up of the sum of a four-vector current density and an axial vector (pseudovector) current density. For the vector part, there is a remarkable connection with the corresponding electromagnetic current density. The weak vector current associated with all the nonstrange particles (nucleons, pions, vector mesons) appears to be essentially proportional to the corresponding electromagnetic current. Therefore, it satisfies the equation of continuity and there exists a weak "charge" that is conserved. It also follows that there are analogues in weak processes to magnetic phenomena such as magnetic dipole transitions. The existence of "weak magnetism" and the concept of a conserved vector current have recently been substantiated by careful measurements of beta-decay spectra of appropriate nuclei.

The axial vector current does not have an analogue in electromagnetism. In particular, it cannot satisfy an equation of continuity. Nevertheless, there is good evidence for a modified form of the conservation law (partially conserved axial current = PCAC), which has recently been used along with other general relationships (commutation relations) between the vector and axial vector currents to establish a striking connection between the constant measuring the weak interaction and the constant measuring the strong interactions, a connection that appears to be consistent with measurements of these constants.

The extension of the notion of universality throughout Table I.1 requires a framework within which the hadrons can be related to one another. In particular, a relationship between nonstrange particles and strange particles (K-mesons, hyperons) is needed. The SU(3) symmetry of the strong interactions, about which more will be said later, provides just this kind of interconnection between the hadrons. The assumption that the same symmetry group can be used to interconnect the weak currents associated with the hadrons has made it possible to formulate universality in a way that leads to relationships between the rates of decay of the various hadrons into certain modes.

Decisive tests of these relationships require careful quantitative measurements on the decay modes of the strange particles. Measurements having the desired precision have become possible in very recent years as the result of improvements in detector instrumentation and the increase in intensity of high-energy accelerators to the point that strange particles are produced in some abundance. However, these experiments are lengthy and difficult, because the particle lifetimes are short (for example, 10^{-10} sec) and many of the decay modes needing careful study are very rare. The results obtained up to the present time appear to be consistent with the universality concept, but much remains to be done. In particular, the more sensitive tests have still to be carried to completion.

The application of SU(3) symmetry to the weak currents offers an opportunity for generalization, which, in turn, suggests new conditions on strong interactions. These conditions arise from an extension of the idea of conserved or partially conserved currents to all the components of the weak current that are interconnected by the symmetry group, and they take the form of algebraic conditions (commutation relations) serving to generate an extended symmetry group. When applied to detailed models, they lead to a number of specific relationships between the weak, electromagnetic, and strong interaction phenomena. Some of these have been supported by experimental data, others not, and still others have not been tested. Much remains to be done to determine the limits of validity of the general principles suggested by the behavior of the weak currents and to

separate these questions from those dependent on the details of the models, which are primarily concerned with the strong processes.

2.10 VIOLATION OF CP INVARIANCE

The remarkable revelation that the symmetry (P) between left-handedness and right-handedness and the symmetry (C) between particle and anti-particle are violated came about through weak-interaction phenomena. It is evidently a universal property of the weak interactions that they are not invariant under the operation P of inversion of the coordinate system or C of charge conjugation, and it is only for the weak couplings that such a violation has been established. On the other hand, almost all weak inter-action processes seem to be consistent with invariance under the com-bined operation of C and P together, the exception being those involving the neutral K-mesons, for which a violation of CP invariance has been clearly established. Whether this is a special property of the weak inter-actions of K^0-mesons or whether it is a general property of either the weak or some other interaction that happens to be most readily observed by means of the K^0-meson phenomena has not been resolved.

The peculiarity of the neutral K-mesons that makes them particularly sensitive to questions concerning CP invariance is that the degenerate K^0 and $\bar{K}^0$ states (particle and antiparticle) are each capable of decaying into the same mode (such as two pions or three pions). This produces a (second-order weak) interaction between the states that removes the de-generacy and mixes their wavefunctions. The result is that they serve as a very sensitive interferometer showing interference between particle and antiparticle decays.

Such an interference phenomenon is illustrated in Figure I.5, which shows the time-dependence of the decay of K^0 mesons into two pions. The effect of interference can be seen by comparing the solid curve with the dotted curve, which is simply the noninterfering sum of two exponen-tial decay curves with lifetimes corresponding to the two characteristic decay rates of the neutral K-mesons. The way in which this interference relates to violation of CP invariance is illustrated by the broken curve representing the expected appearance of the phenomenon in the anti-world obtained by applying C and P to the world. The fact that there is a difference (just the sign of the interference term) shows that the world and antiworld are distinguishable. In a universe of antimatter which was otherwise equivalent to ours, an apparently identical experiment would be expected to yield the broken curve rather than the solid one.

The dashed line corresponds to the result expected if one began with
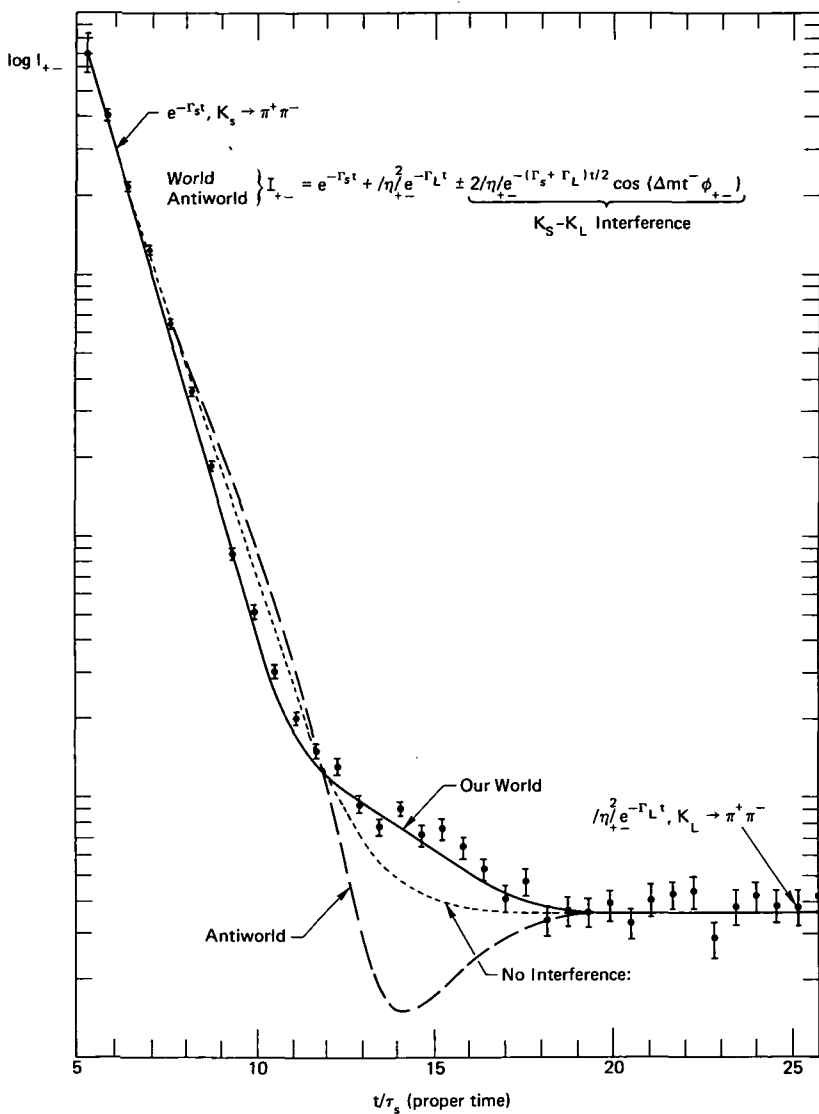
FIGURE I.5   Decay of K^0 into 2π as a function of time. The dashed curve shows how the data should appear in the antiworld obtained by replacing every particle by its antiparticle. The dotted curve corresponds to two noninterfering decay systems indicating how the data would appear if two uncorrelated particles of different lifetimes were being observed.

mesons at $T = 0$. Performing this experiment with $\overline{K}$ mesons, however, is not equivalent to performing the experiment in the antiuniverse. We cannot demonstrate that the preponderance of matter in and about our laboratory does not influence the experiment. One could well imagine that in an identical antiworld the presence of the antimatter could reverse the effect so that no difference in the antiworld experiment would be observed!

Additional, independent evidence for CP violation has been obtained by measuring the charge asymmetry in the leptonic decay modes of the residual long-lived component of a neutral K beam. Both the positive and negative leptonic charge states occur in these decay modes. The rates of decay into the two states of opposite charge were expected to be the same for a given mode if CP invariance were valid. The rates are found to be different, the magnitude of the difference having about the same measure as the CP-violating term in the 2π decay mode.

In order to determine the origins and nature of the CP-violating interaction, a variety of difficult experiments have been performed in the recent past and are continuing. These include measurements of the relative phase of the interfering amplitudes illustrated in Figure I.5 and the difference in masses (of about 10^{-6} eV/c^2 i.e., about one part in 10^{14}) between the two species of neutral K-meson. They also include corresponding phase measurements on the decay into two neutral pions (the above measurements refer to $\pi^+\pi^-$ pairs). Measurements on other decaying particles to determine whether the effect is limited to the K^0 and measurements on decay modes of the K^0 other than the 2π mode have produced no convincing evidence of CP violation in these processes.

Although these experiments have helped to narrow the field, as yet there has been no decisive result that can be used to identify the CP-violating interaction.

There are also other unanswered questions suggested by the observation of CP violation. For example, there is the possibility of a superweak interaction leading to decays in which the strangeness changes by two units ($\Delta S = 2$ transitions). The decay modes observed so far have $\Delta S = 0$ or 1, as can be seen from Table I.1. The expectation would be that the $\Delta S = 2$ transition would be very rare and therefore much more difficult to observe.

In addition to this possibility, there is still another rule for the decay of strange particles that has been the subject of many experiments—but without a convincing result. This is the $\Delta S = \Delta Q$ rule that the change in strangeness of the hadron in the transition has the same sign as the change in electric charge. This rule has important implications for the universality of weak interactions as well as for the interpretation of CP invariance,

and its limitations will probably be explored successfully in the near future.

Other experiments having a bearing on this question are those designed to investigate the selection rules on the strangeness in the strangeness-changing decay processes. Extension of the concept of weak currents to strangeness-changing currents leads very naturally to two selection rules for the strangeness S of the hadrons:

$$(1) \quad \Delta S = \pm 1, 0,$$
$$(2) \quad \Delta S = \Delta Q,$$

where ΔQ is the change in electric charge of the hadrons. Violations of these rules, which would occur as $\Delta S = \pm 2$ or $\Delta S = - \Delta Q$ transitions, would be of interest in themselves for the information they would convey about the concept of weak currents. Furthermore, a transition represent-ing a violation of either rule might be directly related to the origins of the CP violation.

Experiments that have been carried out to date do not provide con-vincing evidence of either type of violation, but much more remains to be done to establish the limits of validity of the selection rules.

2.11 IRREVERSIBILITY OF TIME AND CPT

One feature of the K^0 system that may turn out to be of vital importance to understanding the CP-violating phenomena is the fact that this system is very sensitive to second-order weak interaction effects that are nor-mally extremely small. Because the K^0 and $\overline{K}^0$ states are degenerate, the second-order self-energy term coupling the states has a large effect on them. The CP-violating phenomena that are observed are, at least in part, associated with this self-energy.

The self-energy involves divergent integrals over virtual processes simi-lar to those that are eliminated by renormalization in quantum electro-dynamics. Since the significant contributions to these integrals arise from virtual processes at high energy, any information that we are able to ob-tain about them relates to the behavior of weak interactions at very small distances. In particular, it could turn out that the observed violation of CP is a manifestation of a significant breakdown of the theory at such dis-tances, or even a breakdown of our concept of the infinitesimal behavior of space–time. For a direct test of this possibility, experiments at very high energy would appear to be required.

Having raised the question about the small distance behavior of the theory, we are led naturally to consideration of the CPT theorem. This theorem states that the requirements of (special) relativistic invariance on quantum field theory as it is presently known guarantees that all physical phenomena are invariant under the simultaneous transformation CP and time-reversal (T). An essential ingredient is that the theory refer to local fields, i.e., those that depend on a single space–time point. Nonlocal effects even over very small distances could undermine the basis of the theorem.

If the theorem is valid, then T invariance must be violated whenever CP is violated. This would mean that the dynamical behavior of a system would be different when going backward in time rather than forward in time. Attempts to observe T violation directly in weak decays have thus far led to negative results. However, the quantitative measurements on the K^0 phenomena indirectly indicate a failure of T, although they are not sufficient to test the validity of CPT. There is the possibility that CPT can be tested more directly by means of the weak interactions of the K^0 system, and experiments of this kind, which are still in the future, would appear to offer a particularly good opportunity to obtain some insight into the small distance behavior of particles and fields or the local structure of space–time.

2.12 NEUTRINO PHYSICS

The most direct approach to the high-energy, small-distance behavior of weak interactions is to carry out experiments with high-energy neutrino beams. The *only* known coupling of neutrinos to matter is the weak interaction, and this is the only known particle for which that is the case. One great advantage of neutrino experiments is that the energy and momentum transfer involved in the weak process can be large and can be varied. By contrast, observations on decay processes are limited to those values of energy and momentum determined through the conservation laws by the mass of the decaying particle.

By means of experiments on the scattering of neutrinos from protons, it should be possible to verify in detail the connections between the weak vector current and electromagnetic current; the same form factors should serve to describe both. Such experiments should also add a new dimension to the investigation of the deep inelastic phenomena described in Section 2.5. Unfortunately, neutrino experiments that have been performed have utilized dense targets made up of complex nuclei in order to obtain an

appreciable number of interactions. To then untangle the interaction with a single nucleon has turned out to be a formidable task, and only a qualitative idea of the behavior of the neutrino–nucleon system has been obtained. Neutrino experiments using pure hydrogen or deuterium targets are therefore essential, and they are the highest priority items on the agenda for the new generation of large hydrogen bubble chambers, which can serve both as target and detector (for example, the Argonne National Laboratory 12-ft chamber contains 1500 kg of liquid H_2).

The most exciting outcome of the neutrino experiments in complex nuclei has been the discovery that there are two distinct kinds of neutrino, one associated with the electron and the other associated with the muon. This possibility had been suggested earlier by indirect evidence and was unambiguously established by the direct observation of neutrino interactions in nuclei; the neutrino produced in association with the muon (in pion decay) produces only muons, no electrons, upon interacting with matter.

One of the most interesting questions pursued by means of the neutrino experiments concerns the possible existence of a particle playing the same role for weak interactions that is played by photons for electromagnetic interactions. The existence of such a particle, the intermediate vector boson, is suggested by the analogies between the two interactions. However, the intermediate boson, or W (for weak) boson, must be massive to account for the nearly local character of weak coupling. Direct evidence for the existence of this particle has not been obtained, but the fact that it has not been detected in neutrino experiments (and in some other, less direct, experiments) has led to an estimated lower limit on its mass of two or three times the mass of the proton.

As neutrino experiments are carried out at higher and higher energies, it is clear that they must eventually disagree with the theory as we know it. The successful theoretical interpretation of weak interaction phenomena is due to the fact that lowest-order perturbation theory can be used, because the interaction is so very weak. However, at increasing energy, the use of this approximation leads to the fundamental difficulty that it violates the conservation of probability, the calculated cross sections exceed the "unitarity limit." This implies that one must improve the approximation by taking into account higher-order terms in the perturbation theory. But there is no known systematic way to do this.

It has already been noted in Section 2.2 that even the existence of an intermediate vector boson would not in itself solve this problem. It can be hoped that neutrino experiments at high energy will produce some striking result that will help to guide the way around this great theoretical impasse.

2.13 THE NATURE OF THE STRONG-INTERACTION PROBLEM

The earliest attempts to describe nuclear forces started from the concept of static potentials acting between pairs of nucleons. Comparison of the consequences of this assumption with qualitative features of nuclear structure (such as the saturation of nuclear forces) quickly led to generalization from static potentials to exchange potentials, acting as operators in the Schrödinger equation and capable of exchanging electric charge or spin or both between nucleons.

The relationship between the potential and the primitive interaction may be described by the diagram of Figure I.6, which represents the interchange of a virtual pion between two nucleons. Comparison with Figure I.1 indicates an apparent analogy with electromagnetic interactions. In the nuclear case, the primitive interaction occurs at the vertex between nucleon and pion; a nucleon is capable of creating (radiating) a pion if it undergoes a sufficient change in momentum just as an electron radiates a photon when it undergoes acceleration.

If perturbation theory is applied to calculation of the nucleon–nucleon scattering described by Figure I.6, it is easily shown that it leads to equivalent static potentials having the desired qualitative properties. However, the quantitative features are not satisfactory in this elementary form because the magnitude of the coupling at the vertex turns out to be so large (some thousand times the corresponding $e^2/\hbar c$ of electromagnetism) that the perturbation approximation cannot be valid.

This is the prototype of the problem we face in dealing with strong interactions, which have been found to act not only between nucleons and pions but between members of the manifold of other particles referred to generically as hadrons. As in the case of the weak and electromagnetic interactions, the primitive strong interaction is capable of creating and annihilating particles; mesons can be created or annihilated singly as indicated in Figure I.6, and nucleons can be created or annihilated in antinucleon–nucleon pairs, as can be seen by manipulating Figure I.6 in the same manner used to obtain Figure I.3.
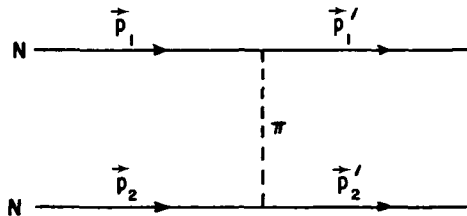
FIGURE I.6 Nucleon–nucleon scattering produced by exchange of a virtual pion.

The possibility of these creation and annihilation processes, when combined with the fact that the interaction is too strong to be susceptible to perturbation theory, means that the strong interaction phenomena are very complex from either the experimental or theoretical side. Experiments at high energy designed to penetrate the inner workings of the interaction lead to the production of an abundance of particles. In addition to having to deal with these multiparticle processes, the theories are confronted with the necessity to describe precisely the effects of large numbers of virtual particles and antiparticles that surround every hadron.

One possible way to overcome these complexities is to deal with hadronic matter having so much internal kinetic energy that the strong interactions do not carry much weight. For this reason, the study of reactions at very high center-of-mass energy is a promising way to obtain simple and understandable results. On this basis, great attention has been given to the details of hadron reactions at the highest available energies by both experimentalists and theorists. The results have led to some important insights, but they also seem to signify that available energies are not high enough to exhibit all the expected simplifications.

In general, the complexity cannot be overcome, it is a necessary and natural characteristic of hadronic matter. When subjected to violent impact, two hadrons produce many hadrons, in many varieties and patterns. These phenomena are observed and are being measured, in all of their richness, to the limits of power of the machines and detectors now available to us. It is remarkable that many simple systematic features of these phenomena have been discerned and have led to interpretations, theories, and further experiments with surprising successes. The purpose of the following sections is to outline some of these features, some of the ideas they have evoked, and some of their implications for the future.

2.14   HADRONS: PARTICLES AND RESONANCES

The particles that serve as the basic material for study of the strong interactions at the present time are classified in terms of those parameters and quantum numbers that are already familiar from atomic physics—such as charge, mass, spin and parity, the additional quantum numbers of isotopic spin introduced in nuclear physics—and that quantum number whose existence has been revealed only by the work in the GeV range, namely, strangeness, S. S plays a role in reactions between hadrons similar to that played by valence in chemical reactions. It is an additive quantum number (i.e., the value for two or more particles is the algebraic sum of the values for each) whose total value is the same on both sides of a strong

reaction. It is also conserved by electromagnetic interactions, but not by weak interactions, as can readily be seen by examination of Table I.1. Strangeness was introduced as a quantum number to account for the phenomenon of associated production of strange particles; the production of the particles identified as "strange" necessarily takes place in pairs (of opposite strangeness) in an interaction between "nonstrange" particles.

The nonstrange particles are by definition those that were more familiar at the time, nucleons and pions, and.they are assigned $S = 0$. Given the $S = 0$ assignment as a starting point, the "stranger" particles are assigned values $S = \pm 1, \pm 2$, etc., the sign being opposite for particle and antiparticle.

In Table I.2 are listed the stable hadrons and some of their charac-

TABLE I.2 "Stable" Hadrons

Name	Symbol	Electric Charge	Mag. of Mc (MeV)	Spin	Parity	Isotopic Spin	Strangeness
pion	$\pi^{\pm}$	$\pm e$	140	0	neg.	1	0
	π^0	0	135				
K-meson	K^+	$+e$	494	0	neg.	1/2	+1
	K^0	0	498				
$\bar{K}$-meson	K^-	$-e$	a	a	a	a	−1
	$\bar{K}^0$	0					
nucleon	p	$+e$	938.3	1/2	pos.	1/2	0
	n	0	939.6				
antinucleon	$\bar{p}$	$-e$	a	a	a	a	0
	$\bar{n}$	0					
lambda	Λ	0	1116	1/2	pos.	0	−1
antilambda	$\bar{\Lambda}$	0	a	a	a	a	+1
sigma	$\Sigma^{\pm}$	$\pm e$	1197	1/2	pos.	1	−1
	Σ^0	0	1192				
antisigma	$\bar{\Sigma}^{\pm}$	$\pm e$	a	a	a	a	+1
	$\bar{\Sigma}^0$	0					
cascade	Ξ^-	$-e$	1321	1/2	pos.	1/2	−2
	Ξ^0	0	1314				
anticascade	$\bar{\Xi}^+$	$+e$	a	a	a	a	+2
	$\bar{\Xi}^0$	0					
omega minus	Ω^-	$-e$	1672	1/2	pos.	0	−3
antiomega minus	$\bar{\Omega}^+$	$+e$	a	a	a	a	+3

a Masses and indicated quantum numbers are the same for particle and antiparticle of opposite charge.

teristics. The term "stable" is used here for those hadrons having a mass smaller than the sum of the masses of any two or more hadrons of the *same* total S into which they could dissociate. With the exception of the proton, all of these particles are actually unstable due to weak interactions, but each decay lifetime is long compared with the characteristic decay time for instability due to either electromagnetic or strong interactions. In fact, the lifetimes are long enough that many of the particles can be used as projectiles for carrying out experiments.

The particles are grouped according to whether they are baryons or mesons. This distinction may be signified in terms of another additive quantum number, the baryon number, B. For mesons $B = 0$, while baryons (nucleons and hyperons) have $B = +1$ and antibaryons $B = -1$. As far as is known, the total B of a system is absolutely conserved by all interactions. The antibaryons have not been tabulated, but it is assumed that there exists one corresponding to each type of baryon. Almost all of the antibaryons have been observed in experiments, the most recent being the anti-Ω^- particle, in December 1970. The bubble chamber event is shown in Figure I.7.

The existence of many of the stable hadrons was first established in cosmic-ray experiments, the exceptions being the π^0, $\Sigma^{0, -}$, Ξ^0, and Ω^-. However, particle accelerators in the multi-GeV range have made it possible to investigate systems of much shorter lifetime; in fact, with such short lifetime that they are usually categorized as "resonances" rather than as "particles." One of the great surprises in strong-interaction physics has been the proliferation of identifiable resonances. The number of resonances that have been identified has increased greatly in the past few years as the result of new experimental techniques for efficiently obtaining enormous amounts of data, improvements in data-processing systems, and development of methods of interpretation.

The resonances may be viewed as excited states of the stable particles listed in Table I.2. A recent summary of the resonances that are reasonably well established and identified leads to the mesonic excited states shown in Figure I.8. There are an additional large number of apparent zero strangeness excitations that have not been clearly identified. The excited baryon states having reasonably clear experimental properties are indicated in Figure I.9.

In both Figure I.8 and Figure I.9 the stable particle of Table I.2 corresponding to a given class of excitations is also indicated, but for some classes the lowest state has a large enough mass to be unstable against decay into particles of the same strangeness. It is apparent that the distinction between the stable particles and resonances is an accident, de-
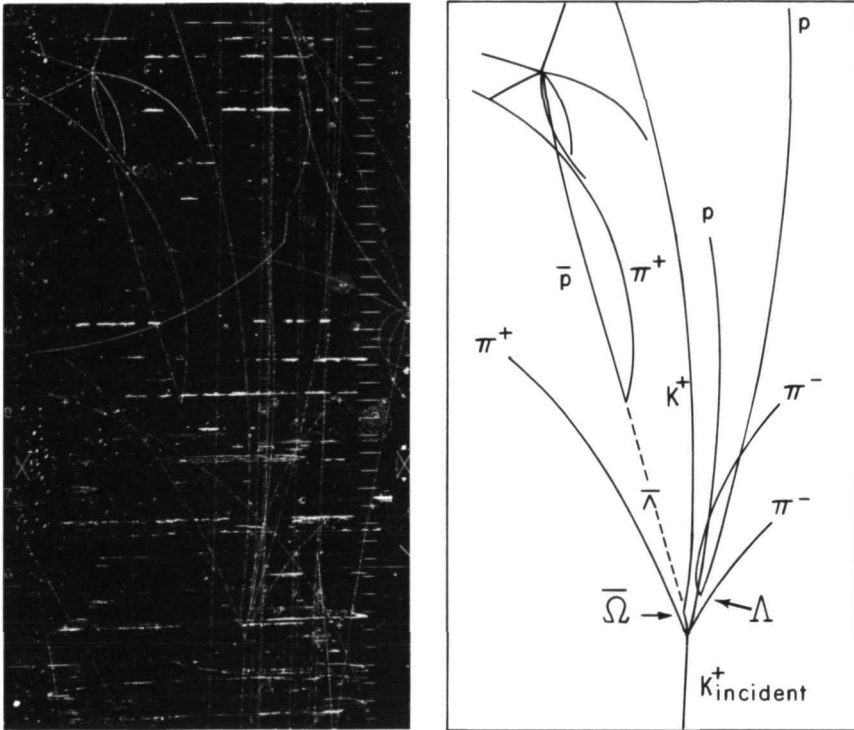
FIGURE I.7 Almost all of the antibaryons have been observed in experiments, the most recent being the antiomega particle ($\bar{\Omega}$), in 1970, by a group from the University of California, Berkeley. The bubble chamber event is shown in the above figure. The experiment involved was a study of the K^+d interaction at 12 GeV/c carried out in the 82-in. Stanford Linear Accelerator Center bubble chamber. The production reaction is $K^+d \rightarrow \bar{\Omega}\Lambda\Lambda p\pi^+\pi^-$, and the decay is $\bar{\Omega} \rightarrow \Lambda K^+$.

pending on what value the mass happens to have. In this respect, each of the resonances may be considered to be a fundamental particle, or, conversely, each of the particles may be considered to be a composite of hadrons.

2.15 THE SEARCH FOR RESONANCES

There are two general experimental methods for searching for resonances: "formation" experiments and "production" experiments. In the former,

E

MESONS AND MESON RESONANCES

GeV

1.6

$\bar{K}_V$ — E — K_V
$\bar{K}_A$ — D / F — A_2 — K_A
B

1.2

ϕ — A_1
X (η')

$\bar{K}^*$ — ω — K^*
ρ

0.8

$\bar{K}$ — η — K

0.4
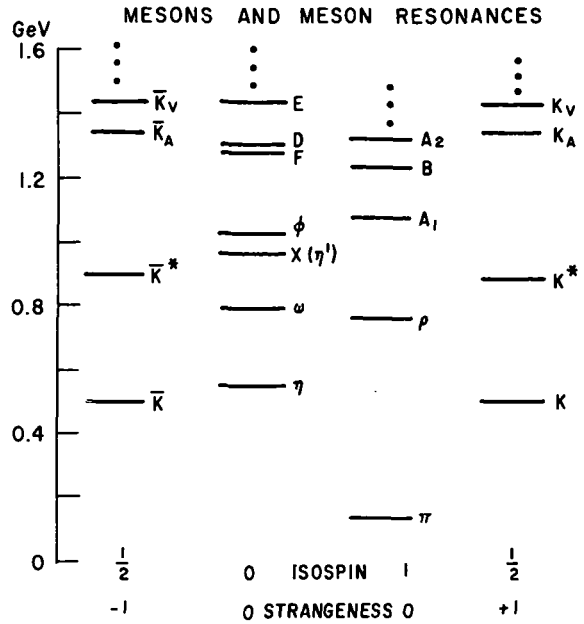
FIGURE I.8   Mesons and meson resonances that are reasonably well established. Many higher-lying states exist, but their detailed properties and quantum numbers are still uncertain.

π

0

	$\frac{1}{2}$	0 ISOSPIN 1	$\frac{1}{2}$
	-1	0 STRANGENESS 0	+1

the resonance state is formed by the addition of the incident particle, A, to the target particle, B, as in the reactions

$$A + B \rightarrow R \rightarrow C + D,$$

where R stands for the resonance. Thus, the intermediate state R is the analogue of Bohr's compound nucleus. The resonance may manifest itself as a "bump" in the cross section for $A + B \rightarrow C + D$ measured as a function of the incident energy, or it may be identified by showing by means of a phase-shift analysis that the phase of a specific angular momentum component of the reaction amplitude as a function of energy passes through 90° in an appropriate way. The expected behavior is illustrated in Figure I.10 under several different conditions. Figure I.11 shows the results of a recent analysis of pion–nucleon scattering data and indicates the position of some of the N and Δ resonances of Figure I.9, as well as some other possible resonances.

In production experiments, the resonance is produced in association with other particles. A typical reaction is

$$A + B \rightarrow C + R,$$

$$R \rightarrow D + E.$$
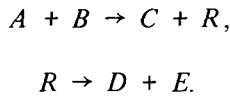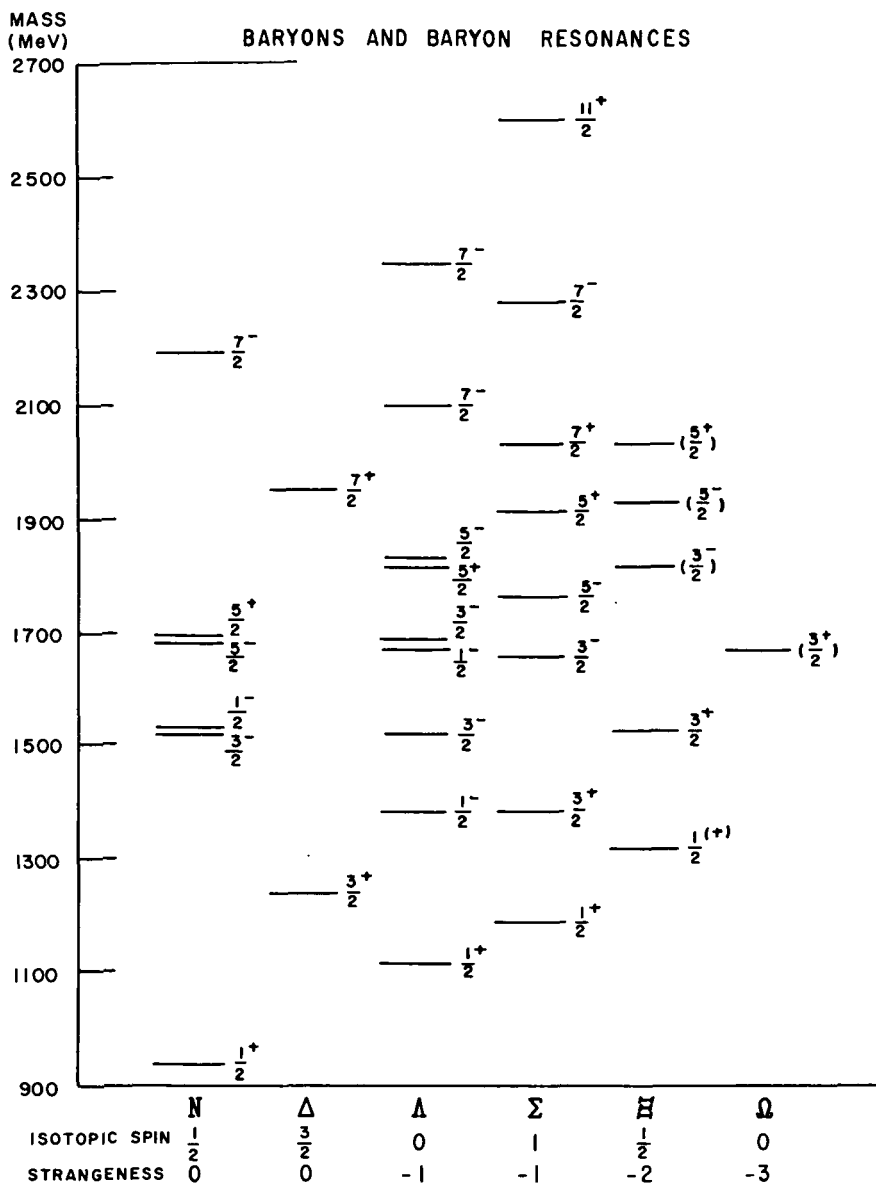
FIGURE I.9 Baryons and baryon resonances. (The numbers in parentheses are Mc^2 in GeV. Spin and parity are also indicated.)

FIGURE I.10   Argand plot of imaginary versus real part of the scattering amplitude $n_\varrho e^{2i\delta}\varrho - 1$ for orbital angular momentum ϱ as a function of energy. The phase shift is δ_ϱ. For a purely elastic resonance process in which the elastic width, Γ_{el}, equals the total width, Γ_{tot}, the curve would follow the outer (unit) circle in a counterclockwise direction. The other cases are (a) pure resonance (Breit-Wigner), $\Gamma_{el} > \frac{1}{2}\Gamma_{tot}$, (b) pure resonance, $\Gamma_{el} < \frac{1}{2}\Gamma_{tot}$, (c) resonance with attractive background, (d) resonance with repulsive background.
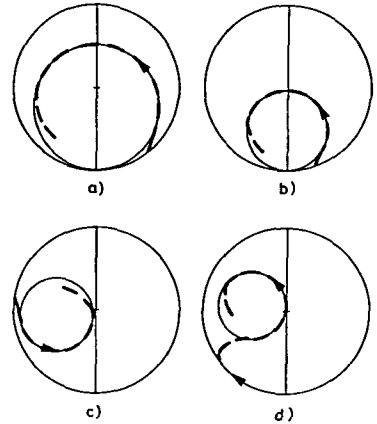
The identification may make use of measurements of the momenta of A, B, and C, which must fit the condition that they combine to form the appropriate momentum of a particle of mass M_R (with a finite width, the width of the resonance). This is the missing mass method mentioned earlier.

A more direct identification is possible when the momenta of the products C, D, and E of the reaction can be measured. Then one can calculate apparent masses for each pair of particles, for example, $M_{DE}^2 = (E_D + E_E)^2 c^{-4} - (\vec{P}_D + \vec{P}_E)^2 c^{-2}$. If the reaction is occurring through the intermediary resonance R, then most of the reaction events should have values of M_{DE} centered around M_R, the width of the distribution being measured by the width of the resonance. (Since it is unlikely that the reaction always occurs in this way, there will usually be a background of events that do not show this behavior.) An example is shown in Figure I.12, where the number of events in the reaction $K^+p \rightarrow K^+p\pi^+\pi^-$) is plotted against M $(K^+ \pi^-)$ showing the existence of the K^* (890) resonance.

Measurements of other details of the reactions, such as angular distributions and branching ratios, as well as comparison of various reactions leading to the same resonance, are required to determine the quantum numbers to be assigned to the resonance. The amount of data that must be processed for this purpose is large, and there are still a number of well-established "bumps" for which much remains to be done to complete the identification. Even some of the well-established resonances are still subject to controversy concerning the possibility that they have structure (see Section 2.19).
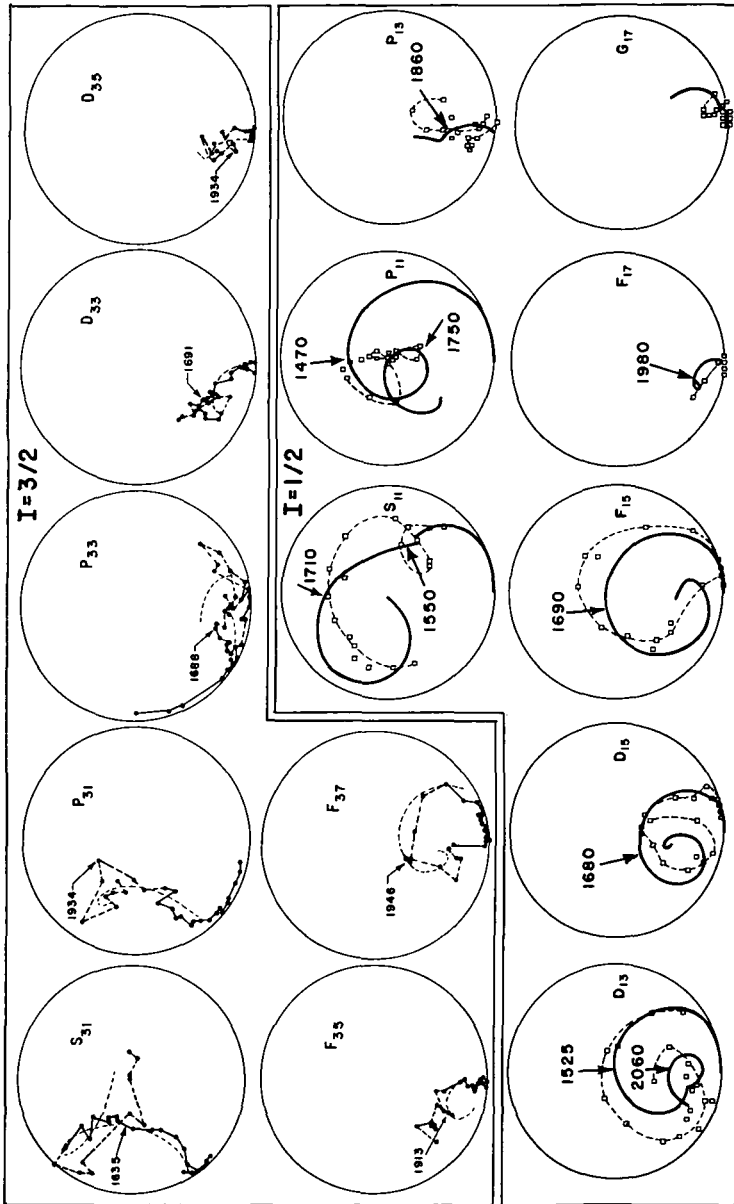
FIGURE I.11   Argand plot for recently determined pion–nucleon phase shifts. The spectroscopic notation $L_{2I,\,2J}$, where $L = S, P, D \ldots$ gives the orbital angular momentum. The arrows refer to apparent resonances at a value of the mass given in MeV.

53
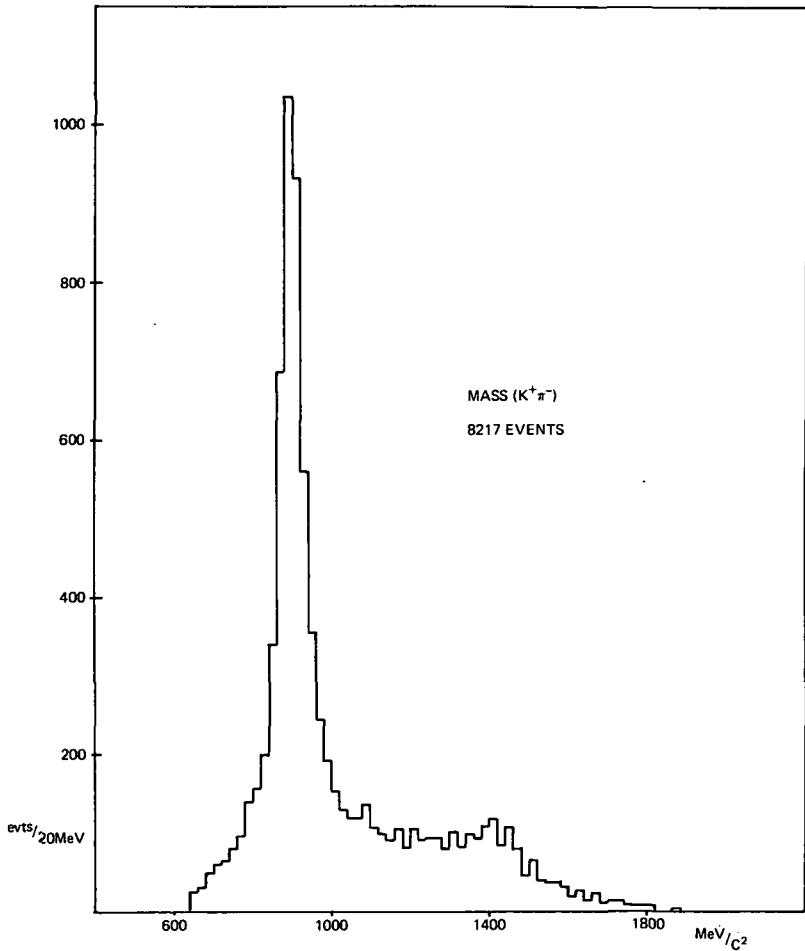
FIGURE I.12 Production of the resonance K*(890) in the reaction $K^+ + p \to K_0^*$ $+ \pi^* + p$, $K_0^* \to K^+ + \pi^-$.

The verification (or contradiction) of proposed models of the structure of hadrons (see Sections 2.17 and 2.18) may depend decisively on the sure identification and classification of the resonances. And an understanding of the dynamics requires much more information concerning mechanisms of their production and disintegration.

2.16 PRODUCTION OF RESONANCES BY DIFFRACTION

The production of resonance states by diffraction scattering is a process illustrating an interesting generalization of the typical diffraction phenomena. The more familiar elastic diffraction scattering of a particle by a nucleus is a typical wave phenomenon associated with the wavefunction of the incident particle. Just as in the case of the diffraction of light by a small sphere, the scattering is strongest in a small angular region in the forward direction, the magnitude of the angular width being determined by the ratio of the de Broglie wavelength to the radius of the target nucleus. Thus, a graph of the scattering as a function of angle shows a strong peak in the forward direction, the width of the peak decreasing with increasing momentum since that corresponds to decreasing wavelength. If the scattering angle is replaced by the transverse momentum transfer as the independent variable, the width of the diffraction peak would be independent of the incident momentum because the momentum transfer is proportional to the momentum at a given angle.

If a hadron has an excited (resonance) state having the same internal quantum numbers as the ground state, the primitive strong interactions will cause even the free particle to spend part of its time in the virtual excited state, just as the photon spends part of its time as a virtual vector meson (see Section 2.6). When the particle undergoes scattering by a nucleus, it may be "caught" in the virtual excited state, which can in the process be converted into the real excited state if the momentum transferred is such as to provide the required excitation energy. At high energy, this process may take place with a relatively small transfer of transverse momentum, and there is a resulting forward diffraction peak in the production of the excited particle.

Since the resonance state is unstable, it dissociates into two or more particles, and the entire process is called "diffraction dissociation." One observes the dissociation products moving in the direction of the diffraction peak and finds that the mass distribution associated with the total momentum and energy of the products is consistent with the mass and width of the resonance.

A very clear example occurs in the scattering of high-energy photons. Since the photon may be excited to a ρ^0-meson state, and the ρ^0 decays into two pions, the phenomenon is observed in the photoproduction of $\pi^+\pi^-$ pairs from nuclei. The diffraction scattering takes place without appreciable depolarization, that is, the polarization of the ρ^0 is found to be the same as the polarization of the original photon in the diffraction peak.

Diffraction dissociation via appropriate excited hadronic state also has

been observed in the scattering of high-energy pions, K mesons, and protons by nuclei. This provides a useful way to search for resonances having the same set of internal quantum numbers as the incident particle.

2.17 THE SU(3) SYMMETRY OF STRONG INTERACTIONS

The notion of an internal symmetry is familiar in nuclear physics, where it was introduced to determine the consequences for nuclear structure of the assumption that nuclear forces do not distinguish between neutron and proton. For this purpose an internal quantum number, the isotopic spin, was used to label the two states of the nucleon, the proton state and the neutron state. The analogy with the spin ½ dichotomy led very naturally to the introduction of two-component wavefunctions, and, just as in the case of the spin functions, these wavefunctions are subject to simple unitary transformations in two dimensions, abbreviated as SU(2). Then the statement that the nuclear energy (Hamiltonian) is invariant under these SU(2) transformations is another way of saying that neutrons and protons are subject to the same forces. This symmetry is clearly broken by electromagnetism since the proton carries charge and the neutron does not. There is also a small difference between the masses of neutron and proton, which is assumed to be electromagnetic in origin (although a completely satisfactory theory of the mass difference has not been given).

The pions fit nicely into this scheme as purveyors of nuclear forces. There are three charge states, $\pi^{\pm}$ and π^0, that can be assigned as members of an isotopic spin triplet (spin 1), and the SU(2) invariance (isotopic spin conservation) has been experimentally verified for interactions between pions and nucleons.

The discovery of the strange particles introduced the strangeness as an additional internal quantum number and led to the possibility of extending the concept of an internal symmetry group. However, in view of the fact that the differences in mass between nonstrange and strange particles are not small, it is clear that any such symmetry is not nearly so perfect as the isotopic spin symmetry, which is broken only by the rather weak electromagnetic interactions. Nevertheless, the assumption that strong interactions are approximately invariant under a simple unitary transformation in three dimensions [SU(3)] has been remarkably successful in accounting for and predicting many of the characteristics of the hadrons.

The approximation used requires the separation of the strong interaction energy into two terms, the larger one being invariant under SU(3) and the smaller having a specified way of transforming under SU(3). If the

smaller "symmetry breaking," term is neglected, the particle masses fall into degenerate multiplets, each multiplet corresponding to an irreducible representation of the group. The quantum numbers characterizing these multiplets are to be identified with isotopic spin and strangeness. Thus, there is an eightfold representation of SU(3), and the octet of mesons $\pi^{\pm}, \pi^{0}, \eta, K^{\pm}, K^{0}, \overline{K}^{0}$ are identified as one such multiplet, while the octet of baryons p, n, Λ, $\Sigma^{\pm}$, Σ^{0}, Ξ^{-}, Ξ^{0} are identified as another. The fact that the particles in the multiplets have different masses is a result of splitting of the degeneracy due to the symmetry-breaking interaction. Appropriate assumptions concerning the nature of the symmetry-breaking term, which is of the order of one tenth of the dominant (invariant) interaction, make it possible to predict successfully relationships between the mass intervals within a multiplet for the meson and baryon octets already mentioned.

It has not only been possible to fit the resonances shown in Figures I.8 and I.9 into SU(3) multiplets, but the interval rules have also served to indicate where to look for missing members of the multiplet. An outstanding success of the interval rules was the prediction of the mass of the Ω^{-}, which, when it was discovered, was found to have a mass within a few MeV of the predicted value.

In addition to predictions of the relative positions of members of the multiplets, the SU(3) symmetry leads to predictions of the branching ratios for the dissociation of the unstable resonances into the various accessible modes. These predictions have also met with considerable success. The important role of the SU(3) symmetry for unifying the description of weak interactions has already been indicated in Section 2.9. These and other important contributions of the SU(3) symmetry to the understanding of the particles and resonances make it seem clear that SU(3) plays a fundamental role in the description of nature. Whether a more basic understanding of this role is to be had remains to be determined.

2.18 QUARKS

The possibility that the hadrons are composed of more elementary particles, called "quarks," has been suggested as a basis for the explanation of the fundamental role of SU(3). The most straightforward suggestion is that the quarks form a triplet which is the multiplet corresponding to the three-dimensional representation of SU(3). Then there are two particles of zero strangeness and one having $S = -1$. The first two form an isotopic spin doublet having the strangeness (0) of the nucleon and are therefore designated by p (for protonlike) and n (for neutronlike), while

the third is an isotopic singlet having the strangeness (-1) of the Λ and is designated by λ. If these are the only three particles, the scheme requires that they be assigned fractional electric charge as follows:

$$p: Q = 2e/3,$$
$$n: Q = -e/3,$$
$$\lambda: Q = -e/3,$$

where e is the charge of the proton. The baryon number of each quark is $B = \frac{1}{3}$. The three antiquarks have $B = -\frac{1}{3}$ and opposite sign of charge and strangeness.

The octets of hadrons may be formed by combining quarks. For example, mesons are made up of quark–antiquark pairs. We note that there are nine such pairs. In addition to the meson octet, this includes a meson belonging to a singlet (identity) representation of SU(3), which may be identified with the X (958) meson of Figure I.8. Similarly, the baryons are formed by combining three quarks, antibaryons by combining three antiquarks.

One may look upon quarks as merely a useful and graphic device for presenting the results of SU(3). Representations of SU(3) formed by combining quarks may be reduced to the irreducible representations that are then assigned to appropriate particles. On the other hand, it is quite possible that the quarks are real particles of large mass bound together by such strong forces that the total masses of such particles as mesons and baryons are smaller than the quark mass. The latter possibility is subject to experimental test: one may search for particles of fractional charge and large mass produced in high-energy collisions. How high the energy of the collision must be depends directly on the mass of the quark, which is evidently too high for it to be produced in appreciable numbers at any existing accelerator. One of the exciting prospects is to see whether the energy at the National Accelerator Laboratory will be high enough. In the meantime, attempts to detect quarks produced in cosmic rays have not led to any convincing evidence for their existence.

If quarks exist, they will have many interesting properties, not the least of them being their ability to exist in matter without being absorbed and without the charge being neutralized (since it is fractional). This could have significant technological implications, which will be discussed further in Section 3.4.

Because the quark forces are so strong, it would be expected that many virtual quark–antiquark pairs would exist in mesons and nucleons. Nevertheless, the very simple model in which such virtual pairs are ignored has

been quite successful in accounting for meson and baryon resonances. In this model, the meson states are obtained by considering all the allowed two-body quark–antiquark states of different angular momenta. Similarly, the baryons are obtained by considering three-body states having all possible angular momentum quantum numbers.

Not only does this simpleminded model seem to lead to a reasonable assignment of many states, it also has the consequence that certain states should not occur. Particles having the quantum numbers of states that should not occur are referred to as "exotic" particles. They would include such particles as baryons with positive strangeness and mesons with double charge. The search for evidence for the existence of exotic particles has been pursued with great vigor in the past few years, and no completely convincing evidence for their existence has been uncovered up to this time.

Whether the substantial successes of the simple quark model mean that quarks are real remains to be seen. However, if it turns out that they do exist, our concept of the nature of matter will have undergone a great change. The quarks would take on the role of the fundamental building blocks of nuclear matter, and the interaction between quarks would be the primitive strong interaction. Presumably it would be a much stronger interaction than the one we have been calling the strong interaction, and this superstrong interaction would be very nearly invariant under SU(3).

Another interesting question concerning quarks is whether they would satisfy the Pauli exclusion principle (Fermi statistics) as would be expected for particles of spin ½. The assignment of allowed states depends, of course, on some assumption about the statistics, and ambiguities in regard to some of the assignments have led to the speculation that the quarks might satisfy parastatistics rather than Fermi statistics. This would allow a different symmetry class of states from the antisymmetric states required by Fermi statistics. The question has not been settled, but if it turns out that quarks do exist and do not satisfy Fermi statistics a fundamental readjustment of our ideas regarding the nature of particles and fields would be required.

2.19 PUZZLES IN MESON SPECTROSCOPY

There are still a number of intriguing basic questions concerning the meson spectrum, so that the orderly pattern shown in Figure I.8 may turn out to be a drastic, perhaps even misleading, oversimplification. Until such questions are clearly answered by a whole new generation of more

accurate experiments, there is a distinct possibility that some important
regularities in the meson spectrum are not yet even suspected. To give
a few examples:

1. Do auxiliary (daughter) states, such as the ϵ meson, which is "hidden" under the ρ meson, occur as commonly as expected?

2. Is the A_1 really a resonance, or is it a kinematical effect? Or is this
even a meaningful question, considering the implications of duality?

3. Are there low-lying narrow states, such as the δ (962), whose role
and classification may be outside the validity of most of the currently
accepted theoretical schemes?

4. Are there narrow heavy mesons above mass 2 GeV? Both experiment and theory need much more work here.

5. Is the doublet structure that was reported for the A_2 meson a correct or spurious result? Present evidence definitely favors the latter, but
more accurate data are needed.

2.20 RELATIVITY, ANALYTICITY, AND THE TWO-BODY REACTION

The simplest particle reactions available for the study of strong interactions are two-body reactions of the type

$$A + B \rightarrow C + D,$$
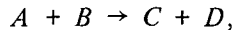
when A, B, C, and D represent particles, stable or unstable. The differential cross sections giving the angular distributions, polarizations (by using
polarized targets), and energy dependences of these reactions are measurable quantities that, for theoretical analysis, may be expressed in terms of
the absolute square of a reaction amplitude or S-matrix element.

Marked progress has been made in recent years in the understanding of
some of the general mathematical properties of these amplitudes. The
mathematical results have been supplemented by a number of speculations or assumptions about their properties, which have served as models
of strongly interacting systems, and these models have been subjected to
intense experimental investigations.

The reaction may be represented by the diagram shown in Figure
I.13(a). If, for simplicity, it is assumed that the particles are spinless, and
that no internal quantum numbers need be considered, the only variables
in the problem are the momenta $\vec{p}_A$, $\vec{p}_B$, $\vec{p}_C$, and $\vec{p}_D$ of the four particles.
The amplitude may be defined in such a way that it is relativistically in-

variant; therefore, it can depend only on invariant combinations of the momenta. There are two such independent combinations that may be taken to be the invariant energy variable

$$s = (E_A + E_B)^2 - c^2(\vec{p}_A + \vec{p}_B)^2$$

and the invariant momentum transfer variable

$$t = (E_A - E_C)^2 - c^2(\vec{p}_A - \vec{p}_C)^2.$$

The similarity between Figures I.13(a) and I.1 is noteworthy. Figure I.1 describes a special case of the type of reaction under consideration. As
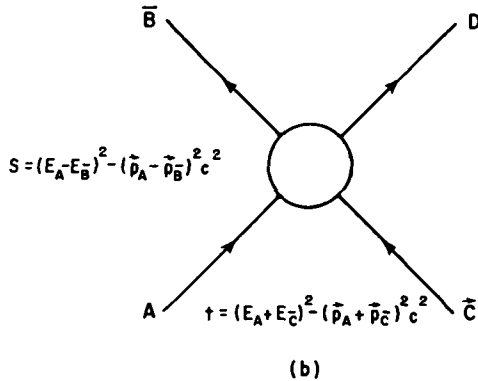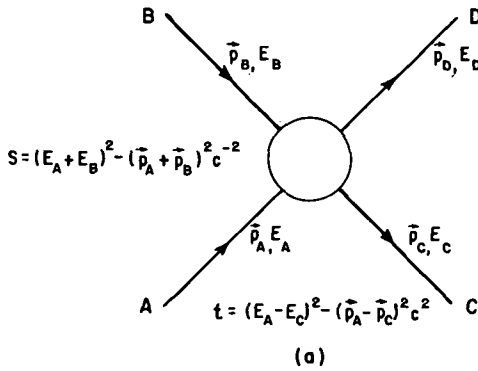
FIGURE I.13 Two-particle reaction amplitudes for reactions (a) $A + B \rightarrow C + D$ and (b) $A + \overline{C} \rightarrow \overline{B} + D$.

already noted in Section 2.8 for the special case, if the masses of particles A and C are the same and the masses of B and D are the same, s is a positive (timelike) quantity and t a negative (spacelike) quantity. When the masses are not equal, s is still positive $(s > (m_A + m_B)^2 \; c^4)$ while t covers the whole range of negative values and a range of positive values not overlapping s $(t < (m_A - m_C)^2 \; c^4)$. The amplitude for the reaction described by Figure I.13(a) is a function of the two variables s and t defined over this limited two-dimensional domain.

With each particle A, B, etc., there is associated an antiparticle $\overline{A}$, $\overline{B}$, etc., and absorption of a particle is closely related to production of an antiparticle. Thus, an incoming particle may be replaced by an outgoing antiparticle, and the original reaction described by Figure I.13(a) is thereby transformed into the "crossed" reaction

$$ A + \overline{C} \rightarrow \overline{B} + D, $$
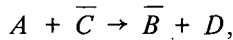
which is described by Figure I.13(b). Now the definitions of s and t are

$$ s = (E_A - E_{\overline{B}})^2 - c^2(\vec{p}_A - \vec{p}_{\overline{B}})^2, $$

$$ t = (E_A + E_{\overline{C}})^2 - c^2(\vec{p}_A + \vec{p}_{\overline{C}})^2, $$

and the energy variable is t, while the momentum transfer variable is s. It is easily seen that the ranges of s and t are now interchanged; t is positive and s ranges from values below the minimum of t to $-\infty$. Therefore, these definitions of s and t serve to extend the physical meaning of each variable over most of the real axis.

On the basis of general assumptions about the structure of the theory, and general principles such as causality, Lorentz invariance, and stability of the vacuum, it is found that the amplitudes for both reactions are given by a single function of s and t when they are extended over the domain of all complex values, and that this function is analytic in much of this domain. Certain singularities (poles and cuts) of the amplitude can also be isolated and identified; indeed in some cases all singularities are determined by the physical intermediate states permitted to occur in the inscrutable circles (known as "blobs") shown at the centers of the diagrams of Figure I.13. It is then possible to establish strong relationships (dispersion relations) between the reactions described by Figures I.13(a) and I.13(b). The first of these is referred to as the s-channel reaction, and the second as the t-channel reaction. More will be said about these connections later.

From the definitions of s and t it will be noted that particle is con-

verted to antiparticle, and therefore the *s*-channel reaction may be con-
verted to the *t*-channel reaction, by simply reversing the directions of the
appropriate arrows in Figure I.13, with the understanding that this re-
verses all four components of the energy-momentum four vector $(E, \vec{p})$.
In this sense, the extension of the domains of the two variables to cover
both situations amounts to a natural extension to include both directions
of the four vector.

Because of the nature of the equations governing the singularities, the
dispersion relations relate many other physical phenomena, but that sub-
ject is too technical to be discussed here.

2.21 THE POMERANCHUK THEOREM

In addition to the *s* and *t* channel, we can consider the channel for the
reaction $A + \bar{D} \rightarrow \bar{B} + C$, called the *u* channel. The variable $u = (E_A +$
$E_{\bar{D}})^2 - (\vec{p}_A + \vec{p}_{\bar{D}})^2 c^2$ is related to *s* and *t*, because there are only two in-
dependent kinematical variables describing a two-body collision process.
The relation is linear:

$$s + t + u = (m_A{}^2 + m_B{}^2 + m_C{}^2 + m_D{}^2)c^4 .$$

The amplitudes for the *s*-channel process $A + B \rightarrow C + D$ and the *u*-channel
process $A + \bar{D} \rightarrow \bar{B} + C$ are related by analytic continuation, for the reasons
discussed in Section 2.20.

For fixed *t*, the amplitude may be expressed as an analytic function
either of *s* or of *u*, and it describes the *s*-channel when *s* has physical
values $[s > (m_A + m_B)^2 c^4]$, precluding physical values for *u*, while the
converse choice of *u* and *s* serves to describe the *u* channel. An interesting
consequence of analyticity follows by considering the behavior at infinite
energy. It is reasonable to expect inelastic scattering (absorption) processes
to dominate over the elastic $(C = A, D = B)$ at high energy because of the
many open channels available there. If the process is completely absorp-
tive as $s \rightarrow \infty$ and as $u \rightarrow \infty$, the elastic scattering amplitude in the forward
direction $(t = 0)$ will be purely imaginary. Analyticity then demands that
the imaginary parts be equal, a result that is known as the Pomeranchuk
theorem.

The imaginary part of the forward elastic amplitude may be re-
expressed directly as the total cross section by means of the optical
theorem, a theorem that is an exact statement of a condition imposed by
unitarity. Therefore, it follows from the Pomeranchuk theorem that the
total cross section for the reaction of particle *B* with target *A* should equal

F

the cross section for the reaction of antiparticle $\overline{B}$ with target A. This is in quite good agreement with most measurements on the reactions of particles and antiparticles with protons. The total cross sections appear to be approaching equality as the energy increases up to energies available at the 70-GeV Serpukhov accelerator. Experiments at much higher energies will be of great interest in this connection.

It is possible to take advantage of the special properties of neutral K-mesons to compare amplitudes for the forward scattering of K^0 and $\overline{K}^0$ and thereby to produce a test of the equality of amplitudes rather than equality of total cross sections. As we have remarked in Section 2.10, the mesons K_S^0 and K_L^0 are in states that are coherent mixtures of the K^0 and $\overline{K}^0$ states. The scattering of a K_L^0 meson is therefore sensitive to the difference between K^0 and $\overline{K}^0$ scattering amplitudes. If they are different (as they are at low energy), the composition of the state changes when the K_L^0 scatters, and it emerges as a coherent combination of K_S^0 and K_L^0 states.

This production of a K_S^0 on scattering of the long-lived K_L^0 is referred to as "regeneration." There will be no regeneration if the K^0 and $\overline{K}^0$ amplitudes are equal, and it is evident that the amplitude for regeneration is given by the difference of K^0 and $\overline{K}^0$ amplitudes. Hence, a measurement of the forward regenerative amplitude gives a direct test of the equality of the K^0 and $\overline{K}^0$ amplitudes.

Preliminary results on the scattering of K_L^0 from hydrogen have been reported from Serpukhov but have not given a decisive indication of the behavior of the amplitude. That experiment is continuing, and its results, along with results to be obtained at considerably higher energy at the N A L, should make it possible to establish the high energy trend decisively.

2.22 POLES, PARTICLES, AND RESONANCES

The simplest kind of singularity that can occur in the scattering amplitude is a simple pole. If the amplitude has a simple pole, i.e., is proportional to $(s - s_0)^{-1}$ for a real value of s_0 lying between the physical values for the s and t channels, it can be shown that the wavefunction of the two-particle systems $A + B$ and $C + D$ is an exponentially decreasing function of the distance between the particles. Thus, this corresponds to a stable state, or a particle, of mass $m = \sqrt{s_0}/c^2$.

A value of s_0 corresponding to a physically accessible value in the s-channel would correspond to an unstable particle since its mass $(\sqrt{s_0}/c^2)$ would be greater than the sum of the masses of the two particles A and B. This instability introduces a negative imaginary part to s_0, and we may

write $s_0 = (m - i\Gamma/2)^2$. Then $(s - s_0)^{-1}$ is the relativistic form of the well-known Breit-Wigner amplitude for nuclear resonance reactions. The position of the resonance is determined by the real part, m, and its width is given by Γ.

This leads to a unified conception of the phenomenological meaning of a particle or resonance in terms of the mathematical properties of the amplitude. The diagram leading to a simple pole in the amplitude has the appearance of Figure I.14(a), where the single line represents the unstable particle resonance, or, in more old-fashioned language, the compound nucleus. The term "intermediate particle" will be used here for virtual stable
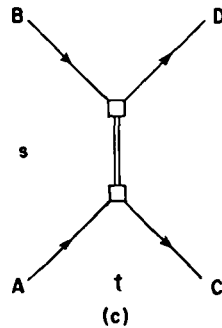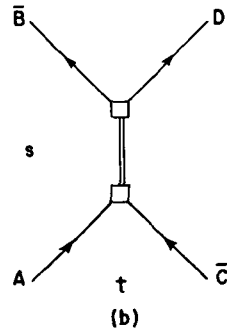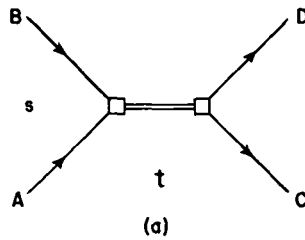
FIGURE I.14   Resonances in the s-channel (a) and in the t-channel (b) and (c).

particle, unstable particle, or resonance, since these are interchangeable in this description.

The situation described by Figure I.14(a) corresponds to the existence of an intermediate particle in the s-channel formed by combining particles A and B or C and D. If particles A and $\overline{C}$ combine to form a particle, then it will contribute as an intermediate particle in the t-channel reaction as shown in Figure I.14(b). This t-channel amplitude also makes its contribution to the s-channel reaction as shown in Figure I.14(c). In fact, if there is an intermediate particle in each channel, the amplitudes corresponding to Figures I.14(a) and I.14(c) must both be included.

The implications of Figure I.14(c) can be better understood by comparing it with Figure I.6 describing the origin of the nucleon–nucleon force. In general, the existence of an intermediate particle in the t-channel leads to forces between the particles in the s-channel. In the particular case of Figure I.6 the crossed (t) channel diagram describes nucleon–antinucleon annihilation into a (virtual) pion.

These considerations serve to illustrate the elegant formulation of some of the interrelationships between physical concepts that follow from very general considerations of the relativistic theory of reactions.

2.23 REGGE TRAJECTORIES

If the resonance in the t-channel shown in Figure I.14(b) has an angular momentum j, then the wavefunction describing the outgoing (spinless) particles has the orbital angular momentum quantum number $\ell = j$. The outgoing wave has an angular dependence proportional to the Legendre polynomial P_ℓ $(\cos \theta)$, where θ is the angle between the incoming particle A and the outgoing particle $\overline{B}$. If, for simplicity, the masses of the four particles A, B, C, D are taken to be equal (corresponding, for example, to the scattering of pions by pions) the relationship between the momentum transfer variable s (we are in the t-channel) and θ is

$$\cos \theta = 1 + s/2 |\vec{p}| c^2 ,$$

where $\vec{p}$ is the momentum of any one of the particles in the center-of-mass system. The resonance amplitude is proportional to

$$|\vec{p}|^{2\ell} P_\ell (\cos \theta)/t - m_\ell^2 c^4 ,$$

where m_ℓ is the mass of the intermediate particle (treated here as stable) having spin $j = \ell$.

One may now determine the contribution of this term in the amplitude to the s-channel, as illustrated in Figure I.14(c). In particular, it is of interest to inquire into the behavior at very high energy, which implies very large s. Since P_ℓ is a polynomial of degree ℓ, the amplitude will be proportional to s^ℓ for large s. Therefore, if the t-channel resonance has an angular momentum greater than $\ell = 1$, the cross section for the process will be a rapidly increasing function of energy. This result would be in contradiction with some very general principles, in particular, with the conservation of the flux of particles, which is referred to as the "unitarity condition" in quantum mechanics. This requires that the amplitude cannot grow as s^λ for large values of s unless $\lambda \leqslant 1$.

Rational behavior of the amplitude can be restored when the nature of hadrons as extended structured objects is taken into account. In particular, one expects hadrons to be able to rotate (and probably vibrate as well) and thus to be members of rotational bands just as in nuclear and molecular physics. The members of the rotational band have angular momenta J, which are functions of their mass m: $J = \alpha(m^2) = \alpha(s)$. The function α is called the Regge trajectory and can be defined for noninteger J as well. The key to solving the problem of amplitudes rising as S^J lies in summing together the amplitudes for exchange of *all* members of the rotational band; one sums at low energy where the sum converges then increases the energy s. One finds that the disastrous behavior s^J is replaced by $s^{\alpha(t)}$, where t is now negative instead of positive because the particle is being exchanged rather than produced. Although $\alpha(m^2)$ is often greater than 1, there is no longer a problem with the energy dependence of the cross sections provided $\alpha(t) < 1$ for $t < 0$.

If the reaction at high energy is dominated by this term in the amplitude, the contribution from the neighborhood of the pole will be proportional to $s^{\alpha(t)}$. For a reaction in the s-channel, the largest value of t is $t = 0$, hence, the unitarity condition requires $\alpha(t) \leqslant 1$ for $t \leqslant 0$. Illustrations of the supposed behavior of $\alpha(t)$ as t ranges through all real values corresponding to both the s and t channels are shown in Figure I.15. The assumption that α is a linear function of t is a simplification that has found some support in the experimental results.

This model of the reaction amplitude may be confronted with experimental data in at least two obvious ways: The determination of the angular momentum of resonances in the t-channel makes it possible to plot points corresponding to integer values of α for positive values of t in Figure I.15. By interpolation, a Regge trajectory is thereby determined for positive t.

For negative t, the differential cross section for large s is expected to be proportional to $s^{2\alpha(t)}$. This form clearly implies that there is a forward
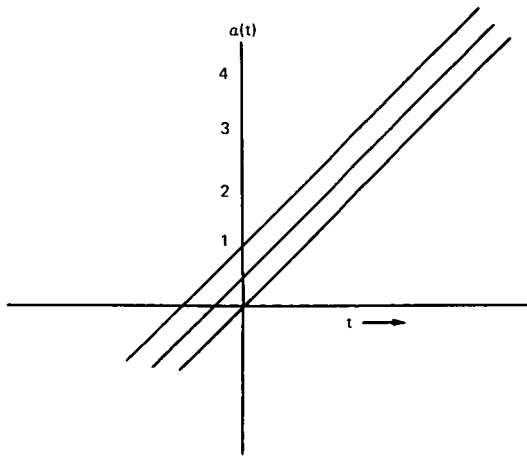
FIGURE I.15   Chew-Frautschi plots of the angular momentum against t.

diffractionlike peak in the angular distribution (t decreases from 0 as the angle increases) dropping off exponentially from the forward direction. The rate of dropoff of the forward peak increases for increasing values of s (shrinkage). In this respect, the result differs markedly from the typical diffraction patterns described in Section 2.16. We noted there that the width of the usual diffraction peak is independent of energy.

Experimentally measured shrinkage of the forward peak can be used to determine $\alpha(t)$ for negative t. In a few cases, experiments of this kind have given results in spectacularly good agreement with an extrapolation of those for positive t. In other cases, it has not even been possible to establish the existence of shrinkage, in fact, evidence for "antishrinkage" or "swelling" has been reported.

Application of both of these methods indicates that there are a number of more or less parallel Regge trajectories as shown in Figure I.15. Each trajectory corresponds to a fixed set of internal quantum numbers. In the approximation that SU(3) symmetry holds, each trajectory is expected to have a degeneracy corresponding to one of the representations of SU(3), since each multiplet of spin j is represented by the point $\alpha(t) = j$ in Figure I.15. Of course, this degeneracy is split by the symmetry-breaking interaction. One sees here how SU(3) not only established relationships within a multiplet, but, when combined with the Regge model, how it established relationships between multiplets of the same internal quantum numbers. Furthermore, the assumption that the trajectories are parallel relates multiplets of different quantum numbers.

The high-energy behavior of the *total* cross section for collision of the

particles A and B also offers an opportunity for comparison between theory and experiment. This is possible because, as we have seen, the total cross section is directly related to the amplitude for forward ($t = 0$) elastic scattering by the optical theorem. Since the amplitude for forward scattering is proportional to $s^{\alpha(0)}$, if the contributions of several Regge trajectories $\alpha_1(t)$, $\alpha_2(t)$, etc. must be included, it is the highest-lying trajectory that will give the largest contributions at very large s. The very highest power that can occur according to the unitarity condition specified earlier is $\alpha(0) = 1$. On the assumption that this does occur, the optical theorem leads to the conclusion that the total cross section becomes constant at very large s, a result that seems to be consistent with total cross section measurements at the highest available energy.

In recent years, there has been an enormous experimental effort put into testing these and related ideas. The models have met with varying success, depending on the particular reaction and the test applied. It is difficult to find a clean and decisive test of the model. Despite these difficulties, there is no question that the model has provided a framework within which an attempt at a quantitative interpretation of the very complex high-energy reactions can be made.

2.24 DUALITY

The primary motivation for the Regge model was concern with the asymptotic behavior of amplitudes. However, given the model, it is natural that every attempt should be made to apply it at lower energy, especially in view of our ignorance of the requirements of "asymptotia"; we do not know how high a value of s is high enough. In perturbation theory, for a reaction in the s-channel, one would add the amplitudes represented by Figures I.14(a) and I.14(c). However, if Figure I.14(c) represents the exchange of a Regge pole, the validity of this procedure is not so obvious since the Regge model must include contributions of many other diagrams (in the sense of perturbation theory) in some way, and it may already have encompassed the amplitude of Figure I.14(a). Therefore, when one analyzes data at intermediate energies where the s-channel resonance amplitudes are expected to be important, the questions arise. Should the known s-channel resonance contributions be added to the Regge contributions associated with all known resonances in the t-channel, or are they already included in the interpolation of Regge behavior at intermediate energies?

One can see how they might be included by considering the interaction

. between particles A and B. We recall that the effective potential acting between these particles is obtained in terms of exchanged particles by determining the scattering amplitude associated with diagrams of the type shown in Figure I.14(c). Given the short-range potential generated in this way, one can solve the Schrödinger equation to determine the bound states of A and B and the scattering of A by B. In general, the phase of the scattering amplitude will be an increasing function of energy that goes through the resonance values $(2n + \frac{1}{2})\pi$. If one takes into account the absorptive effects associated with inelastic channels, these values will correspond to true resonances. Thus, the higher-order effects (as represented by solving the Schrödinger equation) generated by the intermediate particles in the t-channel lead to the generation of stable states and resonances in the s-channel; they are not independent effects.

The notion that the introduction of s-channel resonances along with the Regge contributions in the t-channel is redundant is referred to as "duality"; the t-channel Regge poles take on the dual role including the s-channel resonances. Specific simple models have been invented that successfully demonstrate that a single amplitude can have these properties.

. One of the beauties of the relativistic treatment of the reaction amplitude is the interchangeability of the s- and t-channels. Thus, duality means not only that s-channel resonances are included in t-channel Regge poles but the converse—the t-channel resonances must be included in the s-channel Regge poles. This crossing symmetry clearly imposes self-consistency conditions on the amplitudes, which will be subject to test as the required detailed experimental information is obtained.

2.25 PROSPECTS FOR THE FUTURE

This brief discussion of some aspects of strong-interaction dynamics should serve to illustrate how simplifications can occur as the energy increases. This is one possible way in which the complexities of strong interactions will yield to analysis. There is much to be done to resolve the questions raised by the Regge model, and experiments at higher and higher energy may serve this purpose. Tests of the Pomeranchuk theorem are being carried out at the highest available proton energy for proton scattering at the intersecting storage rings at CERN and for other particles at Serpukhov, and similar tests are on the agenda for the NAL. The results of these experiments and other measurements at high energy, such as the measurement of the shrinkage of the diffraction peak, will provide detailed quantitative tests of these ideas and may suggest new approaches if they are needed.

There is no doubt that strong-interaction dynamics are so complicated that simple theoretical models may prove to be inadequate. However, the search for resonances serving to establish basic symmetries and the investigation of phenomena in the asymptotic energy region seem to offer the best hope for establishing a deeper understanding of strong interactions.

There are also more dramatic possibilities. If the search for quarks should bear fruit, the opportunity to study them directly will revolutionize the physics of strong interactions. It will probably mean, among other things, the existence of superstrong interactions binding quarks together. This may imply that the true asymptotic behavior of nuclear particles will be attained only at very much higher energies than those at which quantitative work is now envisioned.

The dramatic possibilities for the future are not limited to the field of strong interactions. The search for the W-boson, the hypothetical quantum of weak interactions, will be pursued vigorously in the next few years. High energy is needed for this because neutrino experiments have already indicated that the mass is too large for it to be produced at existing machines (with the possible exception of Serpukhov). The most obvious way to go about this is by means of neutrino experiments at high energy. These experiments are not limited to a W-boson search, they should also provide the quantitative information that reveals some of the underlying mysteries of the weak interactions: How are the weak interactions moderated at high energy so as to be consistent with the requirements of unitarity? What feature of the interactions carries the burden of the CP violation?

In electromagnetism, too, there are mysteries about the high-energy behavior. The success of renormalized quantum electrodynamics does not override the need to understand what it is that causes an apparently divergent theory to yield convergent results. Again, the difficulties with the theory at small distances suggests the existence of a massive particle, in this case, a photon of large mass, and one can hope that such a particle would be produced in the collision of particles having a high enough energy.

Another tantalizing feature of electromagnetism concerns the possible role of magnetic monopoles in physics. Although there is no evidence for their existence, particles carrying magnetic charge may be part of our universe. Presumably, if they do exist, their mass is so large that they can be produced only in very-high-energy collisions. Cosmic-ray investigations, including the study of cumulative effects in terrestrial and lunar samples, have led to negative results. However, the high-energy machines offer a different kind of opportunity to look for them, an opportunity that must

be pursued at the highest energies available because of the profound implications that the discovery of the monopoles would have for our understanding of physics.

Although it is clear that a straightforward and dramatic result bearing on these and many other questions of physics is more likely to occur as the available energy is increased, it should be kept in mind that many of the exciting discoveries establishing the very nature of the problems have been, and are being, made by careful quantitative work at the lower energies. An example is the discovery of the first particle resonances, which opened up the whole field. It is therefore of prime importance to continue to obtain data of great precision at lower energies in order to gain new insights into the basic phenomena.

In trying to assess what the future holds we have been guided by ideas generated by past experience. But if that experience tells us anything, it is that there are surprises ahead, that the behavior of matter under new conditions will not be confined to those patterns that we can conceive today. Therefore, the information must be gathered, without prejudice, for whatever good reason we can recognize. The information accumulated in this way may lead to deep insights into the laws of nature and will certainly lead to an enrichment of our knowledge of nature.

3   Interaction with Technology

The vast technological side of elementary-particle physics associated with the construction of large accelerators and other machines of the trade and with the design and development of sophisticated equipment, special computer software, and so on has led to a symbiotic relationship between technology and high-energy physics. It is not always clear in each case which came first, the need or the technology, but it is clear that it works both ways—the high-energy physicist often makes use of the most advanced engineering developments, and he often makes important contributions to technology having extensive applications outside of high-energy physics.

3.1   PARTICLE PHYSICS RELIES HEAVILY ON ENGINEERING AND TECHNOLOGY

As the magnitude of the energy needed in particle physics increases, the scale of the apparatus required to carry out experiments increases too, as

does the degree of ingenuity required to find methods to make the neces-
sary measurements. Particle beams of very high momentum must be
manipulated both in the accelerator and in the external beam systems.
This requires large magnets, high magnetic fields, radio-frequency electric
fields, computer control, and so on. The detection of the particles and
measurements on them require special detection devices such as bubble
chambers, Cerenkov and other counters, spark chambers, on-line com-
puters, and large magnets to analyze the particle momenta. Measurement
of film containing the results of the experiment has required the develop-
ment of special pattern-recognition devices that are able to scan and
measure the pictures in the numbers required to obtain meaningful quan-
titative results. The raw numbers produced by whatever method are sub-
jected to "number-crunching" computer analysis that is unusual in that it
requires massive data analysis and elaborate numerical calculations simul-
taneously. This has strained the capabilities of the largest computers.

The large scale of these devices, the magnitude of the technical prob-
lems, and the unusual character of many of the demands make it evident
that progress in experimental particle physics has been, and continues to
be, completely dependent on modern technology in almost all of its
physics- and engineering-related aspects. The engineering requirements run
the gamut, including high-precision surveying (for accurate determination
of beam angles over very large distances), mechanical engineering, cryo-
genic engineering, high-power electrical transmission (for large magnets),
radio-frequency engineering, electronic engineering, and engineering of
control systems. Applications of physics technology include high-precision
devices for magnetic-field measurement, superconducting magnets, solid-
state detectors, solid-state electronics, solid-state devices to produce
polarized targets, low-temperature thermodynamics, high-resolution opti-
cal systems, high-efficiency light collectors, lasers both for alignment and
for producing high-energy beams of polarized photons, and many others.
In fact, it would be difficult to name a field of physics or engineering that
has not made a significant contribution to the technology of particle
physics.

There are many examples of technological developments resulting from
the severe specifications set by high-energy physics that were successful
only because strong engineering and development support was available
from industry. One may cite the need for very high vacuum over very
large volume for the design of accelerators. A particular case is the de-
velopment of the diffusion-bonded, titanium vacuum chamber for the
12-GeV Zero Gradient Synchrotron at Argonne National Laboratory
(ANL). Although the aircraft industry had extensive experience with dif-
fusion bonding, entirely new methods had to be developed for this pur-
pose, and it is not too presumptuous to assume that other uses will be

found for these new methods. Apart from such special problems, the vacuum requirements for accelerators have placed heavy demands on vacuum pump manufacturers resulting in substantial developments in vacuum systems.

Accelerator physicists have depended on (and contributed to) the development of entirely new rf systems, for example, the reliable, long-lived, high-power klystrons required for the Stanford Linear Accelerator. The development of pulsed-magnet power supplies for accelerators has certainly had the help of the best engineering offered by the power-generator industry. A single such power supply produces more than 5 million 10-MW pulses each year! This is rotating machinery, undergoing a pulsed load, and it is operating well beyond the standard limits set in the mechanical engineering handbooks.

3.2 THE FEEDBACK INTO THE STREAM OF TECHNOLOGY

Because of his dependence on new developments in other fields of physics and engineering, the well-trained experimentalist working in particle physics is usually conscious and appreciative of them. He is on the alert for new applications that can be exploited for his own purposes, and this has had a very beneficial effect on related technologies in many fields quite remote from particle physics. Because the needs are unusual and often go beyond existing technology, because the people involved are strongly motivated and have a good understanding of the fundamentals of physics, and because funds have been available to support new developments that would serve to reduce costs on expensive items of equipment, the high-energy physicist has succeeded in pushing some aspects of physics-related technology in new directions at a considerably faster pace than would have occurred otherwise. Many of the technological gains have applications far beyond the need of the particle physicist.

An excellent example is the development of reliable, well-engineered, and very large superconducting magnet systems that was initiated as a technological development by particle physicists immediately after the fundamental discovery of hard superconductors by solid-state physicists. Two reasons for the interest of high-energy-physicists in this development were (1) the cost of power for the large magnets needed in the work (a 10-MW magnet power supply is not unknown in this field) and (2) the need for higher magnetic fields to bend particle beams of very high energy. Figure 1.16 shows the 184-in. superconducting magnet (18 kG) built for the Argonne National Laboratory 12-ft hydrogen bubble chamber.

All the technical ramifications of this development are not yet known, but it is clear that the availability of reliable, large superconducting sys-
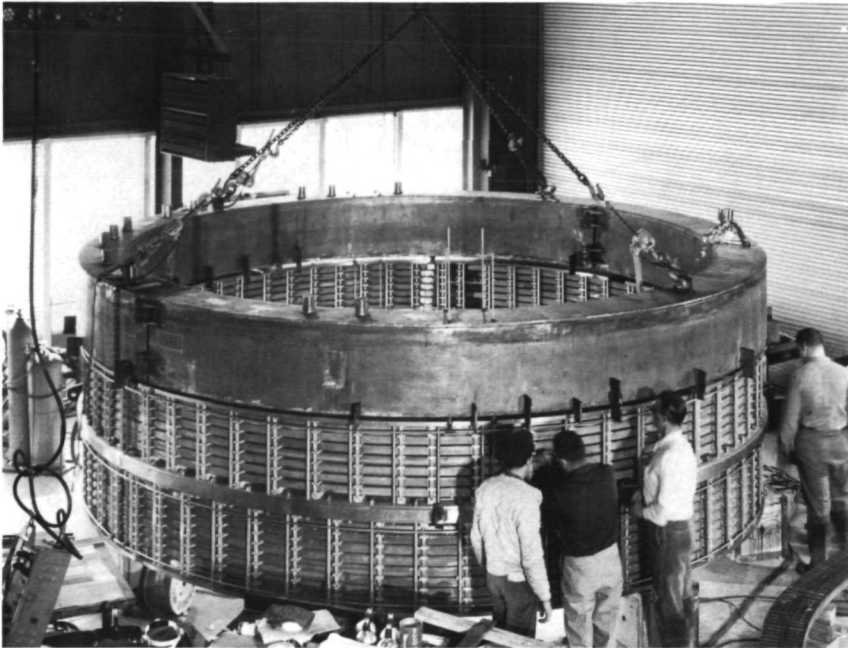
FIGURE I.16 The 184-in. superconducting magnet (18 kG) built for the Argonne National Laboratory 12-ft bubble chamber. The 100-ton magnet consumes only 10 W of power, much less than the several megawatts required for conventional magnets of the same field strength. This is the largest superconducting magnet operating in the world.

tems will be important in any technology that requires large magnets or transmission of large amounts of dc electrical power. Current work on ac and rf superconducting systems could increase tremendously the range of technical applications.

Particle beams and the techniques for handling them have potential for application in other fields. Intense beams of π-mesons (pions) may turn out to have certain advantages for use in radiation therapy (see Section 3.4), and the particle physicist's experience with beam optics will be needed to handle them. Methods developed for focusing and manipulating high-energy particle beams have recently been utilized in the development of a new type of high-resolution electron microscope.

Another example is an important medical application that has arisen in connection with the development of techniques for making and handling thin but very tough plastic films as support for multiwire particle detectors, plastic plumbing for very rugged thin-walled targets, and other plastic systems. Figure I.17 shows a preliminary version of a small, inexpen-

FIGURE I.17 Preliminary version of a small, inexpensive artificial kidney, which has been developed at the High Energy Facilities Division of Argonne National Laboratory.

sive ($15) artificial kidney, which has been developed in the High Energy Facilities Division at A N L. It has been used successfully by nine patients at an Illinois Veterans Hospital. It has a small enough volume (6 $\times$ 2 $\times$ 2 in.^{3}) to be suitable for a child, and it is hoped that it can be made cheap enough to be used daily. The basic configurations of these dialyzer designs are also being explored for their potential as membrane oxygenators for heart–lung machines.

3.3 INSTRUMENTATION

Particle physics shares with the rest of physics the need for new devices for control, detection, data taking, data recording, data analysis, and so on. The experimental work in this field has benefited greatly from instrumentation developed in other fields, especially solid-state and nuclear physics. The demands of physicists in general for special instruments have encouraged the development of sophisticated instruments that have many applications outside of physics. The remarkable growth of the modern instrumentation industry testifies to the usefulness of these devices both for research and for everyday uses.

Many of the contributions of particle physics are closely intertwined with those of nuclear physics, since one need not go back too many years to find that the fields merge. Thus, the development of important instruments such as scalers, pulse-height analyzers, and various kinds of data recorders have grown out of this common effort.

Because of the large scale and cost of high-energy physics experiments, there has been exceptionally strong pressure for the development of cheaper, faster, more sophisticated instruments, as well as for the development of special instruments. While the former have had a continuing impact on the industry, many of the innovations resulting from the latter developments have only recently found their way into more general use. For example, devices developed for analyzing film from spark chambers and bubble chambers are finding application in chromosome counting and radiographic analysis, and they probably could be used for fingerprint analysis. Experience with the development of these devices led the Lawrence Berkeley Laboratory (LBL) to the conception of a mass information storage and retrieval system making use of film, a system that has been developed by IBM under contract to LBL.

The newest development in wire-chamber instrumentation, the multiwire proportional chambers, may turn out to have uses in many fields outside of high-energy physics. Its possible use for low-dose x-radiography has been pursued by a group at LBL, who have produced the radiographs of philodendron leaves shown in Figure 1.18. It is to be noted that these pictures are photographs of a cathode-ray-tube readout of information collected by the wire chambers from a low-intensity soft x-ray source (0.03-mR exposure at 5.9 keV). This would appear to make it possible to obtain good x-ray images with much lower intensity than is required for photographs, thereby significantly reducing the radiation dosage to the patient. The information can be collected and stored in digital form. It can be imagined that many other uses will be found for these chambers. This is just a recent example of the fact that most of the major innovations in techniques for detecting ionizing radiations have originated in particle and nuclear physics, and these techniques have almost invariably found applications in other fields since the phenomena being exploited are universal with respect to all ionizing radiations including x rays and the emanations from ordinary radioactive substances.

3.4 ACCELERATOR PHYSICS AND ENGINEERING

The science of charged-particle acceleration had its beginnings in the universities, initiated by physicists interested in accelerating both nuclear
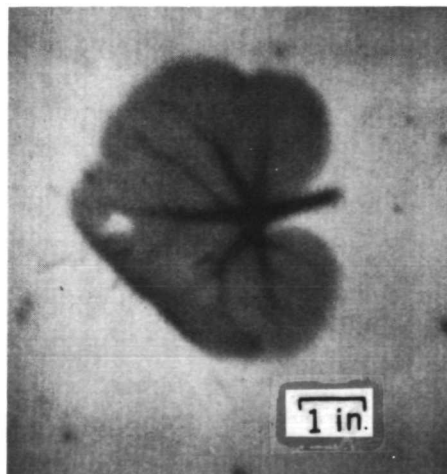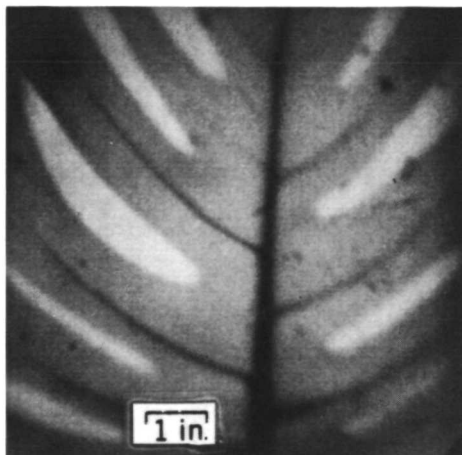
FIGURE I.18 Radiographs of
philodendron leaves. These pictures
are photographs of a cathode-ray-
tube readout of information collected
by wire-chamber instrumentation
from a low-intensity soft x-ray
source.

particles and electrons for studies of nuclear properties. The early accel-
erator physicists participated actively in both the research on accelerator
design and the use of the accelerators for research with the accelerated
particles. As the field has moved to higher and higher energies, accelerator
physics and engineering research has required the full-time effort of many
physicists, who have become accelerator specialists. At the same time,
there are still a number of active particle physicists who spend a substan-
tial amount of time on accelerator physics and engineering. Most of the
physicists responsible for research and innovations in the accelerator field

still identify themselves with the field of particle physics both because their research concerns the behavior of the particles in electric and magnetic fields and because the prime motivation and support for their research arises from the need for higher energy, more intensity, and better control of particle beams.

The number of physicists participating in accelerator research, design, and development is relatively small compared with the total number of particle physicists, possibly equivalent to 200 full-time PhD physicists in the United States. However, their contribution to the field of particle physics is clearly very large in proportion to their number. These are also the particle physicists who have made, and will continue to make, the most direct contribution to technology.

Accelerator physics involves the study both of methods for producing high electromagnetic fields and of the orbits of charged particles in static and time-dependent fields. The methods of orbit theory have become quite sophisticated and make it possible to determine, for example, the conditions under which resonances associated with the periodicity of a machine will occur. At high intensities there are also plasma interactions that can lead to beam blowup.

These effects must be understood thoroughly in order to be able to guide the beams through their various possible oscillations and instabilities while they are being accelerated. It should be realized that this requires precise guidance of a bunch consisting of a large number of particles (10^{13}) over enormous distances (at NAL, 273,000 miles) at tremendous speeds (very close to that of light) without any significant loss of the particles, which (at NAL) will be constrained to a vacuum pipe of dimensions 2 in. × 5 in. Any straying of particles from the specified region will lead to radiation damage and unacceptable levels of radioactivity in the surroundings.

A thorough understanding of particle-beam physics is also required in order to extract the beam from the accelerator after it is at full energy and to manipulate it to match the needs of the experimentalists who are studying the properties of the particles.

The activity of the accelerator physicists includes the development of instrumentation not only for the above-mentioned control of the beams but also for diagnosis of their behavior. This work has led to computerized feedback systems that have served to simplify greatly the operation of accelerators.

Accelerator physics and engineering is deeply imbedded in modern technology, and there has been a strong interplay between them, as discussed in Section 3.1. At this point we wish to emphasize that accelerator physics has contributed *accelerators* to modern technology. High-voltage

transformers (Cockroft-Walton machines), electrostatic generators, and betatrons have found a multitude of uses in industry and medicine, but the motivations that led to their original conception and initial development were generated by the desire to study nuclear particles. There are possible applications of beams of higher-energy particles, and unstable particles such as pions, that would require the use of the higher-energy and higher-intensity accelerators that are now available because of the innovations of the accelerator physicists. Application of very energetic (in the billion-volt region) heavy ions to industry or medicine is not out of the question, and beams of such ions (nitrogen) have recently been produced both at the Princeton-Penn Accelerator and at the Bevatron.

Since the very-high-energy machines require lower-energy machines of very high intensity for injection (at N A L, the beam is injected at 750 keV by a Cockcroft-Walton into a 500-MeV linear accelerator and then accelerated to 8 GeV by a synchrotron booster to be injected into the 200–500-GeV ring) there is continual innovation and development of the theory and practice of the lower-energy machines, all of it being applicable to accelerators produced for technological purposes. Even the methods developed for the highest-energy machines often have their uses at the lower energies. Furthermore, the growing usefulness of accelerators for technology, which was not widely anticipated when they were first conceived, is an indication of the possibility that applications of the highest-energy accelerators may arise in the future.

3.5 WILL THERE BE DIRECT APPLICATIONS OF PARTICLE-PHYSICS DISCOVERIES?

If one asks what applications outside of physics will be made of the end product of particle physics, namely, the properties of the particles, the answer is, very few if any are known at the present time. In a search for precedents to justify a belief in the ultimate application of discoveries in this field one is led very naturally to cite examples from other older fields that do not now evoke an association with high energy. In fact, elementary-particle physics is the newest and still open link in the chain starting from cathode rays, going through atomic physics and then through nuclear physics. The work leading to discoveries in each of these fields was done when the applications were not, in fact, could not be, foreseen. The field of nuclear physics provides the natural antecedent events since high-energy physics has grown from the nuclear physics of an earlier day. There is no clearer example of the discovery of an elementary particle that had a profound influence on technology than that of the neutron. It should be

noted that this discovery was neither made nor celebrated because of its technological implications. In fact, only some of the most original and imaginative physicists working in the field seem to have realized that there might be such implications.

The dramatic discovery by itself was not enough to establish a technology. That required the steady flow of discovery associated with a vigorous program of original research in many fields ranging from nuclear physics to metallurgy, including, of course, another discovery, that of nuclear fission. A similar follow-up along a broad front would be required to bring about successful application of any new dramatic discovery in particle physics.

Some more recent examples of discoveries having possible application can be cited from the field of particle physics. There was a flurry of interest in μ-meson (muon) catalysis of exothermic nuclear reactions and its potential applications as a source of energy, but the numbers conspired against success. The possibility of thermonuclear catalysis would be reopened by the discovery of any new negative stable particle having a mass greater than about 100 electron masses. The conspiracy of numbers is often the root of exploitation or failure; what a difference it would have made if the neutron multiplicity in uranium fission had been slightly smaller!

Among the first of the "unusual" particles to come out of particle physics is the pion. Because its mass is some 270 times greater than that of the electron and because it is absorbed by nuclei (via the strong interaction) with the emission of large amounts of energy, the negative pion would have unique properties as a radiotherapeutic device. More study will be required with intense pion beams to demonstrate that the therapy is indeed as effective as suggested. In this case, the physics of the pions is known well enough for the purpose, and what is needed are biological and medical studies within intense beams.

We cannot predict what new particle or phenomenon may have important applications as the result of favorable properties. There has been a general assumption that the new particles produced by high-energy processes are too unstable to have useful application. However, if a particle such as the quark exists, it is expected to be stable. Not only that, but it should be able to reside in matter without being annihilated unless it should meet with an antiquark or other quarks. It would be expected that the quark–quark interaction is of such short range that the Coulomb barrier would keep "thermalized" quarks of the same kind from getting close enough to each other to form a baryon plus antiquark.

The assumed stability of the quarks leads to the possible existence of an entirely new form of matter made up of (negative) quark molecules

bound together by protons. The chemistry of these molecules would be quite different from any chemistry we know. However, chemical reactions making possible thermonuclear catalysis by quarks can be visualized. A hydrogen-like Q-ion formed by binding a $\overline{Q}$ (antiquark) of charge $-\frac{2}{3}$ to a proton would have a very small radius and a net charge of $\frac{1}{3}$. At a sufficiently high temperature protons or other very light nuclei might be able to penetrate the greatly reduced Coulomb barrier to react exothermally, thereby releasing the quark for further action. This process could be important for the understanding of stellar evolution. In a dense medium of such antiquarks, two might be attached to a deuteron to form a molecular $\overline{Q}_2{}^{-1/3}$ ion of net charge $-\frac{1}{3}$. Such a negative ion could induce an exothermic reaction since it would readily capture another deuteron to form the positive $\overline{Q}_2{}^{+2/3}$ molecular ion, or even the $\overline{Q}_3{}^0$ molecule, allowing the two binding deuterons to react at their leisure. Although the quantitative aspects of such a process would depend on a number of questions, such as the ratio of Q mass to nucleon mass (assumed rather large here), a favorable determination would have terrestrial significance.

There are several other most unusual particles, the nature of each either having been suggested by the systematics of experimental results or having been imagined as necessary to satisfy a need for completeness, elegance, or beauty of the theory. These include magnetic monopoles, intermediate bosons, heavy photons, μ-mesons of higher mass, and hadrons of very high mass but low baryon number. The discovery of any one of these could have technical implications, depending always on the properties that they are found to have. For example, the strong long-range magnetic forces exerted by magnetic monopoles would certainly be useful.

Most of these speculations, or possibly they should be called fantasies, are based on the assumption that it will be possible to produce some of these particle types in quantity at an accelerator. That certainly may turn out to be a false assumption, but will the truth be less fantastic?

4   Interaction with Other Research Fields

The dependence of high-energy physics on technology is matched by its dependence on developments in other fields of research both because the technology leans so heavily on the other fields and because elementary-particle physics depends on all the antecedent fields for its methods and ideas. The compliment is returned largely in the direction of nuclear

physics but also in some surprising ways to other fields of research quite remote from the purposes of elementary-particle physics.

4.1 INTERACTION WITH NUCLEAR PHYSICS

Since one starting point of high-energy physics was the desire to understand nuclear forces, it is not surprising that there is a continuing generic relationship between elementary-particle physics and nuclear physics. There is no question but that a complete understanding of the forces in nuclei must take into account the great variety of particle states that are known or will be found in the future. It is especially clear that vector mesons must play a significant role in nuclear forces at short range, just as the pions are dominant at "long" range.

The existence of hypernuclei, those nuclei in which a neutron is replaced by a lambda-particle, offers new possibilities for studying nuclear structure and nuclear forces. Nuclear structure is also being investigated by studying the spectra of mesic-atoms, atoms in which an electron is replaced by a μ-meson, π-mesons, or K-meson. Because of the higher masses of the mesons compared with the electron, the atomic orbits are very small in radius, and the influence of the nuclear size and structure on the orbit is much greater than in the case of the electron. Most recently even Σ^- atoms and $\bar{p}$-atoms, atoms in which a Σ^- hyperon or an antiproton, respectively, replaces the electron, have been studied.

There are less direct connections between particle physics and nuclear physics, too. The discovery of parity violation, which is fundamental to all of physics, was suggested by the observation of peculiarities of the K-mesons and verified as a phenomenon pervading all of weak interaction physics by an experiment in nuclear beta-decay. It now provides a tool for analysis of beta-decay phenomena involving polarizations and angular correlation measurements.

There has been a strong interplay between the high-energy and low-energy nuclear physicists in regard to techniques of instrumentation and data handling. Workers in each field have learned much from those in the other field, especially in regard to new developments in counters and detectors. The development of polarized targets, already mentioned for its connection with the physics of condensed matter, has proved to be suitable for fundamental experiments in high-energy physics and in nuclear physics at intermediate energies (100–500 MeV), as well as in nuclear physics at lower energies. The polarized hydrocarbon targets that have been developed recently will doubtless be useful for experiments in nuclear physics.

Finally, the contributions of accelerator physicists continue to be as important to the development of nuclear physics as to high-energy physics and, in fact, are often the result of collaborations between those working in the two fields. The development of high-energy proton linear accelerators, represented at their highest energy and intensity by the Los Alamos Meson Physics Facility (LAMPF), is an example. The strong interest in heavy-ion acceleration for nuclear-physics research also has led to new accelerator developments, the most recent being the acceleration of nitrogen ions in the Princeton-Penn Accelerator (PPA) and the Bevatron to energies at the billion-electron-volt level. There has also been a recent flurry of interest in the possible production of superheavy elements by secondary heavy-particle reactions induced by primary beams of protons at very high energy. In addition to these indirect and exotic uses of the high-energy accelerators, the high-energy particles, especially electrons, have been used extensively for nuclear structure studies,* and this use can be extended to higher and higher energy beams as the beam time and techniques become available.

4.2 INTERACTION WITH ASTROPHYSICS

Many connections also exist between astrophysics and particle physics. Measurements of high-energy radiations in space make use of techniques originally developed in the high-energy physics laboratory, just as the early techniques of high-energy physics were adapted from cosmic-ray work. We have already noted that the earliest discoveries in what is now called high-energy physics were made in cosmic-ray investigations. However, quantitative studies required the control provided by accelerators, and now, as astrophysicists become involved with strange stellar objects of higher and higher energy density, they need the quantitative knowledge of high-energy reactions gained in the laboratory in order to analyze stellar processes. Not only is this knowledge required, but also the knowledge of *what* particles are reacting is needed. This includes the many new species of particles identified by the elementary-particle physicist, such as antiprotons, antineutrons, hyperons and antihyperons, and mesons of all kinds, and resonance states of all kinds.

Elementary-particle theory really begins to impinge on astrophysics

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*Although the 1-GeV electron linear accelerator at the Stanford High Energy Physics Laboratory (HEPL) does not meet the over 1.5-GeV criterion of Table I.4, it is classified as a high-energy laboratory and is largely used for nuclear-physics research. The costs of this laboratory have been included in the budget figures of Chapter 9.

when we turn our attention back to the early universe. If current versions of the big-bang model are correct, then the universe was *once* vastly denser and hotter than now.

In order to be able to solve the gravitational field equations, we need to know the equation of state of the matter and radiation present in the early universe, but our present understanding of particle physics is inadequate to determine the equation of state at temperatures above $\sim 10^{12}$ K to 10^{13} K, i.e., at temperatures high enough so that strongly interacting particles are produced copiously in thermal equilibrium. One way to deal with this problem is to treat the matter as consisting of a number of species of highly relativistic free particles. But how many species? If we take a fixed number of species (say, photons, gravitons, leptons, and nucleons) then the temperature of the universe is inversely proportional to its radius. If we take as many species of particles as would exist in thermal equilibrium according to the currently fashionable model of the strong interactions, then the temperature of the universe varies inversely as some power of its radius depending on the details of the model. More likely, no free-particle model makes sense, and to understand the early universe the elementary-particle theorists will have to leave the familiar conceptual framework of S-matrix theory and venture into the uncharted territory of relativistic many-body physics.

Because of the high-energy density in stars, even weak interactions play a significant role, and, in particular, the interactions of the neutrino become important. The study of the fundamental neutrino interactions is a prime objective of experiments carried out at the largest accelerators. Furthermore, the fundamental ideas undergoing tests in these and other experiments involve such notions as a direct coupling between neutrinos and electrons, which would play a significant role in stellar processes by causing neutrino–electron scattering.

The discovery of antiparticles has had a special impact on astrophysics and cosmology that goes beyond the quantitative treatment of matter at high temperatures. Antimatter adds a conceptual dimension to cosmology. It suggests the possible existence of bizarre antistars, antigalaxies, and antiuniverses. Collisions of worlds of matter and antimatter evoke images of explosions beyond imagination. The possible existence of a real antiworld (Section 2.10) leads to notions of a world in which the laws of physics are different from the ones we know because of C and CP violation, therefore, to a universe in which the cosmology might be different from ours.

The existence of antimatter has also led to speculation about antigravity, suggestions that particles and antiparticles might have the opposite sign of gravitational mass. However, the experiments on neutral K-

mesons demonstrate unequivocally that this is not true, at least for those mesons. If the gravitational potential energy of the K^0 and $\bar{K}^0$ were opposite, their apparent mass degeneracy would be broken by the external gravitational field, and the characteristic oscillations in interference experiments such as the one illustrated in Figure I.5 would be so rapid as to wash out the interference phenomenon and the essential difference between K_S and K_L. In particular, there could be no long-lived component of K_L.

Another important recent development that could be of importance to cosmology is the evidence for an apparent breakdown of the principle of time-reversal invariance (Section 2.11). There is still little known about the ways in which this breakdown occurs. It is evidently a small effect, at least for the conditions under which it is observed. As indicated in Section 2.7, there is a possibility that what is observed under present conditions is a manifestation of a much larger effect in so small a domain of space–time that the tools used thus far are incapable of revealing some of its most significant aspects. Of course, work on this tantalizing subject is continuing and will be extended more deeply into the microscopic domain as the tools become available. No matter what the outcome of these investigations may be, it is clear that this microscopic phenomenon can have profound implications for cosmological theories and, as further insights into its origins become available, must be woven into the fabric of cosmology.

4.3 INTERRELATIONSHIPS WITH OTHER ASPECTS OF THEORETICAL PHYSICS

It is not surprising to find the theoretical ideas and methods of one branch of physics finding application in another, since the whole history and beauty of physics has been characterized by the unity and generality of the theories. The methods of field theory, developed to answer the most fundamental questions about the particles of nature, are no exception. Theoretical investigations of the nuclear many-body problem, many-body problems in condensed matter, superconductivity theory, and statistical mechanics have not only been stimulated by developments in field theory but have also provided physical insights that have had an important impact on the interpretation of theories of particles and fields.

The mathematical properties of the amplitudes for reactions between particles have been subjected to intense scrutiny by theoretical particle physicists in recent years in a search for useful approximation methods to handle strong interactions. This has led, for example, to the development of dispersion relations which are broad generalizations of the Kramers-

Kronig dispersion relations describing the dispersive properties of matter for electromagnetic radiation. The understanding of scattering and reaction theory resulting from this work gives an insight into many matters such as resonance states and unstable states of systems—matters that are also of interest to other fields of physics, especially nuclear physics, and to an increasing degree to atomic and molecular physics.

Although the foundations of theoretical physics are well established at the level of the quantum mechanics of particles, there are serious unsolved problems at the deeper level of quantum field theory. Since quantum field theory is needed to describe radiative processes of all kinds— molecular, atomic, nuclear, or other—these problems have their impact on all fields of physics. The structure of atoms, for example, is described very well by ordinary quantum mechanics until one seeks the precision associated with radiative corrections (for example, the Lamb shift). The radiative corrections are not calculable in a direct way in terms of existing theory—the results of such calculations are not finite. However, these divergence difficulties can be circumvented by making use of renormalization procedures that are *ad hoc* methods adapted to the particular approximation methods being used. Despite the fact that the agreement between these calculations and experiments is remarkably good to the limits of precision attainable in both, this situation is conceptually unsatisfactory and represents one of the serious unsolved problems of physics. The need for infinite renormalization may be related to unrecognized aspects of the laws of physics, which could imply a substantial change in physical theories. These changes are expected to manifest themselves much more directly in phenomena taking place at very high energies. So far, high-energy tests of the validity of quantum electrodynamics, which is the form of quantum field theory most easily subject to test, have failed to reveal any weakness. However, some insight into the meaning of renormalization theory can be expected as both the precision and energy at which the tests are carried out are increased.

4.4 INTERACTION WITH APPLIED MATHEMATICS

The precision tests of quantum electrodynamics have demanded theoretical calculations of great complexity to match the increasingly accurate experiments. These calculations are well-defined in principle and typically involve three steps: (1) computing traces of large polynomials of 4 × 4 matrices, (2) reducing the terms to a standard form, and (3) using the result as a part of the integrand of multidimensional (typically six- or seven-dimensional) integrals.

All three steps have been carried out successfully using computers. The second step, which allows one to do elementary algebra by computer, has had applications in many fields; already the program has been used in such fields as mechanical engineering. The integration routine, which by the way must cope with singular integrands, likewise appears to be highly advanced relative to the general status of the field of numerical analysis.

4.5 INTERACTION WITH OTHER SUBFIELDS OF PHYSICS

The application of accelerators to such studies as radiation damage and dislocations produced by energetic particles impinging on solids is well known. Possibly a less widely known application of the work of the accelerator physicists concerns the development of storage rings.

We have already emphasized the importance for particle physics of intersecting storage rings in Section 2.7, and in Section 8.4 we shall discuss their prospects for further development in the future. These developments have offered the accelerator physicists one of their most interesting challenges because useful storage rings require beams of high intensity, with great stability and long life, and under precise control. One of the aspects of electron storage rings (or any other circular electron machine) that is a problem for high-energy physics is the energy loss due to radiation (synchrotron radiation) by the electrons moving in an orbit in the machine. However, this problem, which can be overcome by introducing an appropriate amount of acceleration into a storage ring, has turned out to be a boon to atomic, molecular, and solid-state physicists.

Because of the interest in development of storage rings, the accelerator physicists of the Midwest Universities Research Association (MURA) some years ago began the development of a model storage ring of high intensity to make fundamental studies of the particle orbits and resonances, stability of the beam, and so on. Electrons were used for modeling to keep the scale and cost down to reasonable size. Because of high intensity in the model, it produced intense synchrotron radiation. The radiation takes the form of a continuous soft x-ray and ultraviolet spectrum, in the range 100 to 1800 Å (peaking at about 200 Å when the machine is run at 250 MeV).

This has turned out to be a very intense laboratory source in the soft x-ray and uv regions, and it has many uses for research in atomic, molecular, and solid-state physics. The storage ring is now being used as a national facility for that purpose. A large number of proposals for such experiments from groups throughout the country have been made, and they are scheduled in much the same way as high-energy physics experiments.

Synchrotron radiations from other electron machines, for example DESY in Hamburg, Germany, and the 1.3-GeV synchrotron from the Tokyo INS, are also being used for studies in atomic, molecular, and condensed-matter physics, and there are excellent possibilities for extending this work to the electron storage rings under development. Figure I.19 shows the intensities to be expected in the x-ray and uv regions from some of these devices.

4.6 INTERACTION WITH BIOLOGY, CHEMISTRY, AND OTHER FIELDS

There are some obvious ways in which the high-energy physicist is dependent on research in nonphysics fields. An important example is the radiation safety problem around accelerators. The results of research on the biological effects of radiation have had, and will continue to have, a direct influence on safety standards.

A most important contribution of research in nuclear chemistry is the information that makes it possible to measure accurately the intensities of
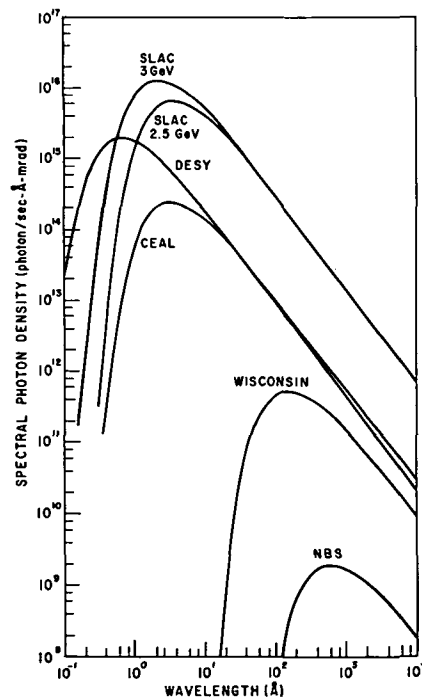
FIGURE I.19 Comparison of intensity of synchrotron radiation from various storage ring and other sources. Owing to the extreme natural collimation of synchrotron sources, they are well adapted to use in a monochromator without too much collimation loss. A conventional x-ray tube on the other hand loses about 10^5 in intensity on being collimated by a monochromator. (DESY refers to the synchrotron, not the proposed storage ring, for which figures are not currently available.)
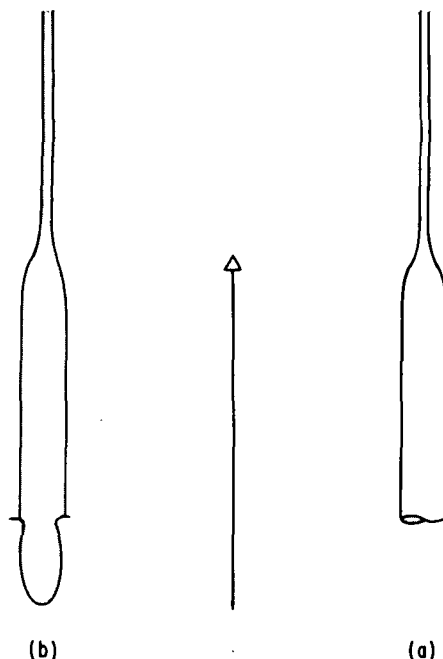
FIGURE I.20 Similarity of an ideal
light collector (a) and a cone from
the retina of the eye (b). Light enters
from below.   (b)   (a)

particle beams in and around accelerators when the levels are too high to
permit the use of direct counting techniques. Foil irradiations are used to
calibrate the monitoring devices, providing direct readout for routine use.

A large component of contributions going the other way is to be found
in the instrumentation developed either for use in high-energy physics or
for peripheral uses that also apply to other fields. The use of particle-
handling techniques has had some impact in electron microscopy with re-
sults that should turn out to be significant for microbiology. Instruments
developed for direct use in high-energy physics that have immediate ap-
plication to biological research are the pattern-recognition devices men-
tioned in Section 3.3.

Another example that may be cited arose from the need for methods of
observing and photographing scintillation tracks. This need led to the de-
velopment of image intensifiers that have proved useful for quantitative ob-
servation of chemical luminescence in biological systems, which have made
it possible to carry out studies of chemical kinetics in organisms and to re-
late the chemical action to the structure of the organism.*

*See G. Reynolds, "Application of Certain High Energy Physics Techniques to Some
Problems in Biology," Princeton U. Tech. Rept. No. 1 (July 4, 1970).

Other methods designed to detect and localize small sparks, such as acoustical detectors, magnetic cones, magnetostrictive detectors, and electrostatic detectors may be found useful for entirely different purposes from those of the particle physicists.

There are cases in which some research objective rather than an instrument is shared between fields so that there are mutual benefits from work done in one of the fields. An example that could have gone either way concerns the development of efficient light collectors. In this case, the need to collect faint light from the Cerenkov radiation of high-energy particles led to the development of the design of a new light-collecting system. Figure I.20(a) shows an ideal light collector. It is interesting to compare this with Figure I.20(b), which shows schematically the shape of the cone in the retina of the eye, which has not been explained. As a result of recognition of this similarity, the analysis of the light collector is now being adapted to the problem of the retinal cone.

We may also call attention to the fact that the discovery of the physical properties of the pion has opened up an entirely new field in radiation biology because of the potential importance of pion radiotherapy already mentioned in Section 3.4. Finally, it has been suggested that high-energy heavy ion beams may be even more effective than pions for radiotherapy, and this is a subject also requiring extensive biological investigation, which now appears to be possible with the high-energy beams of heavy particles that have been produced at the PPA and the Bevatron.

5   Interaction with Society

5.1   SOCIETY'S VIEW

Questions about the value of science and the scientist in the context of the critical problems of society have led recently to speculation in the press and elsewhere on an alleged public disenchantment with science. That there has been some disenchantment with physics among the college-age population, as evidenced by a drop in the number of students majoring in physics and applying for entry into graduate schools, cannot be denied. These changes are undoubtedly influenced by changes in employment opportunities and funding patterns as well as changes in social attitudes. However, this Panel does not see any convincing evidence that the generally educated public has turned against science. It seems likely

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to us that the leadership of the United States in basic science, as indicated, for example, by the award of Nobel Prizes, is still a great source of public pride, and there still exists a latent belief in science and scientists. It is certainly true that continual reiteration of the view that disillusionment is prevalent in the land, combined with the consequent withdrawal of support, serves as a self-fulfilling prophecy.

It is more constructive to recognize that the desire for creative activity is strong in man, and that an atmosphere in which creativity is encouraged enhances the quality of life. Without the sense of personal satisfaction that comes with an environment of intellectual, artistic, and cultural growth, the feeling of progress in the society, indeed the *actual* progress, may come to a standstill, with serious consequences for the overall well-being of the society.

Basic research in physics has been one of the most successful creative American enterprises in recent times. Certainly among the activities that have contributed to this success, elementary-particle physics has played a leading role. The efforts of other countries of the world to emulate our progress testify to this. It is a highly creative enterprise in which this country has been showing the way, and in this respect certainly exemplifies the scientific leadership that our people are led to believe is a symbol of progress and a great cause for pride.

5.2   ROLE OF PARTICLE PHYSICS IN REGARD TO PROBLEMS OF SOCIETY

Because it is concerned with the behavior of matter on a distance scale 10 orders of magnitude removed from direct human perception, elementary-particle physics seems to have little relationship to pressing social problems such as pollution, racial conflict, war, and over-population.

Its social thrust at this stage is largely intellectual and cultural as it was for the pioneering discoveries of Fermi and Lawrence, Rutherford and Einstein even though the world has been transformed by the impact of their work. Certainly they were not motivated primarily by the social implications of their research; in fact, Rutherford, the discoverer of the nucleus, did not believe that nuclear reactions would ever be a practical source of energy.

The following excerpts from an exchange between Senator Pastore and Robert Wilson, Director of the National Accelerator Laboratory, when the latter appeared before the Joint Committee for Atomic Energy serve to illustrate how very personal are the inspirations that move the field:

SENATOR PASTORE: Is there anything connected in the hopes of this accelerator
that in any way involves the security of the country?

DR. WILSON: No, sir; I do not believe so.

SENATOR PASTORE: Nothing at all.?

DR. WILSON: Nothing at all.

SENATOR PASTORE: It has no value in that respect?

DR. WILSON: It only has to do with the respect with which we regard one another,
the dignity of men, our love of culture. It has to do with: Are we good painters,
good sculptors, great poets? I mean all the things that we really venerate and honor
in our country and are patriotic about . . . it has nothing to do directly with defend-
ing our country except to help make it worth defending.

Similar individualistic statements might be made about the motivations
of the people working in any of the basic sciences. Crossing the boundary
at any frontier is so very difficult and demanding, and requires such per-
sistence in the face of disappointment, that the motivation to carry on
must come from the spirit rather than from any narrow objective. But
that does not imply the absence of social accomplishment. The very fact
that spiritual as well as material motivations are recognized and supported
by society is an accomplishment.

It may be true that the "virtues" of basic natural science are recognized
by society because of the outward contributions to its physical and ma-
terial well-being. However, there has been a more subtle, and perhaps more
important, influence of basic scientific work on the patterns of thought
and procedure. The successes of natural science have encouraged the con-
cept of rational and logical solutions to social and economic problems.
Particularly in this area, natural science still has much to offer to society.
Not the least of those things that still must find a proper place in the gen-
eral use of "scientific method" is an understanding of the limitations of
the methods used in natural science. A crucial requirement for success in
the natural sciences has always been recognition of these limitations.

This is a manifestation of the general role played by natural science in
the development of our concepts of the nature of knowledge. In the field
of particle physics one is constantly entangled in deep* questions of
understanding such as the meaning of distance and time on a very small
scale, the meaning of violation of time-reversal invariance, the connections
between space–time and charge symmetries, internal symmetries and
Lorentz invariance, the connection between particles and fields, and even
the meaning of quantized fields. In these and other problems there occur

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*In regard to the use of the word "deep," Niels Bohr is alleged to have remarked that
in order to define a deep statement one should first define a "clear" statement: "A
clear statement is one to which the contrary is either right or wrong. A deep statement
is a statement to which the contrary is another deep statement."

crises of understanding that serve to contribute to our understanding of
the nature of "understanding."

5.3 ABILITY OF THE FIELD TO MAKE FURTHER
CONTRIBUTIONS

How can physics in general, and particle physics in particular, contribute
to the indirect benefits of natural science to society? The influence will
need to be on the one hand symbolic, mainly by setting a style for meet-
ing new problems, and on the other, practical, by producing innovative
people who are trained in that style. Some of the stylistic characteristics
that are part of the research culture of the physicist and could well be of
use in approaching many problems of society are a healthy skepticism
toward supposed technical barriers, a constant awareness of one's own
fallibility, recognition of the value of the judgment of one's peers, under-
standing how to make a semiquantitative estimate when an exact evalua-
tion is out of the question, recognition of the limitations of such esti-
mates, especially the limitations of statistical arguments, and the desire to
take into account all available evidence.

In this context, one can ask whether elementary-particle physics has
anything special to offer. We think that *each* subfield of physics has some-
thing special to offer because of its different slant on research and because
of the different personal characteristics inclining the individual toward one
subfield or another. Elementary-particle physics provides a unique com-
bination of high-flying abstract imagination with vigorous and exciting
critical standards of validity. It also exemplifies the physicist's strong
faith in the ultimate intelligibility of any situation and a belief in simple
and elegant models. The challenge of the exploratory character of par-
ticle physics has traditionally served to develop the maverick mind, which
can be highly useful in meeting other innovative needs. Furthermore, his
exploration into the domain of the unknown frequently leads the particle
physicist to false tentative conclusions, and he is brought face to face with
his own fallibility often enough to be quite aware of it. Skepticism
toward the insolubility of tough technical problems is another require-
ment for the successful experimentalist in this field, as should be evident
from the discussion of Chapter 3.

Government has made use of these special capabilities and perspectives
of the physicists in areas outside their immediate specialties. So far the use
has been greatest in the field of military technology. However, many prob-
lems of the environment are subject to attack by the methods of opera-
tions research, systems-analysis hardware design and construction that are

suggested by experience in physics. Unfortunately, the focus for such an activity has not been as well defined as for national defense, where the Department of Defense has both a mission and money. The existence of the Environmental Protection Agency and Research Applied to National Needs programs of the National Science Foundation may serve to define the clear mission to carry out an imaginative attack on these problems, which would unquestionably bring a positive response from the physics community.

5.4   AN INTERNATIONAL ROLE

All of natural science has played a continuing role in the communication and maintenance of contact between nations. In this regard, particle physics offers many opportunities because of special features of the sub-field, especially its remoteness from technology having a military potential.

The large scale of the accelerators and many of the experiments require cooperation between large numbers of people and the pooling of re-sources. This has had a substantial effect on the sociology of high-energy physics in the United States, which will be discussed in more detail in Chapter 6. It has led to the existence of an international community ex-changing people, ideas, technology, and methods. Collaboration to an un-usual degree is taking place between the countries of Western Europe, be-tween Western Europe and the United States, between Western Europe and Russia, and between Russia and the United States.

The existence of formal agreements among the governments of the countries of Europe to collaborate in funding, building, and using a com-mon elementary-particle physics research facility at CERN in Geneva, Switzerland, is a clear indication of the importance that those governments attach to the kind of success that has been attained in the field in the United States. The purpose was to place Western Europe in a position to compete with the United States for the benefits that flow from a success-ful high-energy physics program. And they have succeeded in arriving at that position. In fact, CERN and the other European high-energy physics laboratories have now passed the United States in the competition insofar as the funding of the field is concerned.

There has been extensive informal cooperation between the Western European and U.S. high-energy physicists. Bubble-chamber film has been exchanged between groups on both sides. Physicists, especially young physicists who are in the best position to contribute and to benefit, have been exchanged between U.S. and Western European laboratories for periods extending up to two or three years. There have been about five

U.S. groups participating in experiments at the unique CERN intersecting storage ring.

The fact that the highest-energy machine in operation is at Serpukhov, U.S.S.R., has led to formal agreements between CERN and the Soviet Union to collaborate on experiments there. Also, there exists a formal agreement, signed personally by DeGaulle, between the French and Russians, for the French to build and deliver a large hydrogen bubble chamber to Serpukhov. After extensive discussions, there is also now an agreement for reciprocal participation of U.S. physicists in the work at Serpukhov and Soviet physicists at the National Accelerator Laboratory (NAL) with each side providing its share of support and equipment.

There are also special official agreements concerning the exchange of individual high-energy physicists between the laboratories of the United States and the Soviet Union on an informal basis. These agreements are an extension of arrangements that were initiated when the United States was in an especially advantageous position in this field and that have been supported vigorously by physicists on both sides. The early arrangements in high-energy physics were followed by exchanges in many other politically neutral fields of natural science, and it seems likely that these newer patterns of providing for the two-way flow of people and information will also extend into other fields.

The potential for ground-breaking in international relations that is offered by high-energy physics is also attested to by the fact that CERN was established by the first agreement among a number of European countries to jointly provide funds for a common enterprise located in one of the countries. Certainly there is a possibility of future collaboration between the United States, the Soviet Union, and CERN on the generation of machines beyond that represented by the NAL. This could provide an opportunity for an exciting study of a pilot operation in international cooperation.

The following is a quotation from an editorial entitled "Cooperation in Science" from *The New York Times* of July 3, 1970:

Supporters of international scientific cooperation have won another important victory. The decision in Washington and Moscow to permit American scientists to work at Serpukhov—site of the world's largest nuclear accelerator—and to have Soviet scientists do research at Batavia, Ill.—where an even larger atom smasher is being built—is a major step forward. The secrets of the atomic nucleus which these giant machines are seeking to unravel have nothing to do with the ideological, political and economic divisions to mankind. It is encouraging that responsible policy-makers on both sides act on that understanding even in the present period of Soviet-American tension.

Soviet physicists, with their families, are planning to move to Batavia

to participate in the first collaboration on an experiment at N A L. This excellent international collaboration has been possible because all participants have felt that they had something to gain. The U.S. bargaining position has been particularly strong in this regard because its high-energy physics program clearly has much to offer. Entré of the U.S. physicists to Russian and European machines depends on contributions of U.S. groups to the field. The world is sufficiently competitive on the national level that the United States cannot be expected to be welcomed as a scientific suppliant. It must offer a scientific and intellectual *quid pro quo* and must be perceived by others as contributing its financial fair share in this field as well as others in order to have the advantage of participation in the international effort.

6   Institutions and Their Relationships

Elementary-particle physics grew out of academic research in nuclear physics and cosmic rays, and in spite of the enormous development in scale of the field, its roots have remained in the universities, the traditional centers for the pursuit of knowledge and the drive to satisfy man's curiosity about the physical universe. The early accelerators and the devices for studying the behavior of the particle beams emerging from them were conceived, designed, and built at the universities. Some accelerators still are located at universities, and these have obvious advantages deriving from their close proximity to the campus and their role in the training of students. However, some of these accelerators have become relatively less important as emphasis has shifted to experiments utilizing the higher energies, greater intensities, and more sophisticated equipment that are difficult to provide at a university laboratory.

6.1   NATIONAL ACCELERATOR FACILITIES

New types of laboratories have been created in order to provide the necessary facilities on a wider scale. The modes of management span the range between a pure university research laboratory serving only one institution and a national accelerator laboratory serving the whole nation. It will be useful to describe briefly the management and operation of the large accelerator laboratories (also see Table I.4).

6.1.1   *Argonne National Laboratory*

The ZGS (Zero Gradient Synchrotron–a 12-GeV proton accelerator) is managed as part of a large multipurpose laboratory by the Argonne Universities Association (AUA), a consortium of midwest universities. The contract is administered by the University of Chicago. The AUA maintains a High Energy Committee, which considers long-range plans and makes recommendations to the parent group. A Program Advisory Committee, consisting largely of outside users, advises the laboratory director for high-energy physics on the experimental program. A totally outside Visiting Committee annually reviews the program and reports to the managing organization. The facility is used by groups in all parts of the country, but with midwestern concentration.

6.1.2   *Brookhaven National Laboratory*

The AGS (Alternating Gradient Synchrotron–a 33-GeV proton accelerator) at Brookhaven is similarly managed by a consortium of nine northeastern universities, Associated Universities, Incorporated (AUI), which, however, has the administrative organization to manage the facility. A Board of Trustees consists of administrators who are in general senior officers of their corresponding institutions and scientists in equal numbers. From time to time, the AUI appoints a special committee to study long-range planning for BNL. A high-energy advisory committee accepts proposals for experiments and advises the director as to long-range policy affecting the program. The facility is available for all groups with no bias as to origin. Typically, 25 percent of the research capabilities are used by in-house researchers.

6.1.3   *The Cornell Electron Synchrotron*

This laboratory is managed as a university laboratory by the Trustees of Cornell University and is devoted exclusively to high-energy physics research. In fact, about 20 percent of the research capabilities of this newest accelerator have been used by outside groups. The NSF contract specifies that this may rise to as much as 50 percent.

6.1.4   *Cambridge Electron Accelerator*

Harvard University is the contracting institution, but the management decisions are made by the Executive Committee alternately chaired by a high administrative officer from either Harvard or the Massachusetts Insti-

tute of Technology (MIT). This consists of three physicists and three administrators from each institution. A Visiting Committee reports to the Presidents of Harvard and MIT. The Program Advisory Committee is equally divided between local and outside people, and in effect, the recent utilization has been equally divided between Harvard, MIT, and outside groups; some of the latter also derive from the greater Boston area.

6.1.5 *Lawrence Berkeley Laboratory*

This is technically a multipurpose laboratory; management is via contract with the Board of Regents of the University of California. The strong physics group at Berkeley dominated the use of the Bevatron (a 6-GeV proton accelerator) in its early years, although there has been increasing use of this facility by outside groups up to the present ratio of about 50 percent. A Schedule Committee about evenly divided between outsiders and insiders reviews proposals for experiments and advises the director on the experimental program and on major policy decisions affecting the facilities.

6.1.6 *National Accelerator Laboratory*

This is a single-purpose laboratory to construct and operate the 200-GeV accelerator. It is managed by a 50-university consortium, Universities Research Association (URA), divided into 15 area groupings. The Trustees (a senior administrator or a scientist from each group) are elected by a Council of Presidents, who additionally elect six "at-large" Trustees from industry or national laboratories. A Program Committee of high-energy physicists from outside the laboratory advises the Director about proposals submitted for experiments. There is also a users group with working committees on various aspects of utilization.

6.1.7 *Princeton–Pennsylvania Accelerator*

This is a 3-GeV proton accelerator. The contract is held by Princeton University; the Director is chosen by consultation between the Presidents of Princeton and the University of Pennsylvania. The major policy committee is appointed by the two Presidents and is composed of scientists and administrators from both institutions. The Science Subcommittee has four members from outside to advise on the program. About 30 percent of the recent experiments were performed by groups other than those at Princeton University and the University of Pennsylvania. The limitation on funds available to the Atomic Energy Commission (AEC) high-energy physics

programs led to the decision to close down this accelerator at the end of fiscal year 1971.

6.1.8 *Stanford Linear Accelerator Center*

This 21-GeV electron linear accelerator is managed by the Trustees of Stanford University but is designated, contractually, as a national facility. A Scientific Policy Committee (SPC) consisting largely of outsiders oversees this by advice to the President of the University and by reports to the AEC through the President. Scheduling is by the Director on advice from a committee having a small majority of outside scientists. The actual usage of the facilities by outside groups, either as entities or in collaborations, is somewhat above 50 percent of the available beam time.

6.2 UNIVERSITY PARTICIPATION

These large national facilities have internal research groups that carry on a substantial part of the research program. Nevertheless, even at these laboratories, most of the physics research continues to be carried out by university groups in spite of the difficulties of working at a distance from the campus and difficulties associated with the increasing magnitude of effort required to carry out meaningful experiments.

University participation is of great benefit to elementary-particle physics, to the universities themselves, and to the national laboratories. Any research program needs the vitality that comes from a continuing supply of young people with fresh ideas, with alert and inquiring minds, and with the enthusiasm for innovation needed to counteract the pressures of conservatism that tend to creep into a more static environment. These young people must come primarily from the universities; they are students and recent doctorate recipients. On the other hand, if the universities are to fulfill their roles satisfactorily as educational institutions and as centers of scholarly research, they must participate in the search for knowledge wherever the frontiers may lie; it would be most inappropriate if their active participation in elementary-particle physics were to diminish.

One of the great assets of a modern university is the scientist-teacher, who combines the obligations of research with formal undergraduate and graduate teaching. In the best examples of this, the two activities mutually reinforce each other. The formal teaching activities with their demand for clarity and incisiveness in the organization of a body of knowledge and in the interaction with ever-fresh young minds must fortify and orchestrate with the frontier research activity and its rigorous demands, excitement,

vitality, and the more intimate association of professor, assistant, and student as collaborators.

As for the national laboratories, a close relationship with the universities is vital. In a recent assessment of relationships between federal laboratories and universities, the Federal Council for Science and Technology concluded* that a different atmosphere exists in those laboratories where this relationship is close.

In talking with persons in these laboratories, one senses a purpose, an alertness, an enthusiasm, a striving for excellence, a dedication, a feeling of accomplishment . . . and excitement, a sense of life and involvement. This atmosphere, fostered by close association with the academic world, highly desirable and not easily attained, was seldom transmitted . . . in laboratories lacking close relationships.

It is indeed fortunate that this close relationship exists in high-energy physics, and it is extremely important that it be maintained.

At the present time, there are approximately 50 universities heavily involved in research in elementary-particle physics, with an additional large number engaged in research at a lower level or with hopes of entering the field. The total number of institutions already participating in some degree is about 125, of which some 90 receive direct federal support.

6.3 MODES OF UNIVERSITY PARTICIPATION

A university may carry out research in high-energy physics in one or a combination of the following ways: by an experimental program based on a local university accelerator; by one or more user groups, who carry out experiments at the large accelerator centers; and by a theoretical program.

6.3.1 *Bubble Chambers*

Until a very few years ago, a majority of the university user groups involved in experimental research at the large accelerator centers utilized bubble chambers for their experiments. The required particle beams, the bubble chamber facilities, and the film development are all provided by the accelerator laboratory, while analysis of the pictures is usually carried on at the home institution. Enormous contributions to our knowledge of elementary particles have been made through work of this kind. The technique has the special advantage for a university group that most of the

Education and the Federal Laboratories, The Federal Council for Science and Technology (March 1968).

work can be performed at home with only a few weeks or less spent at the accelerator to obtain the photographs. Another advantage from the point of view of the laboratory is that many groups in sequence can use a given beam and bubble chamber facility to obtain many sets of pictures without major changes in the installation. Also, with one set of pictures (usually several hundred thousand) the university group frequently can obtain more than one type of result and several publications—an advantage for graduate students' theses. A typical university bubble chamber group may consist of about three senior physicists, two younger PhD's, and six to eight graduate students. In addition, the scanning and measuring effort required to extract data from the photographs will need further personnel, so that a total of more than 30 people may be involved, and the yearly budget for the group can be well over $300,000. (A small group will, of course, spend considerably less.) A crucial requirement for each group is access to adequate computer facilities. There are large variations in size among the groups engaged in this type of research, and recent years have brought considerable change in the methodology and requirements. In particular, a number of these university groups have acquired automatic scanning and measuring machines that make it possible to handle many more bubble chamber events with fewer service people.

In the last two or three years, the role of bubble chamber physics has been changing, largely because most such experiments require very large numbers of pictures to resolve the questions of physics to which they are directed. A need for as many as one million pictures in a single run is not unusual, and these megapicture exposures require elaborate automatic scanning to extract the physics from the film in a reasonable length of time. The number of these devices that can be used effectively is limited, and only a few universities are equipped to use such complex machines efficiently. The smaller runs that can be handled by most university groups tend to be noncompetitive in terms of physics. This disadvantage overrides the many advantages of bubble chamber physics for many university-based groups, and some of them have been shifting their attention to other techniques requiring much more on-site participation of the group at the accelerator.

6.3.2 Counters and Spark Chambers

The other common technique employed by university experimental groups involves the use of counters, spark chambers, and complex electronic systems to obtain the experimental data. Usually the array of equipment, some of which may be provided by the accelerator laboratory, is set up in a beam that has been specially designed for the specific experiment

to be performed by the group. Another group with a different experiment may be able to use part of the beam transport equipment (magnets and vacuum pipe, for example), but that usually requires considerable rearrangement and a completely different array of detecting apparatus. Although much of the preparation for experiments of this type can be carried out at the university, many components being constructed there, a more or less extended stay at the laboratory is required not only during the data-taking stage but during a prior period of installation and testing. The total time for such an experiment from initial proposal to publication of results frequently amounts to two or three years, during which attendance at the laboratory by some of the group will be needed for perhaps one quarter to one third of the time. While the overall group may be larger, the active participants in a given experiment are, on the average, five physicists, two graduate students, one engineer, and two technicians. Again, there are wide variations in the size of groups. Backup support at the university will include a machine shop, an electronics shop, computer services to analyze data, and perhaps scanning and measuring equipment to reduce data from spark chamber photographs. Because recent developments in instrumentation make it possible for these groups to accumulate data at an enormous rate, once their apparatus is set up and debugged, the need for large computer capacity to analyze the data has become just as crucial as it is for bubble chamber physics. A typically active group may have a yearly budget of $200,000 to $300,000 or more.

6.3.3 *Theoretical Work*

Since theorists do not need expensive equipment, a theoretical group can be supported on a relatively small budget. However, theoretical and experimental groups complement and support each other, so that most successful university programs include both. It is not easy to establish a good theoretical group in high-energy physics in the absence of an experimental program. Nevertheless, a university with a small budget can make a start in the field with only theoretical staff, who, at the same time, make a significant contribution to the educational effort. However, a decision to proceed in this way at this time should take into account the special problems in regard to job placement of particle theorists (see Section 7.3).

6.3.4 *Collaborations*

University user groups frequently carry out their experiments as collaborative efforts, either by two (or more) university groups making a joint undertaking or by a university group collaborating with a research group

at a national laboratory. There are decided advantages to these arrangements that provide opportunities for a group to increase the number of experiments it can undertake (with given funds and manpower), to broaden the types of physics it can investigate, and to widen the experience of both professors and students. Members of a collaboration can also benefit from certain specialties in which a given group may have strength, such as advanced techniques in electronics, detector design, data analysis, computer skills, or beam design.

6.4   SCHEDULING OF EXPERIMENTS

The final decision to perform a given experiment is made by the Director of the accelerator laboratory at which it is to be performed. The formal process by means of which the Director arrives at this decision begins (possibly after many informal discussions have taken place) with the submission of a written proposal to the Director by the group or groups wishing to carry out the experiment. Such a proposal is required of any group whether it be one from within the laboratory or from outside.

The proposal presents in some detail the physics objectives of the experiment, its design, estimates of the capabilities of the various components of the proposed apparatus, the design of special beams or other facilities that will be required, the available manpower with an indication of special capabilities of individuals, the level of support that the group can command, the level of support expected from the laboratory, the amount of running time needed, an estimate of the date at which the group will be prepared to begin the experiment, and so on.

The proposal is usually examined by members of the staff of the laboratory who are capable of independently estimating what will be required of the laboratory if the experiment is carried out. It is also studied by the members of the Program Advisory (or Scheduling) Committee. In both of these studies, members of the group proposing the experiment may be consulted to obtain details or clarification of specific points.

The Program Committee meets at regular intervals with the Director and at each such meeting examines collectively, and disposes of, a batch of proposals. The information gathered by the laboratory staff is presented, and the opinions formed by members of the Committee are discussed. The proposers may be called into such a meeting to answer questions that have arisen about their proposal or to make a more detailed presentation if that seems to be needed.

At such a meeting, the Committee reaches recommendations based on its overall view of the program of the laboratory and the way in which a

particular proposal fits into and will affect that program. They try to evaluate the quality of the physics, the reliability of the group, and all other relevant factors. They may recommend that the proposal be changed or that several groups be encouraged to get together and write a new, joint proposal or a variety of other nonnegative arrangements that one can easily imagine. Of course, the Committee may recommend approval or outright rejection.

6.5 POLICY MAKING

Since their research opportunities and, in fact, their careers, depend so strongly on the policies of the accelerator laboratories, such as the makeup of the Program Committees, the high-energy physicists have a deep and abiding interest in the way in which policy is made in each major laboratory. This and other aspects of the "sociology" of high-energy physics such as the issue of apportioning of responsibility for new major facilities between in-house and outside groups, the balance between exploratory and quantitative experiments (see Section 6.6) and the associated issue of optimizing the support of available facilities in the country, questions concerning the balance in influence between established groups and new groups, and all other decision-making processes in elementary-particle physics will continue to call for very close attention from the entire high-energy physics community. For this purpose, a basic requirement is good communication between the agencies supporting high-energy physics, the national laboratories, the universities, and the individuals working in this subfield.

   A number of mechanisms are at work providing for communication and helping in the decision-making process. Policy matters at each of the major national laboratories are either in the hands of a consortium of universities or some other group commissioned to act in the interest of the larger physics and science communities. Each of the laboratories has a more or less formally organized user group, and some of these groups participate very actively in planning and policy making. The laboratories are also very dependent upon an assortment of advisory committees, many of them formed with the help of the appropriate user organization. Another mechanism for communication is provided by the Division of Particles and Fields of the American Physical Society, which is an open organization and has the other advantage that it is independent of any one laboratory or institution. Finally, there is the High Energy Physics Advisory Panel (HEPAP), which is advisory to the Research Division of the U.S. Atomic Energy Commission (AEC). It is made up of a broad range of elementary-

particle physicists from institutions throughout the country, and has the opportunity to establish contact between the physicists and the A E C, which is the government agency carrying most of the burden of support of the high-energy physics program and is sometimes considered the Executive Agent of the entire program.

Because the A E C is the principal source of funds for high-energy physics in this country, H E P A P is in a position to have a strong influence on decisions concerning the distribution of available funds, as long as it speaks with a strong voice and with the support of the community. These decisions determine such important matters as the construction of new facilities, the balance in utilization of different existing major facilities, and even the question of maintaining or closing down existing accelerators.

As far as the physics program itself is concerned, the most influential of the advisory committees is the Program or Scheduling Committee of each of the laboratories, whose functions were described in detail in Section 6.4.

The rather complex social system that has been described here seems to work reasonably well as judged by the success of the U.S. program in elementary-particle physics. In many respects this system has stimulated scientific collaboration and a unity of goals in the national particle-physics community to a degree that is perhaps unique in science. Attempts have certainly been made to emulate the system in the international community, as we have already indicated in Section 5.4. However, the system is starting to show some signs of strain as the result of such issues as the reduction in available funds and services for each group, the intensification of competition for accelerator time, underutilization of accelerators, and more limited employment opportunities.

It will be important for the future of the subfield that the system of communication continue to demonstrate its viability or, if it is not viable, that it be corrected. There are several aspects of the system that are particularly vulnerable and should be mentioned: The selection of committee membership is usually made on the basis of the scientific reputations of candidates. This tends to select established physicists who then become more visible and are selected for additional committees. One of the problems here is to find candidates who are not only knowledgeable and experienced enough to provide good advice but also are willing and able to do the necessary work at the expense of their personal research activities. This bespeaks the choice of people who have already taken on some administrative responsibility. Thus, there are real dangers, even within the rather elaborate system of communication that has been set up, that decisions will be made on the basis of advice from physicists who are losing

touch. Every effort must continue to be made to bring fresh views into these committees.

A problem on the other side can arise when committees are formed with too much emphasis on equal representation for all parts of the high-energy physics community. Such a committee might have difficulty agreeing that certain proposals should be rejected outright, especially when it means that groups will be eliminated. The view that carrying out an experiment at a national accelerator is a right, a view sometimes expressed by marginally productive groups, is not tenable and, if accepted, would seriously water down the quality of the physics.

Another tendency that committees must be prepared to resist, especially in hard times, is the feeling that an experiment or program must be one that is guaranteed to produce definite results. This can militate against the high-risk experiments that, in the long run, are essential to the life force of physics.

As long as there is support from the community for avoiding these lines of least resistance, it can be assumed that the traditions of physics will prevail, and that the ultimate criterion for recommendations and decisions will be the quality of the physics that can be expected.

6.6 EMERGING PATTERNS OF POLICY AND PARTICIPATION

The pattern of relationships between the established accelerator laboratories and the universities has become rather well formed, but the normal evolution of these patterns is expected to be interrupted and changed as the result of the underutilization of all the accelerators, including complete shutdown of at least one, caused by funding problems. This will reduce the number of opportunities for university groups to participate in high-energy physics research, thereby also reducing the chance to train new people and to try out new methods. At the same time, the National Accelerator Laboratory (NAL) is expected to introduce a countervailing influence, since it will offer exciting new opportunities; but because of the scale of most experiments at the new laboratory, these opportunities will take on a rather different character.

The NAL will be the spearhead of the exploratory work in the United States, while the other laboratories, working at energies less than 33 GeV, will become more and more involved in quantitative experiments.

A balance must be maintained between the exploratory experiments and the important quantitative work discussed in Section 1.4, and the choice of a balance point will have a profound influence on the subfield because the quantitative work will in general occupy many more groups

than will the exploratory work. Since the quantitative experiments are more certain of definite and useful results, they offer a good opportunity to produce good physics, to train new people, and to try out new methods. They also offer younger experimental groups an effective way to establish themselves as candidates for critical but high-risk experiments.

It becomes clear that we are in a good position to exploit this mode of operation while at the same time making full use of NAL when we note the magnitude of university interest in the field described at the end of Section 6.2. Those numbers include about 50 universities that have declared explicitly their interest in NAL by joining URA, the corporation set up to manage this laboratory. Membership in URA is not a requirement for activity at the laboratory, and it can be assumed that many other universities of the 125 have groups hoping to participate in experiments there.

During the first few years of experimentation at NAL it is likely that, aside from bubble chamber experiments, *at most* 20 experiments per year will be carried out there. This represents about one fourth of the number of high-energy physics counter and spark chamber experiments being carried out per year in all the other laboratories at the present time. The number of bubble chamber pictures to be produced during the early years at NAL cannot be estimated reliably at present. Construction of one large hydrogen bubble chamber is under way, and the 30-in. hydrogen bubble chamber was moved there from Argonne to run as soon as a beam became available. It is expected that the rate of picture taking will be appreciably smaller than one fourth of the peak rate produced at all the other laboratories taken together.

Clearly, it will not be possible to accommodate all the groups desiring to work at NAL within a reasonable length of time. The problem may be mitigated to some extent because many experiments will require groups of appreciably larger size than now exist, and some consolidation of groups from different institutions will take place. However, this makes the situation difficult for the small, independent groups and for young groups trying to make a place for themselves.

That difficulty is further intensified by the long lead time usually required for planning high-energy experiments, especially since the lead time now includes the time required to develop, design, and construct any special facilities that will be needed to carry out a program of experiments. Since the scale of most such facilities will be very large at NAL, and the technical problems very difficult, decisions must be made early, and the groups proposing the facilities must be expected to make some contribution to their development, design, and testing. Decisions on the choice of facilities based on early proposals tend to fix the character of the physics program for some time to come, especially when the number of large facil-

ities is severely limited by funding considerations. The smaller groups that are not in a position to make an early proposal or to provide help for new facilities are certainly put at a disadvantage.

Since the exploratory and innovative character of elementary-particle physics, which is one of its strongest justifications, requires the fresh ideas and open mindedness provided by a steady influx of new people, it is imperative that the system should be responsive to the needs of smaller and younger groups. This injunction applies to all the accelerator laboratories but may be most difficult to impose at NAL both because of the pressures from established groups and because of the large costs associated with a given experiment. Its implementation requires that the universities, which are the sources of new people, continue to play a very strong role in all aspects of high-energy physics.

It is natural that a subfield as fundamental as elementary-particle physics should be centered in the universities. Conversely, because it is so fundamental a field, universities offering graduate programs in physics must be in a position to keep in touch with the subject. The interest of an increasing number of universities in establishing this contact is manifest from a comparison of the 90 universities receiving direct federal support cited in the HEPAP Report (1969) with the 40 universities cited in the Walker Report (1966).* (The 1969 report cites 125 universities as having "activity in the field.") This increase is, of course, a consequence of national policy, in the past decade, to broaden and strengthen the university base of science in the United States. However, the growth trend cannot continue under present funding restrictions since existing groups are operating at constant or reduced levels, and very few new groups are being supported. Therefore, new relationships must be established between the universities themselves as well as between the universities and laboratories in order to accommodate universities that have not yet started work in this field and those with inadequate support.

There is constant pressure for increasing the total number of research groups. Where this pressure can be justified, for example by emerging new talented groups, a positive response is desirable in spite of funding strictures. This response should be made in the context of the total program. That implies a continuing review of all programs by the funding agencies, leading to reduced support wherever there is clear evidence of reduced scientific productivity. Difficult as they are, these decisions will have the backing of the community at large as long as it is clear that all groups, including the well-established ones, are subject to the same treatment.

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*"Elementary-Particle Physics," in *Physics: Survey and Outlook. Reports on the Subfields of Physics* (National Academy of Sciences–National Research Council, Washington, D.C., 1966), p. 53.

7 Training and Manpower

7.1 NATURE OF THE QUESTIONS

In 1966, the predecessor to this Panel (the Walker Panel), reported that in connection with manpower in high-energy physics it had been asked to address itself to the following two questions:

1. Will there be an adequate supply of trained doctorates from which a selection can be made to staff the various programs envisioned in this [the Walker] report?
2. Can such a supply be achieved without diverting large numbers of scientists away from other fields of research?

The Walker Panel gave affirmative answers to both questions, and subsequent events have borne out those answers. In the 1969 report of the High Energy Physics Advisory Panel, it was found that about one half of those receiving the PhD on the basis of a thesis in high-energy physics were being employed in fields other than high-energy physics.

Since 1969, the manpower situation has changed in all fields of physics in such a way as to make the questions asked of the Walker Panel seem almost ludicrous. In view of a difficult funding and employment situation faced by all research fields, there no longer appears to be a question about an adequate supply of trained scientists in any field. However, we should like to take a stand against facing these questions on the basis of numbers alone. We feel that the real manpower questions concern quality rather than quantity: Will the people who can maintain the highest standards of performance in the field continue to be attracted to it? Will their training qualify them for a broader role? How will the quality of physics as a whole be affected by a major cutback in opportunities to work in the field?

7.2 SOME FACTS

To put our attempts to answer some of the questions concerning training and manpower into perspective, we cite some of the relevant information concerning the existing manpower situation. The manpower involved with elementary-particle physics (fiscal year 1970) is summarized in Table I.3.

There are approximately 1700 PhD physicists who can be identified as working in institutions having projects supported by federal high-energy physics funds. It can be assumed that this includes most of the experi-

TABLE I.3   Manpower in Elementary-Particle Physics (Fiscal Year 1970)

	Total Manpower	PhD Manpower[a]	Graduate Students[b]
1. National Accelerator Laboratory	695[c]	56	0
2. Stanford Linear Accelerator	1330	104	28
3. Brookhaven AGS	1276	103	0
4. Argonne ZGS	897	62	4
5. Berkeley Bevatron	1145	102	92
6. Cornell Synchrotron	?	20	10
7. Cambridge By-pass Storage Ring	146	18	0
8. University groups	(3610)[d]	1255	1103
TOTAL	~ 10,000	1720	1237

[a] The PhD classification includes physicists who hold PhD degrees and/or are working at a PhD level.
[b] The graduate student classification includes students possessing an MS degree or equivalent training. Good statistics on graduate students in the university groups are lacking. Note that all graduate students performing experiments in high-energy physics are in some way associated with one of the accelerator laboratories, but they are counted here on the basis of the channel through which they receive support.
[c] NAL will not be in operation until fiscal year 1972.
[d] Estimated.

mentalists in the field, since the federal government is almost the only source of support for them. The special interests of the 1700 physicists are distributed roughly equally among the general areas of specialization—theoretical, bubble chamber, and counter plus spark chamber work—except for a relatively small but important group working in the field of accelerator design and development. There are indications of a trend away from work with the bubble chamber into work with the counter and spark chamber, although this trend has not been documented.

There are approximately 1100 graduate students working at the thesis level on projects supported by federal high-energy physics funds. A substantial number of students supported by other funds should probably be added to this. Well over 250 PhD's per year are being granted in the field. Of these, approximately half have been going into other fields, as we have already noted.

Most of those who have stayed in the field appear to have taken temporary postdoctoral appointments such as fellowships or research associateships. These appointments normally last for two years but are often followed by an additional one or more years of postdoctoral work at the same or another institution. This postdoctoral opportunity appears to be almost a necessity for the training of anyone planning to stay in the field and even for some who later leave the field. The available information on employment indicates that most of the experimentalists in this field have succeeded in finding such appointments immediately after receiving the

PhD. The theorists have been finding it more difficult in the past year or two. Those who have completed the postdoctoral internship have not been finding many openings, and some have been switching to other work. The numbers are not determined, although several organizations are trying to determine them. The situation is even more difficult in regard to tenure appointments.

While the situation described here is a difficult one, it should be recognized that there is still competition among the institutions in the field for very good or very promising physicists. An increasing amount of this competition is coming from Europe because of the strong support for particle physics there. However, the overall funding and employment trend is in such a direction as to increase the flow to other fields of those trained at the doctorate level in elementary-particle physics.

7.3 ROLE OF UNIVERSITIES

In view of the foregoing information, which reflects a situation existing not only in particle physics but in all of physics and other sciences too, one may ask: Why should training in the field be encouraged? An answer to this has been eloquently set forth in an editorial titled "The Real Brain Drain" in *The New York Times* of November 15, 1970:

> Large numbers of scientists, engineers and educators are being shaken out of their jobs in an upheaval that must be viewed as something far more serious than a temporary dislocation in employment born of the business slowdown.
>
> The issue transcends the personal hardships of highly educated professionals seeking to support their families by working as handymen, or of foreign scientists returning to their home countries in the hope that they may be able to use their once prized talents.
>
> The deeply troubling, fundamental question is whether a nation can allow itself to be the pawn rather than the master of its destiny. Young Americans today are told that Ph.D.'s are a drug on the market. Only a few years ago, to become an engineer or a scientist or indeed a teacher was not only to fulfull one's goal of intellectual satisfaction but to serve nation and society. The inevitable conclusion, if the downgrading and disuse of highly educated manpower gathers force, will be that advanced skills are relevant only to competition for hegemony in arms or space.
>
> Such a concept is absurd when there is such apparent need for the application of scientific, technological and philosophical brain power in the pursuit of peace and dignified living conditions.
>
> American cities are in aimless drift. The slums not only dehumanize those who live in them but contaminate the physical and economic security of the entire urban area. Transportation and communications are declining as though their problems were unrelated to the huge untapped potential of innovative planning. Pollution of air and

water eats away at the environment, just as the pollution of mind and word corrodes relations between people.

In the face of such desperate, though easily defined, needs it is intolerable that brain power be put in mothballs like unwanted warships. If under such conditions the brain drain is allowed to proceed, this will clearly tell new generations that America no longer wants the best of their talents, inventiveness and reason. To be incapable of employing the country's intellectual resources for peace and progress as readily as for war and national glory is to clamp an embargo on hope and faith.

In regard to this point of view, one may again ask: Why particle physics? There are several reasons. One relates to the general need to provide physics education in depth in order to develop the "scientific, technological and philosophical brain power" to which the editorial refers. Particle physics is one of the subfields that goes to the philosophical and epistemological roots of physics, and every physicist should be given the opportunity to learn about it.

The existence of a viable, creative field of elementary-particle physics depends strongly on the flow of young people having the creative drive as well as the courage and stamina to overcome the severe demands associated with its large scale of operations. This increasing scale has led to continual changes in style to which the younger physicists adjust more readily than their teachers. Furthermore, it is frequently the younger generation that introduces the new perspectives, asks the fresh questions, and arrives at the entirely new approaches that give any scholarly field its vitality.

To maintain the required flow of people into elementary-particle physics, the universities having the required resources must continue to provide vigorous training and research activities in the field. In fact, since the new horizons associated with studies near the boundaries of knowledge attract the most creative and imaginative students to physics, it is essential that universities keep open the options between the various challenging sides of physics, including elementary-particle physics, if students of the highest quality are to be attracted. The students must feel that they will have the opportunity to learn about, and test themselves against, one of the great unknowns of nature. Every effort must be made to keep alive this challenge that characterizes what is best in physics.

7.4 CHARACTER OF THE TRAINING

Any description of the character of the training in elementary-particle physics must make a sharp distinction between theorists and experimentalists. The training of the experimental high-energy physicists probably war-

rants the closest attention here because it requires all the paraphernalia of high-energy physics.

The nature of a high-energy physics experiment is such that a graduate student specializing in this subject inevitably works within a group, although the size of a group may range from just a few people (say 4 or 5) to as many as 25. He usually participates in a series of experimental runs at an accelerator before he is given the responsibility that justifies his making a thesis out of some particular experiment included in one such run. He will be involved in the planning, data taking, and the data analysis in some or all of these runs. If it is a counter spark chamber group with which he is associated, he may help in the design and development of equipment, and he may participate in computer interfacing and software development. In a bubble chamber group, he may spend a great deal of his effort on developing programs or procedures for sorting out the information contained in the pictures. In either case, he may become deeply involved in the design of an appropriate particle beam used for a run, and he may be required to take some responsibility for monitoring the beam. In the planning phase of an experiment, the student frequently becomes familiar with the intricacies of the cost analysis. In the data handling, he often becomes an expert on the capabilities of large computers.

It is not surprising that those who represent the best products of this training require more than a period of graduate study to become expert in all its aspects. Therefore, it is almost standard practice for the PhD to seek a postdoctoral research appointment for a period of at least two years. Frequently, the period is extended to three or four years.

The well-trained high-energy experimentalist is likely to acquire some experience in such diverse disciplines as theoretical physics, electronics, solid state, cryogenics, electrical engineering, computer science, information theory, cost accounting, or even psychology! He has been educated in the search for new and unusual methods for solving a technical problem and in the ways to stretch existing techniques to the utmost, as well as getting the most out of available funds. He is disciplined, as required for a large-scale group effort, in all the troublesome aspects of technical teamwork.

In contrast to the experimentalist, the theorist must be prepared to work by himself. It is characteristic of theoretical physics that it is a relatively lonely game even though it is often carried out in a large laboratory.

The training for work in particle theory requires an exceptional amount of formal study because almost all of the well-organized theory in physics should form the underlying basis for research in this field. In order for him to acquire the variety of experience needed to be a truly independent scholar, the particle theorist usually finds it desirable to spend two to four years in postdoctoral training.

When this extensive training is carried out in depth and breadth as it should be, the particle theorist is in a good position to serve universities in the role of an educator, who can provide the student body with contact with elementary-particle physics—even in those universities lacking the resources needed for an experimental program in this subject, since the theorist's research does not require large financial resources.

The well-trained theorist should be familiar with the experimental side of the subject and able to handle both phenomenological and fundamental problems. His experience with new mathematical techniques and new concepts of nature would then prepare him especially well to deal with unusual problems whether these are problems in his special field or in applied science.

7.5 PhD MANPOWER NEEDS

Federal funding patterns for elementary-particle physics in the next few years will determine the manpower needs or, more realistically, the employment opportunities for experimentalists holding a doctorate in this field. Several possible patterns will be discussed in Chapter 9, and we limit our remarks here to the impact of three of these representing the full range of possibilities:

1. The "full capability" program described in Section 9.5 would require an increasing number of PhD physicists at a rate corresponding roughly to the rate of increase of funding (in terms of purchasing power) for operations and equipment, namely, about 10 percent per year.
2. The "level" pattern of Section 9.7, by the same token, would require a rate of increase of about 6 percent per year.
3. The "6 percent annual decline pattern" discussed in Section 9.8 would require about the same number of PhD's as is working in the field at the present time.

To see how these figures compare with the available manpower, we may use the estimates of Section 7.2. The estimate of 300 for the number of new graduates compared with the 1700 PhD's identified as being supported by federal high-energy physics funds, would correspond to a 16 percent per year rate of increase. This is a reasonable rate to apply to the experimentalists alone, since the distribution between theorists and experimentalists of the identified graduate students and postdoctorals is about the same. Therefore, the scenario of case 1, the full capability program, would require most of the new graduates. Case 2 would require about half of them, and that would correspond roughly to past history; half of the

people trained in this field have been going to other fields. Case 3 would offer no new employment opportunities. Only by releasing substantial numbers of those who are now employed would it be possible to make room for young people with new ideas.

It should be noted that recognition of the present funding situation has already had an impact on the number of new graduate students going into physics. The rate of production quoted above refers to the students now irrevocably committed. The rate should show a substantial decrease after about four years (compare Table I.7). If, at that time, there is good support for a sound program in elementary-particle physics we could find that there is a shortage of manpower.

The situation with regard to particle theorists is different in several ways. There is no reason to expect that the total operating expenses for high-energy physics are closely correlated with the number of theorists. The need for theorist manpower is largely determined by the educational demand since some particle theory is essential to any graduate study program in physics. In many universities the only faculty member having an active connection with elementary-particle physics will be a theorist.

Since the theorist who wants to remain active in research hopes to have research students when he is in a university teaching position, this educational function has tended to produce a high multiplication rate for particle theorists. This is not reflected accurately in the figures given in Section 7.2 because there are many theorists and theoretical students who are not supported by federal funds. Although those having such support comprise roughly one third of the total, we find that about 55 percent of the theses in elementary-particle physics in 1969 are on theoretical subjects.

The result is a rapid exponentiation of the number of particle theorists, and it is difficult to see a way in which to continue to absorb all of them into this field if the number of universities and colleges seeking staff in the field does not continue to increase rapidly too.

There will be some increase in the demand for theorists associated with any growth in the experimental program both because new data will generate new theoretical problems and because new theoretical ideas are always needed to stimulate new experimental programs. There is probably a greater and more constant need for fresh ideas in particle theory than in any other field of physics, since progress is made by trying to probe with many ideas in many directions. The need for new ideas carries with it the need for succeeding generations of theorists.

Although it is clear that there is a need for new particle theorist manpower, it is virtually impossible to give a quantitative estimate in terms of program requirements. A growth rate substantially smaller than that for

experimentalists would appear to satisfy identifiable needs, but a con-
tinuing production of both theorists and experimentalists is essential if
the field of elementary-particle physics is to retain its vitality.

8 Facilities for the Future

8.1 OPERATING ACCELERATOR LABORATORIES

The most essential tool for elementary-particle physics is the accelerator
and its ancillary experimental facilities. Table I.4 shows the operating
high-energy accelerator facilities of the world. In addition to these, the
200–500-GeV proton accelerator at the National Accelerator Laboratory
(NAL) was put into limited operation in 1972.

8.2 NEW ACCELERATOR TECHNOLOGY

The possibility of going to even higher energy, or of upgrading existing
accelerators in one way or another depends on the development of new
accelerator technologies because the cost of further extensions by con-
ventional (i.e., well-established) technology would appear in most cases to
be prohibitive. There appear to be several promising methods that are now
in various stages of investigation. The successful development of these
methods would allow economies in the construction costs of new accel-
erators and would also lead to a substantial reduction in the consumption
of power.

In addition, these new techniques would permit the conversion of exist-
ing accelerators to higher energy. In such conversion, the magnet ring of a
synchrotron would be replaced with new cryogenic or superconducting
magnets that can operate at fields three to four times higher than conven-
tional magnets. Such replacement of the magnets allows the tunnel and ex-
perimental areas to be preserved, giving considerable savings over entirely
new construction. Such replacement at a suitable time could boost the
energy of the Brookhaven AGS to 100 GeV or the NAL to 1000 GeV. In
a similar way, developments of superconducting low-loss rf cavities would
permit higher energy and high-duty-cycle conversion of linear accelerators.
For example, at a suitable time, SLAC could be converted to 100-GeV op-
eration with a duty cycle in excess of 6 percent.

The new techniques of magnet construction seem especially well adapted to the construction of storage rings. Advances also seem likely in the techniques of accelerating the beams in storage rings after stacking, leading to greater freedom in choosing the injection energy.

It must be emphasized that the costs directly associated with the construction and operation of the accelerator itself are only a fraction of the total investment in an accelerator laboratory. The savings in construction of new accelerator facilities that can be made by improvements in accelerator technology alone, therefore, are not so great as one might hope. However, some of the technical developments associated with the accelerators, especially the development of superconducting magnets for beam

TABLE I.4   Accelerators Operating above 1.5 GeV

Accelerated Particle	United States			Western Europe			Soviet Union		
Proton	PPA[a]	3	GeV	Saturne	3	GeV			
Proton	Bevatron	6.2	GeV	Nimrod	7	GeV	ITEP	7.5	GeV
Proton	ZGS	12.7	GeV				JINR	10	GeV
Proton	AGS	33	GeV	CERN-PS	28	GeV	Serpukhov	76	GeV
Electron				Bonn	2.3	GeV	Kharkov	2	GeV
Electron	CEA[b]	6.3	GeV	NINA	5	GeV			
Electron	Cornell	10	GeV	DESY	6.2	GeV	Yerevan	6	GeV
Electron	SLAC	22	GeV						

[a] Shut down at end of fiscal year 1971.
[b] Being used only as an intersecting beam device.

Abbreviations, Names, Locations

United States:

PPA	Princeton–Pennsylvania Accelerator, Princeton, New Jersey
Bevatron	Lawrence Berkeley Laboratory, Berkeley, California
ZGS	Zero Gradient Synchrotron, Argonne National Laboratory, Argonne, Illinois
AGS	Alternating Gradient Synchrotron, Brookhaven National Laboratory, Upton, Long Island, New York
Cornell	Cornell University, Ithaca, New York
CEA	Cambridge Electron Accelerator, Harvard University, Cambridge, Massachusetts
SLAC	Stanford Linear Accelerator Center, Stanford, California

Western Europe:

Saturne	Commissariat à L'Energie Atomique, Saclay, France
Nimrod	Rutherford Laboratory, Chilton, Berkshire, England
CERN-PS	Proton Synchrotron, CERN, Geneva, Switzerland
Bonn	Physikalisches Institut, Bonn, Germany
DESY	Deutsches Elektronen-Synchrotron, Hamburg, Germany
NINA	Daresbury Nuclear Physics Laboratory, Daresbury, England

Soviet Union:

ITEP	Institute of Theoretical and Experimental Physics, Moscow
JINR	Joint Institute of Nuclear Research, Dubna
Serpukhov	Institute of High Energy Physics, Serpukhov
Kharkov	Physical Technical Institute, Kharkov
Yerevan	Institute of Physics (GKAE), Yerevan, Armenian SSR

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transport and superconducting rf systems for beam separation, can allow additional substantial savings in the power costs for operating experimental areas.

The methods that are presently being investigated are discussed below.

8.2.1 *Superconducting Alternating Gradient Synchrotron and Storage Rings*

The size of a synchrotron is governed by the maximum strength of its magnetic field; conventional machines, such as the Brookhaven A G S, the PS at CERN, and the NAL accelerator have conventional, iron–copper electromagnets. Superconducting coils have already produced much higher *steady* magnetic fields than is possible with conventional magnets, but, until recently, superconducting magnets for time-varying fields have shown excessive power losses. Since an advantage of superconducting magnets is their potential for almost eliminating the high costs for power of the conventional type of magnet, further reduction of these losses is important for their use in accelerators. Recent work with highly stranded, twisted conductors has indicated that this problem is well understood. However, the requirements of uniformity and reliability in pulsed magnets for accelerators are very stringent and suggest that much lower repetition rates will be appropriate. Working models of pulsed synchrotron magnets have still to be constructed in order to investigate their uniformity and long-range stability under pulsed conditions. Since the problem of losses does not occur in storage rings, it may be possible to use superconducting coils for their construction in the near future. However, the cost of meeting the also stringent requirements for uniformity and reliability of storage rings is still to be determined.

8.2.2 *Cryogenic Alternating Gradient Synchrotron*

At low temperatures, the resistance of some very pure metals is reduced, by orders of magnitude, over its value at room temperature. This offers an alternate approach toward improving the economics of design for a high-field, pulsed magnet suitable for accelerators. These magnets operate at higher temperatures, and added losses due to time-varying fields are not important. The possible success of such a system depends on the availability of extremely pure metals in sufficient quantity, of engineering practicality, and of not too great cost. High-purity aluminum appears to be a possible candidate because its cost of manufacture has been significantly reduced recently. Success also depends on the ability to control various effects such as stress, impurities, and radiation damage that might be deleterious to the low-temperature resistance of the metal. Again, for use

in accelerators, the magnets must exhibit uniformity, consistency, and reliability for continual operation.

8.2.3   *Superconducting Microwave Linear Accelerator*

At present, electron linear accelerators are limited by the high dissipation of power in the walls of the accelerating structure. Because of this, the resultant beams have a short duty cycle, and the range of possible experimentation is, in turn, limited. Moreover, expensive radio-frequency sources of power are required. Superconducting walls, operating at low temperatures, in a linear accelerator could result in nearly continuous beams, and the linear accelerator could be constructed for higher energy in a given length. The beams from such an accelerator could combine some of the characteristics that are now found only separately in beams from conventional linear and circular accelerators.

Tests of single, microwave cavities with niobium walls have shown encouraging results. However, problems of fabrication, field emission, damage to surfaces by vacuum accidents, and control problems require further study and investigation.

8.2.4   *The Electron Ring Accelerator (ERA)*

It has been recognized for some time that if protons could be captured in a cloud of electrons, which is then accelerated to high energy, the captured protons would attain an energy considerably greater than that given to the electrons. In this way, one might be able to build an accelerator that would yield protons of extremely high energy, with a structure of moderate length. A practical approach to such a system, that now appears very promising, is the electron ring accelerator in which the capturing electron cloud is in the form of a ring composed of high-speed electrons. Initial success in producing and compressing such a ring and capturing protons has been attained, both in the Soviet Union and in the United States. Many problems remain concerning the transfer of such a ring into an accelerating structure besides questions of stability, and they are under investigation. The form of design of a total accelerator and its probable cost are still uncertain. An intense development program on the ERA is being carried out at the Lawrence Berkeley Laboratory.

8.3   FUTURE ACCELERATORS

There are two areas to which the application of these new accelerator techniques would be desirable. One is to the modification of existing ac-

celerators to increase their energy, intensity, duty cycle, or other param-
eters; the other is to any future superaccelerator that might be built when
the N A L accelerator has to be superseded.

The existing accelerators are going to be the source of detailed informa-
tion about phenomena within their highest energy capabilities. As new
discoveries are made in new energy regions, and as new questions are
raised about the associated physics, there will be a corresponding need for
a broad quantitative study of these higher-energy phenomena. The capabil-
ity for such work could be provided by increasing the energy of an existing
accelerator to, say, 100 GeV by making use of one of the new technolo-
gies..This would serve both to test the technology in a pilot model and to
relieve the pressure on N A L by providing additional intermediate-energy
beams that could be used to seek answers to the multitude of questions
concerning this energy region.

History tells us that experiments at N A L are likely to raise important
questions calling for even higher energy. The new technologies may make
it possible to push the N A L accelerator as high as 1000 GeV; but if ener-
gies much greater than that are needed, consideration will have to be
given to building even another accelerator unless the need can be satisfied
by means of intersecting storage rings. The choice of energy for that super-
accelerator cannot be made now; however, it would clearly need to be well
above 2000 GeV to justify the venture since that would represent only a
factor of 2 in center-of-mass energy. The new technology and perhaps new
styles of construction and operation would have a definitive influence
here. Even with the new technology, costs would be expected to be high
enough to justify an international, or even a world, collaboration. That
would not only be a challenge to the international character of high-
energy physics, it would be a most unusual opportunity to put interna-
tional cooperation in science to a significant test.

8.4 STORAGE RINGS

We have already remarked in Section 2.7 on some of the interesting phys-
ics that has been done with electron–positron storage rings. Very re-
cently, exciting new results have also been obtained at the CE R N inter-
secting proton–proton storage ring. The great advantage of the colliding
beams that can be attained in storage rings is that the laboratory beam
energy is used effectively in the reaction process. If two particles of equal
mass and total energy E collide head on, the total center of mass energy
is $2E$. In the more usual case where one of the particles (of mass M) is at
rest, the reaction energy is $\sqrt{2MC^2E}$, where E is the energy of the moving

TABLE 1.5 Colliding-Beam Storage-Ring Projects

Area	Location	Maximum Beam Energy and Particle Type	Relative Luminosity	Status	Comments
Western Europe	Frascati, Italy	1.5 GeV, e^-e^-	1.	Operating	Most powerful of operating storage rings; taken as unit of relative luminosity. Improvement program to increase luminosity by order of magnitude
	Orsay, France	0.55 GeV, e^+e^-	0.05	Operating	Improvement program under way to increase luminosity by an order of magnitude
		1.5 GeV, e^+e^-	30.	Authorized	Probable completion in 1975
	DESY, Hamburg, West Germany	3.5 GeV, e^+e^-	1000.	Under construction	Scheduled completion 1974
	CERN, Geneva, Switzerland	28 GeV, pp	10.	Operating	
Soviet Union	Novosibirsk	0.75 GeV, e^+e^-	0.1	Was operating	Being modified to device described below.
		0.75 GeV, e^+e^-	10.	Modification of above under way	Scheduled completion 1972
		3.5 GeV, e^+e^-	10.	Under test	Scheduled operation 1971(2)
		6-10 GeV, e^+e^-	10.	Under construction	Approximately 1972 — These two projects use same magnet ring
		24 GeV, $p\bar{p}$	Uncertain	Under construction	Completion date uncertain
	Kharkov	2-3 GeV, e^+e^-	30.	Under construction	Completion date unknown
United States	CEA, Cambridge	3.5 GeV, e^+e^-	30.	Under test	Scheduled operation 1972
	SLAC, Stanford	2.5 GeV, e^+e^-	300.	Under construction	Scheduled operation 1972

122

particle (which energy for simplicity is assumed to be large compared to the rest mass of the particle).

Colliding beams of electrons and positrons can circulate in opposite directions in a single magnet ring. The CEA has been converted to such a storage ring with each beam at $E = 3.5$ GeV giving a total reaction energy of 7 GeV. A positron beam would require 5×10^4 GeV laboratory energy to have the same reaction energy with an electron at rest. A pair of intersecting storage rings at NAL with 100-GeV protons in each would give a proton–proton reaction energy of 200 GeV. A proton energy of 2×10^4 GeV would be required to give the same reaction energy in a collision with a proton at rest.

Thus, we see the great advantage to the storage ring. There are, however, limitations to the technique. First, the interaction rates that can be achieved are of the order of 10^6/sec or less. When a proton beam hits a stationary target, almost all of the beam can be made to interact (typically 10^{13} interactions/sec). Second, one can achieve this high center-of-mass energy only in collisions of stable particles. Thus colliding beams do not constitute a substitute for higher-energy accelerators.

On the other hand, for the first exploration under controlled conditions at very high energy, the storage ring is ideal. In view of this, it is most unfortunate that the United States has fallen behind in the use of storage rings after having taken a lead in their development. The existing world situation with regard to storage rings is shown in Table I.5. It is clear that future planning should include a higher-energy electron colliding beam facility ring to be built at NAL, or at BNL if it proves feasible to accelerate stacked beams in the storage ring with no penalty in the characteristics.

9 Funding Considerations

9.1 THE STARTING POINT

The splendid accomplishments in U.S. high-energy physics during the past 20 years demonstrate the effectiveness of both the research and educational traditions of the United States. They also demonstrate the existence of the exceptional base of physical sciences and technology on which the work rests, and they remind us of the generous public and federal support that has made it all possible. As a result, the available resources, both in terms of people and facilities, are impressive.

The greatest single resource that enters into our projections into the immediate future is the 200–500-GeV accelerator at the National Accelerator Laboratory, whose existence represents a special responsibility for the high-energy physics community. This unique facility together with the lower-energy accelerators discussed in Chapter 6 provide a strong foundation for continuing excellence in high-energy physics research. The 6.2-GeV accelerator at the Lawrence Berkeley Laboratory has prime responsibility for research requiring incident protons between 1 and 6 GeV; the ZGS facility at Argonne National Laboratory provides for proton energies between 6 and 12.5 GeV; the AGS facility at Brookhaven National Laboratory is primarily responsible for work requiring incident proton energies between 12 and 33 GeV. As for the electron accelerators, the 6-GeV CEA synchrotron is preparing electron–positron colliding-beam experiments; the 10-GeV electron synchrotron at Cornell University has a high duty cycle characteristic of synchrotrons, and the research there complements that done at the high-intensity (but low-duty-cycle) 22-GeV electron linac at SLAC.

Unfortunately, budgetary limitations have become so severe in the last few years that most of these excellent high-energy physics resources are seriously underutilized. The combination of reduced funding for operations of the national laboratories and for support of research in universities has resulted in the closing of one accelerator laboratory (PPA), a severe cutback in the operations of another (CEA), the shutdown of a number of intermediate-energy accelerators, the slowdown or outright cancellation of many promising research projects in progress, a substantial reduction in the manpower and ancillary equipment available to support experiments at the major laboratories, and abandonment or slowing of exciting development projects in midstream.

To meet the impact of the President's fiscal year 1972 budget, the national laboratories have found it necessary to make additional severe cutbacks in staffing (the reduction in total manpower, physicists, other professionals, and nonprofessionals since fiscal year 1969 has been 23 percent, see Table I.7), operating schedules, new equipment and replacements, and development and realization of new facilities.

This is the point at which we find ourselves in trying to evaluate the needs for the future. The most recent earlier attempt to project the federal funding required to exploit the opportunities in elementary-particle physics, or more precisely, in the high-energy component of that subfield, was presented in the 1969 Report of the High Energy Physics Advisory Panel (HEPAP) of the Atomic Energy Commission. The general features of the HEPAP study can still be considered the basis for a plan to realize the potential of the U.S. high-energy physics capability, although the large

program reductions that have taken place since that study was made make it necessary to consider substantial extensions of the timetable.

This report will treat a number of alternative funding situations, each of which will be based on the assumption that a funding trend will persist over a 5-year period. These cases are (1) full-capability case (based in part on the 1969 HEPAP report); (2) a case based on an average rate of increase in total costs of about 6 percent per year; (3) a holding case (constant total costs); and (4) a declining case, total costs declining by 6 percent per year.

We will indicate in general terms the effects of these various possible funding situations on the high-energy physics research enterprise. It is important to note that these estimates of impact are based on the assumption that each of the budget projections is given in terms of purchasing power (i.e., 1971 dollars). There is no sure way to guess inflationary trends, but the impact of recent rates of inflation on these programs would be devastating if the figures were interpreted as actual dollars.

The projections for the level and declining cases are presented with deep misgivings, for we believe that long-range plans should be based on the requirements for achieving continued vitality and productivity in any research field, and especially a field like elementary-particle physics that is one of the important and natural parts of the whole organism of science.

Under level or declining budgets, it will be seen that some excellent ongoing programs would need to be sacrificed and that significant detailed decisions would have to be made concerning which of the specific programs would be supported. How acute this problem would become will be recognized from the fact that costs of the high-energy physics program are heavily quantized because the program is based on a very small number of large facilities. Each of these facilities requires substantial funds just to cover fixed costs. As a result, the research output per dollar of a given accelerator laboratory decreases substantially as the total funding to the laboratory declines to a subsistence level. Therefore, it is not possible to squeeze all the laboratories equally without inviting an increase in the unit cost of the research that can be produced. Consequently, termination of some accelerators would become necessary to continue the rest at a reasonable level of efficiency.

However, it should be noted that removal of any one of the present active laboratories would take the United States out of an important area of investigation. Each of the existing laboratories is playing a significant and nonduplicative role in high-energy physics and can be expected to do so for several years. If the hard decision to close out a laboratory must be made, it should take into account the best scientific and technical information available at the moment of decision, and it must involve several

levels of the federal government. From all points of view, a decision of this kind should not be made abruptly. It takes years to build up these laboratories, to assemble groups of inventive physicists and engineers, and to build the esprit that makes them successful. The abrupt demise of any one of them would be costly in terms of research, money, and manpower, and experience indicates that it would not necessarily protect the rest of the program against further abrupt budget cuts.

9.2 FINANCIAL HISTORY

Expenditures of federal funds from fiscal years 1965 through 1970, estimated expenditures for fiscal year 1971, and the President's budget for fiscal year 1972 (ending June 30, 1972) are shown in Figure I.21 in actual dollars. Figure I.22 shows the totals corrected to 1971 dollars by making use of the Consumer Price Index (all items).

Each of the appropriately labeled bands at the bottom of Figure I.21 represents the costs of running one of the accelerator laboratories. The way in which these costs are categorized will be discussed in Section 9.3. Above the total costs for all the accelerator laboratories are shown the costs of the university high-energy physics programs (which make up most of the elementary-particle physics costs in the universities). And the costs of construction of new facilities are indicated as the upper band in Figure I.21. Figure I.22 shows the total costs and construction costs in 1971 dollars.

The major item of construction in the early years is the AGS conversion to high intensity at Brookhaven National Laboratory. After fiscal year 1967, the costs of construction of the 200–500-GeV accelerator at NAL become the major construction cost.

Figure I.21 and Table I.6 show that operating, equipment, and accelerator improvement project (AIP) funds for the seven laboratories (ANL, BNL, LRL, SLAC, CEA, Cornell, and PPA) increased at an average annual rate of about 11 percent during the 1965–1968 period. This increase exceeded the 3 percent annual drop in purchasing power, to leave a growth rate of about 8 percent per year to cover the expansion that took place in capability, especially the costs of one major new installation, SLAC, and the increasing costs of elementary-particle experimentation.

From 1968 through 1971, operating, equipment, and AIP funds for these laboratories decreased at an average annual rate of about 2 percent. (Equipment funds, exclusive of very large computers, have dropped by about 14 percent per year since 1968.) This declining pattern of support, coupled with inflation, leaves the effective support for these laboratories in fiscal year 1971 about 25 percent below the fiscal year 1968 level.
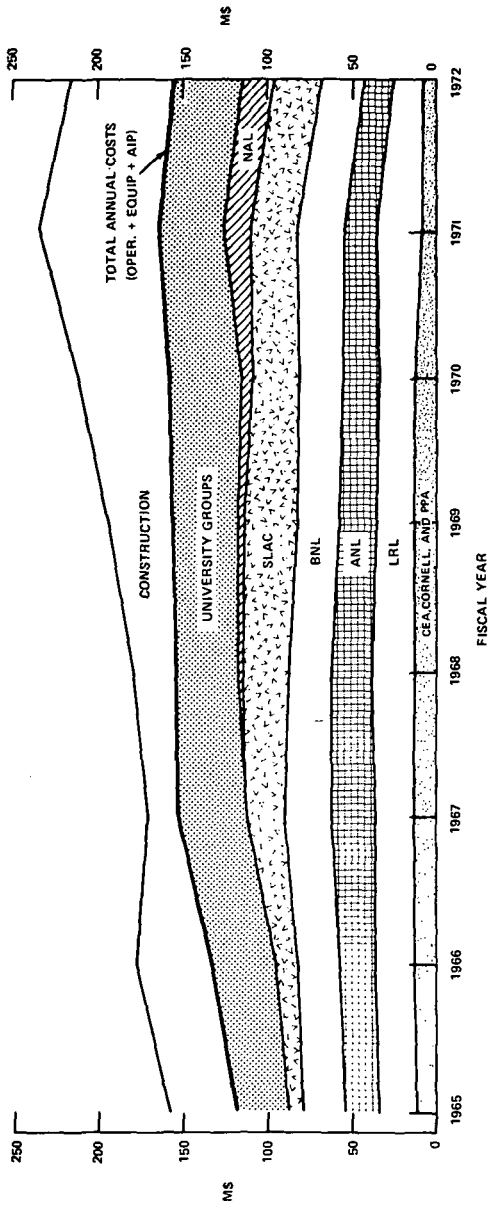
FIGURE 1.21   High energy physics total federal expenditures (actual dollars)

127
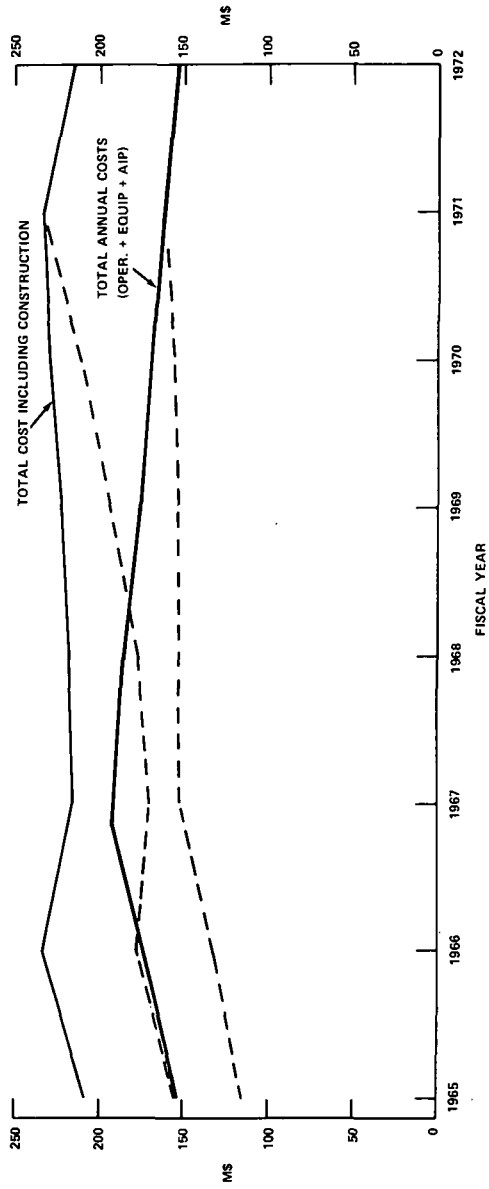
FIGURE I.22 High-energy physics total federal expenditures. ———, fiscal year 1971 dollars; – – – –, actual dollars.

128

TABLE I.6   High-Energy Physics Total Federal Expenditures (Actual Dollars)

	FY 1965	FY 1966	FY 1967	FY 1968	FY 1969	FY 1970	FY 1971	FY 1972
CEA, CORNELL, and PPA	12.4	13.3	13.6	13.4	14.3	13.6	9.1	8.3
LRL	22.8	23.4	22.9	25.7	21.9	19.7	26.4	17.0
ANL	19.6	20.7	26.4	23.4	21.9	22.1	19.7	17.4
BNL	24.3	25.1	26.6	25.7	24.8	25.8	27.2	24.7
SLAC	8.5	13.0	22.7	27.0	30.6	26.6	27.0	27.7
NAL	–	–	–	1.6	4.3	8.3	14.8	19.4
University groups	29.9	37.2	39.7	36.3	36.1	40.1	37.1	38.9
Construction	40.8	44.5	18.5	25.7	39.7	55.5	72.0	62.2
TOTAL	158.3	177.2	170.4	178.8	193.6	211.7	233.3	215.6

Further reductions in both operating and equipment funds are projected for fiscal year 1972. In terms of real purchasing power (Figure I.22), the funding of these laboratories is projected to be 10 percent lower than the fiscal year 1971 level, which is 25 percent below fiscal year 1968 level. Thus, over a four-year period the effective level of support (in fiscal year 1971 dollars) will have dropped by about 35 percent. This decline in effective support may be appreciably greater, since we have included only the effects of inflation as they apply to the economy at large.

Concomitant with funding trends has been a steady loss, about 5 percent per year, of personnel at the AEC laboratories (Table I.7). Some of the professional people have moved to NAL, many have been forced out of the field. Most importantly, there has been a sharp drop in the opportunity for young PhD's to work in, and contribute to, this field. The maintenance of opportunities for young particle physicists in the face of current budget stringencies is a problem of vital concern. If the opportunities are unduly restricted for even a few years, innovations in the field, which are its touchstone of success and its reason for being, may become stultified.

Support for university programs increased at an average rate of 15 percent per year to fiscal year 1967 and has remained essentially constant in actual dollars since that time. The total number of people associated with these programs has decreased at an average rate of 8 percent per year since fiscal year 1968. Table I.7 also shows that the number of graduate students being supported in AEC high-energy physics programs is decreasing. From this it can be surmised that the number of students entering the field has dropped sharply—about 10 percent per year.

The steady diminutions in purchasing power over the past few years, in the face of sharply increasing needs of high-energy physics associated with

TABLE I.7   Manpower[a] Supported by the A EC High-Energy Physics Program

	FY 67	FY 68	FY 69	FY 70	FY 71	FY 72[b]
TOTAL MANPOWER						
Laboratory						
PPA	336	320	295	95	0	0
CEA	~ 233	230	216	146	127	123
ANL	1232	1150	1089	897	847	740
LRL	1481	1350	1291	1145	1025	919
BNL	~1250	1305	1365	1276	1228	1029
SLAC	~1350	~1300	1397	1330	1287	1239
(Subtotal)	5881	5655	5653	4889	4514	4050
NAL	–	200	410	695	850	920
(Subtotal)	5881	5855	6063	5584	5364	4970
University Program	2682	2759	2606	2378	2130	1884
TOTAL	8563	8614	8669	7962	7494	6854
PhD MANPOWER						
Laboratory						
PPA	7	7	7	4	0	0
CEA	~ 18	18	18	18	11	10
ANL	49	55	64	62	65	68
LRL	108	105	103	102	100	98
BNL	~ 100	105	110	103	99	91
SLAC	~ 85	~ 90	99	104	99	95
(Subtotal)	367	380	401	393	373	352
NAL	–	~ 15	36	56	63	100
(Subtotal)	367	395	437	449	436	452
University Program	645	659	641	639	604	539
TOTAL	1012	1054	1078	1088	1040	991
GRADUATE STUDENTS						
Laboratory						
PPA	0	0	0	0	0	0
CEA	0	0	0	0	0	0
ANL	31	20	3	4	2	2
LRL	111	110	104	92	87	79
BNL	0	0	0	0	0	0
SLAC	~ 20	~ 30	38	28	35	35
(Subtotal)	162	160	145	124	124	116
NAL	0	0	0	0	0	0
University Program	647	660	626	594	484	399
TOTAL	809	820	771	718	608	515

[a] This number represents the head count at the end of the fiscal year. For information on manpower in institutions other than those directly supported by the AEC see Table 1.3.
[b] Fiscal year 1972 is estimated on the basis of the President's budget.

an increasing accelerator capacity, have had a drastic effect on the utiliza-
tion of the excellent research facilities available, as shown in Table I.8.
SLAC utilization, which was 74 percent in fiscal year 1969, will drop to about
57 percent in fiscal year 1972. CEA operation has been reduced in scope
to colliding-beam development and experiments. PPA has been dropped
from the program. The average projected operating level of the remaining
accelerators is only 60 percent of capacity.

The national laboratories are at the present time carrying a large back-
log of highly competitive experiments, which they are unable to schedule,
and they are facing further severe reductions in program. The result will be
that many excellent opportunities will be missed, and some very special
facilities will be underutilized or not utilized at all. We noted earlier the
severe cutback of CEA and the shutdown of one major accelerator, PPA.
The closing of PPA was based on the need for providing urgently needed
support for higher priority programs at other laboratories, but additional
support has not gone to them; in fact, the loss in total operating support
has exceeded the cost of operating PPA.

The downward trend in support of operations (see Figure I.22) has
taken place over a period when the largest electron accelerator in the
world (SLAC) has gone into operation; while a major improvement pro-
gram, intended to double the capability of the AGS, is being completed;

TABLE I.8 Percentage of Utilization[a] of High-Energy Physics Accelerators

	FY 70 (%)	FY 71[b] (%)	FY 72[c] (%)
CEA	73	50[d]	50[d]
ZGS	77	66	54
Bevetron	83	83	70
SLAC	60	60	57
AGS[e]	48	74	63
PPA	63	55	0
Cornell	85	90	90

[a] The percent utilization is defined as the percentage of hours that are actually scheduled for ma-
chine operation out of the practical total that could be scheduled with sufficient funding. The
practical total corresponds to the total hours/year (8770) minus holidays and normal require-
ments for routine maintenance and improvements. The total is ~7700 hours for proton accel-
erators and ~7500 hours for electron machines.
[b] Fiscal year 1971 numbers are based on actual numbers for the first half of the fiscal year
with an estimate for the remainder of the fiscal year.
[c] Fiscal year 1972 numbers are preliminary estimates of the maximum utilization permitted by
the fiscal year 1972 President's budget request.
[d] For fiscal years 1971 and 1972, CEA operation has been reduced in scope to colliding beam
development and experiments.
[e] The AGS numbers are anomalous because of the scheduling of shutdown periods for the AGS
conversion project. Fiscal year 1970, 6-month shutdown; fiscal year 1971, 2-month shutdown;
fiscal year 1972, 4-month shutdown planned.

while the largest proton accelerator in the world (NAL) was approaching the operating state; while the first electron–positron colliding beam facilities (CEA by-pass and SLAC-SPEAR) in the United States are being completed; and while the first bubble chamber in the world of an entirely new generation (12-ft chamber at ANL) was completed. The original concept of intersecting storage rings at SLAC had to be abandoned in favor of the present, much more limited, single ring SPEAR project, resulting in a loss of luminosity and no possibility of studying electron–electron collisions for comparison with results that will be obtained with electron–positron collisions.

Not only has the decreased funding resulted in a severe underutilization of existing accelerators, but it has caused some serious problems in planning for experiments at NAL. There has been good reason to hope that some experiments will begin there ahead of schedule. As a result, preparations were started early on a number of important NAL experiments proposed by university groups; but the initial funding of these groups, of course, did not take into account the possibility that the accelerator might come in early. These groups, in their desire to meet the newest challenges, have placed the highest priority on their NAL experiments. However, there is not enough support for both the NAL experiments and other good experiments at the other laboratories that have been planned for some time.

The lack of support for work of the user groups at the laboratories combined with damage already done by the direct budget pressures on these laboratories can wreak havoc from which the U.S. program will be slow to recover. We find ourselves faced with the paradox that the community of high-energy physicists will not be able to take full advantage of an exceptional opportunity to advance the field on a broad front.

9.3 BUDGET CATEGORIES

In order to understand the information on expenditures and budget plans presented here, it is necessary to recognize clearly certain features of the federal funding process. Funds are allocated to the AEC (which carries by far the largest part of the elementary-particle physics budget) in three distinct categories, which, by act of Congress, are not interchangeable. They are categorized as operating, capital equipment, and construction funds.

Operating money includes salaries and wages of research and service personnel and the cost of electric power and expendable supplies.

Capital equipment money pays for major items of equipment, including computers. Experiments in high-energy physics involve such massive installations that many of the costs inherent in the normal experimental pro-

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gram appear as large enough individual items to be formally budgeted as capital equipment items. These would include beam-transport equipment such as bending magnets, focusing quadrupoles, velocity selectors, beam splitters, and general-purpose analyzing magnets.

Capital equipment funds also support the fabrication and procurement costs of major special items of equipment and of new items needed to replace those that rapidly become obsolete because of the pace of developments in the field. In this regard, there is an important difference in concept between the role of capital equipment in high-energy physics research and its role in industrial production. In industry, it is used to expand operations and has permanent value. In research, much of it is needed for the more transitory purposes of upgrading beams and facilities and providing more sophisticated particle-detection and data-handling equipment, in order to keep up with innovations and evolving research demands.

Construction funds provide for the construction of permanent installations. The funds for a very large unit may be appropriated and obligated in one or several years, but the expenditure will spread out over the period of years required to carry out the construction. Thus, the construction budgets, which reflect appropriations, may show large fluctuations, but the actual year-by-year expenditures (costs) are a relatively smooth function of time.

In addition to funding specific construction projects, construction funds appear in the AEC high-energy physics budget for each of the laboratories as accelerator improvement project (AIP) funds. These are used for continual upgrading of accelerator performance in a manner similar to the support of research by means of the capital equipment funds. They serve to provide for the additions and innovations that are essential to combat obsolescence of the facilities, thereby providing for the changes that are vitally important to the experimental program.

The information presented in Figures I.21 and I.22 and in Table I.6 shows actual or estimated *costs* rather than the mixture of costs (operating) and obligations (equipment and construction) that would normally appear in a budget. Estimated expenditure rates will also be the basis for projections indicated later in this chapter. Furthermore, we combine operating, equipment, and AIP construction costs, because that combined figure represents the annual outlay of money to keep the enterprise going.

It is important to realize that equipment and AIP funding, which appear as separate budget categories, have been more and more severely limited, to an extent even greater than the limitations on operating money. This reduction in funds that could have been applied to the use of new technology and methods has been costly in terms of operating efficiency and effectiveness. We are falling behind in making use of new methods, such as integrated electronic systems, that would reduce costs and also in taking

advantage of some older ones, such as dual liquid bubble chambers that offer an opportunity for high-efficiency detection of neutral particles produced in reactions. Not only is there a serious need to compensate for this in the future, but the need for major items of standard equipment at N A L is enormous because of the scale of the operations. Therefore, high priority should be given to the capital equipment item in the high-energy physics budget.

9.4   GENERAL ASSUMPTIONS ABOUT SCIENTIFIC PRIORITIES

In order to provide projections of capability and needs for the future, it is necessary to establish some priority guidelines. These guidelines are clearly influenced by the funding history; the decline in support for existing facilities that has taken place during the past few years is having a profound influence on future capabilities. The suddenness of this decline in financial support was unanticipated and made it impossible to complete or fully exploit some of the opportunities or facilities as planned. Nevertheless, almost every major undertaking in this field has been successfully brought to completion within the conditions that were set at the outset.

Discovery, especially unanticipated discovery, is the ultimate measure of success in any exploratory field. This has been the general principle underlying priority decisions made in the past, and its correctness is attested to by the success of the program. Therefore, we continue to follow the successful principles in trying to set priorities for the future; the opportunities for innovation and unanticipated discovery must be emphasized, but with as strong a background of exploitation of existing facilities as is possible.

9.4.1   *National Accelerator Laboratory (NAL)*

In all respects, these principles bespeak giving highest priority consideration to the accelerator and experiments at N A L. Experience has been that the opening up of a new energy domain for controlled experimentation leads to unanticipated discoveries. The promise and opportunity for exploration and innovation offered to U.S. high-energy physicists by the availability of this highest-energy accelerator must be exploited.

9.4.2   *Users*

The accelerator users consist of both in-house research groups at the laboratories and, to a larger extent, university groups. An in-house staff active

in high-quality research in an accelerator laboratory is essential to the maintenance of the standards and effectiveness of the laboratory. At the same time, a viable high-quality research effort in elementary-particle physics requires the strong participation of the academic community. This field was spawned in the universities, and it shares with the universities the spirit of their scholarly traditions. About 75 percent of the active research in the field is done by university-based physicists using the national facilities.

A proper balance between the needs of the universities and those of the accelerator laboratories is of the utmost importance to the success of this field. The work cannot be done unless the laboratories have sufficient support to provide the beams, the facilities, and the associated manpower support for the experiments set up by the users. And unless the users, most of whom come from the universities, are adequately supported they will not be able to bring in their students and research associates or develop, design, and build the apparatus that they need to carry out the experiments.

It must be kept in mind that the universities are the only source of new talent and the new ideas and perspectives associated with it. In spite of the reduction that is taking place in the total number of people associated with the program, there must continue to be an opportunity for those with outstanding motivation and talent for innovation, original work, and intellectual effort to choose to work in this field. Otherwise all of physics will become less attractive to such people. Therefore, high priority must be given to support for those university groups offering that opportunity.

In the past, the balance between accelerator laboratory and university support has been such as to ensure the vigorous development of the field. Looking to the future, we believe there will be an incremental increase in university needs as a result of the increased complexity of the NAL experiments. It is assumed that, as a trend, the university needs will be proportional to the total accelerator laboratory operating budgets.

9.4.3 *Other Major New Facilities*

The AGS at Brookhaven National Laboratory has been undergoing a major conversion that is now nearing completion. The purpose of this conversion is to provide such high-intensity beams and increased experimental capability that it will be possible to carry out research of a qualitatively different character that should open new portals of understanding.

Another important new facility is the electron–positron colliding beam facility at SLAC, designated as SPEAR. This will also open up new opportunities to carry out electron–positron experiments of a different charac-

ter if it can be exploited. The same can be said of the colliding beam facility at CEA, which is now the focus of the entire effort at that laboratory. The 12-ft hydrogen bubble chamber at Argonne, which is the first of an entirely new generation of bubble chambers, also falls into the category of major new facilities whose exploitation is just beginning.

Incremental funding (both operating and equipment) to take advantage of these and any other such opportunities will be indicated explicitly in the projections that are presented here, indicating that higher priority should be given, in general, to the new opportunities relative to extensions of the old ones.

9.4.4 Accelerator Laboratories (other than NAL)

It is essential to the health of the field that work be continued over the entire spectrum of available energies. A rational research program will continue a vigorous activity at these energies in the same way as productive research activity continues in virtually every field of physics. Therefore, the small number of existing accelerator laboratories should have continuing support at a viable level until the special functions that each performs can be handled by another accelerator.

There are several points to be made in this connection:

1. Although the greatest exploratory power is at the highest energy (NAL), important explorations can also be carried out at lower energies if high beam intensities and long running time are available. These will make possible the exploration of very rare phenomena and highly quantitative effects in known phenomena (Section 1.4). Furthermore, we should be prepared for history to repeat itself. As new and unexpected phenomena unfold at NAL, it seems likely as before that there will be new questions that can best be answered at lower-energy machines. An excellent example from the past is offered by the way in which parity violation was discovered. The Lee-Yang suggestion of parity violation arose from the discovery in cosmic rays and at the highest-energy machines of the puzzling properties of charged K-mesons. The definitive experiments following this suggestion, however, made use of the methods of low-energy nuclear physics and intermediate-energy physics (at the proton synchrocyclotrons).

2. The very small number of machines involved offer a variety of complementary approaches to the lower-energy problems. For example, there is a close and complementary relationship between the work on vector dominance (Section 2.6) with electron machines and the work on vector meson resonances with proton machines (Section 2.14).

3. These lower-energy machines offer by far the best opportunity for promising young people to get their training and display their talents. The

N A L might accommodate as many as 25 percent of the active high-energy groups, but so much is at stake there that wild ideas are likely to be pursued only if the proponents have clearly demonstrated some of the characteristics needed to make a success of a radical experiment or to establish an unexpected result with reasonable confidence. One such success at a lower-energy machine can do much to establish a reputation of that kind. This is an important by-product of the good physics that can be produced by these smaller machines.

4. Higher-energy accelerators and facilities can in principle be designed to produce low-energy beams; however, each of the facilities has been designed to cover a specific area of physics or some special need. The cost for use of the larger machines for the same purposes as the lower-energy machines would generally be much greater than continued operation of the lower-energy accelerators. Furthermore, substantial new capital assets would first need to be added to the larger machine to make the lower-energy beams available. There are also serious and costly scheduling and administrative complications, including a great loss of flexibility, associated with trying to fulfill too many purposes at a single accelerator.

5. The diversity of scientific styles and decision-making mechanisms of the present pluralistic U.S. high-energy physics program is clearly one of its greatest reasons for success. This weighs heavily against "economy of scale" arguments, which, in any case, often turn out to be ephemeral when applied to research activities.

6. In view of the above, the shutting off of an accelerator prematurely reduces costs relatively little, if at all, unless at the same time, research in an entire research area is curtailed or abandoned. In a tough budget situation, reduced operating levels, while leading to inefficient utilization of machines at least keeps the entire range of energies open for study and leaves the option of returning to efficient use at a later date. They also leave the option for converting the machine to uses in other fields such as nuclear physics, medical physics, or space radiation physics, while a shutdown will usually result in the total loss of important technical and scientific resources.

7. A decision to shut down any accelerator should be made on the basis of decreasing scientific interest relative to higher priority programs and on the basis of a lack of uniqueness of the facility. Such decreased scientific interest is frequently signaled by the declining number of high-quality experiments proposed for execution at the facility.

9.4.5. *Particle Physics at Intermediate-Energy Accelerators*

Although the focus of experimental particle physics is at the high-energy accelerators, with primary beams of energy greater than 1 GeV, some part

of the experimental activity at the intermediate-energy machines, such as the 180-in. cyclotron at Lawrence Berkeley Laboratory, the cyclotron at Nevis, and the meson factory (LAMPF) at Los Alamos is now, or is expected to be, directed toward the problems of elementary-particle physics. These machines produce, or will produce, intense beams of protons, pions, and muons, which make it possible not only to do precise experiments that will contribute valuable information to the field but also may still turn up some surprises. Furthermore, they may again, as in the past, be called upon to explore important questions raised by new discoveries in the high-energy domain.

The particle-physics experiments constitute a relatively small part of the program at these machines and cannot be made the primary basis for giving them a high priority for funding. The justification for funding must come largely from the needs of nuclear structure physics and other fields depending on intermediate-energy particle beams for their front-line research activities. On the other hand, the discussion presented above in paragraphs 1 through 7 supports the need to maintain some of this capability for experiments directed toward the goals of elementary-particle physics.

9.4.6   *Cosmic-Ray Physics*

The only source of information concerning the behavior of particles at energies beyond the reach of the accelerators is cosmic radiation. As we have mentioned before, some of the most important discoveries about elementary particles were made in cosmic-ray experiments before machines at particle producing energies were available. In more recent history, the machines have proved to be by far the more productive vehicle for discovery in this field, although a continuing activity in cosmic-ray physics has produced some intriguing results that may turn out to have an important bearing on elementary-particle physics.

Most of the activity in cosmic-ray physics is not concerned with the problems that motivate elementary-particle physics. However, the relatively limited number of experiments bearing directly on such problems are intended to take advantage of the very high energies of cosmic-ray particles. Since this is the only source of information concerning phenomena at these very high energies, such work should be continued, with the emphasis on the possibility of observing qualitatively new phenomena. Since each such experiment requires a very substantial facility, every new proposal for a new experiment of this kind should be given very close scrutiny before it is approved.

9.4.7 *New Construction*

Several construction projects that could be started within the next five years should be kept in mind (see Chapter 8). A number of considerations, aside from the availability of funding, enter into the determination of priorities between these projects. The most important is whether the required technical developments are completely successful. This, for example, would apply at this time to pulsed superconducting magnets, superconducting rf cavities, and electron ring accelerators.

Another consideration is the physics results that will be obtained in the immediate future at NAL. It might happen that some unexpected result opens up an entirely new field of research at, say, 100 GeV, making it desirable to give very high priority to conversion of one or more existing machines to higher energy. And the recent exciting success of the intersecting storage rings at CERN suggests that high priority be given to the construction of intersecting storage rings in this country at even higher energies.

The storage rings have the additional advantage that the major technical problems have been solved, perhaps even for superconducting storage rings. Another project for which the technical problems seem to be in hand is the very large detection device, needed especially for high-rate neutrino experiments at NAL. Perhaps this should be a super-bubble chamber or a combination of a large bubble chamber with wire chambers (hybrid system). Much more will be known about the importance of such a system for physics after some results are obtained with the more modest devices now under construction. This experience will also be useful in making a choice between different possible devices that could be used for the purpose.

In view of their short-term character and the fluidity of the technical situation, the funding projections that follow will not include any priority ordering for new construction.

9.5 A FULL-CAPABILITY PROGRAM

By a full-capability program we mean a program allowing for optimum utilization of existing facilities and new facilities that have been authorized and are being built. We also imply a program of new construction and equipment that would follow the general outlines set forth in the HEPAP 1969 report but brought up to date to take account of subsequent technological developments and other irreversible changes that have occurred since then. Reductions in program and manpower due to continuing budget restrictions have eroded our capability, but the facilities, per-

sonnel, and state of the art are still such as to allow the possibility of a strong program.

Table I.9 is intended to give some notion of the present capability for high-energy physics experiments at accelerators other than NAL in the United States. A "slot" indicates a place in which an experiment can be available, and a "large facility" indicates a facility that is more or less perpetually set up so that the total numbers of both indicate the number of positions at which experiments can be in the process of setting up, testing, tuning, or taking data at a given time. It should be kept in mind that the number of slots at a machine, the properties of the slots, and the characteristics of some large facilities are constantly changing in response to new developments and new ideas concerning the physics to be done.

A rough rule of thumb is that a counter experiment may be on the floor for about one year and taking data at a significant rate intermittently during half that time. A large facility, such as a bubble chamber, will accommodate about five to ten different experiments per year. Counter experiments may require as much as one or two years for the preparation of apparatus at the physicists' home base, and both counter and bubble chamber experiments may require an equal number of years for data analysis, although there are some on-line counter experiments that allow much faster handling of the data. The number of physicists required to carry out one such experiment varies from a minimum of three (one for each of three shifts during around-the-clock data taking) to as many as twenty. The number of physicists who can be occupied in making full use of the capability indicated by Table I.9 is therefore quite substantial.

Figure I.23 and Table I.10 indicate the funding required to provide for optimum use of the capabilities set forth in Table I.9 and to provide also for optimum use of new facilities that will be available but are not described in Table I.9. The costs shown for each of the accelerator laboratories are meant to include expenditures for operating, capital equipment, and accelerator improvement projects. The figures for the last two items include estimates of what is needed to compensate for the lag in such funding during the past few years.

The PhD manpower required for such a program is already in the pipeline, as indicated in Section 7.5, but sharp reductions in the student population in the past few years will soon change this picture.

Figure I.23 also shows the planned expenditures for new construction that would be required to maintain and regenerate the full capability in this field. These expenditures combined with all other costs of the full-capability schedule shown in Figure I.23 would lead to an overall increase in total costs for high-energy physics at an average rate of 10 percent per year in fiscal year 1971 dollars.

TABLE 1.9 Rough Estimates of U.S. Accelerator Capability (Excluding NAL)

Machine[a]	Primary Particle Accelerated	Energy	Slots for Counter Experiments	Large Facilities[b]	Simultaneous Experiments (Beams)	Character of Physics		
						Weak Interaction	Electromagnetics	Strong Interactions
AGS	P	30 GeV	11	4	~6	40%	10%	50%
ZGS	P	12 GeV	14	3	~6	20%	0	80%
SLAC	E	20 BeV	12	4	~4	20%	50%	30%
Bevatron	P	6 GeV	11	1	~6	40%	0	60%
Cornell	E	12 GeV	(6)	0	~3	0	85%	15%

[a] PPA has been omitted from this table because of its imminent shutdown as a particle-physics machine. That eliminates about 10 slots for counter physics from the table. CEA has also been omitted because its program is now limited to intersecting-beam experiments.

[b] Large facilities include bubble chambers, large spectrometers, streamer chambers, and other facilities that may be programmed for extended series of experiments by a number of different groups.

FIGURE I.23 High-energy physics cost projections for federal support. Full-capability case, 10 percent annual growth (fiscal year 1971 dollars).

142

TABLE 1.10 High-Energy Physics Cost Projections for Federal Support. Full-Capability Case, 10 Percent Annual Growth (Fiscal Year 1971 Dollars)

	FY 1971	FY 1972	FY 1973	FY 1974	FY 1975	FY 1976	FY 1977
Presently Operating Laboratories[a]							
CEA, Cornell, and PPA	9.1	8.3	10.0	11.0	11.0	11.0	11.0
LRL	26.4	17.0	22.0	23.0	23.0	23.0	23.0
ANL	19.7	17.4	22.0	23.0	23.0	23.0	23.0
BNL	27.2	25.3	37.0	37.0	34.0	34.0	34.0
SLAC	25.5	26.5	37.0	37.0	36.0	36.0	36.0
Subtotal	107.9	93.6	128.0	131.0	127.0	127.0	127.0
Increment for Exploitation of Other Major New Capabilities							
Operating	–	–	5.0	12.0	22.0	31.0	36.0
Equipment	1.5	1.5	5.0	7.0	10.0	10.0	10.0
Subtotal	1.5	1.5	10.0	19.0	32.0	41.0	46.0
NAL							
Operating	9.2	12.9	26.0	34.0	41.0	45.0	46.0
Equipment	5.6	7.5	25.0	25.0	20.0	20.0	15.0
Accelerator improvements	–	–	–	2.0	5.0	8.0	10.0
Subtotal	14.8	19.4	51.0	61.0	66.0	73.0	71.0
University Groups							
AEC operating	25.1	23.9	30.0	33.0	36.0	39.0	42.0
AEC equipment	1.0	1.0	2.0	2.0	3.0	3.0	3.0
Other agencies	11.0	14.0	17.0	18.0	19.0	20.0	21.0
Subtotal	37.1	38.9	49.0	53.0	58.0	62.0	66.0
Construction							
AGS conversion	7.0	3.2	–	–	–	–	–
NAL	65.0	59.0	42.0	20.0	6.3	–	–
New projects	–	–	6.0	28.0	59.0	79.5	100.0
Subtotal	72.0	62.2	48.0	48.0	65.3	79.5	100.0
TOTAL	233.3	215.6	286.0	312.0	348.3	382.5	410.0

[a] Total annual costs, operating + equipment + accelerator improvements.

143

We have already noted that the specific construction projects that should be undertaken will best be determined by the latest technical developments and physics results. Under the funding for new construction included in the full-capability program, a good start could be made during the next five years on several of the following projects that are under consideration (see Chapter 8): BNL storage rings ($40 million), LRL new technology accelerator ($50 million), multithousand GeV accelerator (planning only), NAL energy increase (to 1000 GeV), NAL large detection device ($30 million), NAL storage rings ($100 million), SLAC energy increase ($25 million), and SLAC e^+-e^- storage rings.

In view of current budgets, the full-capability program may be viewed as optimistic, although a rate of increase of 10 percent per year would have seemed very modest just a few years ago. This program does serve to set a standard against which less optimistic projections may be measured. It should be recognized that a higher growth rate for this field is current at this time in Western Europe, and their base of capability has grown to be nearly equivalent to ours.

9.6   SIX PERCENT PER YEAR GROWTH PROGRAM

In view of the excellent opportunities and facilities that exist for work in this field, it is possible to envisage a program that, while showing less growth than the full-capability program, would still permit a good recovery for elementary-particle physics. This would require total expenditures increasing at an average rate of about 6 percent per year. However, this averaging must begin at fiscal year 1971 and should, therefore, include compensation for the sharp decrease in the President's fiscal year 1972 budget. The most effective (and, in the long run, least expensive) way to compensate for this decrease is to provide for a sharp increase in fiscal year 1973, thereby returning to a normal growth curve of about 6 percent per year.

Costs for such a program are indicated in Figure I.24 and Table I.11 in terms of combined operating, equipment, and accelerator improvement program (AIP) funds for the accelerator laboratories, combined operating and equipment funds for the universities, and construction funds that are needed to complete the AGS conversion and the basic facility at NAL, as well as those that would be available later for new projects. The distribution of costs is based on optimal utilization for NAL starting in fiscal year 1973 and substantial utilization of the new capabilities associated with the AGS conversion and SLAC-SPEAR. Substantial increases in equipment and AIP costs are included in order to show what would be needed to bring the program back into balance.
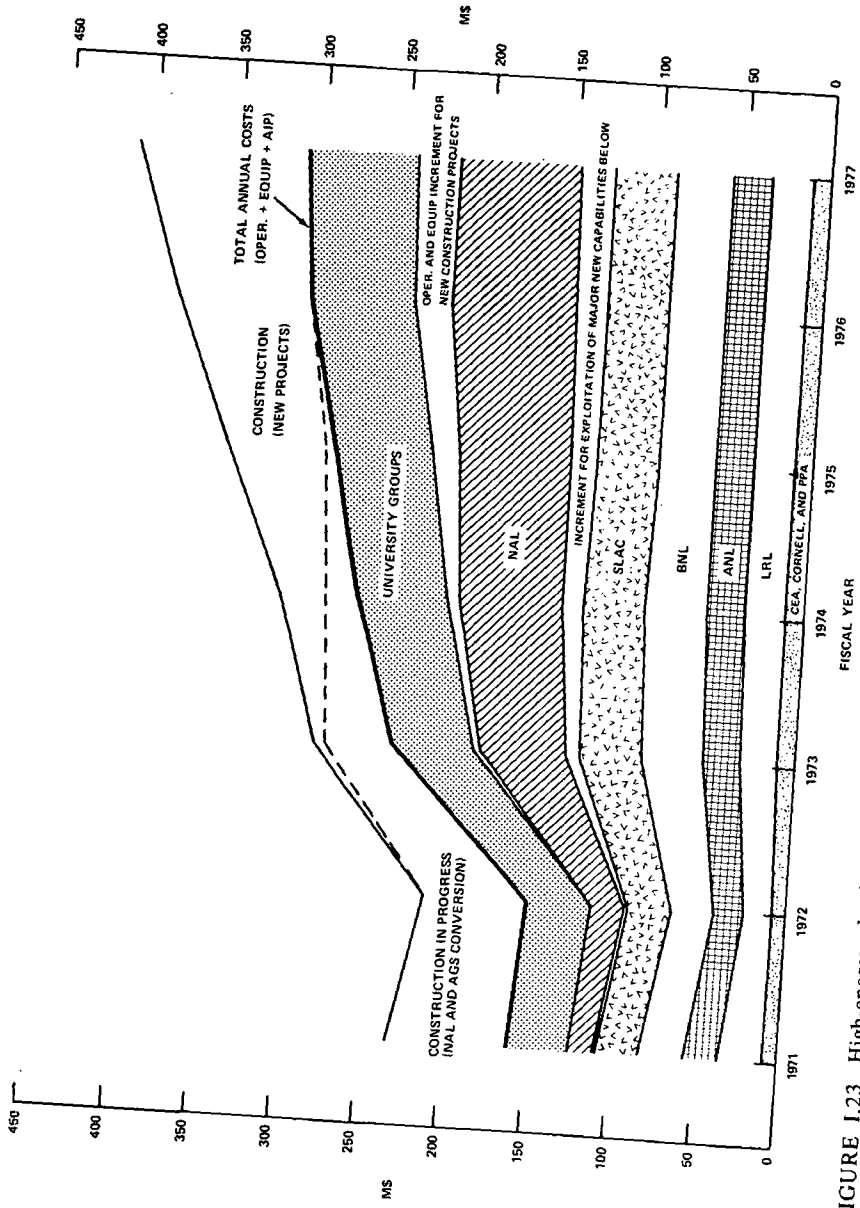
FIGURE 1.24 High-energy physics cost projections for federal support. Six percent annual growth case. (Fiscal year 1971 dollars.)

145

TABLE 1.11 High-Energy Physics Cost Projections for Federal Support. Six Percent Annual Growth Case (Fiscal Year 1971 Dollars)

	FY 1971	FY 1972	FY 1973	FY 1974	FY 1975	FY 1976	FY 1977
Presently Operating Laboratories							
AEC operating	84.3	78.6	91.0	93.0	94.0	94.0	94.0
AEC equipment	15.5	5.6	11.0	12.0	12.0	12.0	12.0
AEC accelerator improvements	3.6	3.4	4.0	4.0	4.0	4.0	4.0
NSF	4.5	6.0	7.0	7.0	7.0	7.0	7.0
Subtotal	107.9	93.6	113.0	116.0	117.0	117.0	117.0
Increment for Exploitation of Other Major New Capabilities							
Operating	–	–	4.0	9.0	12.0	15.0	17.0
Equipment	1.5	1.5	4.0	5.0	6.0	6.0	5.0
Subtotal	1.5	1.5	8.0	14.0	18.0	21.0	22.0
NAL							
Operating	9.2	11.9	25.0	34.0	40.0	43.0	45.0
Equipment	5.6	7.5	20.0	20.0	20.0	20.0	15.0
Accelerator improvements	–	–	–	2.0	4.0	5.0	5.0
Subtotal	14.8	19.4	45.0	56.0	64.0	68.0	65.0
University Groups							
AEC operating	25.1	23.9	29.0	32.0	35.0	35.0	35.0
AEC equipment	1.0	1.0	2.0	2.0	2.0	2.0	2.0
Other agencies	11.0	14.0	17.0	18.0	19.0	19.0	19.0
Subtotal	37.1	38.9	48.0	52.0	56.0	56.0	56.0
Construction							
AGS conversion	7.0	3.2	–	–	–	–	–
NAL	65.0	59.0	42.0	20.0	6.3		
New projects	–	–	2.0	13.0	25.7	42.0	62.0
Subtotal	72.0	62.2	44.0	33.0	32.0	42.0	62.0
TOTAL	233.3	215.6	258.0	271.0	287.0	304.0	322.0

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The funds needed by the university users for their operations are assumed to be roughly proportional to the total amount of operating money available to the accelerator laboratories, because the rate at which the users can proceed is so closely tied in with the amount of beam time that the accelerators can provide.

Aside from the incremental costs assigned to the new opportunities offered by new major facilities, the costs at all laboratories except N A L are assumed to level off after they are brought back to a reasonable level of efficiency by the compensating surge in fiscal year 1973.

The funds for new construction projects would allow, roughly speaking, for two of the items listed in Section 9.5, but which of them would be included would be determined by developments during the next year or two. The intersecting storage rings for N A L appear at this time to be an excellent candidate, both because they will open up an entirely new domain of energy and because the success of the CE R N proton storage rings augurs well for the future of this device.

Although this program would not make full use of existing capability, it would be consistent with the objective of making good use of the talent and special apparatus that will be available during the next few years, and, furthermore, it would provide opportunities for some of the new talent that is so important to the future of the field.

9.7 A LEVEL PROGRAM

For this projection we assume that the total budget (operating and equipment and construction costs) will be held at level purchasing power for the next six years.

Figure I.25 and Table I.12 illustrate the program as follows: N A L would be scheduled for strong utilization, an increment would be provided to take advantage of other new major facilities, and all other operations (except the closed-down PPA) would be reduced below their present marginal levels during the succeeding six years. Allowance has been made for the fact that the support of users would need to be increased at about the same rate as total operating funds if the research produced at N A L is to be commensurate with the increased operation of this facility.

Construction *costs* for completion of the basic accelerator at N A L are also shown. In addition, a band of other construction *costs* is shown based on the need for some new experimental capabilities. As before, we do not specify these capabilities but assume that they will be selected from among the candidates listed in Section 9.5.
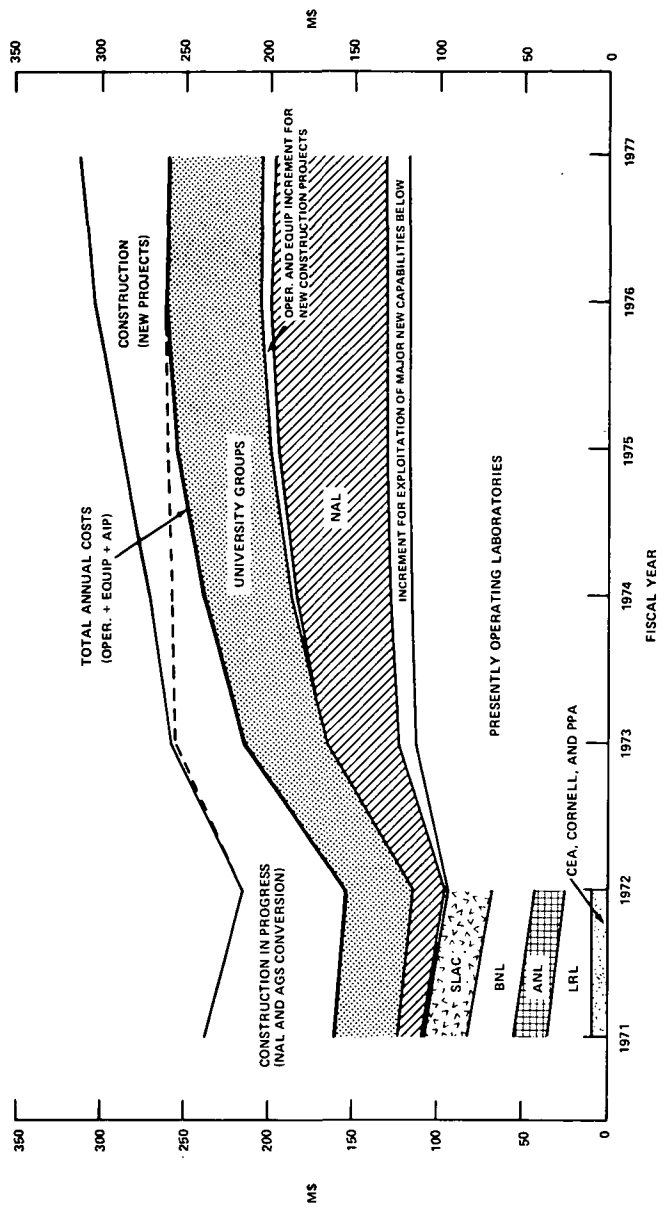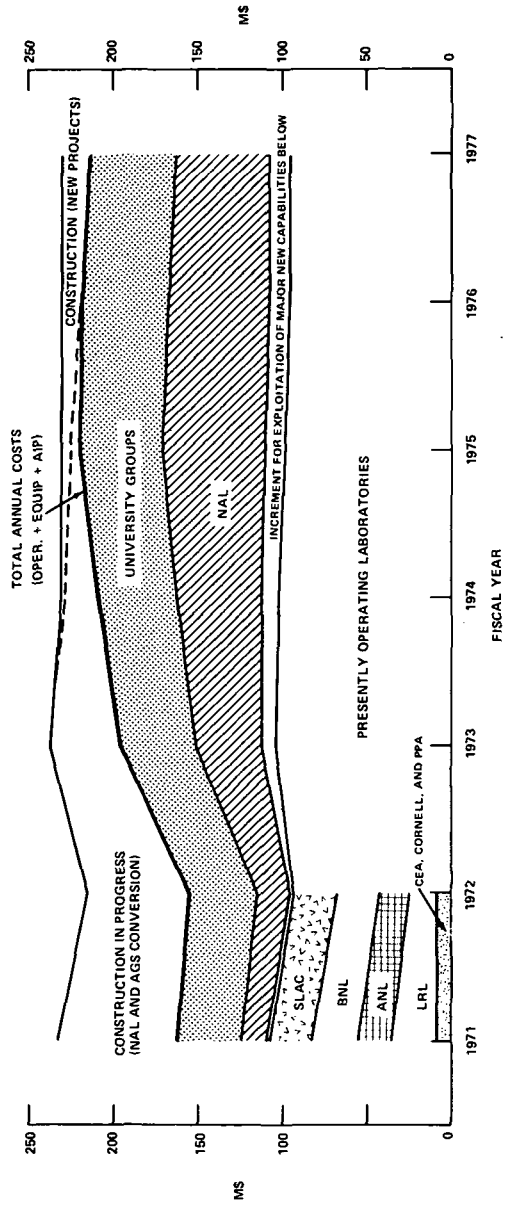
FIGURE I.25 High-energy physics cost projections for federal support. No growth case. (Fiscal year 1971 dollars.)

148

TABLE I.12 High-Energy Physics Cost Projections for Federal Support. No Growth Case (Fiscal Year 1971 Dollars)

	FY 1971	FY 1972	FY 1973	FY 1974	FY 1975	FY 1976	FY 1977
Presently Operating Laboratories							
AEC operating	84.3	78.6	84.0	84.0	80.0	80.0	80.0
AEC equipment	15.5	5.6	10.0	9.0	9.0	8.0	8.0
AEC accelerator improvements	3.6	3.4	4.0	3.0	3.0	3.0	3.0
NSF	4.5	6.0	6.0	6.0	6.0	6.0	6.0
Subtotal	107.9	93.6	104.0	102.0	98.0	97.0	97.0
Increment for Exploitation of Other Major New Capabilities							
Operating	–	–	4.0	6.0	10.0	10.0	10.0
Equipment	1.5	1.5	4.0	4.0	2.0	2.0	2.0
Subtotal	1.5	1.5	8.0	10.0	12.0	12.0	12.0
NAL							
Operating	9.2	11.9	24.7	31.0	37.0	40.0	40.0
Equipment	5.6	7.5	15.0	18.0	20.0	15.0	10.0
Accelerator improvements	–	–	–	1.0	4.0	5.0	5.0
Subtotal	14.8	19.4	39.7	50.0	61.0	60.0	55.0
University Groups							
AEC operating	25.1	23.9	26.0	27.5	29.0	30.0	30.0
AEC equipment	1.0	1.0	2.0	2.0	2.0	2.0	2.0
Other agencies	11.0	14.0	16.0	16.5	17.0	17.0	17.0
Subtotal	37.1	38.9	44.0	46.0	48.0	49.0	49.0
Construction							
AGS conversion	7.0	3.2	–	–	–	–	–
NAL	65.0	59.0	42.0	20.0	6.3	–	–
New projects	–	–	–	2.0	4.7	12.0	17.0
Subtotal	72.0	62.2	42.0	22.0	11.0	12.0	17.0
TOTAL	233.3	215.6	237.7	230.0	230.0	230.0	230.0

9.7.1 *Impact*

This funding pattern would include a substantial shift of users' support from old program to programs exploiting the major new capabilities (including N A L). It implies that to retain a semblance of balance in the program even N A L would be underutilized at its inception. It also implies marginal utilization for the remaining accelerator laboratories or the shutdown of one or more of the few major accelerators, and it leaves little room for construction of the new capabilities which are so critically important to this type of frontier research effort.

The level funding indicated here would be a continuation of recent funding history, which has been discussed in Section 9.2. That Section describes the serious losses that have been, and continue to be, incurred as a consequence of that history, and these losses would continue, and would be compounded, by a policy of level funding. The need to assign a major portion of the operating funds to N A L is just beginning to be felt, and it will cause even more serious problems than the turning on of other accelerators in the past because of the magnitude of the undertaking.

Elementary-particle physics at the lower end of the energy spectrum would be likely to suffer great damage under level funding. In particular, the elementary-particle research at the intermediate-energy (100–1000-MeV) accelerators would be hard hit if we are correct in our surmise that the university users will give highest priority to the use of their limited funds at the highest energy. That seems to be indicated by present trends and is consistent with the current notion that most of the action in particle physics is to be found above 1 GeV.

Costs for constructing and operating the intermediate-energy accelerators have not been included in either our funding history or projections* because these accelerators are used for many good purposes other than particle physics. As we have already noted, some of these accelerators have been shut down and more are scheduled to be shut down. There are three such major facilities (Berkeley 180-in. cyclotron, Nevis cyclotron, Los Alamos Meson Physics Factory) intended to be used in part for elementary-particle physics that will continue to be in operation or start operation in this period. They are or will be an excellent source of pions or muons capable of continuing the fundamental studies of muonium (Section 1.4), mesic x rays, precision pion–nucleon and nucleon–nucleon scattering, and so on. Some of the highest precision work, leading to primary determinations of fundamental constants (such as the fine-structure con-

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*However, a substantial part of the costs of university groups that are or might be users of these machines are included.

stant) have been carried out recently with these methods. Such work probably could not survive the pressure of a level budget, although there will continue to be useful and interesting problems in this field.

Since the level program of Figure I.25 allows for little correction of the current situation at the existing accelerator laboratories, it would cause a continuation of the underutilization of these laboratories at a figure close to that shown for fiscal year 1972 in Table I.8. This would imply that a substantial fraction of the slots and facilities tabulated in Table I.9 would stand idle or be used at a very low efficiency. This will discourage the use of these facilities for some of the most imaginative new experiments. It would also discourage the development of new methods and new ideas at these laboratories.

The long-term loss to physics that would result from these discouragements is not to be underestimated. As we have already pointed out in Section 9.4, we depend on the programs at the lower energy end of the spectrum to develop the new people and new methods feeding the more costly effort at the top of the energy spectrum, which is essential if it is to continue to be an original and exciting program. It must be recognized that many of the most original ideas and devices have been produced in these lower-energy laboratories. Along with these ideas and devices comes the identification of some of the most talented physicists, which is so important. And the development of powerful, simplified methods at these lower-energy laboratories may actually decrease the engineering demands on the experimenter, a change that would have an important impact on the program at N A L because it can reduce the burden associated with the greater overall complexity of experiments carried out there.

In addition to causing the loss of opportunities to try out new ideas and people, the underutilization of accelerators would severely limit the number of experiments devoted to answering quantitative questions, as well as limit the amount of time that could be assigned to each experiment, thereby reducing the statistical precision. The consequent loss to U.S. physics may be indicated by some examples of precision work that should be pursued vigorously to follow up the unsettled questions of today:

1. Spectroscopy of meson and baryon states. The understanding of the resonances requires much more definitive information. The methods discussed in Section 2.15 need to be pushed to more precise levels. The quantitative work is best done in terms of phase shifts (Figures I.10 and I.11), but this type of work has hardly begun for strange-particle resonances (K-meson scattering on nucleons).

2. Search for exotic hadrons. This seems to be a needle-in-the-haystack

problem and therefore requires better statistics, but it holds the key to the validity of simple quark models. (See Section 2.18.)

3. Mysterious fine-structure effects. (Section 2.19.)

4. The general systematics of hadron–hadron processes at low energy. There are many structural features that have been observed only roughly and should be studied much more closely. Precise quantitative data are needed to evaluate dispersion relations and sum rules as tests of various aspects of the theory. (Section 2-20.) The very powerful theoretical concept of duality (Section 2.24) may point the way to a much deeper understanding of hadron processes and is sorely in need of more accurate data on many hadronic phenomena at medium energies.

5. Detailed studies leading to information on pion–pion scattering, K-pion scattering, and so on. These studies also lead to information on mesonic resonances.

6. Increased precision in studies of the CP- and T-violating processes in K^0 decay. (Sections 2.10 and 2.11.) More careful determinations of the parameters are still needed, especially such quantities as the asymmetry between the modes $\pi^+ \ell^- \bar{\nu}$ and $\pi^- \ell^+ \nu$, where ℓ is electron or muon. Also, the parameters determining K_L decay into two neutral pions.

7. Precision measurements of the beta- and mu-decay spectra of K-mesons and hyperons. (Section 2.9, Table 1.1.)

8. Precision measurements on vector meson photoproduction and electroproduction. (Sections 2.6 and 2.8.)

9. Detailed studies of hadron states in deep inelastic scattering. (Section 2.5, Figure 1.2.)

This may give some idea of the kind of physics that will be delayed or set aside indefinitely by a budget plan that throttles the opportunity to carry out these highly quantitative measurements. Note that each item actually represents a *field* of study, not just one or two experiments. The level-funding plan would provide for only a fraction of the need. It would probably result in bringing competitive U.S. work in a few of the listed areas to a halt, since performing high statistics experiments at the required slow pace would cause such unreasonable delays that the thread of the work would be lost by any one group.

All of these operational problems that would be caused by the level funding as shown in Figure 1.25 and Table 1.12 would be compounded in the future as the result of limitations imposed on new construction. Already authorized construction projects would consume most of the construction money. The total amount permitted for new projects by the plan as outlined is $36 million spread over a four-year period. This might

be compared with the $145 million spread over a five-year period that would be available under a 6 percent per year growth rate (Table I.11). It clearly is hardly enough to bring to completion even one of the major projects listed in Section 9.5.

The net effect of the level funding would be a damaging continuation of the slowdown of activity in the field during the next few years combined with an equally damaging failure to provide the means for a future recovery.

9.8 CONSEQUENCES OF A SIX PERCENT PER YEAR DECLINE IN FUNDS

We turn now, with strong misgivings, to a funding program based on a decline in total expenditures at the rate of 6 percent per year. This is the equivalent to level funding in actual dollars if the recent rate of escalation of costs should persist. We should emphasize here that the discussion of the level program of Section 9.7, and of all the other funding programs, is based on 1971 dollars, which is intended to be a measure of purchasing power, rather than actual dollars.

Another way to arrive at the 6 percent decline in total expenditures is to assume that the total operating, equipment, and accelerator improvement program expenditures in fiscal year 1971 dollars will be held roughly at the fiscal year 1971 level, and that no new construction will be included after completion of the authorized construction. This is illustrated in Figure I.26 and Table I.13.

9.8.1 *Impact*

As can be seen from Figure I.26, the consequences of such a funding pattern would be disastrous for the field. All the difficulties of recent years, which are described in Section 9.2, would be exacerbated. Although the distribution of funds indicated in Figure I.26 still emphasizes the importance of exploiting major new facilities, the underutilization of these facilities, especially NAL, which was foreseen in Section 9.2 for fiscal year 1972, would be extended both in time and in magnitude.

The indicated suppression of existing programs would have an impact in the same direction, but going far beyond, the impact of the level funding pattern on these programs, as described in Section 9.7. It would be expected that elementary-particle physics in the intermediate-energy region would be virtually wiped out. Many of the slots and facilities indi-
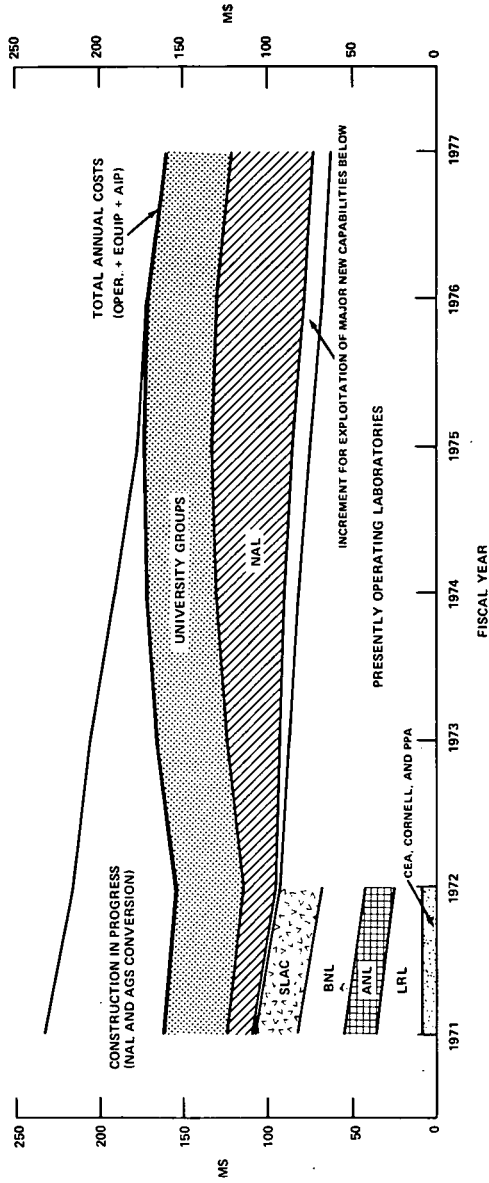
FIGURE I.26 High-energy physics cost projections for federal support. Six percent annual decline case. (Fiscal year 1971 dollars.)

154

TABLE 1.13 High-Energy Physics Cost Projections for Federal Support. Six Percent Annual Decline Case (Fiscal Year 1971 Dollars)

	FY 1971	FY 1972	FY 1973	FY 1974	FY 1975	FY 1976	FY 1977
Presently Operating Laboratories							
AEC operating	84.3	78.6	73.0	68.0	62.0	58.0	53.0
AEC equipment	15.5	5.6	7.0	6.0	5.0	4.0	4.0
AEC accelerator improvements	3.6	3.4	2.0	2.0	2.0	1.0	1.0
NSF	4.5	6.0	6.0	6.0	6.0	5.0	5.0
Subtotal	107.9	93.6	88.0	82.0	75.0	68.0	63.0
Increment for Exploitation of Other Major New Capabilities							
Operating	–	–	2.0	5.0	7.0	8.0	8.0
Equipment	1.5	1.5	3.0	3.0	3.0	2.0	2.0
Subtotal	1.5	1.5	5.0	8.0	10.0	10.0	10.0
NAL							
Operating	9.2	11.9	19.0	25.0	30.0	34.0	35.0
Equipment	5.6	7.5	12.0	15.0	15.0	15.0	10.0
Accelerator improvement	–	–	–	1.0	3.0	3.0	3.0
Subtotal	14.8	19.4	31.0	41.0	48.0	52.0	48.0
University Groups							
AEC operating	25.1	23.9	24.5	24.5	24.5	24.0	23.5
AEC equipment	1.0	1.0	1.0	1.0	1.0	1.0	1.0
Other agencies	11.0	14.0	14.5	14.5	14.5	14.0	13.5
Subtotal	37.1	38.9	40.0	40.0	40.0	39.0	38.0
Construction							
AGS conversion	7.0	3.2	–	–	–	–	–
NAL	65.0	59.0	42.0	20.0	6.3	–	–
Subtotal	72.0	62.2	42.0	20.0	6.3	–	–
TOTAL	233.3	215.6	206.0	191.0	179.3	169.0	159.0

155

cated by Table I.9 would necessarily stand idle. The opportunities for giving sway to the imagination and innovative talents of young physicists would all but vanish. Competitive U.S. work in many of the quantitative fields described in Section 9.7 would come to a halt. The attraction of the field for graduate students would disappear along with the thesis opportunities. The roots of the field, needed to produce new people, ideas, techniques, and information, would wither.

It should be clear that the impact on the broad spectrum program would be catastrophic. Although the distribution of funds suggested by Figure I.26 would force the underutilization of NAL and other major new facilities, it still shows that any significant utilization of these facilities would consume a substantial part of the resources that would be available for the entire program. Therefore, adequate funding would not be available for the existing programs at the accelerators other than NAL.

Further damage to these existing programs would be caused by the need of the university groups to divert the limited funds away from such programs in order to mount experiments at NAL. It can be assumed that the proportion of the university funds needed for the latter purpose will be at least as great as the proportion of the total funding going to NAL. We say at least as great because both the scale of the experiments and the problem of distance are greater (for most groups) at NAL.

In addition to the generalized damage to the field, this declining budget would undermine the careful planning of the past years that has led to a well-balanced development of appropriate special capabilities at each of the major laboratories. The shutdown of smaller accelerators and the curtailment of larger ones required by declining budgets would, in each case, eliminate some unique feature of the program. Examples are as follows:

1. The accelerators that are the most likely candidates for shutdown are

CEA: The storage ring (by-pass) project, which is on the verge of producing physics, would be eliminated. This is presently the only research activity at CEA.

Cornell: This is the only active high-duty-cycle, high-energy electron accelerator in the country. The broad-spectrum activity in electron physics in the range 1 to 10 GeV would be seriously damaged.

Bevatron: Contributes at high efficiency to the broad-spectrum activity for proton energies in the range 1 to 6 GeV, and the secondary beams produced by these protons. The traditions and people held together by the accelerator are unique. The Bevatron continues to serve as a focus of activity of one of the most innovative groups of physicists and accelerator designers in the world. The opportunity for the training of students is also unique at this accelerator.

ZGS: A most efficient source of beams of protons in the range 6 to 12 GeV and associated secondary beams. This accelerator is a major source of physics in this energy range. It also includes, as a unique and essentially permanent feature of the facility, the 12-ft hydrogen bubble chamber, which offers the first opportunity to carry out significant neutrino experiments in hydrogen. It is the first of a new generation of chambers.

2. Although our suggested budget distribution provides for some of the major new facilities other than NAL, the magnitude of the available funds would be inadequate. The possibility of taking advantage of the SLAC storage ring SPEAR would be minimal under this plan. Experiments on production in e^+e^- collisions, for which SPEAR is especially suitable, would probably be set back by years, while the corresponding work continues to move rapidly ahead in Europe.

3. A corresponding statement would apply to the AGS improvements, which are just coming to completion. The facility would be brought up to full intensity more slowly, and the equipment needed to take advantage of the new capability would be lacking, with the result that a number of promising new beams would be delayed or lost. The manpower required to make use of the new capability would not be supported. Special facilities such as high-energy, long-duty-cycle beams based on the new technology that could be used with the high intensity would not be possible. The loss to physics can be indicated by some of the experiments made possible by the improvement program: Measurements of very small K and $\bar{p}$ cross sections, which allow the study of these reactions at large momentum transfer and the study of resonance fine structure with rare particles; the study of very rare weak processes such as those with $\Delta S = 2$, $\Delta S = -\Delta Q$, neutral currents, $\Delta S = 3$, etc. (see Sections 2.9 and 2.10); the use of tertiary beams, for example, for K_S^0 studies on weak interactions, CP-violations and time reversal (see Sections 2.10 and 2.11); and many other very fundamental experiments that have not been feasible at available accelerator intensities.

4. Insofar as NAL is concerned, the need to give highest priority to its groundbreaking program has been recognized in terms of the proposed distribution. Nevertheless, the limitations are such that the accelerator would have to be deliberately underutilized—and seriously so. The result would be that the excellent opportunity offered by the good start that this accelerator has had would be dissipated, many opportunities would be missed, discoveries could not be fully pursued.

5. The plan allows *no* provision for future new construction. There would be no possibility for taking advantage of any of the several technological advances that are now in exploratory development. The future of the subfield would not be promising in these circumstances because

the knowledge, technical manpower, and industrial experience needed to make use of these new technologies would soon diffuse away from this country.

As stated earlier in this Section, a budget plan based on declining total expenditures would be disastrous for this subfield, and the consequences would eventually be felt throughout many other areas of science and technology.

9.9 CONCLUSIONS

In this Chapter we have reviewed the funding history of the field of elementary-particle physics and have tried to show how recent trends in funding are leading to a paradoxical situation in which the field has had remarkable support, especially in its need for machines at higher and higher energy, but is now being stifled by funding that is not adequate to provide for the use of the exceptional facilities that have been provided. This has happened despite an outstanding record of scientific and technical productivity in the field.

The lack of adequate funding has led to the closing down of one accelerator and underutilization of all others. It has reduced the opportunities in the field and made it less attractive to new blood. Large numbers of people working in the field have lost their jobs, and this is not limited to the PhD physicists, who in fact represent a relatively small fraction of the people employed in this endeavor. There are engineers, technicians, programmers, scanners, riggers, pipe fitters, and a great variety of other artisans who have been employed in a highly creative capacity in this field. Many of them have been let go as operating support for the program has declined (see Figure I.22 and Table I.7).

Much of the damage results from the fact that the facilities were established on the basis of clearly stated plans, which have not been supported in subsequent years. The lesson is clear, longer-term budgetary commitments are needed to avoid repetitions of the current situation. What is not clear is how this can be accomplished under federal budget procedures.

Although the present situation is difficult, it is not beyond hope, and we have attempted to make projections for future funding that will allow recovery. We have also considered various other funding patterns and what their impact on the field might be. The only one of the funding plans considered that is restrained but still can be recommended in good conscience would require an increase of 6 percent per year in total expenditures (in actual purchasing power). Even a level rate of expenditure would require spartan measures to allow the work to go ahead.

No attempt has been made to extend the projections beyond the five-year period. It is not implied that the curves of funding versus time should be extrapolated into the future, because future plans will depend very strongly on what happens during the next few years. The question for the future is: Does this field continue to hold great promise, and, if it does, what funding is essential to allow it to live up to its promise?

We think that the next few years will yield an affirmative answer to the first part of this question, and that the field will continue to illuminate some of the most promising aspects of physics for many years to come.

II
Nuclear Physics

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GEORGE L. ROGOSA, Atomic Energy Commission

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A. SUNYAR, Brookhaven National Laboratory
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B. BERTOZZI, Massachusetts Institute of Technology
E. A. KNAPP, Los Alamos Scientific Laboratory
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163

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L. J. RAINWATER, Columbia University
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D. M. ROBINSON, High Voltage Engineering Company
H. A. SCHWETTMAN, Stanford University
A. M. SESSLER, University of California, Berkeley

Consultant

R. G. HERB, University of Wisconsin

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NEEL W. GLASS, Los Alamos Scientific Laboratory
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164

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ALEXANDER ZUCKER, National Academy of Sciences, *Chairman*
G. L. BROWNELL, Massachusetts General Hospital

165

166

Preface

This report is being developed in a time of crisis for nuclear science. The crisis has two antithetical elements. On the one hand, new opportunities— and new facilities uniquely suited to exploit these opportunities—of great potential importance have been clearly identified. On the other hand, the funding for nuclear science has not increased in recent years, and our ability to react to the new opportunities and even to maintain our present program is in serious question.

The strong national support that nuclear science has received for some two decades has been used effectively to train a group of nuclear scientists and to establish a network of nuclear facilities at the forefront of the field. Major advances have been made in the fundamental understanding of nuclear phenomena; in parallel, nuclear science has had profoundly important effects upon our society, from the provision of nuclear energies to the provision of means for the alleviation of suffering and disease.

As in any other science, the investigation of its frontiers is vital to nuclear physics. New facilities are now being designed or constructed to open new areas of the field. Equally important to nuclear physics, however, is the broad multifaceted program that is based on a much larger number of smaller and very versatile facilities that carry through the many complementary activities necessary to progress in depth. However, the major new facilities are large complexes that are expensive in both scientific manpower and in dollars; these represent a new departure for nuclear physics and one that will pose new problems in their exploitation.

What are the consequences of current funding levels as measured against the long-range scientific and societal goals of the field? Responding to the

charge from the Physics Survey Committee, we have evolved a range of contingency alternatives for the field; we delineate these in four budgetary projections.

Full exploitation of the field, which makes maximum use of the trained manpower available, would require sizable annual increments in operating funds over the next several years. This level of expenditure would permit the exploitation of the new high-energy nuclear regions, full entry into the field of heavy-ion nuclear physics, and expansion of the multifaceted effort into areas as yet only tentatively explored.

An intermediate budget, with scientific manpower held approximately constant over the 1969–1977 period considered, would give up this bold and full exploitation of opportunities but would still provide a reasonable base for progress on the presently identified major fronts of nuclear physics. This funding level, requiring 6 percent increases per year, would not be adequate to ensure the United States pre-eminence but would ensure a highly competitive effort.

A constant budget pattern poses severe problems because of the necessity to add in the costs of the major new facilities to the costs of the broad range of activities under way. Clearly, this can be accomplished only at the expense of a sizable redistribution of the research effort. It cannot be done without very significant losses in people and in opportunities. A reasonable distribution of these restricted resources among the new and classical areas of nuclear physics requires by the end of six years the restriction of federal support to one third of the existing facilities and about a third fewer scientists than are now in the field. The research programs would lose much of their flexibility, and coverage of this widely spread field would be much thinner. Much of the research would be concentrated at the few larger facilities, and many groups, now with in-house facilities, would have to change to a user's mode of operation. The advantage of in-house efforts at many institutions would be lost. The advantage of the close presence of an active accessible scientific effort for the education of a modern man would be correspondingly lost.

Budgets decreasing at the rate of 5 percent a year (measured in dollars of constant purchasing power) would have, over the 1969–1977 period projected here, still more drastic effects. If research efforts are reasonably distributed over the various areas of nuclear physics, only minimal attacks on the new frontiers would be possible even though the present support of some 88 facilities would be reduced to support of about 33. Only about half of the number of scientists now working in nuclear-physics research could be supported. Coverage of the field as a whole would be minimal, and response to new discoveries and opportunities might well be uncertain. Further reductions would leave gaping holes in coverage, and it would be necessary

for the country to develop a new pattern that gives up active attention to the whole field and finds a more limited place in the international effort.

Quite evidently, the prospects for continuing progress in nuclear physics will depend sensitively on the level of funding and on the distribution of the available resources. Since 1968, the funding pattern has been one of constant or diminishing dollars in the face of inflation. Dismemberment of the nuclear enterprise has already begun. The future may well see a diminishing budget with the constricting consequences detailed in this report.

This report is part of a survey of physics being conducted under the aegis of the National Academy of Sciences Committee on Science and Public Policy.

The Nuclear Physics Panel of the Physics Survey Committee was organized in June 1969 to provide its special part of the overall Physics Survey and to respond to a request from the President's Science Advisory Committee for a preliminary report on nuclear physics for consideration in parallel with a recent report on elementary-particle physics developed by a High Energy Physics Advisory Panel to the U.S. Atomic Energy Commission. The Panel was asked to report on both low-energy and intermediate-energy nuclear physics and to provide objective bases for policy deliberations.

The Panel, in turn, organized specialized subpanels to report on various aspects of nuclear physics—its organization and funding and its relevance to other science and technology and to society in general. The subpanels met at irregular intervals during the remainder of 1969 and the first half of 1970. Some of the subpanels submitted formal reports; others submitted informal working papers only. In February 1970, by means of a questionnaire, the Panel solicited the views of some 150 university, federal, and industrial groups on personnel and program trends, the actual and potential output of various research activities, and the impact of funding changes on their programs. In July 1971 a group of consultant-readers, selected in consultation with the Chairman of the Division of Nuclear Physics of the American Physical Society, were asked to comment on a preliminary version of this report. Their comments were carefully considered by the Panel, and the results of these deliberations are incorporated in the report. The work of the subpanels, the contributions of many people who participated informally in this study, the comments of the consultant-readers, and the response of the nuclear-physics community to our questionnaire provided important input to the Panel's considerations. However, the responsibility for this report lies entirely with the Panel.

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Contents

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II

NUCLEAR PHYSICS

1 Summary

1.1 INTRODUCTION

Because the nucleus is an important part of our physical world, we must continue our efforts toward a fuller understanding of its features and its potentialities for the benefit of man. Nuclear science has already made important contributions—in big and little ways, through fundamental discoveries and steady developments—to a wide range of human endeavors. The discovery of fission phenomena and their use in harnessing nuclear energies came early in the development of nuclear physics when the gross, qualitative facts about the nucleus were being uncovered. The impact of these findings on the development of new sources of energy, on the nature of war and peace, on industry, and on medicine is now well known. Over the years, explorations to learn more about the nucleus have generated a steady flow of ideas, innovations, and applications. The breadth of current and projected activity and the strength of the field itself promise continuing benefits to science and society.

1.2 CHALLENGE OF NUCLEAR PHYSICS

Where in its development does nuclear physics now stand? The great advances in experimental means of the last two decades, but especially of re-

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cent years, have made it possible to discover and measure many fundamental properties both of naturally occurring nuclei and of nuclei made only in the laboratory. An empirical model has emerged that provides a central framework in which to fit the constantly emerging facts. But much more than an empirical understanding has been achieved. A firm, but not quantitatively exact, connection between the fundamentals of nuclear structure and the fundamental forces between nucleons is being built. These advances, in what may be called "classical" nuclear physics, still leave unanswered questions and puzzles. They point to a deeper set of questions about the structure of nuclei and the nature of nuclear dynamics. The answers can be found by picking apart and by building up nuclear states or modes of motion with the nuclear projectiles provided by a new generation of versatile accelerators. To accomplish this, complementary approaches based on a range of experimental facilities are intrinsically necessary.

1.3   NEW FRONTIERS

Other and quite different frontiers are now apparent. The characteristics of the deeply hidden, short-distance properties of nuclei are little known. New high-energy electron and proton facilities, which will provide the short-wavelength microscopes needed to explore them, will soon be in operation. Intense beams of mesons will probe aspects of the nucleus now only dimly seen. Another completely different class of information will be obtainable from beams of heavy ions from new facilities. The presently known nuclear world is only a small fraction of what could be studied, for the means are now known by which to produce and study some 6000 nuclear species as compared with the 300 found in nature and the 1300 more that have been produced with available techniques. Superheavy elements that lie well beyond our periodic table may exist. Heavy-ion facilities provide entry into this field. But even more important, they provide the capability for studies of the virtually unknown interactions of massive amounts of nuclear substance with one another. The study of nuclear excitations can be greatly extended. For example, because heavy-ion projectiles bring in such large amounts of angular momentum there is the exciting possibility of quantitatively exploring the very high angular momentum states of nuclei. As of now it is possible only to creep up to states with some 20 units of angular momentum; states whirling with 30, 40, or even 100 units will be studied to find out how the nucleus responds to such huge centrifugal stresses.

Nuclear science, which now rests on a solid base, is ready to move out in many directions. Its further development requires a diverse effort. We do not know what will be discovered; however, it appears certain that entirely

G

new and unexpected classes of phenomena important to the science itself
and rich in applications will continue to emerge.

1.4   CONTRIBUTIONS TO SOCIETY

There are many benefits to be derived as nuclear science evolves. Because
nuclear power is a billion-dollar industry, relatively small refinements in the
knowledge of nuclear properties can lead to annual savings of many millions
of dollars. New designs for nuclear research accelerators turn into sources
of radiation treatment for the 300,000 cancer patients who need them each
year—sources that are more effective medically and less costly to operate.
Advanced accelerator concepts—for example, the cryogenic accelerator—
point to technological applications in the field of power transmission. The
constant flow of systematic information on the properties of isotopes con-
tinuously opens new doors. Technetium-99 is a case in point. The proper-
ties of this isotope, which had been established in the 1950's, offered spe-
cial medical advantages that were recognized in 1964; it is now being de-
veloped into one of the very important diagnostic tools of nuclear medicine.
The Anger camera, an array of nuclear detectors, is used for 10 million
scans a year. High-resolution semiconductor detectors, developed in the
1960's to increase the accuracy of nuclear measurements, have found many
other uses; they are now used extensively in medical programs, in the anal-
ysis and monitoring of bomb tests, and in the important field of isotopic
analysis by nuclear activation.
    It is possible to put dollar values on some of these applications. The oper-
ating budgets of medically used accelerators total some $30 million a year.
The nuclear instrumentation business earns $100 million a year and saves
industry many times that amount. Radioisotopes are a $30 million a year
industry, and the products and equipment involving their use total several
hundreds of millions of dollars annually. But the importance of nuclear
applications cannot be reduced to a simple balance between these hundreds
of millions of dollars a year in economic activity and the $70 million in-
vested annually in basic nuclear research. The patients saved, the defense
of the nation, and the unshackling of mankind from his otherwise limited
energy resources could each be cited as more pertinent testimony to the
value of nuclear research.
    If the basic research programs in the United States were to falter, foreign
programs could provide, with some time lag, some of the developing nu-
clear information of interest to technology, medicine, and national defense.
However, the match to our technological needs would be poorer and slower.
Newly trained scientists in the United States would know less of the new

frontier ideas and methods. Technological advances would filter through nuclear industries that would grow in other countries, with adverse effects on the U.S. balance of payments. Re-establishment of U.S. pre-eminence would be difficult and would require considerable time and funds.

1.5   IMPACT ON OTHER SCIENCES

Nuclear physics is a basic part of physical science and, like every healthy and developing science, enriches and feeds on the stream of ideas and techniques that it shares with other sciences.

The nuclear nature of astronomical energy sources makes nuclear astrophysics an intrinsic part of the study of our universe; the recent interpretation of pulsars as neutron stars emphasizes the rich variety of nuclear phenomena that exist in stars. Study of the space environment calls on many familiar nuclear techniques. Nuclear physics is intimately connected with many other branches of physics and science in well-known ways. The nature of the nuclear forces led naturally into elementary-particle studies. There are still fundamental links between discoveries in these fields; nuclear phenomena provide the means and place to study the fundamental nature of the strong, electromagnetic, and weak interactions that are important in a domain much larger than nuclear physics. The intrinsic interests that nuclear physics focuses on quantum-mechanical many-body effects interconnect nuclear physics, solid-state physics, and statistical physics. The experimental tools of nuclear science connect with all the physical sciences, earth sciences, and life sciences, as well as with a host of applications ranging from those in archaeology and forensics to art. Nuclear phenomena and nuclear tools are part of a wide variety of research fields.

Nuclear physicists play essential roles in training modern scientists and engineers both in the classroom and in the research laboratory.

1.6   FUNDING PATTERNS AND THEIR CONSEQUENCES FOR NU-CLEAR-PHYSICS RESEARCH

This study sketches the developments in nuclear physics and their meaning for man, as well as the opportunities for further advances that are now at hand. To exploit these opportunities fully will require substantial increases in the people and money devoted to nuclear research. A strong reversal of the downward budgetary trend of the last three years can keep this country in the pre-eminent position it now holds in all aspects of the science. If, on the other hand, budgets are kept at constant buying power, or reduced,

severe shackles will be imposed on nuclear physics, bringing the very real danger of lost scientific and technical opportunities.

We believe it essential that the new ventures immediately before us be pursued vigorously. At the same time, we believe that it is important to maintain the breadth of present programs, for the very nature of nuclear investigation makes it clear that there is no single experimental approach that can provide all the answers. There are many problems in the already opened areas that remain unsolved.

Nuclear physics is in a particularly difficult position because major accelerators, authorized several years ago, are soon to come into operation, and these operations will require substantial funding, roughly equal to the average annual cost of their construction. The scientific justification for these facilities is as strong now as then; it might be noted that other countries, having recognized this, are building or planning similar ones. To exploit their manifold opportunities will require increases in the people and money devoted to nuclear research. Even to fund these new directions and the present broad programs in a merely adequate manner will require substantial increases of funds. Such increases are made especially necessary after the past several years of stringent budgets that have inhibited the upgrading of facilities commensurate with the growing complexity of the scientific questions.

To bring out clearly the sensitivity of nuclear research prospects to the level of funding, we have analyzed in Chapter 6 four levels of budget that are assumed to develop over the years 1969 through 1977. At each level, it has been necessary for definiteness to choose a specific distribution of funds. These distributions attempt to meet, within the constraints, the requirements of new research directions, breadth of current programs, and efficiency of operation. The detailed choices of distributions are, of course, controversial and debatable. We do not claim that they are necessarily the best possible or put them forward as a suggested blueprint for the future of nuclear physics. We believe, instead, that they are reasonable definite choices on which to form a basis for discussion of the effects of different funding policies on the health of the nuclear research program and its flow of applications.

Before going into these analyses, two special points should be noted. First, the most costly of the new facilities, the Los Alamos Meson Physics Facility (LAMPF), will have sizable programs in areas outside of nuclear physics—particle physics and biomedical and weapons research. We have, however, included the total expenditures for LAMPF in our considerations. Second, as will be seen, the mission-directed laboratories maintain nuclear research programs consonant with their applied objectives. It may well be that these laboratories cannot redirect their activities, since they

have other criteria to consider, and for this reason we have not included them in discussing possible responses to various funding levels. These programs are, however, an important supplement to the program we analyze.

The first of the budgets we analyze would permit full exploitation of the opportunities now before the nation in nuclear physics. Aggressive explorations in the use of high-energy nuclear probes and heavy-ion physics together with a prudent expansion of the present multifaceted program based on conventional accelerators are the major components of this program. There exists already an adequate base of facilities to support most of this ambitious effort; the rate at which the new fields can be explored is limited primarily by manpower. Such a program would require increases in funding levels so that by 1977 the annual operating budget would be slightly more than double that of the base year, 1969 (see Tables II.17 and II.22); it would ensure, we believe, an active and productive enterprise with the prospect of important new discoveries and many benefits to science and technology.

Recognizing that the required budget increases may not become available, we have analyzed an intermediate budget—intermediate between the expanding budget just discussed and the current slowly declining one. This budget is drawn under the constraint of holding the scientific man-years of effort approximately constant. It would permit adequate exploitation of the new high-energy facilities and contemplates a new heavy-ion physics facility. The budget for the other aspects of the broad nuclear program is held nearly constant; to do this in the face of the increasing sophistication and, therefore, costs of scientific investigations would require concentrating funds and manpower on the most versatile installations. This funding level would have to rise by about 40–50 percent by 1977, an average rate of increase of about 6 percent per year in the 1969–1977 period (see Tables II.18 and II.23). Such a budget would preserve the national investment and would provide a reasonable base on the major research fronts. It would not permit full exploitation of the opportunities of nuclear physics. A number of problems would emerge, problems that would be very sharply exacerbated in the lower level budgets that are examined below. With scientific manpower held constant, there would be only limited opportunity for young people to find a creative place in the research program.

In the third budget analyzed, expenditures are held at constant purchasing power. Under this constraint, the new and expensive ventures considered necessary for the viability of the field could only be carried out at the expense of drastic contraction of the on-going programs. Any reasonable distribution of effort among the various kinds of programs would result in forcing the shutdown or drastic restriction of the activities of many

facilities and forcing many talented people out of the field. The remaining program would become heavily concentrated in a few of the best and most versatile facilities, obliging many highly productive groups to change into mainly a users mode of operation. It is important to note that in our comment on limiting the number of supported facilities to the best, "best" means best in quality and not necessarily the largest. It seems, however, only prudent that at least a few small facilities be maintained to permit the small-scale innovativeness that has been an important part of the nuclear research pattern. While the new large facilities are funded in this analysis, it would be necessary to do so at a reduced operational level to maintain the breadth of the whole field. Both in these and the older broad programs, only a fraction of the potential of the field could be exploited. Sizable dislocations of institutions and people would occur. A simple but shocking summary of the effects is given by the fact that such a budget situation would require that almost a third of the present scientific man-years of research be cut away (see Tables II.20 and II.24).

Finally, we have analyzed the effect of a budget declining at the rate of 5 percent over the eight-year period. The effects can only be described as catastrophic to both nuclear physics and its practitioners. With the greatest reluctance, we have concluded that it would no longer be sensible to operate the newly constructed LAMPF because of the large fixed costs that are incurred before any research can be done. The capability even to probe toward the new frontiers would be almost entirely removed. Even so, the broad, multifaceted program would have to be drastically curtailed and the total number of scientific man-years in nuclear research cut in half. Should such a budget become reality it is very likely that the field would become completely disorganized (see Tables II.21 and II.25).

Paradoxically, the possibility of restricted support comes just as nuclear physics is prepared to yield new returns. Nuclear physics is important to the future of our civilian power needs, the strength of our national defense, a growing host of technological benefits, and our scientific understanding. The importance of maintaining a broad-scale attack on nuclear problems is clear in terms of the potential results both in new nuclear knowledge and in practical applications. It is reasonable to argue that the nation cannot afford to slip in any major area of the nuclear sciences. The stakes—scientific and practical—in this gamble with the future appear too great.

Yet, the dismemberment of the nuclear enterprise has already begun. It could be seen a year ago in the difficulties that the most recent graduates experienced in finding places to use their scientific talents to maximum effect. It can be seen in the emergency measures to which the nuclear laboratories have had to turn in order to avoid the disruption of their vital

programs. These temporary measures have been based on the hope that the difficulties are only temporary, but they cannot continue without permanent damage. Only a reversal of the budget declines can save the situation.

2   The Science of Nuclear Physics

2.1 SCOPE AND NATURE OF THE FIELD

Because nuclei are important constituents of the physical world, their study is an essential part of science. The study of nuclei is inescapably connected to other fields of physics. The very nature of the force between the individual constituents of nuclei forms a bridge into particle or high-energy physics. A body of related phenomena that can be studied in both fields makes the ties even stronger. The central role of nuclear processes in energy transformation in stars produces a common ground of interest and research with astrophysics. The applications of nuclear properties, phenomena, and techniques in other sciences and in a very large number of technological specializations have created overlaps and bonds with many fields. However, in this chapter we shall concentrate on the study of the intrinsic properties of nuclei.

Nuclear physics studies the atomic nuclei found in nature and produced in the laboratory. From the data and the characteristic phenomena that are uncovered are developed the concepts needed to build a quantitative physics to confront quantitatively both old observations and new predictions. The goals of this physics are to identify and describe the laws of nuclear structure underlying the symmetries and the fundamental modes of nuclear motion and to establish firmly their connection with the basic forces between nucleons.

The domain of nuclear physics is very large, and it is not enough to explore only a part to understand the whole. It is necessary to study a great number and variety of nuclei and a wide spectrum of excitations within a nucleus. Some nuclear phenomena appear uniformly in almost all nuclei, some are limited to small islands of the nuclear periodic table. Some occur over a broad range of excitation energies; some appear and vanish within very small changes in energy. Analyses of many nuclear phenomena are possible only because a large number of reaction probes, each yielding its own type of distinctive information, have been developed. No one nucleus,

no particular way of looking at nuclei carries the magic key to understanding.

The need to explore a large domain, to develop a large body of systematic information for the development of nuclear physics itself, accords well with the external uses to which that information can often be put, as will be shown in the following chapters of this report. A wide knowledge of nuclear properties is vital to applications in industry, medicine, and other sciences. It is important also to the study of fundamental physical phenomena in which the nucleus serves as a microscopic laboratory, since one must then know how to choose advantageous and well-understood conditions.

Progress in the exploration and understanding of this domain is made, as in other fields of physics, by a combination of approaches. Systematic experimental and theoretical work follows known phenomena into unexplored regions. Close comparison between theoretical and experimental views has served as an important source of direction to both. At the same time, investigations motivated only by the need to explore the unexplored have frequently uncovered new phenomena and given us new insights later to be built into the pillars of nuclear physics. The theorist too can systematically investigate the quantitative bounds and applicability of a formulation or try to leap over the difficulties and restrictions with a new synthesis or hypothesis. Nuclear physics has progressed by both the systematic evolutionary ventures and the revolutionary changes that provide new insights and openings into entirely new areas. Maintenance of a proper balance between the two classes of ventures is part of the art of both experimental and theoretical nuclear physics.

A third class of nuclear investigations must also be included—that loosely called programmatic. Although carried out for well-defined, practical engineering purposes, these investigations provide an important body of precise, systematic nuclear data that can point the way to entirely new ideas. Nuclear physics has profited greatly from its close relation to applied fields, and two important recent results achieved in this way are part of the later discussion.

2.1.1   *Overview and Summary of Progress in Nuclear Physics*

The techniques of experimental nuclear physics have, in this last decade, undergone changes that can only be described as revolutionary. New accelerator developments have not only vastly extended the range of energies in which fine resolution measurements can be performed but have also made possible the acceleration of many types of nuclear projectiles under more flexible and variable conditions. New detectors have made it possible

to achieve orders-of-magnitude improvements in precision. The development of computers and their incorporation as integral parts of experimental arrangements have opened entirely new methods of analysis and data handling, which have, in turn, brought about an enormous improvement in the completeness and overall quality of the data. This technical mastery has led to a deepening knowledge of the static and dynamic characteristics of a wide range of nuclei in both their ground and excited states.

A set of reactions, steadily increasing in number and subtlety, has been developed, each yielding a specific kind of information about the nuclear state studied. By using as many reactions as can be brought to bear, each probing a particular facet of the state involved, a wide variety of nuclear excitation modes has been discovered and studied. So far only the lower-lying excitation regions have been accessible in this thoroughgoing way, but this boundary is being erased as time and technique move forward.

A surprisingly simple model, known as the shell model, has emerged as a basic element in summarizing the ever more subtle body of empirical knowledge that is being developed. Like the model of the atom, the nuclear shell model is based on the concept of individual nucleons orbiting in a field of force. The field of force has been found to be spherical in some nuclei, deformed into a pancake or football-like shape in others. It has recently been discovered that at least some nuclei display both forms among their excitations. The description of the excitation spectra depends intimately on the interactions between the nucleons in the outer orbits. In fact, these residual interactions, as they are usually called, are determined empirically by fitting to the spectrum and its characteristics.

The simplest nuclear excitations have simple shell-model descriptions in terms of the motions of a few nucleons. There are others in which many particles move together in "collective modes." Such collective modes can be visualized as rotations of deformed nuclear shapes, as waves rippling the nuclear surface, as spin oscillations in the nuclear substance, or as vibrations of the neutrons and protons against each other. The analysis of these collective modes in terms of the shell model appears to be at least qualitatively possible—even if complex—and indicates that a basic unity of the underlying description may be achievable.

The aim of nuclear-structure physics is to go further, both in uncovering new phenomena and in unifying apparently different aspects of nuclear behavior. Explorations with more and different kinds of reactions will reach new aspects of known states. The complex regions of higher-lying excited states await detailed study by all the nuclear means possible. It is possible to produce and investigate nuclei with ranges of neutron and proton composition very different from those now known. We do not know whether these discoveries will fit tamely into our present views. In-

deed, nuclear excitations have been uncovered recently that apparently do not.

These are important directions for nuclear physics. Reactions capable of providing specific information about more complex forms of nuclear motions are being developed. For example, reactions in which one or two nucleons are injected to produce an excitation are becoming familiar tools. Reactions in which three, four, or more nucleons are transferred together will soon be used to pick out states and components that were previously inaccessible. The new generation of electrostatic accelerators capable of providing the higher but still very sharply defined energies and heavier projectiles under conditions of easy variability is crucial.

Little is yet known of the high-lying modes—for example, corresponding to excitations of deeply buried single-particle orbitals or couplings with collective modes. Even the low-lying states may contain small amounts of tightly bound multinucleon aggregates. The new intermediate-energy cyclotrons are designed to explore these still obscure aspects of the nucleus.

A quite different set of probes is being readied in new, high-energy nuclear facilities. New accelerator sources and detection devices now under construction will permit precise nuclear investigations by high-energy protons and extend the current limits of study with high-energy electrons. Examining the nucleus with these two different short-wavelength beams will be like putting the nucleus under microscopes illuminated by complementary radiations. New copious sources of mesons can be used to probe the nucleus for still different specific components of nuclear motions. Mesonic atoms, formed by mesons orbiting the nucleus, sampling and reporting on the nuclear matter that they traverse, provide still different information. The intensive study of hypernuclei, formed by replacing one of the constituent neutrons or protons by a "strange particle"—a hyperon—can provide still another view from deep inside the nucleus.

All of our ideas of nuclei and, therefore, of nuclear physics, are based on only a small fraction of the total number of nuclei that it will be possible to produce and study. Technical means are at hand to expand the nuclear species vastly from the presently known 1600 to over 6000 and to bring us into nuclear regions far from those that nature and art have, so far, provided for study. We do not yet know what phenomena or what potential applications are to be found in this new world. The possibility of the existence of a region of stable superheavy nuclei—that lie beyond the heaviest now known—has been much discussed. If our current ideas on element production are correct, the superheavies can only be created by a leap over the regions of instabilities that surround them. This leap can be achieved by the bombardment of massive nuclei with massive projectiles

at energies sufficiently high for them to overcome their mutual repulsion and fuse together. Such collisions between massive nuclei appear also to be fruitful sources of the many other nuclear species we seek. The new heavy-ion facilities now being planned will begin to open these possibilities.

Nuclear reactions are more than a tool to be employed in nuclear-structure investigations. They contain intrinsic information on the dynamics of nuclear motions and interactions. Nuclear reactions are not nearly so well understood as is nuclear structure. However, it is possible to distinguish different classes of phenomena. Nuclear-reaction cross sections show both extremely sharp energy dependences, called compound nuclear resonances, and very broad energy structures. These are taken to be the corresponding manifestations of very complex and of very simple modes of motion. The simple modes are described in terms of simple orbits in a field of force, which is the extension of the shell model into the region of energies at which reactions take place. The simple modes have, however, a very special importance for nuclear-structure studies because the direct nature of the transfer of energy, angular momentum, and particles provides specific mechanisms for exciting specific modes of motion in the nuclear system. Indeed "direct reactions" are one of the most important tools of nuclear-structure work.

Most of the reactions studied are initiated by a light bombarding particle. It is now becoming possible to study the reactions between massive nuclei. As we have noted, there are many technical benefits to be garnered from such work. But an additional and very important reason for such experiments is simply that we know so very little about such reactions. Work with the lighter nuclei that are now available points to the existence of surprisingly simple modes of motion. We have still to learn the basic facts of the regime of interactions between massive amounts of nuclear substance. The nuclear regions of high angular momenta of states with 30, 40, and more units of $\hbar$ are as yet unentered but would be opened by heavy-particle reactions. It is for these reasons that the nuclear community has been very interested in acquiring the facilities that have the reach—in energy, precision, and variability—required to explore this new domain of nuclear physics. Part of this research is provided by several of the most recent machines; but if the whole is to be studied, a much larger capability is required.

We have spoken so far of empirical knowledge and empirical models. How to relate this phenomenology to the basic internucleon forces is a fundamental problem of theoretical nuclear physics. The shell model, built as a summarizer of experimental information, goes much further and appears as a halfway meeting ground between experiment and theoretical fundamentals. Theoretical methods have been and are being developed to

connect the simple, gross properties of nuclei—their sizes and the strengths with which their constituents are bound together—with the forces between nucleons. The agreement achieved is far from perfect but sufficiently impressive to indicate that we are moving in the right direction and to motivate strong efforts against the obstacles. Further, these methods also show how the main concepts of the shell model—the field of force, individual nucleon orbits, and the residual interaction—can be connected with the internucleon forces.

While only prudence motivates the plans for the future, the possibility that these new and powerful capabilities may uncover things wholly new should not be entirely ignored. Who is to say that entirely new states of matter and energy do not come into play in the nuclear interior—states that may, and presumably do, have very small effects on gross nuclear properties but that can be searched out in the nuclear domain and whose very existence may have profound impact on our view of nature. Until all is known, even the familiar may contain the strange. How much more pressingly valid this is in the still strange domain of the nucleus!

2.1.2 *Nuclear Concepts and Discoveries and the Unity of Physics*

Out of the study of nuclei and nuclear reactions there have been distilled concepts and characteristic phenomena that apply in a domain of much wider extent than nuclear physics. These "universals" have been carried over into other fields—sometimes almost directly and sometimes as parts of a generalized law. None of this should be surprising for the chief problems of modern physics are concerned with the behavior of quantal systems, even though they are the objects of study of fields as different as the physics of solids and liquids or of atoms and molecules or of nuclei or of elementary particles. The visibility of the phenomena and the immediacy of the concepts will, of course, vary from field to field as the foci and emphases in the fields differ. In what follows, some of the universals that originated in nuclear physics will be described, together with examples of their applications elsewhere in physics.

Atomic physics added to classical physics the intrinsic spin of a particle as a degree of freedom. Nuclear physics added isobaric spin to describe the proton–neutron states of the nucleon. The near symmetry between these two states is the basis of the approximate invariance, or symmetry, law of isobaric spin conservation that is one of the major principles of nuclear structure and dynamics. The still more approximate symmetry combining spin and isobaric spin was discerned in the supermultiplets of nuclear states and described in terms of a unitary spin. Isobaric spin conservation is broken by the weaker Coulomb interactions, and the concept of such a bro-

ken symmetry has had an important place in nuclear physics and, more recently, in particle physics. Particle physics has discovered the added internal dimension of hypercharge. The generalized symmetry based on spin, isobaric spin, and hypercharge has formed the basis of a unitary representation for the elementary particles that provides a far-reaching classification.

The connection between nuclear and particle physics can be seen again in the weak interactions. The weak interactions were first discovered in the radioactivity of nuclei and many of the chief properties established in nuclear experiments. Weak interactions are now known to be of universal occurrence in the elementary particles, and probing the nature of these interactions has emerged as one of the central problems of high- and low-energy physics. In these continuing studies, nuclear physics has determined some of the most crucial aspects.

It is curious that nuclear physics provided the arena in which both the weak and the strong interactions were discovered. Both classical and atomic physics were described in terms of the electromagnetic and gravitational forces alone, both long-range. The strong, short-range interaction between nuclear constituents is understood to be mediated by the mesons—the study of whose properties and interactions are at the center of present particle physics.

The dynamics of interactions forms a common area of study. The sharp compound nuclear resonances were first observed in nuclear reactions, and the formalism for their description was developed by nuclear theorists. The direct reaction had been known from the study of atomic collisions, but its coexistence with the compound resonance was first discerned in nuclear processes. The specificity of the direct reaction for the measurement of a specific property is one of the major discoveries and has formed the basis for one of the most important investigative tools of nuclear physics. Both the direct reaction and the compound resonance are now part of the phenomenological approach used in the study of atomic and molecular physics and elementary-particle reactions. At high enough excitation energies of the compound system, statistical descriptions, including the concept of temperature, were found useful for nuclear reactions. A modified form of this approach has been employed in chemical physics and highly inelastic hadron–hadron collisions. Intermediate structure was discerned in nuclear cross sections but has now been seen in molecular reactions.

The identification of the fundamental modes of motion, or elementary excitations, is crucial to the understanding of any quantum system of particles. The nucleus, quite remarkably, showed the applicability of the shell model and the importance of the single-particle orbital—in spite of the absence of an atomlike force center and the presence of strong short-range forces. The history of nuclear structure is to a great extent that of the un-

covering of the single or few particle modes—and the co-existence with them of collective modes in which many of the constituent particles participate. These apparent contradictions cannot only be reconciled; the two modes are contained in a single, unified formalism. The collective mode has been taken over to form a part of modern atomic and molecular theory. In turn, nuclear physics has directly benefited from the discoveries of solid-state physics. Thus, the explanation of the phenomenon of superconductivity in terms of correlated pairs of electrons was taken over into nuclear physics as a basic component of the description of the correlated structure of the low-lying states.

The central theoretical problem of any field of physics is to form a clear connection between the phenomenological models or descriptions and calculations based on the fundamental forces between its constituent particles. The fact that the forces interact strongly between nucleons and are of short range means that the averaging methods of atomic physics are not capable of making the connection. A set of developments of many-body theory that could take into account singular interactions between pairs of nucleons that move in the averaged field of the rest of the nucleus forms the basic first step. These methods, based on summing the repeated interactions between pairs of particles, have been applied to the properties of liquid helium and to those of other quantum liquids, to the calculation of the energy of an electron gas, and to the correlation corrections in atomic binding energies.

New areas are about to be explored in nuclear physics. In part these explorations will be searches to evolve the new descriptions necessary, but, in important part, they will be searches for new universals—perhaps new static and dynamic symmetry laws, new modes of motion, or even, *mirabile dictu,* new forces.

The next pages are devoted to fuller descriptions of the progress of nuclear physics and reveal the breadth and promise of nuclear physics in some detail.

2.2 NUCLEAR STRUCTURE

2.2.1 *The Birth of the Shell Model in Its Single-Particle Form*

By 1949, a considerable body of knowledge had been accumulated on the simpler properties of the nuclei in nature. Masses had been measured and used to deduce the energy with which their constituent neutrons and protons are bound together. Nuclei with particular neutron or proton numbers, so-called "magic numbers," were seen to be especially tightly bound. Radii were roughly known. The angular momenta of the ground, or most

stable, states of many nuclei had been determined. For some nuclei, static electromagnetic moments that describe the shape of their distribution of charge or magnetization were known. However, with the exception of a few long-lived or "isomeric" states, very little was known of the properties of the excited states of nuclei.

While only a modest amount of detailed information was available for any one nucleus, knowledge of the systematic variation of the known characteristics with neutron and proton number led Mayer in this country and Jensen in Germany to propose a general model of the structure of nuclei. The shell model that they proposed took over from atomic physics the basic idea of individual particle orbits in a central force field. The form and nature of this force field had to be such that the sequence of orbits filled by the neutrons and protons generally reproduces the angular momenta of nuclear ground states. Further, the energies of the orbits were *required to clump together into reasonably well-separated bands, or shells,* in such a way that the filling of each shell coincides with a magic neutron or proton number, much like the closure of atomic electron shells at the noble gases. All this, it turned out, could be accomplished with a relatively simple form for the underlying force field. This shell model has been central in the development of our understanding of nuclear structure.

2.2.2   *Advances in Experimental Technique—the 1950's*

Advances in experimental technology in the 1950's expanded the field in several ways. New nuclear species (both new isotopes of known elements and those of previously unknown elements) were discovered with the postwar reactors and accelerators. High-efficiency detectors of nuclear electromagnetic radiations and of nuclear particles were developed that had the energy discrimination required to resolve the separate transitions that make up a nuclear spectrum. This, together with the necessary fast electronics, made it possible to identify the excited states, to unravel the level schemes of wide ranges of nuclear species, and to isolate specific nuclear properties for detailed study. Accelerators that could bombard nuclei with projectiles of high but sharply defined energies were being developed. These, together with detectors that could measure accurately the final products made it possible to study specific nuclear excited states—and to do so in a number of different ways.

2.2.3   *Advances in Experimental Technique—the 1960's*

The 1960's saw still another wave of radical innovations and developments in the technology of accelerators, detectors, and data analysis. New accel-

erators were engineered that could produce higher-energy projectiles, allow easy variation of the projectile type and projectile energy, and still retain the sharp energy definitions required in nuclear physics. New solid-state detectors have made it possible to achieve order-of-magnitude improvements in precision; whereas in the 1950's it took great effort to measure the energy of a gamma ray to 1 or 2 percent, 0.2 percent is now routine. Computerization, both in the analysis of nuclear data and in the on-line control of actual data acquisition, has made it easier to recognize more complicated nuclear patterns and to improve the completeness and quality of the data.

As a result of these technical advances, the number of nuclei studied has been increased from the 300 found in nature to around 1600; accurate and systematic information has been acquired on the position and properties of the excited states of many of these nuclei. The lighter nuclei, whose levels are more widely spaced, more easily resolved, and more accessible to low-energy projectiles have been more thoroughly explored. In light nuclei, the region explored extends up to 5 to 10 MeV. In heavy nuclei, it does not extend beyond 1 or 2 MeV.

2.2.4 *Nuclear Spectroscopy and Nuclear Reaction Studies*

Nuclear levels have a wide variety of characteristic properties, and these must be studied in many ways and with many probes. The simplest properties of a nuclear level are its energy, the angular momentum, and parity, i.e., the quantum numbers describing the state of the nucleus. To these exact numbers we add the isobaric spin quantum number associated with an approximate symmetry among neutrons and protons. The recent discovery of states in heavy nuclei that are the neutron/proton analogues of one another has shown isobaric symmetry to apply to a much wider range of nuclei than the low-mass region for which it was first proposed. These quantum numbers as well as more intimate relations between nuclear levels are inferred from the classical tools of spectroscopy based on the study of electromagnetic (gamma-ray) and weak-interaction (beta) decay, and from the more recently developed reactions studies.

How a state is excited in a reaction tells us about its nature. Electromagnetic methods have been extended by using the strong transient Coulomb field carried by nuclear projectiles to excite states of the target nucleus. This electromagnetic probe selects out strongly collective modes and has been an important tool in their discovery and study.

Clear information of another type comes from the so-called direct reactions, in which the state is formed from the target state by adding or subtracting nucleons without disturbing those already present. One-nu-

cleon transfer reactions have been developed into quantitative tools of wide applicability by the work of more than a decade. More recently, two-nucleon transfers have been under study. While not yet able to provide a quantitative measure, they have been important in establishing qualitatively how two nucleons are fitted in to build up the nuclear states that we are trying to analyze. Other multinucleon transfers are now being examined. The decay of the recently discovered analogue states provides still another distinctive and useful reaction. So will the whole range of new reactions that become accessible only with the very much higher-energy projectiles soon to be available from the larger accelerators. Each of the many possible reactions brings with it different, specific information, all of which is needed to fix the character of the state. This need for the use of a large number of different methods and reactions is one of the characteristic complexities and opportunities of nuclear physics. A high premium is thereby placed on a many-sided national program with flexible and versatile research programs and research facilities.

2.2.5 Rise of the Shell Model in Modern Form

Out of this series of studies a surprisingly simple physical picture of the nucleus, centered around a generalized version of the shell model, is emerging.

The shell model was first proposed to explain primary ground-state properties of nuclei. As information about excited states accumulated, it was found that the angular momenta and parities of nearby excited states could be interpreted as the excitation of a neutron or proton from one orbit to another. Serious attempts to obtain quantitative descriptions of these states in terms of many-nucleon shell-model calculations began, mostly in Britain, the United States, and Israel for the very light nuclei—those of atomic weight below oxygen-16. The main ingredients of such a calculation include not only the central force field but also an interaction between the nucleons of the outer shell—called the residual interaction. The few parameters that define the effects of the residual interaction are fixed by the requirement that the calculated energies reproduce those of the low-lying excitations of a large number of nuclei. Confidence in the model comes from its wide applicability achieved with only a small number of adjustable parameters. After the first successes based on fragmentary data, there began in this region of light nuclei a very close partnership between experimental discovery on the one hand and interpretation and prediction by the model on the other.

At the same time, there was a series of discoveries of nuclear collective phenomena that clearly involved the cooperative motion of many nucleons. At first sight, such modes of motion appeared to be outside the

framework of a shell model based on independent orbits. Over the years many of the collective phenomena have been seen to fit naturally into an extended shell model that includes residual interactions. Experimental and theoretical work continues on the search for new modes of nuclear motion, on the interactions between different modes, and on their description in terms of the movements of individual nucleons.

2.2.6 Strongly Deformed Nuclei and Rotational Modes

By 1950, it was known that nuclei in the rare-earth region of atomic weight between 150 and 180 had large static quadrupole moments, showing that a sizable part of the nuclear charge distribution was far from spherical. This was understood in shell-model terms by taking the force field to have not a spherical but a deformed spheroidal shape. The corresponding neutron and proton orbits then adjust to and align with this shape. There followed in the early 1950's the discovery of distinctive sequences of levels in the spectra of the rare-earth nuclei that were reminiscent of the rotational bands of molecules. Very strong electromagnetic radiative transitions between levels of the rotational bands showed the existence of large dynamic quadrupole moments. A group at Copenhagen interpreted these bands as rotations of the deformed nuclear shape. A model based on individual orbits in a deformed force field, together with collective rotational motion of the deformed shape, has been found over the last 15 years to apply very successfully to the rare-earth nuclei, to the very heavy nuclei around uranium, and even to certain light nuclei.

More recently, rotational bands have been discovered among the excited levels of nuclei that are not deformed in their ground state. Oxygen-16 shows a near-rotational sequence beginning at an excitation energy of 6 MeV. The reverse situation is suspected in the rare-earth nuclei, which are deformed in their ground states and appear to have spherical excited states. The shape is, then, a dynamical property, differently manifested at different excitation energies. The systematics needed to understand this phenomenon fully are still to be gathered. It is an important fact that the shell model for oxygen-16 has shown itself rich enough to describe both the excited deformed rotational band and the nondeformed ground state. The rotational band appears as the excitation of many nucleons out of the closed shell, the excited nucleons being tied together by the residual interactions to result in an overall deformed state.

2.2.7 Weakly Deformed Nuclei and Vibrations

Nevertheless, it should not be concluded that everything connected with the dynamical motions of nuclear shapes is clearly seen or well under-

stood. Outside the small islands of nuclei that show large deformations, the pattern of levels ceases to exhibit the simple rotational sequence, although a pattern can be discerned that varies systematically from nucleus to nucleus. It had long been known that strong electromagnetic transitions are involved, as in the case of the rotational nuclei. All this strongly suggested that motions of the shape are involved but in some form different from the rotation of a simple stable deformation. The fact that the excitation spectra of some of these nuclei have many of the characteristics to be expected from a quantum-mechanical vibrator rather than a rotator suggests a description of harmonic oscillation about a spherical equilibrium shape. Recently, however, measurements of the static quadrupole moments of the first excited state of a number of these nuclei, in the cadmium region, were made in this country, in Europe, and in Israel; ingenious use is made of the Coulomb field of heavy projectiles first to excite the nucleus and then to produce scattering from the quadrupole charge distribution of the excited nuclear state. These moments, which would vanish for harmonic oscillations about a spherical equilibrium shape, turn out to be large and clearly indicate a significantly deformed shape, although less so than in the rotational islands. In the meantime, experiments at Berkeley, using heavy-ion projectiles, excited high-energy and high-spin members of rotational bands in deformed nuclei; the energies of these levels show considerable deviations from the simple rotational pattern, the amount of deviation increasing with increasing angular momentum. A set of empirical relations has been found to fit the sequences. Recently, it has been observed that a quantitative relation also fits the level energies of many of the nuclei both inside and outside the rotational islands; this indicates that a similar, as yet unknown but important, mechanism is at work in both regions. Further work on the highly excited, high-angular-momentum members of these sequences of level is essential.

2.2.8   *The Shell-Model Description of Collective Vibrations*

There are other modes of nuclear excitation that indicate the cooperation of many nucleons. It was known in the early 1950's that the absorptivity of nuclei for electromagnetic radiation is concentrated into a band whose main excitation energy varies smoothly and regularly with neutron and proton number—from around 25 MeV in the light nuclei to around 15 MeV in the heavy ones. For this strength to be concentrated in one nuclear excitation, that of a large oscillating electric dipole, the protons must vibrate as a group against grouped neutrons. This giant-dipole state was the first collective excitation discovered. Since then, an electric octupole collective mode has been identified, and other modes are suspected. The two special characteristics—the collective cooperation of many nucleons and the

smooth systematic variation with proton and neutron number—seemed to set such important aspects of modes apart from the individual orbital picture; semiclassical descriptions seemed more appropriate. It was soon understood, however, that such modes can have a simple shell-model description. In shell-model terms, the giant dipole and octupole collective modes are superpositions of simpler modes involving the excitation of a nucleon out of a filled shell or subshell leaving a hole behind. The superposition is such that the electromagnetic amplitudes of the simple modes reinforce each other. The residual interaction has, then, the task of producing the superposition. The superposition of many simple modes weakens the dependence on orbital idiosyncrasies and produces a smooth variation from one nucleus to the next.

How far can this shell-model description of collective modes be carried? Recent experimental work has been addressed to the analysis of how the collective modes are constructed out of individual orbital motions. A few examples will illustrate the possible attacks. From measurements on the scattering of high-energy electrons, the spatial form of nuclear excitation can be mapped out. This bears directly on the nucleon orbits involved. Only a few light nuclei have yet been studied, but high-energy electron scattering holds large promise for studying nuclear excitations.

The structure of the giant dipole excitation can be studied by observing its de-excitation by re-emission of electromagnetic radiation or, more frequently, by emission of neutrons or protons. The inverse of the latter reaction (proton→gamma ray) has recently been made accessible by the availability of proton sources with the requisite energy and energy definition and of efficient high-resolution detectors. Detailed studies have so far been possible only in restricted sets of nuclei. In the light nuclei, the observed structure is in accord with that based on the shell-model description. However, for heavier nuclei, around silicon-28, the shell model appears inadequate for a detailed description of the giant dipole excitation. Are other modes seriously involved? Are still deeper mechanisms manifesting themselves? It is an interesting mystery about this important mode of nuclear excitation.

It is likely that there are other collective modes that have still not been found, much less studied in detail. These include the shape oscillations, spin waves, and still other modes involving more intricate cooperative movements of the nucleons. The detailed and systematic analysis of the collective excitations is still largely a task for the future.

2.2.9 *Nuclei in the Lead Region*

As examples of recent studies we sketch some in heavy nuclei. The region of nuclei around lead-208 has recently been opened to detailed study by

the development of accelerators having the requisite combination of sufficiently high energy and sufficiently sharp energy resolution. The low-lying states of these nuclei have been interpreted in surprisingly simple fashion in terms of the basic single nucleon and collective modes of the shell model. This simplicity agrees with the shell-model picture of lead-208 as a double closed-shell nucleus—with both proton and neutron shells filled. The concepts and mechanisms that had been developed for other nuclei apply here in especially pure and uncomplicated form. The importance of these discoveries is essentially that these nuclear modes of motion can be examined experimentally and theoretically in a more thorough and quantitative fashion. For example, a collective octupole mode appears throughout this nuclear region. By making use of isobaric analogue states, in a way that will be described later in Section 2.3, the many particle–hole components are clearly delineated. The complete analysis, which requires a complex set of experiments, is now being undertaken. States have been shown to be well described as couplings between particle excitations and the collective quadrupole and octupole modes. Because the coupling turns out to be weak, a detailed but simple structure is revealed, permitting a thorough analysis of the coupling mechanisms.

Whether these ideas will suffice for levels above the low-energy region of simple excitations is not now known, but beginning explorations of higher excitations in the lead region nuclei suggest more simplicity than had been suspected. The simplicities discovered in the lower excitations of the nuclei of the lead region might well make this a good jumping-off base for investigations of the higher ones.

2.2.10   *The Reality of the Inner Shells*

Thus far only the nucleons in the outer shells have been discussed. The nucleons in the inner shells are also becoming open to increasing and precise study. High-energy protons and electrons have been used to knock protons directly out of the core in (p→2p) and (e→e'p) reactions. Precise measurements on the two emerging reaction products determine the energy and momentum the ejected proton originally possessed when bound in the nucleus. Recent (p→2p) measurements have shown that the momentum and energy distributions are roughly those anticipated for shell-model core levels. The internal shell structure of the core remains well defined for nuclei as heavy as samarium, with some 150 nucleons. It will be of substantial interest, as higher-energy projectiles become available, to extend such studies to the heaviest nuclei to determine where and if the shell structure of the core dissolves.

2.2.11 *Developments in Basic Nuclear Theory*

We have described our rapidly growing knowledge of the empirical prop-
erties of nuclei, the interpretation of this knowledge in terms of models,
and the trend toward a unification of the various different models in terms
of the shell model. Thus the problem of relating the properties of nuclei
to the force between free nucleons—a problem central to the theory of nu-
clear structure—becomes that of laying the theoretical foundations of the
shell model.

Two sorts of nuclear property should be distinguished—gross properties
such as size, shape, and binding energy and the detailed level properties
with which the many-particle shell model deals. A theory of nuclear struc-
ture must first achieve rough agreement for the gross properties, because
otherwise the nucleus of the theory bears no resemblance to the nucleus of
nature.

2.2.12 *Adaptation of the Self-Consistent Field Approximation to Nuclear Interactions*

The basic theoretical approach is a modification of a method developed in
atomic physics. In its first approximation this self-consistent field, or
Hartree-Fock method, reduces the complexity of the many-body problem
by averaging the many interactions undergone by any one particle. The
motion of the individual particle is, then, an orbit in this average force
field. The omissions of the averaging approximation are rectified by cor-
rections that, if the basic method is to make sense, must turn out to be
small.

It is not yet known whether the true nucleon–nucleon interaction can
be treated adequately by the Hartree-Fock method. This depends on the
nature of the interaction at very short distances, a subject about which
nothing is known. However, some of the most widely used interaction po-
tentials are strongly repulsive at short distances, and for these the Hartree-
Fock method certainly breaks down. A modification devised by Brueckner
some 15 years ago is designed to take care of just this difficulty by includ-
ing the interactions of a pair of nucleons exactly while they move in the
average force field. This procedure replaces the original intractable inter-
action by a smooth effective interaction to which the Hartree-Fock
method is applicable.

Whether or not the interaction must be replaced by a smooth effective
interaction, the basic physical concept of individual-nucleon orbits is in-
troduced by the self-consistent field approximation. The Brueckner effec-
tive interaction will be necessary if in real nuclei there are significant cor-

rections to the individual-nucleon motions arising from close encounters between nucleons. The existence of such short-range correlations is an experimental question, yet to be answered, whose resolution will require the short-wavelength, high-momentum probes now becoming available.

2.2.13 *Nuclear Matter, a Simplified Testing Ground, Finite Nuclei*

This theoretical approach was first applied to an extrapolation from real nuclei known as nuclear matter. By using the semiempirical mass formulas, together with parameterizations of nuclear radii mostly from high-energy electron-scattering work, the complexities of surface, shape, local-shell-structure effects, and the Coulomb-energy contributions are all stripped away to reveal the bulk properties of an infinite nucleus characterized by a binding energy per nucleon and a density. It was intended as a simplification on which to sharpen the theoretical techniques before tackling real nuclei. The program of calculation is generally called the Brueckner-Bethe program, after its two chief architects. Using parameterizations of the internucleon forces that accurately describe the scattering and binding of one nucleon by another, the binding energy per nucleon is calculated. The result depends on the particular parameterization used and may vary by about 5×10^6 eV. With quite reasonable parameterizations, and with the inclusion of higher-order corrections, the result is within approximately 2×10^6 eV of the empirical 16×10^6 eV. In recent years, such calculations, known generally as Brueckner-Hartree-Fock calculations, have been applied to the simpler, closed-shell nuclei, with results very comparable with those for nuclear matter. At least in one calculation not only the binding energy of the finite nuclei but also the charge distribution of the protons has been well reproduced. When it is recalled that the binding occurs as a balance between large repulsive and larger attractive contributions, qualitative agreement must be viewed as a considerable achievement. It provides a motivation to examine the fundamentals of the method and the approximations inherent in its application.

2.2.14 *Nucleon–Nucleon Interaction, Many-Body Forces, and Nuclei*

There is, however, a very important point that has become clearer as the methods have become systematized. Different choices of the nucleon–nucleon interaction remain possible if only the two-nucleon data are considered, and these different choices lead to different effects in many nucleon systems. This is at once a complication for nuclear binding calculations and an opportunity to learn more about the interaction. Part of the latitude of choice corresponds to gaps in the pertinent data on the two-

nucleon system at lower energies. Such deficiencies can, of course, be remedied. More interesting and important is the basic ambiguity in the determination of the nucleon–nucleon forces from the two-nucleon data alone. Some additional information is required.

The meson theory of nuclear forces has provided a partial guide. Long-range parts of the nucleon–nucleon interaction are taken as theoretically determined by the exchange of the least massive mesons. However, since the short-range behavior is most likely to involve many mesons in a complicated fashion, it is determined by empirical fitting. This is done in a variety of different ways, leading to the variety of interaction forms that constitutes the ambiguity mentioned above. As the necessary experiments are undertaken and the uncertainties in the calculations reduced, a more unique choice of interaction may yet be possible.

Since two nucleons are not enough and our understanding of an infinite number still in too delicate a condition, it is natural to look for enlightenment to the three-nucleon system or that of two nucleons plus the electromagnetic field. In fact, efforts are evolving in both these directions. The first is hampered by the well-known technical difficulty of the three-body problem, but there has been a renewal of activity based on new theoretical methods. The second requires the isolation of situations sensitive to the unknown parts of the interaction and not overwhelmed by the imperfectly known. An experiment of the requisite sensitivity has not yet been performed.

The nuclear calculations have been entirely based on pure two-body forces. It is possible that there are additional relatively weak forces between three or more nucleons. While it is believed that such many-body interactions are quite small, perhaps of the order of 5 percent, there is not specific evidence at this time to permit a precise determination of the strength of multinucleon interactions. As these ambiguities are resolved, the properties of the three-nucleon system will allow us to bound the contributions of three-body interactions. Similarly as it becomes possible to remove the effects of multinucleon correlations induced by the main two-nucleon interactions, true multinucleon effects will be revealed.

2.2.15   *Calculation of Nuclear Shapes*

We are left then in the midst of a hopeful complexity concerning calculations of binding energies and sizes or densities of nuclei. The question of the ground-state shapes of nuclei has so far been approached only with simplified interactions for which the Hartree-Fock method is directly applicable but which fit the two-nucleon data only qualitatively. It is, however, an important result that the calculated deformations reproduce the

observed trends. The gross nuclear properties of binding energy, size, and shape seem, then, to be within at least the qualitative reach of nuclear theory.

2.2.16 Foundations of the Shell Model

The two main ingredients of the shell model—the average field and the residual interactions—can both be derived from nuclear many-body theory. The Brueckner-Hartree-Fock calculations that have recently been carried out permit comparisons between the calculated and observed central-field properties. The first results show a qualitative, tantalizing likeness, although the spin-orbit term in the single-particle shell-model potential still awaits quantitative interpretation. But even more interesting than the individual orbits is the residual interaction between the outer nucleons. The connection between the real internucleon forces and the residual interactions has been examined in detail, so far, only for the simplest class of nuclei possible—those with two nucleons away from doubly closed shells. The close agreement obtained for the spectra of these nuclei is very encouraging, but the role of couplings involving excitations out of the closed shell core is still not understood. Still remaining to be investigated are the sensitivity to different forms of the nucleon–nucleon interaction, the importance of various corrections, and extensions to more complex nuclei. However, a connection between the empirical shell model and the basic nucleon–nucleon force has been forged.

We have sketched the working program of experimental and theoretical nuclear structure. There are gaps and difficulties in both branches. The new experimental techniques open up new areas of investigation about which our present models and concepts will make predictions. Whether we shall find fundamental contradictions with our present ideas we do not know.

2.3 NUCLEAR REACTIONS

The study of nuclear reactions cannot be separated from the study of nuclear structure. Most of the information on nuclear structure indeed comes from nuclear-reaction studies. The basic physical picture of nuclei must finally apply to both aspects or be valid for neither. The division is a matter of viewpoint and emphasis. In the study of nuclear reactions the interest focuses on the dynamics, on the processes that occur when nuclear particles are brought together.

2.3.1   *Early History of Reactions*

Before 1947, the reaction phenomenon that held the center of attention was the compound nuclear resonance. It appeared in its most striking form in the response of a nucleus to bombardment by low-energy neutrons. Sources of low-energy neutrons, of the order of electron volts, had been developed that made it possible to do experiments with neutrons of well-defined variable energies. Neutrons, being uncharged and, therefore, un-affected by the Coulomb field could easily penetrate into the nucleus even at low energies. The cross sections revealed truly spectacular, very narrow resonances standing well clear of backgrounds. Such narrow resonance widths correspond to the formation of configurations that live a relatively long time before decomposing with the emission of a neutron (described as elastic scattering) or in some other way (described as absorption). These configurations, called compound nuclear states, seemed to involve the dissolution of the incoming neutron motion into a complex internal motion with a corresponding long lifetime.

The postwar developments in experimental technique made possible neutron beams of a much wider energy range, still with variable energy selectivity. A new phenomenon was uncovered that was complementary in character to the nuclear resonance. By examining neutron cross sections over a wider energy region, from 0 to 2.5 MeV, with a correspondingly broader energy resolution that averaged over narrow resonances, a broad energy structure could be discerned. This broad structure varied gradually and regularly with the atomic number of the bombarded nucleus. These regularities together with the angular dependence of the elastic-scattering cross sections could be well described in terms of the scattering of the neutrons by a smooth, central force field of nuclear dimensions. This description is usually called the optical model. While such a model had been proposed earlier for high-energy scattering, the neutron data indicated its applicability for low energies. There was, then, a second mechanism operating in nuclear reactions, in addition to the formation of long-lived configurations. The breadth of the energy structure and the successful description in terms of scattering in a force field both imply that the reaction takes place promptly. The result of averaging over the narrow resonance structures is to stress the prompt process.

The accelerator and detector developments made possible precision studies of the scattering of protons, deuterons, and alpha particles by more complex nuclei. These, too, were found to be well described by optical models. It appears, then, that the prompt mechanism is very prevalent in nuclear physics. The representation of the interaction between a particle and a target nucleus in terms of a smooth force field leads to the

same physical picture as does the shell-model description of nuclear structure but extrapolated to the positive energies at which reactions take place.

2.3.2   Direct Reactions

In the 1950's, reaction studies flourished, and it gradually appeared that many other kinds of reaction also showed regularities that were simply described in terms of prompt mechanisms. At the same time, a vast number of possibilities emerged that could be exploited to obtain nuclear-structure data. Inelastic scattering, in which the bombarding projectile excites the target nucleus into definite excited states, revealed cross sections that varied smoothly with energy but showed strong dependences on the angle of scattering. This is opposite to what would be expected if the reaction proceeds through complex, long-lived configurations. The reaction characteristics are, however, well described by viewing the nuclear excitations as taking place in a direct interaction with the projectile, the path of which is determined by the central force field of the optical model. Such "direct reactions," as they are usually called, are important sources of information on nuclear excitations.

2.3.3   Transfer Reactions

Reactions in which nucleons are transferred between projectile and target exhibit the characteristic energy and angular dependence of direct processes. One of the earliest studied is that in which a bombarding deuteron is stripped of its neutron, leaving the proton to register the event. Such (deuteron→proton), and their inverse (proton→deuteron), reactions appear to take place by direct transfer of the neutron between the deuteron and a bound nuclear orbit, both the deuteron and proton moving in the paths determined by the optical force field. Many other neutron and proton transfer reactions have been studied since then. Those such as (triton↔deuteron), (helium 3↔deuteron), and (alpha↔helium-3) are now considered standard direct reactions. The angular momentum of the bound nucleon orbit determines the angular distribution of the reaction. The observation of this characteristic angular dependence was important in establishing the reaction mechanism. Turned about, it provides an important method for determining the angular momentum quantum numbers of the orbits. The angular distribution contains only part of the information on the transferred angular momentum. The determination of the orientation of the intrinsic spins of the transferred particle usually requires the use of polarized projectiles, which are now being developed mostly in this country, for use in reaction studies. They will make possible finer studies of the reaction mechanism.

While, at first, direct reactions could only be used to fix the energy and angular momentum quantum numbers of the states of the nucleus, they have been developed to go far beyond determinations of quantized parameters and to provide quantitative information on the structure of the nuclear states involved. Because the reaction mechanism is direct single-nucleon transfer, the cross section for excitation of a nuclear state is a measure of its single-nucleon component. The experimental and calculational programs necessary to permit the extraction of this important quantity had, by the middle 1960's, succeeded in making direct nucleon-transfer reactions one of the most important sources of nuclear-structure information.

Most recently, reactions involving the simultaneous transfer of two closely paired nucleons have been investigated and show the empirical features characteristic of direct processes. Especially interesting results have been provided by proton⟷triton reactions. Since the spins of the two neutrons in the triton are oppositely directed, the direct reaction will excite nuclear states formed by adding or subtracting two neutrons paired off to zero resultant angular momentum. While the development of two-nucleon transfer into a quantitative measure of the strength of this paired component in nuclear states is not yet complete, the qualitative results are already very interesting. The large cross sections for the excitation by these reactions of ground and particular excited states in the heavier nuclei reveal a collective excitation built up by a constructive superposition of many states in which two nucleons are paired to zero spin. That nuclear states had such pairing characteristics was theoretically deduced from several lines of indirect evidence and developed in analogy to the description of superconducting states of metals. Their direct observation is an important verification of the complex ideas that led to these descriptions.

Transfer reactions are not limited to one or two nucleons. Processes involving three, four, or more are being studied. Very recently, alpha-particle transfers were effected by reactions that demonstrated the characteristics of direct processes. Each of these reactions carries its own specific information: thus (deuteron⟷proton) tells us about single-neutron orbits, while (triton⟷proton) tells about paired neutron components. This specificity is expected in the multinucleon reactions now being studied. Together these direct reactions provide an array of methods that permit us to analyze how different components enter into the structure of a nuclear state.

2.3.4   *Intermediate Structure—Analogue State Resonances*

The 1960's addressed themselves also to the identification and study of energy structures in cross sections intermediate between the very broad

ones and the narrow compound nuclear resonances. Two recent discoveries, isobaric-spin analogue-state excitations and neutron-fission resonance structure, are very relevant examples. The isobaric analogue resonances are especially important and will be described in some detail. The intermediate-energy structure is interpreted as corresponding to nuclear configurations of complexity intermediate between the simple individual nucleon orbits of the optical model and the complicated intricacies of the narrow resonances. These intermediate phenomena are important both because of the nuclear dynamics involved and because of the nuclear-structure information they provide.

The phenomenon of analogue state excitations is itself remarkable, and its discovery is one of the important surprises in the nuclear physics of the past decade. They were discovered by a group at Livermore that was embarked on a programmatic investigation of (proton→neutron) reactions on a large range of nuclei. The outgoing neutrons signaled a strong predilection of the residual nucleus for a particular narrow region of excitation. This behavior occurred for many nuclei, in all regions of the periodic table, and was clearly a general nuclear phenomenon. The Livermore workers correctly interpreted the preferentially excited nuclear configuration as the isobaric analogue of the target nucleus. By the isobaric analogue of a state is meant one whose structure is the same except that a neutron has been changed into a proton, and it is easy to see that a (proton→neutron) reaction would tend to excite such a configuration. The symmetry between neutrons and protons, which is labeled by the symmetry quantum number known as isobaric spin, is broken by the Coulomb force through the admixture of different isobaric symmetries. It had been thought that the whole concept of isobaric spin would be useful only in light nuclei where Coulomb forces are weak. The analogue state discovery at once showed the very wide applicability of this symmetry in nuclear structure.

Analogue state resonances were subsequently discovered in proton elastic scattering and (proton→neutron) reactions by a group at Florida State University using the very good energy resolution of a Van de Graaff accelerator. In these experiments the actual narrow width of the energy structure was displayed. The analogue excitation is embedded in a background of complex excitations of different, lower isobaric spin, and its narrowness corresponds to the extreme weakness of the isobaric-spin breaking mechanisms that mix the two together.

The explanation of the weakness has two parts. The Coulomb potential, while large, is also spatially very smooth. Since mixing requires strong gradients, the Coulomb force is a much less effective mixer than first appearances suggest. Also, higher nuclear charges go with larger differences be-

tween neutron and proton numbers, which has been shown to stiffen the nucleus against the mildly effective Coulomb force. The two effects together explain the extension to heavier nuclei of the usefulness of the concept of isobaric spin symmetry.

2.3.5 *Significance of the Analogue Phenomenon*

The importance of analogue state resonances for nuclear structure is enormous. Each low-lying state in heavy nuclei has its analogue in the neighboring nucleus, which can be excited in a variety of reactions. The analogue configurations are as well known as the low-lying parents but are raised in energy by one proton's worth of Coulomb energy (around 15 MeV in the lead nuclei). They can then decay, and how they do so tells us how the final states are related to the analogue states. The specific nuclear information is similar to that afforded by the direct nucleon transfer reactions but can be exploited for many more pairs of states throughout the periodic table.

2.3.6 *Reaction Theory*

The broad- and intermediate-structure reaction phenomena have picked out simpler modes of nuclear motion that appear to be closely connected with those discovered in nuclear-structure work and described by the shell model. How to incorporate these shell-model descriptions into a detailed theoretical formulation of nuclear dynamics is an active, current concern of theoretical nuclear physics. Formalisms are being developed that show conceptually how such a marriage between simple (optical), intermediate, and very complex modes (compound nuclear) might be arranged in the nuclear dynamics of a reaction.

However, even the simpler features of nuclear reactions have not been understood on the level of first principles. The quantitative connection of the optical model, as developed and used in low-energy nuclear physics, to the fundamental interactions between nucleons is incomplete but under active study. Current work seeks to adapt the shell-model concepts to the study of nuclear reactions.

2.3.7 *Recent Work on Compound States*

The study of the compound nuclear states themselves has been largely confined to the interpretation of the experimental data. The great density of these compound levels and their intrinsic complexity encourage a statistical approach to both description and analysis. The basic descriptions

are stated in terms of the distribution of resonance widths and heights, spacings between resonances, and correlations. The statistical phenomena that are uncovered in this way turn out to be those resulting from a number of independent random variables. It might be remarked that the study of such statistical distribution has developed into a significant subfield of its own between physics and mathematics.

Similar statistical considerations have proven useful at the higher energies, where resonances overlap one another. The sharp variations of the separated resonances give place to a cross section that varies with energy in a smoother but still fluctuating way. These fluctuations, themselves of interest and under analysis, permit the extraction of the width of the underlying resonances, although these are not directly visible.

At still higher energies, the compound nuclear process is eclipsed by the direct reaction. In the in-between region, the modes of decay appear to follow neither the high- nor low-energy experience. The very mode of formation of the compound states in the intermediate region appears important to the final decay pattern. Doorway modes of intermediate complexity certainly seem to be involved.

Connection of these statistically treated phenomena and concepts with a basic nuclear Hamiltonian has not yet been seriously attempted. Developments that relate optical model parameters to detailed nuclear structure have been made with qualitative success. But it is still much too early to feel that a fundamental theoretical understanding in any depth has been established.

2.3.8   *High Energies and Heavy Ions*

New accelerators and detection devices are making it possible to use high-energy projectiles in nuclear investigations and thus to place the nucleus under a microscope sensitive to the short distance structure of the nucleus. Precise observations on the scattering of protons with energies up to 1 GeV from a variety of nuclei have been made. At high energies, the complications that stand in the way of connecting nucleon–nucleus scattering directly to the nucleon–nucleon interactions diminish, and a derivation of the optical model or a direct description of the scattering process itself becomes possible.

As nuclear physics goes on to make more searching use of other projectiles, both experimental and theoretical methods will need to be correspondingly developed. Pi-mesons raise interesting possibilities that can probably be explored with refinements of existing concepts, although there is the possibility that phenomena will be uncovered that cannot be described in terms of nucleon coordinates alone.

2.4   EMERGENCE OF NEW FRONTIERS

Current ideas about the nucleus are based on limited evidence. New technical means are at hand to learn about nuclei very different from those that we have so far been able to study. New types of nuclear reaction induced by bombardment with different projectiles will yield new types of information about nuclei and will reach parts of the nucleus about which we know very little. Some steps have already been taken in these directions, and it is these and the frontiers toward which they tend that we discuss next.

2.4.1   *Beyond and Away from the Valley of Stability, Expansion of the Periodic Table*

We have so far been able to study only a small fraction of possible nuclei. Some 300 stable nuclei occur in nature. Nuclear physicists have produced about 1300 radioactive species that are unstable against weak beta decay and that live from fractions of seconds to thousands of years. Some 6000 more could be produced that would enormously widen the nuclear horizons. Figure II.1, originally prepared by G. N. Flerov of the Soviet Union,
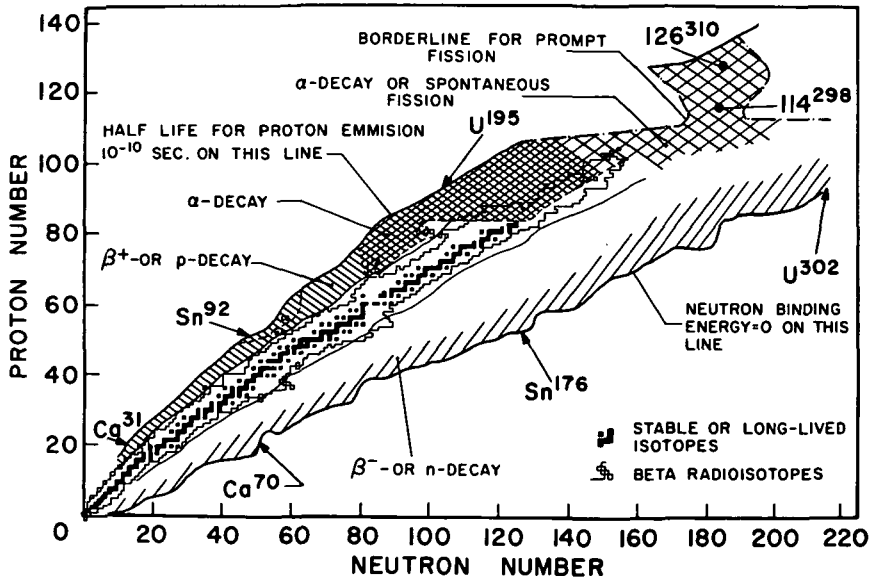
FIGURE II.1   Stability diagram for atomic nuclei.

tells the story. The stable nuclei and the beta-decaying radioisotopes are plotted according to their proton number, Z, and neutron number, N. Also indicated are the limits of stability imposed by a variety of possible particle emissions. If the energy of binding were plotted as the third dimension, it would be seen that the stable nuclei lie clustered around a line of deepest binding in a valley that rises on either side of this line. At the high end the valley is cut off by fission and alpha decay, along the sides by the instability against neutron and proton emission. The narrowness of the region explored to date means a corresponding narrowness of the N, Z variations from which all our ideas on nuclei are drawn. For example, of the tin isotopes only those with mass number from 110 to 132 are now known; however, it can be calculated on present ideas that the tin isotopes from mass 92 to 176 should be stable against nucleon emission. This example illustrates how the possible range of neutron–proton asymmetry, measured by $(N-Z)$, would be tremendously enlarged beyond that now available. What is hidden there we do not yet know.

Many ways are being used to climb up the sides of this valley, a few steps at a time. One approach is based on spallation reactions in which very energetic protons plow into a nucleus, ejecting nucleons and leaving small numbers of slope-dwelling nuclei. Also, reactions in which small clusters of nucleons are transferred are being investigated.

The bombardment of very massive nuclei with massive projectiles, perhaps uranium on uranium, has been proposed. Such a reaction would result in the production of a smear of perhaps 5000 nuclear species. Fast separation techniques could make many of these available for individual study.

2.4.1.1 *Heavy Elements* A massive effort has gone into climbing out of the stable valley at its upper end in the direction of new heavy elements. This has taken the form of a dramatic competition, primarily between the U.S. effort at Berkeley and that of the Soviet Union at Dubna. Conflicting results and conflicting claims have surrounded the discoveries and studies of elements 102 and, more recently, 103, 104, and 105. The reason for conflicts lies in the great difficulty of the experiments. The basic process of adding nucleons to available isotopes is carried out by bombarding plutonium or heavier nuclei with projectiles such as carbon, oxygen, or neon. Only very few atoms of the new element are produced, and the identification is difficult. Chemical identification requires working out the chemistry of the new element and developing techniques appropriate for short half-lives that will work with one atom at a time. If the new element has new specific radioactivities, a definite identification is possible. If instead it is a fissioning element, the broad smear of fission products makes iden-

tification a subtle matter. All these developments go on together to form
a turbulent and exciting special field, characteristic of the early days of
a frontier.

2.4.1.2 *Superheavy Elements—Can They Be Produced?* Beyond the
transuranics and the new elements, very fast fission ends the chain of nu-
clear species. It is thought, however, that there are islands of stable or
quasi-stable superheavy nuclei beyond this region of instability. Current
calculations suggest that the closest island surrounds proton numbers 114
and possibly 126 and neutron number 184. Our main ideas on the super-
heavies are based on theoretical extrapolations of binding energies out of
the stable valley. These extrapolations are formulated in terms of model
prescriptions tested within the valley. In one of the most detailed proce-
dures, binding energies are given by combining a "liquid-drop" dependence
on neutron and proton number, with finer corrections based on the shell
model. The detailed structural contributions are small but vital in predict-
ing the island of stability. A major effort involving Swedish, Polish, Rus-
sian, and U.S. groups recently has been directed toward carrying out such
calculations. Their results form the basis of predictions of the properties
of the superheavy nuclei.

   Reaching this island requires a vault over the intervening unstable re-
gion. This is very different from the successive additions of a single nu-
cleon that we now believe to be the stellar process for creation of heavy
elements. Instead it appears necessary to put together large nuclear masses
with sufficient energy to overcome their Coulomb repulsion. Facilities
capable of producing intense beams of heavy projectiles, or heavy ions,
are the *sine qua non* of such an effort. The converted Berkeley HILAC
will be capable of entering this region, and the Soviet Union and West
Germany are each mounting drives toward this end. Indeed, this endeavor
is a major part of the overall Russian effort in nuclear physics.

   It is not possible to tell now whether it will be possible to put together
even a few of the superheavies by fusing together the component parts
while discarding only a few nucleons, as in the suggested possibility (eu-
ropium-155) + (europium-157) → (element 126, A = 310) + 2 neutrons. Al-
ternatively, the uranium on uranium reaction, which, as noted, is expected
to lead to a broad smear of everything and so also the superheavies, may
be the most promising method.

   The need to study the superheavies is clear. How to do so is not. The
position and existence of the island of stability is uncertain. The reaction
mechanisms too are unknown; they are interesting subjects for study
rather than familiar tools. These are, of course, excellent reasons for fur-
ther study, and it is important that we do so in as flexible and wide-rang-

ing a fashion as possible. The properties and usefulness of the superheavies in some future technology can only be matters of speculation now. However, a measure of the possibilities that may be offered is revealed by the frequently suggested large neutron multiplicity in the fission processes of the superheavies.

A number of laboratories have initiated searches for possible superheavies that might be found to occur in very small numbers. Flerov at Dubna has looked for fission tracks in old lead-glass that resided in ancient buildings, found more tracks than could be accounted for by the uranium content, and interpreted this excess as evidence for the presence of superheavy chemical homologues of lead. Balloon flights of emulsions by a Bristol group, and then by a combined Bristol and U.S. group, indicate the presence in cosmic rays of very small amounts of very heavy elements and perhaps have even identified the track of one superheavy nucleus. Such portents make all the more interesting the coming efforts to enter the superheavy region by laboratory reactions.

The efforts to extend the region of known nuclei along the slopes and beyond the valley of stability will be voyages of exploration into a territory largely unknown. What scientific and technical riches lie there remain to be revealed.

2.4.1.3 *Fission Isomers* In 1962, experimenters at Dubna discovered a new kind of fissioning nuclear state in americium-242—one that lived a long time compared to usual gamma-ray lifetimes but that decayed by fission very much faster than was characteristic for low-lying states. The state was somehow very different from other low-lying states. By 1967, a sizable number of such states, known as fission isomers, had been discovered in the uranium and transuranic nuclei in Denmark, the Soviet Union, and this country. While knowledge of the systematics of these isomers is still incomplete, it is clear that fission isomers are quite a general phenomenon in fissionable nuclei.

It is now believed that the explanation first put forward by Strutinsky is correct, at least in its general features. He argued that as the nucleus deforms from its original shape on its way toward separation into fission fragments it is trapped in an intermediate shape that is especially stable— usually referred to as the "second minimum." The fission isomeric state is the lowest-lying excitation corresponding to this shape. If the explanation is correct there should be a whole set of such excitations all corresponding to the intermediate shape. This second nuclear world has not yet been directly demonstrated but is a subject of great interest. The excitations of the intermediate shape have, however, been seen as intermediate structures in reactions at the neutron facilities at Geel, Belgium, and Saclay, France.

It is worth noting that the very striking cross-section structures were first revealed in measurements that were part of a programmatic survey of (neutron–fission) reactions.

The existence of the second-minimum shape was, in fact, predicted on the basis of energy calculations very similar to those described for the superheavy nuclei. A very important ingredient in both calculations is the delicate, but crucial, effect of shell structure. The second minimum appears then to provide interesting evidence for the validity of shell-model concepts even at the large deformations of a fissioning nucleus. Because of the similarity of the underlying theories it offers a measure of support for speculations about the existence of superheavy elements.

2.4.2 *Interactions between Massive Amounts of Nuclear Matter*

Interactions between heavy nuclei are largely in the *terra incognita* of nuclear physics—an unknown awaiting a facility capable of accelerating beams of heavy nuclei to energies high enough to penetrate the Coulomb barrier of the heaviest target elements. Simply knowing that such an unknown territory exists is a strong justification for the nuclear community's intense interest in a heavy-ion accelerator designed for nuclear experiments. But the justification has a broader base: The unique properties of accelerated heavy ions, i.e., high electric charge, large amounts of linear and angular momentum, can be used to extend greatly present investigations of a wide range of nuclear problems. It is this twofold set of possibilities that makes this field so attractive.

The electric excitations produced by the Coulomb charges of light and medium ions—protons, helium, on through sulfur and argon—have yielded many electromagnetic properties of nuclei. The collective states that have large electromagnetic strengths stand out clearly in Coulomb excitations, and, indeed, many of the properties of the rotational levels were first seen in such experiments. As has already been noted, the shapes of short-lived excitations, established by the Coulomb excitation mechanism, have turned around our ideas on the nature of the basic shape oscillations of nuclei. These are only beginnings of a series of researches that the immensely stronger electric field of the heavier ions would make possible. For example, multiple processes, in which excited states act as the beginning target, stand out, thus providing the means to observe higher nuclear states built up of two or a few low-lying collective modes. The analyses of such states in which several excitations are simultaneously present will provide the opportunity for basic new nuclear physics. Little is now known of the interaction of the nuclear modes with one another—fundamental though this knowledge is to the whole concept. Other examples

abound for which the strong Coulomb excitations provide the investigating tool.

There are other properties of heavy ions that can be put to excellent use. Their large mass means that for a given energy of excitation, large amounts of linear and angular momentum are delivered to the final product nucleus. This large momentum exchange is important because the excited nuclear system can then be made to penetrate materials and probe their electric and magnetic fields or, conversely, to use known fields to measure the properties of very short-lived nuclear excitations such as magnetic dipole moments. The time the excited system remains in motion before being brought to a halt can be used as a clock against which to measure the lifetimes of the excitations—the motion of the excited system being signaled by the classical Doppler shift of the frequency of emitted radiation.

The high angular momentum carried in by the accelerated ions can produce very interesting nuclear states of correspondingly high angular momenta. Instead of being able to merely peer at angular momenta of 20 units, states with spins 30, 40, or even up to 100 would be open to study. The importance of observing and measuring both the energies and the electromagnetic characteristics of the high angular momentum members of rotational bands has been touched upon. Even more important is the capability of exploring types of nuclear motions which as yet have been only tentatively examined. The new range of angular momenta associated with heavy ions takes us into a new territory; some questions are, as we have seen, ready and waiting, but some surprises may be in store.

It has been realized for a long time that a great deal could be learned about the surface structure and dynamics of nuclei if those interactions that arise when particles come together in a grazing collision could be separated out. The strong Coulomb fields of heavy nuclei can be exploited to keep the nuclei at distances far enough apart so that the surface regions barely touch one another. By gently varying the energy, the distance of interpenetration can be correspondingly varied. However, even at energies that lead to more penetration it is likely that the reactions in which clusters of nucleons are transferred from surface to surface will stand out clearly. Very exciting problems present themselves. For example, a basic concept in nuclear structure is the tendency of nucleons in the unfilled, outer shells to pair off. The existence of such pairs in the "superconducting" nuclear ground states and excitations will manifest itself by the strength with which the tunneling of pairs between heavy ions occurs. Very recently, in experiments with the limited range of ions now available, another kind of phenomenon was revealed. When observing reactions in which an alpha particle was transferred, certain nuclear states stood out

from the many others—revealing an unsuspected structure that must still be systematically studied and tested against the concepts that have succeeded elsewhere.

An important special purpose of heavy-ion work is to reach into the regime of superheavy nuclei. The possibility of producing such nuclei in the aftermath of the fissioning of the system formed by smashing the heaviest nuclei together—uranium on uranium, for example—has been touched upon already. Whether more delicate reactions that approach a superheavy with the discard of only a few nucleons will work cannot be judged now because the properties of such reactions are as yet unknown.

Beyond these exploratory uses of heavy ions, beyond the definite questions already asked and partly revealed, lies the stark fact that very little is known of how heavy nuclei interact with one another. The massive amounts of nuclear substance hurled together form nuclear systems very different from those as yet explored. Experiments between lighter nuclei, within the means now at hand, show striking and surprising cross sections that point to interestingly simple configurations. We do not know whether such modes will continue to evince themselves in reactions between the heavy nuclei. The manner in which heavy nuclear masses deform and distort each other, how they exchange and dissipate energy and nuclear substance, are now matters of conjecture but will be subjects of precise study.

2.4.3 *Nuclear Physics and High-Energy Probes*

Studies of the scattering of high-energy particles by nuclei provide a direct means of observing the spatial structure of the nucleus. To measure on such a fine scale, the wavelengths of the incident particles must be fractions of the size of the nucleus and the energy resolution good enough to resolve characteristic excitations of the nucleus. High-energy electron scattering was developed in the 1950's and has been steadily refined and extended. In the 1960's, high-energy proton scattering was begun when suitable detection systems were devised. New facilities will provide intense beams, and energy resolution has been sharpened by almost two orders of magnitude, thereby offering the promise of a precise nuclear probe. The two methods, electron and proton scattering, provide partly overlapping but mainly complementary kinds of nuclear information.

2.4.3.1 *Charge Distributions from Electron Scattering* Our best knowledge of the size and form of nuclear charge distributions comes from elastic electron scattering. From the work of the Stanford group, and other groups in this country and in Europe, has emerged our modern picture of nuclei throughout the periodic table: a near-constant charge density inside

the nucleus with a fairly sharp drop-off at the nuclear surface. This has been checked and strengthened by work with muonic atoms. However, we do not yet know all that we can and need to know. Studies of the isotopes of calcium have shown that the change in the charge distribution has an interesting dependence on the neutron number; the systematics of this neutron–proton effect are as yet little known but clearly constitute a central problem of nuclear physics. Recent observations have been extended to the very small cross sections that correspond to scattering with very large momentum transfer. Higher momentum transfers correspond to shorter distances, and the new observations indicate that the charge distribution in the nuclear interior has a somewhat rippled structure. Such rippling is qualitatively consonant with shell-model descriptions and Hartree-Fock calculations, but the amplitude of the ripples is still subject to controversy. Information on the distribution of nuclear magnetization is also fundamental, but few of these more difficult observations have yet been made.

2.4.3.2 *High-Energy Nucleons and Short-Range Correlations* High-energy protons interact much more strongly with the nuclear constituents than do electrons. The incident proton has, then, a greater probability of interacting with more than one nucleon before leaving the nucleus. Such processes, involving two or more nucleons, are affected by the tendencies of the nucleons to cluster together or repel each other. The relative importance of nucleon correlations is, then, an important feature of high-energy proton scattering. The strong interaction between the incident proton and both neutrons and protons is also complementary to the much different view the electron takes of neutrons than it does of protons. Preliminary experiments some years ago with the bombardment of the helium nucleus and other light nuclei by 1-GeV protons were very important in indicating the feasibility of such precise observations. The main features of the observed intricate dependence of the cross section on the angle of scattering are well described by formalisms that connect directly with nucleon–nucleon scattering. This makes it possible to go on to further refinements necessary to pick out the delicate interesting effects—of which correlations between the constituents of the nucleus are prominent.

Experiments with either high-energy electrons or protons that involve the direct knockout of nucleons are important in identifying the energy and momentum distributions of the nucleons in their nuclear milieu—a point that has already been noted in earlier sections. The knockout of heavier clusters, such as deuterons and helium-3 nuclei, has been observed. The analysis of the knockout experiments is complicated by the possibility of secondary pickup interactions within the nucleus. Whether it will be

possible to remove the effects of such secondary interactions is not now known, but the study of momentum transfer and its relation to the energy dependence of the knockout process is an important subject.

2.4.4   The Interactions of Mesons with Nuclei

Meson reactions with nuclei can significantly augment the information accessible to more conventional probes. The intensities of the mesons that have been available have limited what can be done, especially with the rarer and more interesting modes of interaction. New facilities that act as "meson factories" should drastically change this situation. In this section we describe how the characteristic properties of mesons can be used to examine specific aspects of the nucleus that could not otherwise be sensitively probed.

2.4.4.1   *Muons as Electromagnetic Probes*   The μ-meson, or muon, has already proved its value in the study of nuclei, and, on the other hand, nuclei have been important in revealing the nature of μ-mesic interactions. The muon interacts with the nuclear constituents only through its electric charge and through a weak interaction. The two different interactions give rise to two distinct branches of muonic nuclear physics.

The negative muon, μ^-, can be trapped by the nuclear charge into atomlike orbits. The heaviness of the muon, 206 times that of the electron, makes the orbit dimensions small, and, in fact, the inner orbits are sensitive to the spatial distribution of the nuclear charge. Both the radial form of the charge distribution and the electromagnetic moments that describe the shapes of the distributions are probed. The two or three innermost orbits, whose sensitivity is greatest, have been studied most extensively--by groups in the United States and in Europe. The necessary precision has been provided by modern solid-state detectors, which are vital to the feasibility of such experiments. The muonic atoms yield information that complements that learned from high-energy electron scattering. In addition to its role as a probe of the nuclear ground-state charge distribution, the muon can produce excitations of the nucleus. Both the transition strength and the charge distribution of these excited states can then be measured— as they can in no other experiment. Such dynamic effects have only recently been identified.

2.4.4.2   *Muons in Weak Nuclear Reactions*   The weak interaction permits a proton to absorb the negative muon to form a neutron and a neutrino. The nature and form of this interaction have been determined in nuclear experiments. The analyses have profited from the known nuclear system-

atics by making it possible to select special transitions between nuclear states of known characteristics that clearly display the effects to be studied. For example, negative muon capture by oxygen-16 in its O^+ ground state (angular momentum zero, positive parity) to a known O^- state (zero angular momentum, negative parity) in nitrogen-16 selects that part of the weak interactions that changes parity without any accompanying change in angular momentum—the important pseudoscalar term. Conversely, the capture process can be used to study the structure of nuclei by measuring the strength with which specific nuclear states are excited in the capture event. Especially important are a variety of nuclear collective modes observed in conventional methods of excitation. The fact that the end products of negative muon capture, neutrons and gamma rays, are hard to detect has limited experimental studies to the measurement of total cross sections rather than the examination of specific excitations. There have been a few recent measurements in the United States and in the Soviet Union of emitted nucleons and gamma rays that follow the capture events. High-resolution muon-capture spectroscopy is a field for the future, but one that should be made more accessible by the copious supplies of muons from the new facilities.

2.4.4.3 *Pions as Nuclear Probes* The strong interaction of the π-meson, or pion, with nucleons permits its use in parallel with conventional nuclear projectiles. The importance of the parallel work arises from the opportunity to extract what should be the same nuclear information from radically different reaction processes. This permits us to check on many of our ideas about nuclear structure and dynamics.

Of more interest are the experiments designed to exploit the special pion properties. The charged and neutral pions (π^+, π^-, and π^0) form a symmetric triplet, described by the isobaric spin quantum number of 1. The nucleons form a system with fewer isobaric degrees of freedom—a doublet consisting of neutron and proton. The symmetry between the interactions of the π^+, π^- with neutrons and protons was used more than a decade ago to examine the relative neutron and proton populations in the surface of the lead nucleus. Direct comparison of neutron and proton spatial distributions provides interesting structure information both for neutron-rich nuclei like lead and for those with more comparable neutron and proton numbers like the calcium isotopes. The greater isobaric spin of the triplet allows the production of nuclear species off the stable valley and of nuclear excitations with a wider range of internal nuclear symmetries.

Perhaps the most interesting of the nuclear interactions of pions are those in which two nucleons participate, for such reactions reach an important but so far elusive set of nuclear properties—the short-range corre-

lations between pairs of nucleons in the nucleus. These correlations have been touched on in the discussion of the connection of the shell model with the internucleon forces. Clearcut experimental demonstrations have been obscured by secondary interactions of the reacting particles within the nucleus. Whether the pion experiments will provide a less ambiguous signal is not yet known, but it is also important to look at the characteristic experiments over a wide range of nuclei and nuclear conditions. Among these experiments are the $(\pi^+ \to \pi^-)$ reactions in which a positive pion enters and a negative pion leaves, two nucleons being needed to take up the two units of charge. Still another investigation is based on the absorption of low-energy negative pions by nuclei, which is known to take place predominantly on nucleon pairs inside the nucleus, the two nucleons being required to accommodate the rest mass of the pion without any accompanying momentum. Such experiments have been carried out in the United States and in Europe but are still in their early stages. In addition to these experiments, whose purpose is clear, there are others whose potential is still unknown—as is characteristic of a fresh field.

2.4.4.4   *Kaons and Nuclear Properties*   The strongly interacting negative K-meson, K^-, may be able to tell us about a part of the nucleus—the outer fringe of the nuclear surface—of which we know very little experimentally and have few sound theoretical opinions. The great strength of the basic interaction makes it possible for a K^-, bound in an outer atom-like orbit around the nucleus, to sample the very thin "nuclear stratosphere." Since the end products of K^- reactions on protons and neutrons are detectably different, the relative neutron and proton populations can be revealed. Recent experiments at Berkeley on the absorption of K-mesons have begun this systematic exploration.

Plentiful supplies of kaons would make feasible a hypernuclear spectroscopy. The Λ-particles, hyperons, when produced by the absorption of K-mesons by nucleons, may remain within the nucleus to form compound systems called hypernuclei. Light hypernuclei have been extensively observed, but the study of the short-lived excited hypernuclear states and heavier hypernuclei require higher primary-beam intensities. The interest in these different experiments derives from the hyperon's role as an *in situ* probe of nuclear structure characteristically different from any other. Perhaps it is closest to a nucleon freed from the need to obey the Pauli principle and so capable of taking on any orbit within the nucleus.

It would be wise to pursue such experiments with kaons, antiprotons, and other particles made available in present high-energy accelerators. The construction of special facilities for such nuclear experiments can be studied as these explorations show what can be done.

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2.5 THE NUCLEUS AS A MICROSCOPIC LABORATORY

The nucleus sometimes appears in a supporting role in the study of other phenomena as the medium in which the reaction occurs or as a device that amplifies the phenomenon to be investigated. To know the nuclei and nuclear states that will best serve, one must know the pertinent nuclear physics and have available a well-developed nuclear systematics. Some past history and current events will illustrate this.

2.5.1 Weak Interactions

Weak interactions are now known to be a universal phenomenon. However, by pressing the study of the weak interactions within the nuclear domain, general principles of wide validity were established.

The properties of the first of the weak interactions, the beta-decay phenomena, were deduced from nuclear processes. The great experiments that first demonstrated the breaking of a classical conservation law, parity conservation, were carried out by seizing on the combination of properties of the beta-decaying nucleus cobalt-60. Other weak interaction properties were established in nuclear experiments. The direct determination of the spin direction of the neutrino involved in beta decays was made possible by transferring the momentum and angular momentum properties of the invisible neutrino to an observable succeeding electromagnetic radiation. For this to be feasible, a complex set of characteristics had to be matched, and, out of the whole systematics, only the europium-152 nucleus had them. But this one observation was one of the crucial steps in establishing the general law of weak beta interactions.

The conserved vector current theory argued for a very close connection between the form and structure of the weak interactions and the interaction of the electromagnetic field with nuclear currents and charges. A direct test was afforded by a triad of transitions—beta decay and electromagnetic transitions—that are the isobaric spin partners of one another. The known validity of the isobaric symmetry in the three nuclei of atomic number 12—boron-12, carbon-12, nitrogen-12—guaranteed the structural identity and permitted the comparisons needed to establish the general law.

There still remain qualitative and fundamental questions about the weak interactions. The weak interaction is understood to occur with the emission of a particle—an electron—and an antiparticle—the usual neutrino. That a particle is always accompanied by an antiparticle suggests a general conservation law, usually called light-particle, or lepton, conservation. The very weak process of double beta decay could test this general law. The question is whether in a process in which two betas are emitted two neu-

trinos or no neutrinos accompany them. The latter, in which just two light particles are emitted, violates the conservation law; the sensitivity of the test is enhanced by the fact that this violating process is inherently a faster one. The catch is only that double beta decay being a doubly weak process could be seen to occur only if a situation existed in which everything else is energetically impossible. Suitable nuclear candidates have been found and indirect evidence for some kind of double beta decay discovered; but the final question of the limit to be set on the rate of the neutrinoless transition remains as yet unanswered.

Modern weak-interaction theories strongly suggest that not only is there a parity-violating weak interaction between nucleons and the beta-neutrino system but also between a nucleon and another nucleon. Such a parity-violating part of the nucleon–nucleon interaction would then come into any nuclear process in both simple and complex nuclei. Since the expected effect is weak (one part in a million), the strategy has necessarily been to use the well-known nuclear systematics to find particularly favorable circumstances where the usually dominant effect is suppressed, thus amplifying the relative importance of the parity-violating term.

It is now possible to say that such parity violation, of the order of magnitude expected, has definitely been seen. These observations constitute strong evidence for the correctness of the lines of thought that suggest a parity-violating nucleon–nucleon force. Parity tests on nuclear processes have been carried out in the United States, Europe, and in the Soviet Union. Several kinds of experiment have been carried out. Searches have been made for nuclear reactions in which nothing would be seen to react unless parity is indeed violated. Another based on observing a correlation between an angular momentum, or a sense of rotational motion, and a rectilinear motion. It is this last type of experiment that, in two nuclei, has successfully demonstrated the parity-violating effect. Other experiments, in the Soviet Union and West Germany, based on observing the directional preferences of radiation following the capture of polarized neutrons are snarled in contradictions, and groups in the United States are preparing to enter the fray. Such fundamental experiments are well worth doing in many nuclei and in many ways.

2.5.2 *Time Reversal and Other Symmetries*

The violation of one of the general invariance laws, parity, brought into question others. The invariance under time reversal, the equivalence between motions of a system and those in which all motions and the sense of time are reversed, also arises in classical physics. A definite violation in the decay of the neutral K-meson has brought the question of T-invariance

in quantum physics to the fore, especially since the source of the violation of time reversal invariance is unknown. Where else do such violations occur? Are nuclear processes involved, or can nuclear effects be ruled out? The weak-interaction beta decays of the neutron and more complex nuclei have been searched to this end. Tests have been made in electromagnetic radiation processes and nuclear reactions in which only nucleons are involved. To within a few parts in a thousand no violation has as yet been seen, but attempts are being made to lower this limit. If both parity and time reversal invariance are violated, static electric dipole moments can occur both for nuclei and for nucleons, but none has yet been observed.

Nuclear physics shares the general interest in the fundamental laws that apply to all of physics. It may well provide an especially good region for observing small violations.

2.5.3 Experiments with Very-Short-Lived Particles

Measurements of the scattering of mesons that live 10^{-8} sec are by now familiar and routine. For lifetimes a thousand times shorter, the particles would not survive the passage across any ordinary experimental apparatus. If the interactions between such transitory particles and nucleons are to be measured, very small laboratories are required. Fortunately, nuclei provide such facilities. The particle is produced in a collision with one of the constituents of the nucleus and scatters from other nucleons on its way out. The cross section can then be deduced if the nuclear physics and the scattering formalisms are well enough understood. The basic production cross section has, of course, to be known from measurements on the very lightest nuclei—protons and deuterium. The nuclear physics and the scattering formalism can be handled with the aid of information from the study of other more familiar scattering processes in the same nuclei. This procedure has already been used to estimate the interaction cross sections on nucleons of ρ-mesons and ϕ-mesons produced in high-energy photoproduction experiments.

2.6 THE DATA OF NUCLEAR PHYSICS—ITS ORGANIZATION AND TRANSMISSION

The domain of nuclear physics embraces a vast amount of empirical information. The very richness and variety of phenomena exhibited by the large numbers of known nuclear species poses a formidable problem in straightforward cataloguing, to say nothing of the enormously greater task of placing the observations in a theoretical context. The statement

that no two nuclei are alike has a nontrivial implication: we are in fact confronted with many-body systems of such complexity that any kind of organization of nuclear species into classes has only limited predictive value. It is just the extreme sensitivity of the nuclear structure to the delicate interplay of competing interactions that offers the greatest challenge to nuclear theory; and it is in the confrontation with a vast amount of detailed empirical information that theories must meet their test. The power of modern experimental techniques and the sublety of the questions to be answered impose severe demands on the compilation and organization of nuclear data.

In the sense just described, we picture nuclear data and nuclear theory as interacting at the battlefront, combining hard fact and logical deduction to develop a deeper understanding of the nucleus. Equally important, of course, is the synthesis and condensation of our advancing knowledge for incorporation into the general culture. In any science, the orderly progression of knowledge from primary literature to textbook is a challenging and arduous procedure involving many stages of data compilation, critical evaluation, scholarly reviews, and treatises. Again, the many-faceted character of nuclear physics and the large variety of interests that the subject intersects pose special problems in selective condensation and presentation of nuclear knowledge.

It is well to recognize that the basic data of nuclear physics have important implications for other fields. One need only mention the needs of nuclear engineers for neutron cross-section values, the importance to users of tracers and radioisotopes of well-documented decay schemes, and the interest of astrophysicists in nuclear-reaction cross sections to exemplify the demands for timely, accurate, and well-organized nuclear data. While many of these enterprises grew up as part of the main development of nuclear physics, they are sufficiently specialized by now to require separate treatment in the information chain.

The pattern for critical evaluation and organization of experimental nuclear information was set in 1937 by the classic article of M. S. Livingston and H. A. Bethe (Rev. Mod. Phys. 9:245–390). At a time when the subject was in its infancy, they meticulously evaluated published experimental data, corrected the reported values with new calculations, and placed the entire body of empirical information in a framework where its relevance to the theory could be discerned. It is not too much to say that this article, and the accompanying treatises (H. A. Bethe and R. F. Bacher, Rev. Mod. Phys. 8:83–229, 1936; H. A. Bethe, Rev. Mod. Phys. 9:69–244, 1937) developing the theory, formed the backbone of nuclear physics for many years.

Nuclear physics has grown since 1937, and it is no longer possible to

encompass the whole knowledge of the subject in one scholarly work. The tables of Livingston and Bethe have their progeny in uncounted specialized volumes on range-energy relations, neutron cross sections, tabulations of atomic masses, energy level diagrams, and radioactive decay schemes. Representing as they do an enormous literature with several thousand new publications each year, these compilations involve a considerable effort on the part of active nuclear physicists. Even with a literature of such comparatively high quality as that of nuclear physics, the extraction and critical evaluation of relevant information is a demanding task and one that requires continuing application. This process of steady consolidation, of reviewing the bases of our knowledge, and of placing facts in the relevant framework constitutes at once a mechanism for identifying the changing forefronts of nuclear physics and a fulfillment of the basic obligation of scientists not only to discover but also to make known.

3 Applications of Nuclear Physics

3.1 INTRODUCTION AND SUMMARY

Unlocking the riches of the physical world and channeling them for man's use is a central aim of physical science. This chapter is devoted to a brief review of the role that nuclear physics—and nuclear physicists—have played in this mission.

How does a science make its contributions to the material needs of human existence? There are four primary ways: through the discoveries of physical phenomena and the general laws underlying these phenomena; through the application of the information and the insight that emerge in the orderly development of the science; through the devices and inventions that are offshoots of the apparatus and machinery devised for scientific investigation; and, finally, through the ingenuity of its practitioners in turning their scientific skills and methods toward overcoming or circumventing the problems of our technically advanced society.

Nuclear physics, a relatively young and developing science, has deeply benefited us in all these ways from its beginning. The breadth of current and projected developments and the vigor of the science itself promise a continuing flow of contributions toward the progressive harnessing of nature.

The discovery of the phenomenon of nuclear fission changed the nature

of both war and peace. It immediately revolutionized the materials of war and methods and concepts of national defense, and, at the same time, and much more importantly, it loosed the constraints imposed on the world by its otherwise restricted and rapidly diminishing supply of fossil fuels.

The major developments are well known; less well known but, in sum, just as important are the myriad small inventions, applications, and ideas that have been a part of nuclear physics since its inception. New isotopes of elements and new elements, artificially made in nuclear reactors and accelerators, are finding rapidly expanding usage in science, industry, medicine, and technology. The effects of nuclear technology are manifest in big and little ways, in ways close to and far from the individual. Industrial uses of nuclear radiations range from the giant irradiation units performing vital but unseen services on production lines to the small radioactivated phosphors illuminating warning signs in airliners. Medical applications run the gamut from the now familiar use of x rays and harder radiations and neutrons in the treatment of cancer to new isotopes whose use permits unraveling finer details of the intricate biochemical and biophysical basis of life itself.

Nuclear physicists, too, have formed a resource, contributing in fields both inside and outside their specialty, ranging from closely allied areas of application to those in which their entering expertise consists only of their style and method in approaching new problems.

3.2   NUCLEAR POWER SOURCES

The discovery of the phenomenon of nuclear fission in 1938 came as the explanation of a series of puzzling and paradoxical results of nuclear experiments. With it began the modern era of large-scale utilization of nuclei. In the history of science and technology there are few instances in which the interval between a basic discovery and its application has been so short. The initial crash program, in the midst of World War II, was, of course, directed toward military application and was spectacularly successful. Nuclear fission, however, also offered the larger gifts of a new copious source of energy and the availability, for the first time, of large quantities of very useful radioactive isotopes.

The nuclear fission reactor, first built to test the feasibility of a self-sustained chain reaction, showed the way to the controlled extraction of nuclear energies. After the war, a new industry arose to exploit this new potential (see Figure II.2 and Table II.1). At the present time, the installed nuclear power capacity in the United States is about 5000 MW. While it is now only a small fraction of our total capacity, it is increasing rapidly. It
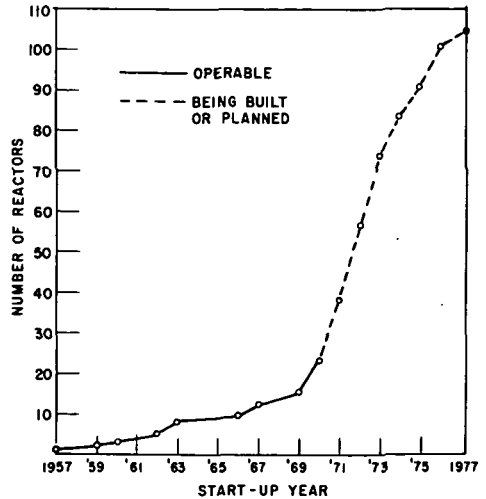
FIGURE II.2   Nuclear power plants built, being built, or planned in the United States as of June 30, 1970. From USAEC Report TID-8200 (22nd rev.).

NUMBER OF REACTORS		PLANT CAPACITY (kW)
OPERABLE	16	5,073,300
BEING BUILT	54	43,756,900
PLANNED	34	32,591,000

has been estimated that by 1980 nuclear energy will be furnishing 22 percent of the total electrical energy required, and about 40 percent by the year 1990. Annual investment in nuclear power plants during the next 10 years is expected to average some $3 billion, and fissionable fuel costs alone will total in excess of $12 billion. "Because of its advanced technology and plant capacity the United States has been in effect the only exporter of enriched uranium for power reactors. Indeed, our export earnings from sales of nuclear power plants, fuels and related services are over $1 billion now and are expected to reach $5 billion by 1975."* Nuclear power is fast becoming a major industry, supplying the energy requirements of a growing fraction of our economy, but even this constitutes only a part of its overall importance. It promises to free humanity from the constraints imposed by a limited supply of fossil fuels at a time when an expanding world seeks vastly more of the basic, central ingredient of progress—energy. Nuclear power promises massive energy generation with additional benefits such as reduced air pollution, virtual elimination of the problem of transporting massive amounts of fuel, and the conserving of fossil fuels for future petrochemical purposes. At this time we have only a glimpse of developments that may occur with the advent of massive nuclear-energy

*Department of State Publication 8571 (March 1971).

TABLE II.1   Nuclear Reactors—1970 (Built, Being Built, or Planned in the United States for Domestic Use or Export)[a]

Civilian Reactors	Operable	Being Built	Planned	Shut Down or Dismantled
Civilian Reactors				
Power Reactors				
Central-station electric power	15	54	32	6
Dual-purpose plants	1		2	
Propulsion (maritime)	1			
Experimental power-reactor systems				
Electric-power systems	3			21
Auxiliary power (SNAP)	1			8
Space propulsion (Rover)		2	2	18
Test, research, and university reactors				
General irradiation test	4		1	2
High-power research and test	11			2
Safety research and test	3	2		7
General research	36			21
University research/teaching	53	4	5	1
Production Reactors				
Materials production	4			9
Process development	5			
Military Reactors				
Defense power-reactor applications				
Remote installations	4			2
Propulsion (naval)	104	25	5	5
Developmental power				
Electric-power experiments				3
Propulsion experiments	7			7
Test and research				
Test	4			1
Research	7			3
Reactors for Export				
Power reactors				
Central-station electric power	10	8	5	
Propulsion	1			
Test, research, and teaching				
General irradiation test	4			
General research	26	2		1
University research/teaching	25		2	

[a] Taken from USAEC Publication TID-8200 (22nd rev.).

generation; possibilities include large-scale desalination installations, electrification of the metal and chemical industries, and more effective means for utilizing the waste products of man's activities. There are serious problems requiring continuing attention. Management of the vast amounts of radio-

active wastes that will be produced in reactor operations is a major consideration; these wastes must be contained and isolated from man and his environment. Thermal pollution must be kept within acceptable limits. There is, however, a solid basis for optimism that the manifold benefits of nuclear power can be realized without unduly affecting our environment. The methods and skills of nuclear science are a safeguard against undesirable developments.

Intensive research and engineering efforts are under way on reactor problems that, when overcome, will greatly enhance the capabilities of these nuclear-energy sources. Waste heat is a major problem with current nuclear installations because structural limitations in the reactor itself limit output steam temperatures to some 600° F compared with the 2000° F in a modern fossil-fueled plant. This reduced temperature carries a penalty in efficiency of extraction of electrical energy and consequently greater release of thermal exhaust energy for a given amount of electrical output. This problem is being faced in developments aimed at increasing the effective working temperatures of the several types of reactor and in the search for profitable uses for the discarded heat.

New kinds of reactors, breeder reactors, are now being developed that can simultaneously generate useful power and breed fissile material from very abundant but previously unusable uranium and thorium isotopes. A major development program is directed toward commercial utilization of these reactors, in recognition of the fact that available resources of uranium-235 are limited. It should be noted that current reactors extract only 1–2 percent of the energy potentially available from their uranium fuel and are dependent upon the availability of large amounts of low-price ores to keep their power costs economically competitive with those of fossil-fuel plants. Breeder-reactor power costs are expected to be relatively insensitive to the cost of uranium and thorium, thus opening up economic use of low-grade ores. Other transuranic elements that will be produced may prove to be useful by-products.

The earliest work on the design, planning, and realization of power reactors was done by nuclear physicists specializing in this new field. With the growth of the field into an industry, a new kind of specialist, the nuclear engineer, has emerged to take on the reactor design and engineering development. Today, nuclear physicists tend to become engaged in the parts of the field that are concerned with instrumentation, mathematical models, cross-section measurements, evaluation, and other aspects that are not directly related to the mechanical design. The reactor industry looks to the nuclear physicist for innovative ingenuity in introducing new ideas into power reactor development.

Nuclear physicists will continue to play a strong role in activities leading

to reactors of improved economy and increased sophistication. Their role in part will be that of providing more precise data on neutron and gamma-ray cross sections, the energy and angular distributions of nuclear particles and radiations, and the yields of neutrons and fission products—the vital ingredients that quantify the basic mechanisms at work in turning nuclear energy into useful heat. Such data, together with theoretical models, are used to determine the economies of a nuclear power reactor: the required fuel loadings, the amount of energy to be extracted before a new loading will be required, the devices needed to control a reactor, the amounts of radioactive materials produced, the shielding needed, and other important details of working plants.

The success of these efforts to date is impressive—so much so that, in the case of conventional reactors based on the use of low-energy or thermal neutrons, the designs are not seriously limited by uncertainties or un-knowns in the nuclear data. But even those nonserious limits involve very high stakes. Thus, among the cost uncertainties there are those associated with nuclear data affecting primarily the fuel costs. These uncertainties are estimated to be of the order of only 0.02–0.03 mil per kilowatt-hour of electric power. But applied to the entire projected nuclear-power capacity of the United States, this trifling uncertainty will amount to $20 million to $30 million in annual fuel costs by 1980, $140 million to $210 million by the end of the century. In the case of the newer breeder reactors, which are based on higher-energy neutrons, the uncertainties are much larger. Here, cost uncertainties, attributable to cross-section data are 0.15–0.25 mil per kilowatt-hour, or up to 30 percent of the total contemplated fuel cycle cost—a matter of billions of dollars annually. Substantial further ef-forts, both experimental and theoretical, on neutron cross sections will be required to reduce these cost uncertainties to an insignificant level.

Such applied usage demands data of the highest quality and determines the priorities for cross-section measurements. These neutron measurements, at the limits of present accuracy, require recently developed techniques, methods, and special skills. In this context, as in many others, the work in the basic and applied research programs overlaps substantially, and, in fact, it is difficult to make a clearcut separation between them. There is no rea-son to desire separation, but there are many reasons to encourage the sym-biosis. Among other things, it ensures a close coupling of nuclear physics and the nuclear power program. As an example of such overlap, knowledge of the neutron capture and fission cross sections of plutonium-239 has had significant impact on the planning of reactor development programs based on more energetic neutrons. It was remarked in the previous chapter that the results of these neutron cross-section measurements have been more important also for basic nuclear physics.

Since the basic phenomenon in reactors is nuclear, it is not surprising that there are many significant applications of recent nuclear developments to reactor problems. Modern high-resolution gamma-ray spectroscopy (made possible by the new detector and instrumentation developments) has been applied to observe the distributions of fission products in order to analyze both normal and abnormal behavior of reactor components. As part of the nuclear reactor development program the more precise analysis of the neutronic behavior of reactors will be carried out in a variety of re-actor-physics experiments. Many of the techniques of nuclear research will be closely applicable. The use of an accelerator as a pulsed source of neu-trons is an especially interesting way to study the dynamic behavior of re-actors under a variety of driving conditions. The uses of up-to-date nuclear techniques in design studies will continue to be important for innovative special-purpose reactors. Nuclear reactor development work and nuclear research are distinct but allied fields, and a strong flow of information and people between them is natural.

There are other vast sources of nuclear energy that have not yet been tapped. Thus far we have focused on the release of nuclear energy through the medium of the fission mechanism. Even more attractive, albeit techni-cally vastly more demanding as far as commercial power is concerned, is the equivalent release of nuclear energy in fusion reactions. It has been known for a long time that reactions in which light nuclei are fused to-gether give rise to energy emission; that fusion processes are responsible for the enormous energy generation of the sun and other stars.

Physicists discovered the fusion reaction in the laboratory before they did fission, but to create and control fusion power is a vastly more diffi-cult task than that of harnessing fission. The fusion reactions of deuterium with deuterium, of deuterium with tritium, and of deuterium with helium-3 have been studied extensively in the laboratory over the past 20 years. Power could be extracted on a massive scale if enough of these nuclei could be brought together with sufficient energy to overcome their mutual repul-sion. Containment in a plasma of sufficient density and at a very high tem-perature may offer a practical way of attaining these conditions. Problems of safety, thermal release, and radioactive waste disposal may largely disap-pear. And there is hope of direct conversion from nuclear energy to electri-cal energy without the intrinsically inefficient thermal intermediary cycle characteristic of fission reactors.

However, the practical utilization of fusion lies in the future. An initial period of enthusiasm and optimism in the late 1950's was followed in the 1960's by increasing realization of the complexity of the necessary tech-nology and a period of disappointment and pessimism. Only recently, as a consequence of significant new developments in the Soviet Union and in the United States has cautious optimism re-emerged about the eventual

controlled release of nuclear energies in a fusion system. The problems are formidable, but so tremendous are the potential benefits that strong scientific efforts to break through the remaining difficulties will not slacken.

While the main effort is in plasma physics, nuclear physics and physicists are involved in a number of essential ways. Nuclear techniques are used in laboratory research with plasma-containment devices. Through the years, workers in accelerator development and plasma physics have been exchanging ideas with profit to both. When a controlled-fusion reactor is achieved, nuclear science will be called on to define the shields and the forms whereby the nuclear components of the reactions will be regenerated.

One can see a steady progression of capabilities in the exploitation of nuclear power. The light-water reactors, already in use, will be the work- horses of the nuclear power industry for some years; however, they must be coupled with more advanced reactor types if future energy needs are to be met. The breeder reactor is being developed to lift the limitations of the current types of fission reactors. The fusion reactor, still in the early stage of laboratory development, stands back of these as a hopeful possibility of the future. There are still further ideas on the far horizon with as yet un-explored possibilities. One that has been considered involves the use of a very large accelerator in a power-producing complex. This follows from the observation that a neutron for eventual use in initiating a power-producing event can be obtained at lower energy expenditure in spallation reactions that follow bombardment of a heavy nucleus by protons than when it is produced in the fission chain reaction itself. The spallation-produced neutrons would then be used to produce power and to breed new fissile material in blankets of appropriate fissile and nonfissile materials assembled around the accelerator target.

However the far future is resolved, the energy sources in the near future will be importantly based on nuclear fission reactors. The unlocking of nuclear energy has already been of enormous importance, and its impact is in the early stages. We do not know with certainty whether nuclear physics has any more such prizes. We do know, as we shall make clear in the next section, that the earnest efforts to explore the field completely result in a series of very useful practical benefits that add cumulatively to an enormous total reward.

3.3   PRACTICAL APPLICATIONS OF NUCLEAR PHYSICS TO MEDICINE, FOOD, INDUSTRY, AND TECHNOLOGY

We have already alluded to the important applications of nuclear physics in the industrial and agricultural economies, in medical diagnostics and

treatment, and in other essential services. Nuclear physics interacts with these areas through five primary channels: radioisotopes as tracers; radioactive nuclei as energy and radiation sources; nuclear methods of materials analysis; the direct utilization of electron, photon, and ion beams from accelerators; and, finally, the broad range of instrumentation developed by or stimulated by the nuclear community. This has been a continuing and developing situation with new discoveries in nuclear science influencing an ever-widening range of external activities. Because of the broad utility of nuclear developments, in this section we can only illustrate their usefulness with a few examples of their transmutation into effective practical benefits.

3.3.1 *Radioisotopes in Medicine*

The availability of many radioisotopes of all elements at low cost has led to revolutionary changes in many fields, but perhaps most strikingly in medicine and in the biological sciences. We put aside here the crucial applications of medicine and biochemistry in untangling the basic processes of life and in the development of new tests, new drugs, and new insights. We focus instead on the parts of medical practice closest to the treatment of patients.

Great progress has been made in nuclear medicine in the use of emissions from radioactive isotopes to "visualize" organs and in the use of radioactive tracers to measure internal body functions and to perform biopsies on tissues taken from patients. Radioisotope scanning to locate tumors and others abnormalities has become routine clinical procedure, some hospitals performing many thousands of diagnostic scans per year. Statistics, however, do not describe completely the growing importance of these procedures. Many radioisotope imaging tests have become the procedure of choice in diagnosing and locating brain tumors. Indeed, any enumeration of the specific human organ systems in which radioisotope scanning has played an important diagnostic role includes practically all such systems, just as the functioning of almost all of them has been traced out with tracer techniques.

One of the most widely used imaging devices at the present time is the Anger camera. This device is used to observe a large number of different bodily functions by injecting a radioactive element into the body and observing its circulation through the organ whose function is being studied. This camera is an array of standard sodium iodide crystal detectors and electronics that were originally developed in nuclear-physics laboratories. About 10 million scans on patients are performed each year (three fourths in the United States). The rate of growth of this number is 15 percent per

year. Sales of these cameras are $37 million for about 600 cameras per year. Foreign sales are about 10 percent of this total and represent the fastest sector of growth.

Radioisotopes appear again in the medical arsenal as a tool of therapeutic treatment. During 1968, over 4 million patients in the United States were treated with radioisotopes. One of the most frequent and now most familiar of these treatments involves the uptake of radioactive iodine by the thyroid. Radioactive gold and phosphorus are among other isotopes used for therapy as well as for diagnostics.

To produce this medical revolution has required the availability of a wide variety of radioisotopes, the cataloguing of their nuclear properties, and means for detecting and measuring their radiations. Special purposes, however, require isotopes with unique properties, and here we find that well-explored nuclear systematics that permit picking out the most useful set of nuclear properties are essential. The nuclear reactor, the nuclear accelerator, and the isotope separators have produced a variety of radioisotopes that span the system of chemical elements. This material is the basis of the current flood of applications.

The discovery of new applications of radioisotopes and new radioisotopes for old applications goes on continuously. Two medical examples will suffice to illustrate this situation. As has been mentioned, millions of thyroid tests based on radioactive iodine "cocktails" are made every year. For many years, the typical iodine isotope used was iodine-131, which, in addition to the desired gamma radiation, also produces beta rays. The beta rays created a problem, since, in order to attain a readily detectable gamma-radiation intensity, the level of beta-ray activity is such as to cause some unwanted radiation effect in the thyroid and surrounding tissue. Recently, however, it was found possible to produce iodine-123 to act as the active ingredient in the "cocktail." This new isotope is now replacing iodine-131, not only for thyroid uptake studies but for brain scans, blood volume measurements, and liver and lung scans as well. Unlike iodine-131, accelerator-produced iodine-123 emits no nuclear particles (or beta rays) that can be absorbed in the patient, and only the easily transmitted gamma ray tags the iodine. Thus, millions of patients will be spared the unnecessary radiation damage associated with iodine-131 radiation therapy and diagnosis.

The most recent agent used in brain scanning, technetium-99, was first introduced in 1964—27 years after the discovery of this isotope following deuteron bombardment of a molybdenum target in a cyclotron. Technetium-99 has unique qualifications for brain scans: it emits only a low-energy gamma ray, which permits its identification with minimal extraneous or unnecessary patient irradiation; it concentrates selectively and

rapidly in diseased tissues; its radiation is easily collimated, permitting precise location of the isotope concentration, and its short half-life (6 hours) allows its rapid removal from the body. In addition, its chemical properties allow it to be incorporated into a variety of compounds that can be placed in many body organs. The short life of the isotope means, however, that it cannot be stored, and, were there no way around this difficulty, its usefulness would be greatly circumscribed. In the search for the most advantageous radioisotopes, this rather stringent requirement must be met. In the case of technetium-99, the isotope is obtained from a reactor-produced radioactive "cow," molybdenum-99. This "cow" is milked several times a day for the technetium-99 to be used for brain scans. The medical use of this isotope has resulted in a great demand for its parent nuclide, molybdenum-98; in 1969 a substantial fraction of the entire Oak Ridge stable isotope separation effort was devoted to meeting this demand.

Recently, nuclear medicine has been moving beyond the use of packaged radioisotopes, whose decay lifetimes are long enough to permit them to be processed and shipped in a more or less routine way. Radioisotopes whose properties are important but whose lifetimes (fractions of hours) are so short as to require production on the spot are now produced as required by small cyclotrons and linacs installed in hospitals. Among the applications of these machines is the production of three short-lived isotopes: carbon-11, nitrogen-13, and oxygen-15, representatives of the building blocks of the organic chemistry of life. These isotopes are crucial in a wide variety of biochemical and physiological studies. They are examples of a large number of other short-lived nuclear species that have been used for both biological and clinical research purposes, some of which have moved into routine use.

This increased local reliance on and utilization of nuclear facilities in medical environments reflect the growing importance of nuclear physics and broadly trained nuclear physicists in medical laboratories and medical work.

3.3.2 Radioisotope Tracers in Industry

The explosive growth in the use of radioisotopes in industry began with the advent of the means to produce a great number of species of known particular properties. Today, approximately 100 private firms, most of them formed over the last 10 years, are engaged in the production, processing, and distribution of radioisotopes and the sale of equipment associated with their use. The marketing of radioisotopes is a $30 million-a-year business,* and sales of equipment and products involving their use are on the

*USAEC, The Nuclear Industry—1969 (U.S. Govt. Printing Office, Washington, D.C., 1969).

order of several hundred million dollars annually. In 1969, roughly half of the 500 largest manufacturing concerns in the United States used radio-isotopes. "About 4,500 other firms also are licensed to use radioisotopes under AEC general licenses. *These thousands of firms benefiting from radio-isotopes come from virtually every type of industry, including metals, electrical and transportation industries, chemicals and plastics, pharma-ceuticals, petroleum refining, paper, rubber, stone, clay and glass products, food, tobacco, textiles, crude petroleum and natural gas, mining, and utili-ties."* This rapid and pervasive growth was based on the research in which the systematics of the nuclear properties and nuclear spectra were estab-lished. The revolution in experimental technology of the 1950's and the consequent opening of nuclear systematics provided the data and instru-mentation needed for the manifold applications of radioisotopes in both industry and medicine. At present, the growth continues as new methods for using radioisotopes are found and as new useful radioisotopes are made available at lower costs. Similarly, improvements in detectors and instru-mentation are opening new areas by improving sensitivity, selectivity, and reliability of radioisotope techniques.

Radioisotopic tracers are now part of a wide range of industrial pro-cesses. They are routinely used to record the movement of material in pro-duction equipment, in laboratory studies of industrial process parameters, and in the study of chemical reactions. Direct introduction of radionu-clides into industrial process streams as tracers permits monitoring the be-havior of large and complex plants without disturbing in any way their usual operation. Prime examples of this technique are flow-rate measure-ments of tagged materials in refinery pipes and in processing machinery and detecting leaks in chemical plants. The savings to American industry from the use of flow measurements alone are already considerable. In 1963, a year for which statistics are available, the estimate was some $30 million to $50 million, and the use of this technique has increased enormously in the intervening period. The monetary advantages, however, reflect only in part the fact that radioisotopic tracers have given rise to a new style and a new scope in industrial and medical practice.

3.3.3   *Radioisotopes as Energy and Radiation Sources*

The uses of radioisotopes as energy and radiation sources range from in-conspicuous to giant production units, from the peripherally important to the vitally needed. The oldest known radioisotopes are used as well as are

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*"The Atom's Expanding Role in Industry," USAEC Release No. 5-24-69. Remarks by Dr. Glenn T. Seaborg, Chairman, U.S. Atomic Energy Commission, at Dedication Cere-mony for Radiation Machinery Corporation's Headquarters and Development Center, Hanover, N.J., July 31, 1969.

recently discovered elements. Their uses in industry are based primarily on two properties of their radiations: the ability of the radiation (gamma) to traverse matter nondestructively, permitting measurement of attenuation after traversal, and the deposition of ionization energy in matter.

The most pervasive industrial application of radioisotope sources is gauges that measure thickness, density, fluid level, water content, and other physical properties by observing the fraction of the radiation scattered or transmitted by the sample material. These techniques are indeed brute-force exploitation of the interaction of radiation with matter; however, when coupled with sophisticated electronic instrumentation, they bring a delicacy of measurement previously inaccessible to routine industrial activity. Their development has been rapid, dating from early successes that began as soon as reliable sources and detectors became available. The kinds of use and the subtleties of use have expanded markedly, and the costs have fallen dramatically with the availability of a wide variety of cheap radioisotopes and of rugged, reliable detectors.

The ease with which these nondestructive tools can be incorporated as control and monitoring devices has made them important parts of industrial production lines. Their use in the steel industry is typical and demonstrates their value. As everyone knows, sheet steel, a critical component in automobiles, household appliances, and most of the contrivances around us, is produced in enormous quantities. The sheets are formed by squeezing and stretching the steel in giant, high-speed rolling mills. It is vitally important to the ultimate user and to the economy of production that the sheet be of specified thickness and uniformity. The key control in this operation is the element that measures the thickness of the metal sheet and commands changes in the roll pressures and sheet tensions that keep the thickness constant. It is impossible to make accurate thickness measurements mechanically on a wide expanse of wildly swaying sheet steel moving at a rate of 50 feet per second. Nuclear gauges installed across the sheet uniquely measure the thickness, without any contacts whatever, by observing the attenuation of the radiation through the material. By electronically feeding the signal to the roll controls, product quality is constantly and accurately maintained.

Such installations find industrial use wherever on-line sensing of the product characteristics is possible. Nuclear gauges are now employed in industries such as those dealing with both ferrous and nonferrous metals, paper, rubber, plastics, tobacco, chemicals, petroleum, and mining. Newer applications are now being found in food and beverage manufacture and in monitoring effluents as one of the measures of pollution control. Virtually every tire made in this country is controlled at some point in its manufacture by a nuclear gauging system. The delicacy of sensing suits nuclear

gauges for monitoring the production of the most fragile paper as well as sheet steel—only the penetrability of the nuclear radiation need be changed, and this can be done simply by changing the radioisotope.

The market for these devices is already impressive and is growing rapidly. There are some 14,000 gauging installations, representing more than a quadrupling in the last decade. The market for radioisotope gauges is estimated at over $35 million a year and appears to be growing at a rate of 20 percent annually. The annual savings to American industry must be several times the $35 million investment.*

A somewhat different major use of nuclear-physics techniques occurs in prospecting for minerals and especially in the search for oil. In past years, the problem of finding and producing oil was complicated by the fact that precise location and analysis of the various strata in a deep hole by core drilling was an expensive and slow process. Nuclear techniques provide the solution. A sensitive nuclear detector is lowered into the hole, and its readings locate the occurrence of natural radioactive strata. If, then, a neutron or gamma-ray source is lowered with the detector but shielded from it, the detector will respond to the neutrons or gamma radiation scattered around the shield by the materials in the walls of the drill hole. On the basis of extensive experience now available, these readings are directly interpretable to yield rock formation, porosity, water, and petroleum content data. Virtually every oil well drilled in the United States is logged via nuclear techniques, in some cases repeatedly, to observe the changes produced by prolonged pumping. The yearly cost of this nuclear logging service amounts to approximately $25 million. The saving to the petroleum industry is many times this amount, since the information derived from logging reduces the number of costly dry holes that would otherwise be drilled.

Neutron logging is of particular interest in the petroleum industry because of the preferential and characteristic scattering of neutrons from the hydrogen in the hydrocarbon deposits. Until very recently, no convenient neutron-emitting radioisotopes were known, and the logging involved lowering a miniaturized accelerator into the well that produced the neutrons *in situ*. A typical well-logging source consisted of a 500-kV Van de Graaff complete with acceleration tubes, ion source, and neutron-producing target—all encased in a tube 3 feet long and 4 inches in diameter. Obviously, this is a delicate device, and the petroleum industry, understandably, has been interested in replacing it; indeed, they have been using isotopic sources such as plutonium–beryllium, but the new transuranic neutron-emitting radiosotope, californium-252 source, since it is much smaller, is expected to gain wide acceptance.

*USAEC, *The Nuclear Industry—1969* (U.S. Govt. Printing Office, Washington, D.C., 1969).

This isotope, first discovered in 1950 through cyclotron bombardments, has potential widespread application as a point source of neutrons. Neutron radiography using californium-252 may complement the presently employed gamma radiography of industrial components. Its use in human therapy is under intensive study because of the great effectiveness of neutrons in destroying anoxic cells. In tumor therapy the neutron source must be placed in the midst of the tumor, and this can be done only with the very intense radioisotopic neutron source promised by californium-252. Because of its potential usefulness, consideration is being given to its production by large-scale reactor methods.

The ionizing uses of radiation, too, have industrial applications of great importance. The differential sensitivity of contaminant bacteria to nuclear radiations has provided an approach to sterilization of materials that would otherwise be weakened or degraded by the more conventional thermal sterilization processes. Both gamma-emitting radioisotopes and electron accelerators are used extensively in this country and abroad in the routine sterilization of medical supplies and materials. The use of such radiations for food preservation, whose future importance we do not now know, is still in an investigative phase. Clearly, we are still only beginning to find new ways of dealing with the bacterial spoilers that have beset man throughout his history.

Agricultural scientists, using gamma radiation from cobalt-60 to sterilize screwworms in the pupal stage, developed a technique that has been used to eradicate the screwworm from the southeast United States and northern Mexico. Less than 10 years ago, the screwworm caused annual losses of up to $120 million in domestic animals and untold losses in wildlife. The technique may find further application to other major insect problems in this country and other parts of the world.

This is an example of just one of the many ways in which nuclear physics has been instrumental in advancing food production. The importance of radiation sources in forcing plant mutation and the use of isotopes in tracing the action of plant processes and in animal physiology are all part of the great advances in this century in the biological sciences and in the special and vital effort to provide more and better food for a still-hungry world.

Large sources of gamma rays have other uses. Just as x rays have become a standard way of examining faults in the human body, more penetrating rays have been used to examine manufactured articles. Radiography utilizing various packaged gamma-ray sources is in general use in industries that make heavy metal sections such as boilers and pipes. Examination by neutron beams will be a most useful probing tool, and the development of neutron generators and neutron sources promises to make this technique available for industrial and medical application.

The energy liberated in the course of radioactive decays can be used directly through thermoelectric conversion to produce electrical power or converted by appropriate phosphors to visible light. Power units of this type are of vital importance in remote places where other sources are short-lived or unavailable. The scientific instruments left on the moon by Apollo 12 are powered by a thermoelectric generator fueled by plutonium-238. Deep-space probes will undoubtedly include such power supplies. However, much smaller units are also important in the maintenance of vital human functions. Isotope batteries, whose high voltages are produced either by the collection of beta rays from a radioactive source or by thermoelectric generators, are extremely long-lived and reliable. These stable characteristics, together with their small size, make them extremely promising as the power units for implanted pacemakers to control abnormal heartbeat.

 Use of radioisotopes to produce visible light is still in its infancy but is already satisfying special needs. Exit signs in commercial airliners are illuminated by phosphors activated by tritium and krypton; here, independence of external electrical connections meets an obvious safety need and permits savings in maintenance costs as well. The experiment packages placed on the moon by the astronauts of Apollos 12, 14, and 15 are being powered by SNAP generators that derive their energy from plutonium-238. Also, the lunar modules of Apollo 11 and 12, returning to the command ships, homed on a radioluminescent docking target activated by promethium-147.

3.3.4   *Activation Analysis*

Nuclear methods have invaded the province of the chemist and are being used to determine elemental compositions. The methods are nondestructive, fast, and very sensitive.

Activation analysis is a technique for quantitatively measuring the amount of a given nuclide in a sample of material. The material is bombarded with particles chosen to produce radioactivity whose characteristic gamma radiation identifies the isotope. The technique was first proposed in the late 1940's and developed in the intervening years. The new semiconductor detectors, whose energy resolution is 100 times better than anything previously available, have worked a revolution in the ease with which it is possible to routinely resolve and identify the gamma-ray spectra of specific isotopes. Under favorable conditions, a few parts per billion are detectable. By virtue of its sensitivity, dynamic range, and comparative independence of the form of the surrounding material, the technique finds wide application in analyses for nearly every element. Its dramatic use in the detection of art forgeries and identification of trace materials left at the scene of a crime attests to the power of the technique.

Neutrons of various energies, high-energy gamma rays, and a variety of energetic charged particles are used to initiate the nuclear reactions for activation analyses. To date, thermal and fast neutron reactions are most generally useful. While very important for sensitive analyses, activation analysis has not been applied to production-line process control, partly because of difficulties in developing reliable, constant neutron sources for such routine use. The development of packaged neutron sources, based on californium-252, offers a possible solution to the problem.

We will note later the special advantages that activation methods based on neutron irradiation of material to be interrogated bring to the intricate problems of the Safeguards program. The problems of accurate analysis for fissionable material in mixtures in which the active ingredients are accidentally or deliberately hidden, without changing or destroying the original forms, are formidable indeed. The nuclear activation methods provide a subtle analyzer to meet a set of complex demands.

In a very real sense, nuclear techniques have brought order-of-magnitude improvement in sensitivity to analysis of elemental compositions. Their potential is limited as yet only by the ingenuity of the user.

3.3.5 Accelerators

Nuclear-particle accelerators were developed in the pursuit of nuclear-physics research. Workers in other fields have been quick to exploit their use in greatly diverse medical, industrial, and technological areas. The listing of accelerators used for these purposes, Table II.2, and their locations, Figure II.3, attest to their widespread utility. The benefits they have conferred have repaid the research and development invested in them many times over. New attacks on disease, new methods in industry, new products, and new ideas are part of the dividends.

Radiation therapy is one of the principal weapons in the fight against cancer. In this country alone there are 700,000 new cancer patients each year. In 1968, approximately 300,000 people were treated by radiation therapy. More than 200 accelerators around the country are being used for this purpose. While monetary measures are not an accurate guide to the importance of the fight for human lives, it is some guide to the value put upon it to note that these accelerators represent a capital investment of some $60 million and an annual operating budget of $30 million. The battle against cancer is by no means won, but improved radiation methods share with chemotherapy and surgery in the success of extending the survival rate against one of man's intrinsic enemies. The higher-energy accelerator, developed in the nuclear program, offers a very great advantage over the low-energy accelerator-produced x rays in the treatment of various cancers. The higher energies are translated into a highly penetrating, forward-peaked

TABLE II.2 Accelerators Used for Other Than Basic Nuclear-Physics Research in the United States—1968[a]

Principal Application Categories	Number of Accelerators	Investment, Accelerators Only $Millions (approx.)
Nuclear Engineering and Training		
Neutron-physics research	80	23.5
Reactor physics and engineering	41	1.6
Injection into larger accelerators	22	8.1
Teaching and training	46	1.9
X Rays and Neutrons		
Radiation therapy	220	31.0
Industrial radiography	176	18.9
Neutron radiography	2	0.3
Radiation effects		
Research in chemistry, biology, food technology	52	6.8
Activation analysis	104	2.9
Isotope production	7	2.2
Space-radiation simulation	46	6.4
Misc. (including weapons effects)	106	18.1
Atomic and Solid-State Physics		
Optical spectroscopy	3	0.8
Ion implantation and channeling	60	1.2
Solid-state R&D	10	1.1
Atom-ion interactions	5	0.2
Radiation Processing		
Chemical (e.g., plastics, textiles)	46	4.9
Solid-state	2	0.1
Radiation facility	12	1.5
	1040	131.5

[a] A. E. Burrill, Austin, Texas, private communication.

pencil of radiation that permits the delivery of more radiation to the malignancy and less to healthy tissues.

New improvements in accelerator designs and concepts are quickly translated into improved radiation facilities. For example, recent advances in the technology of linear accelerators developed for Los Alamos Meson Physics Facility were immediately applied to inexpensive hospital-based electron accelerators for therapy. Sales to date (64 accelerators) have totaled $10 million, and 50,000 patients per year are being treated. New radiations are becoming available to the therapist. For example, there is now being investigated a completely novel and potentially powerful modality in radiation therapy made possible by the new medium-energy accelerators. This involves the use of negative pions to selectively irradiate a tumor volume with minimal damage to surrounding tissue.

FIGURE II.3   General locations of accelerators for other than basic nuclear-physics research—1968. Number of accelerators, 1018; investment (accelerators only) $128 million.

The use of charged particles in the analysis of materials is new but rapidly expanding. Here use is made of the differing energy losses when charged particles are scattered off sample nuclei of differing mass. The technique is particularly powerful in searching for heavy nuclear contaminants in a lighter matrix. Specific examples of current use are the quantitative examination of the diffusion of uranium atoms into the aluminum cans of reactor fuel assemblies and determination of mercury contamination of lake fish. The composition of the moon's surface in Surveyor flights was first analyzed with this technique.

Industry uses accelerator-produced radiation for a great variety of purposes. The uses include radioisotope production, radiographic inspection of fabricated parts for hidden flaws, part of the process of sterilization of medical supplies, and, very importantly, radiation processing to provide physical or chemical changes in production processes. Radiation curing of coatings and finishes, especially of building materials, textiles, and metals, is a fast-growing activity. Irradiation of plastics and applications in packaging materials and electrical insulation are other examples. A very interesting development is the processing of paint by electron curing, thus eliminating solvents and providing a step forward in eradicating industrial pollutants. Of the more than 1000 accelerators in this country, about a

third are owned by private industry; over 40 of these are devoted to radiation processing on a production scale. It is estimated that about one quarter of a billion dollars of radiation-processed products are produced annually.* By the mid-1960's, over a dozen companies were manufacturing accelerators, thus establishing a sizable industry in its own right.

Many application developments have not yet been completed. A particularly interesting set of such ideas does not involve the application of accelerators *per se* but rather of accelerator concepts first proposed to solve design problems on new high-energy machines. Two such developments stem from the spiraling need for more electric power in already overloaded parts of the country.

Remote hydroelectric sites are being constructed—the Churchill Falls site in Labrador is a case in point. In the transmission of power from remote sites, a high premium in reduced line loss is obtained as the transmission voltage is increased. While 250 kV was standard a decade ago, it has been steadily moving upward to the 750 kV pioneered by Swedish industry. A number of accelerator companies, using their experience in high-voltage accelerator development, are actively at work on transmission-line components to be used at higher than a million volts.

Another attack on the power problem is much more daring. To solve the problems of transmitting high-frequency electric power for nuclear-particle acceleration without prohibitive line losses, ways of taking advantage of the superconducting properties of metals cooled to liquid helium temperatures are being developed. The technology here has been largely developed in connection with the transmission of power and the design of large superconducting magnets for nuclear-particle transport. The application of these developments to the transmission of commercial electric power over long distances throughout the country by means of very low-loss, superconducting networks is now in the explorative stages. Such networks may well be the solution to the double problem posed by the need for power in highly populated areas and the desire to keep the power source very far from these areas.

These are far from the only technologies that have sprung from accelerator design activities. These designs have always pushed at the frontiers of technology and, at the same time, have been the beneficiary of technological developments brought about by other pressures on these frontiers. It is difficult to describe these symbiotic bonds adequately, so intimate and pervasive are they, and we must be content with a few illustrations.

High-power modulators and klystrons demanded by linear accelerator

designs and developed to meet their taxing requirements have pushed forward the power levels by giant steps, hence the capabilities of our communication and national defense systems. The large vacuum enclosures required in accelerators have forced the development of large vacuum pumps and of high-vacuum technology generally. This technology is used throughout industry and was basic to the development of spaceflight capability. The techniques of beam handling involved in accelerators and the necessary stable voltage supplies are being applied to a totally new generation of electron microscopes with enormous potentialities for the life sciences. These are only a few examples of the technological transfer from the accelerator field.

3.3.6 Plowshare

Plowshare is the overall identification of the U.S. Atomic Energy Commission program for the development of peaceful uses of nuclear explosives. Central to this program is the use of a nuclear explosive underground to shake, break, and move earth around in massive amounts. Traditionally, as man's ability to manipulate the terrain has grown, so has his ability to mine the riches of nature and fight off its ravages. Do the awesomely greater powers of nuclear explosives offer a leap forward in this ability?

The Plowshare program is still in an early stage, and it is too soon to assess whether its goals can be attained. While the major problems of explosive design and containment, earth movement, and general ecological questions are beyond the expertise of the nuclear physicist, they present important areas to which he can contribute. One of the problems is radioactive contamination, which must be accurately predicted and rigorously controlled. In part, this will be a matter of containing radioactive debris. Nuclear design is important in minimizing neutron activation of surrounding materials and in the requirements for shielding.

A number of immediate applications have been identified, and some have been tested already. The use of nuclear explosives to liberate natural gas from underground rock formations has been tried, amidst storms of controversy, however. Efforts to fracture oil-bearing shales or mineral-bearing ores have not yet been made. Construction of huge underground reservoirs for oil and gas storage is proposed. Large-scale excavations of canals, harbors, and other such massive earth removals with a few, well-fashioned explosives are possible applications that are still in the future.

A by-product of the Plowshare and other tests that have been conducted has been a much-expanded understanding of seismic phenomena. Another has been the production in quantity of heavy elements having both scientific and practical value by using the extremely large neutron fluxes available in specially designed nuclear explosives. At present, some of these ele-

ments can be made by reactors only in extremely small quantities or with large impurities; for example, curium-250 cannot be made at all in a production reactor but was produced by piling up neutrons from an intense underground explosion. Neutron cross sections of very short-lived nuclear species, produced in the explosion itself, are of vital importance in the design of high-flux reactors and in work related to nuclear defense problems. These cross sections cannot be measured except in experiments using nuclear explosives.

3.3.7   *Nuclear Instrumentation*

Many of the applications already described directly involve in some central way the instruments of nuclear research—the new high-resolution gamma detectors, charged-particle detectors, and the information-handling electronic circuits that were developed by nuclear physicists in the course of their experiments.

Perhaps of greatest importance has been the work of nuclear physicists in assembling complex and intricate instrument systems both with and without specific computer components. Their experience has found its way into many areas, from national defense networks through the control complex of a modern industrial plant.

Ingenious devices now being readied for accurate imaging of otherwise inaccessible body organs and malignancies are interesting examples of nuclear techniques in action. These devices take advantage of radioisotopes that decay by positron emissions. The positrons slow down and annihilate close to the point of their origin, emitting two gamma rays that come off back to back in a straight line. These gamma rays are detected in sodium iodide gamma-ray detectors and, by means of a coincidence electronics developed in nuclear spectroscopy, are matched into corresponding pairs. From an array of such detectors, bound together with the necessary informational electronics and a small computer, an image is reconstructed of the internal activity. Whether as a diagnostic tool or as a guide to a surgeon's hands, their importance is clear.

Numerous other devices that are not directly used to produce or measure nuclear radiations have grown out of the designs and concepts of the nuclear laboratory. Two examples typify this second-generation adaptation: The electronic circuits that were first devised to count nuclear events were incorporated into the basic unit of digital computers—as is more fully discussed in the next section. Quite recently, the very precise methods involved in beta-ray spectroscopy have been applied to measuring the energies of electrons in chemical compounds and, in this way, to analyzing unknown chemical bonds.

There is a very close and fruitful interdependence between the nuclear-

research program and the instrument companies. Many of the ideas for new instruments and, in most cases, the early prototypes came from research physicists measuring nuclear properties. The national laboratories, in particular, have been the leading source of important instrumentation development as part of their research programs. The commercial manufacturers have provided the design engineering and production know-how that make these devices available to a wide market for routine use. Further, the nuclear-research groups, by offering a sophisticated market and testing ground for valuable innovations, foster the rapid commercial application of new concepts. These improvements then find their larger market in industry in the United States and abroad.

The scientific instrumentation industry is a key industry, providing the tools on which technological progress depends. "The scientific instrument industry represents a small part of the total manufacturing activity of an economy. Nevertheless, it is of strategic importance because of its ability continuously to produce new tools for research and automation, thus contributing in a substantial way to the increase in overall productivity."*

The nuclear-instruments section of the scientific-instrumentation industry is a rapidly growing part of the U.S. economy. In 1965, its annual sales volume (independent of accelerators) was $60 million. These annual sales are expected to reach $100 million by 1970 and $300 million by 1980. However, the larger benefit to the national economy lies not in the magnitude of the instrument industry but in the savings realized by the rest of industry through the use of nuclear techniques in providing goods and services.

Because of the pre-eminence of the U.S. nuclear-research effort, nuclear-instrumentation sales abroad are correspondingly significant; however, this situation is changing rapidly. As the European commitment to a nuclear-research program increases, the flow of instrumentation devices is becoming more of a two-way exchange between Europe and the United States. This is in itself a natural and healthy development. However, it is unfortunate that this comes at a time when, because of funding stringencies, our laboratories are cutting back their instrumentation efforts in order to preserve their research momentum. Since the long-term consequences of this situation will surely be detrimental, the steps necessary to change this downward trend are of vital concern in future funding patterns for nuclear research. This concern is discussed further in Chapters 5 and 6.

3.3.8 The Computer and Nuclear Physics

As in many other areas of society, the computer has had a profound influence on the physical sciences. At the same time, the physical sciences, and

*Gaps in Technology—Scientific Instruments (OECD, Paris, 1968).

in particular physics, have been major contributors to the influences that have shaped the development of the computer and its utilization. Nuclear physics, both in its basic and applied aspects, has been heavily involved in this interaction.

Much of the logical circuitry that forms the basic electronic structure of digital computers had already been developed by nuclear physicists and was then taken over as the actual realizations of the mathematical operations of early computer designs. Nuclear physicists had sought circuits capable of counting the randomly occurring events characteristic of nuclear phenomena. The bistable multivibrator counting circuits and electronic switching gates developed by them in the 1930's found their place during the 1940's as the basic binary units of computers. The electronics developed before World War II for timing the flight of nuclear particles found direct application as shift registers, a fundamental component of modern computers. Analogue-to-digital converters, which translate instrument outputs stated as electrical amplitudes into the digital language of the computer, were first developed as the pulse-height analyzers needed by nuclear physicists to measure the energy deposited by a particle or gamma ray in a detector.

Nuclear-physics research has also participated in less tangible but equally important ways. The need to accommodate high data rates from many simultaneous channels has forced nuclear physicists to develop, with the computer industry, a number of techniques that have subsequently found wider application. These techniques include buffers for intermediate storage and presentation of data to the computer at speeds commensurate with computer capabilities, simultaneous processing of many information channels, and techniques such as the light-pen, involving the interaction of the human operator and the computer. The use of a computer not only to analyze raw data instantly but also, acting on the basis of these analyses, to control the adjustable parameters of experiments, was powered by nuclear physicists who found increasingly that the gap between the parameters that they were actually measuring and those that were of interest to them was so wide that on-line data processing was essential for decision-making. These techniques are now common throughout industry, wherever such process controls can serve, in high technology as well as in the production of basic commodities.

Nuclear physicists, as consumers of the products of the computer industry, have been very demanding technically. The continual pressure for increased performance encouraged many innovations and improvements that are by now commonplace. The imaginative utilization of computers for nuclear-research purposes has established new patterns in a much wider sphere. The current widespread acceptance of digital computers as an integral part of many kinds of laboratory experiments and process-control sys-

tems has been more than a little influenced by their successful usage in nuclear facilities.

Many of the problems of nuclear physics, especially applied nuclear physics, demanded very large computers and sophisticated software to permit their effective utilization. The accurate calculation of the characteristics of nuclear explosions was beyond anything but the largest and fastest computers. The urgent need for such information was the primary stimulus to the early development of large digital computers and the accompanying numerical methods. These needs continue and will grow under a test ban.

At the other end of the scale, the rapidly expanding minicomputer market owes at least part of its deserved success to the pioneering use of these small processors at nuclear installations. It was the early work that demonstrated the extreme utility of even a very limited programmable memory.

3.4   NATIONAL SECURITY

The short and intense period of scientific and technical effort from the discovery of fission in 1938 to the testing of the first atomic and thermonuclear devices in 1945 and 1952, respectively, has had the most profound effect on the character of national security. Military technology and international diplomacy have been revolutionized by the advent of nuclear weapons. Further development and evaluation of nuclear weapons, on the one hand, and, on the other, the limitation and control of this weaponry are primary components of the problem of national security. While at present emphasis must be given to these weapons, eventually the controlled release of nuclear energy in fission reactors, and possibly in fusion reactors, may be of great importance to the security of the nation because of the great economic and technical benefits to be derived.

Initially, the application of the phenomenon of fission to weapons was primarily a nuclear-physics problem. Feasibility itself depended on relatively primitive determinations of such parameters as fission cross section and the number of neutrons per fission. Once confident of feasibility, the formidable problems of detailed design began. The nonnuclear design was separable from the nuclear but heavily influenced by it. As one of many examples, the nuclear physics of the spontaneous fission of the plutonium-240 impurity present in reactor-made plutonium-239, on which the plutonium bomb was to be based, dictated the actual weapon design, in turn affecting the aircraft form required to accommodate the shapes and sizes of the final product.

Today, almost all fields of physical science are involved in describing a nuclear explosion and its effects. Nuclear physics, however, remains cru-

cially important at every step from conception through manufacture, test, means of delivery, and evaluation of effect. Nuclear physics is equally relevant to the establishment and the monitoring of international agreements, the intention of which is to lessen or eliminate the chance that nuclear explosives will ever again be used in war.

3.4.1   *Nuclear Weapons Design*

Efforts to make nuclear devices more efficient, more economical to build, and suitable for specialized purposes began immediately after tests of the prototype devices. A wide range of physics, chemistry, engineering, and mathematical expertise has contributed to this effort. The central aspects are basically nuclear, and nuclear physicists have continued to play a central role in design calculations, measurement and calculation of appropriate cross sections, and test and evaluation work. All the tools and techniques developed in the nuclear-research laboratory are brought to bear in support of weapons planning and test work.

Modern design requires theoretical descriptions, incorporating the most accurate nuclear-physics data possible within current measurement capabilities. As the sophistication of design has increased, so has the sensitivity of design to the knowledge of nuclear properties. For example, difficult experiments in scattering or production cross sections can lead to improvement of a few percent in accuracy but may have a substantial effect on weapon configuration, efficiency, and effectiveness.

Should underground testing cease, even greater demands are likely to be placed on theory and laboratory experiment, with still more utilization of nuclear physics and physicists to keep up with the technology and minimize the danger of a technological surprise.

3.4.2   *Nuclear Weapons Testing*

The primary motivation for testing is to obtain performance information along with observations of weapon effects. Detailed diagnostic measurements of a physical nature and radiochemical examinations of the radioactive debris are necessary in determining the efficiency of new weapons. Most of these measurements involve the direct application of nuclear physics and nuclear techniques in their design, execution, and interpretation. Nuclear processes provide the direct connection back to the source of the measurements, and much of the instrumentation and its calibration are the work of the nuclear laboratory. One of the techniques being applied increasingly in the nuclear test program is based on the new solid-state detectors produced by the revolution in the experimental technology of nuclear

physics. Because of their very high resolution, these detectors can be used to identify the constituents of complex mixtures through their characteristic radioactive emission spectra. From the composition of the final debris material much can be learned about the processes that take place during a nuclear explosion.

Although the testing and diagnoses of nuclear weapons have been under way for a long time, some of the required nuclear physics is not yet known accurately enough. Detailed measurements of the primary fission fragmentation in general have been extremely difficult and are possible now only with the latest instrumentation. Very accurate cross-section data are still needed to make as much as possible out of test observations, our own and others. Because of the extremely high neutron fluxes generated in a nuclear explosion, knowledge of multiple neutron and charged-particle reactions is pertinent to the design of the weapon and to the evaluation of its performance. Since the intermediate nuclei live for only a very short time, these cross sections are not normally amenable to laboratory experiment. Some have been determined using bomb test fluxes. Recourse must be taken to nuclear structure and nuclear-reaction models for estimates of many cross sections of interest. The need for further accurate measurements and for development of nuclear theory illustrates again that nuclear physics, although it has already made enormous contributions, has still been only partially explored and exploited.

3.4.3   Weapons Effects and Civil Defense

In recent years, much attention has been given to the effects of weapon-emitted neutrons, x rays, and gamma rays on the environment and on many kinds of materials and systems generally concerned with the question of missile vulnerability. The problems are no longer clearly identifiable as nuclear-physics problems but are usually so closely related to nuclear physics and its instrumentation that the field benefits enormously from the flow of nuclear developments. So long as weapon testing cannot be done in whatever natural environment might be applicable, it is inevitable that the many remaining unanswered questions concerning interactions of nuclear radiations with the environment and with other weapons systems must be approached by theoretical methods and by laboratory or underground tests.

Protection against weapons effects remains an important problem. Nuclear physicists have contributed their knowledge, gained in part from the shielding requirements of nuclear facilities, to the devising of ways of shielding military structures and personnel against prompt and long-lived nuclear radiations. They have been an integral part of the efforts to deal

with the problem of shielding civilians from the same radiations, especially from fallout.

3.4.4   *Test Ban Monitoring and Surveillance*

The development and evaluation of weapons systems is one aspect of our national security. The limitations and bans placed on such devices is another. An important step has been taken in the test ban agreement, under which nuclear detonations in the atmosphere, under water, and in space have been renounced by the United States, the Soviet Union, and many other nations. Such agreements are based in part on the existence of methods and surveillance of clandestine nuclear detonations. The surveillance methods require an evolutionary growth in sophistication as the technology evolves.

Again, the nuclear-physics content of this endeavor is small but central. The scientific part of the program concerned with surveillance is planned, executed, and analyzed by those trained as nuclear physicists. While the cost of this portion of the effort is small compared with that of the total program, it must be remembered that the concern here is with a nuclear-physics requirement of other technologies. The special position of nuclear physics depends further upon the fact that much of the detection instrumentation has been developed in nuclear-physics laboratories and tested and calibrated by means of nuclear facilities.

3.4.5   *Safeguards*

By the late 1970's, power reactors around the world will be producing plutonium as a by-product of their power output at the rate of more than 200 pounds per day, sufficient in itself, for the production of a substantial fraction of the world's electric power—or for the production of tens of nuclear weapons a day. The promise of nuclear energy brings with it the need for strict control, lest diversion of fissionable material circumvent international arms limitation or reappear as bootlegged holocausts. The U.S. Safeguards program has been designed to this end.

An effective Safeguards system must detect, identify, and quantitatively measure fissionable materials in a great variety of configurations and mixtures. The analytical methods must be capable of being carried out under a wide range of conditions—e.g., in materials processing, fabrication, and recovery plants; in fuel shipping and transportation facilities; and at reactor sites. These techniques include the direct observation of natural emissions from the fissionable species. Another avenue of approach involves active interrogation of the material in question by highly penetrating beams

of neutrons or photons from external sources. Characteristic responses detected from the interrogated material could serve as quantitative identifying signatures. Many of the methods, detectors, and instrumentation devices of nuclear physics are now being developed for the solution of a difficult problem. Since physical rather than chemical techniques are required, nuclear physics and physicists are importantly involved.

3.4.6   *Arms Limitation*

Because central considerations in arms limitation negotiations rely upon nuclear physics, nuclear physicists have played important roles in these negotiations. They provide advice, information, and quantitative analyses to the negotiators of relevant international detentes and have served on U.S. delegations in such efforts. They will also provide much of the expertise in the arrangements finally devised to maintain agreements when and if they are reached.

We have already noted the Safeguards program, which can be regarded as part of an arms limitation program because it is designed to help to prevent any circumvention of limitations by clandestine diversion of fissionable material into nuclear weapons uses.

3.4.7   *Nuclear Power for Military Purposes*

National security has been served by nuclear power in ways other than weapons systems. Much of the early development of power reactors was motivated by the need to achieve a practical model suitable for submarine use. It had been quickly recognized that a reactor heat source that made no demands on scarce supplies of oxygen and had negligible fuel consumption would have a revolutionary effect on submarine performance. The first system was in action by 1954, and, in 1960, world-circling underwater journeys signaled the arrival of a new kind of underwater vessel free of range and submersion-time limitations and capable of speeds hitherto unobtainable. Today it is a major arm of the nation's defense forces. It may be noted, incidentally, that the development effort invested in the submarine nuclear reactor was an important stimulant to the production of the large, land-based nuclear-power reactor.

Reactor power sources are also applicable to naval surface vessels and to static and portable power systems for field use in remote civil defense and civil disaster situations. Energy sources powered by radioactive nuclides specially produced in long reactor irradiations are of greater and greater interest for such facilities as satellites, buoys, and weather stations. For all these systems, extensive design requirements depend on basic nu-

clear physics and on shielding and integral reactor-physics measurements and calculations.

4   The Impact of Nuclear Physics on Astrophysics and Other Sciences

4.1   NUCLEAR ASTROPHYSICS

4.1.1   *Introduction*

The nuclear physicist can claim for his discipline the study of most of the matter and energy of the universe. Reactions between atomic nuclei supply the energy of the normal stars; the nature and changes of nuclei determine the elementary composition of all matter in earth and heaven; and nuclei comprise nearly all the mass that is the source of the star-binding gravitational forces. It is no wonder that nuclear physics has led to a specialty called nuclear astrophysics, which applies the results achieved by the nuclear physicist to the domain of astronomy, including geology on the one side and cosmology on the other.

4.1.2   *The Evolution of the Stars*

Cosmology is the most encompassing discipline of all science; it tries to give a picture of the whole universe. It became a science with the advent of general relativity, and general relativity still plays an overwhelming role in the formation of the overall picture. However, the details of the picture, the inner constitution of the stars and nebulae, are preponderantly under the influence of nuclear physics, and many of the basic properties of stars— and particularly of the newly discovered varieties of stars—could not be the subject of science without our knowledge of the behavior of nuclear matter. In this way, nuclear physics supplements, and in some cases overrides, the earlier basic science that orients us on much of the constitution and properties of ordinary stars—that is, spectroscopy.

Since the rise of spectroscopy, the sequence of atomic line and color relationships in the spectra of normal stars has suggested an evolutionary, not merely a logical, ordering of stellar stages. It became clear with modern atomic physics, about the time of the old quantum theory, that the absence of helium lines in cool red stars, and their abundance in hot blue

ones, had nothing to do with intrinsic composition. The pioneers of stellar interior theory came to the understanding that deep within the stars there had to be an energy source derived from the nonchemical "burning" of ordinary matter, mainly hydrogen, at kilovolt temperatures. At the time of the first quantum understanding of the nature of radioactivity, the Gamow-Condon-Gurney theory, astrophysicists began to see what kind of energy release must be fueling the stars, for the suggestion of an adequate source from natural radioactivity from the heaviest elements was implausible. By the time of the Second World War, Bethe and von Weizsäcker had been able to apply the new knowledge of nuclear physics to try to make precise statements of the governing reactions. Now a generation of work has given us a strongly linked, if only partly tested, picture of the whole topic of stellar energy generation. It is more or less complete for long-lived and hence common states of stars but fades to uncertainty about the rarest, most explosive stellar states. The experimental nuclear work in this field is concerned with reactions at very low energies and hence low cross sections. To carry out these difficult measurements, charged-particle beams of high current and the lowest workable energies are most useful.

The difficult and specialized nature of the nuclear methods oriented toward astrophysical problems, rather than those central to the study of the nucleus, have tended to set this work off as a subfield somewhat apart from the main lines of effort of nuclear research. The Kellogg Radiation Laboratory at the California Institute of Technology has been largely given over to the questions posed by nuclear astrophysics, and other laboratories have invested parts of their program in them. As nuclear physics becomes increasingly capable of answering the more detailed questions asked by astrophysicists, this relatively small effort is growing, and the subfield of nuclear astrophysics is becoming firmly bridged to the whole of nuclear physics.

The report, *New Uses for Low-Energy Accelerators,** outlines the ways in which low-energy machines, outdistanced in the race to the forefront of nuclear research, can do pioneering work on nuclear astrophysical problems. At Orsay there is a new accelerator devoted to these problems. In related theoretical work, there is already a sizable international effort.

The earliest stages of stellar evolution are those of lowest internal temperature. How stars form out of cold galactic gas is still poorly understood. But once a star is close enough to a long-lived state to burn abundant nuclei at its center, the known thermonuclear reactions take hold and the regime of detailed study begins. Nowadays, the physics is used as input to computer calculations with realistic, not merely simplified, analytic de-

* Ad Hoc Panel on New Uses for Low-Energy Accelerators. NRC Committee on Nuclear Science, *New Uses for Low-Energy Accelerators* (National Academy of Sciences, Washington, D.C., 1968).

scriptions of the nuclear reactions and the atomic–photon interactions in the star.

Rather than outline individual reactions that produce radiation and kinetic energy deep in a star, a few of the key questions that remain not quite settled will be presented here. The most usual problem is that of a main sequence star, formed with a mass of the order of the sun and still in its first burning phase, turning hydrogen into helium. Thermal energies of reacting ions in such stars do not exceed some tens of kilovolts. The particle reactions, when they involve only low-charge nuclei—hydrogen or helium isotopes—have effective proton on proton reaction cross sections of 10^{-26} barn, helium-3 on helium-3 reaction cross sections of 10^{-12} barn. The competition among such reactions, of which (helium-3 + helium-4 → beryllium-7 + γ) versus (helium-3 + helium-3 → helium-4 + 2p) is an important example, determines the details of the overall reaction chain which both supplies energy and modifies the composition of the star. This is the driving force of stellar evolution. But the slow reaction that determines the overall rate for temperatures up to some kilovolts is the initial, neutrino-emitting reaction (p + d → d + positron + neutrino). A long chain of hypothesis and theoretical calculation connects the few solar constants with the nuclear mechanisms at work. The energetic neutrinos escape the interior and bring out direct information. One of the heroic experiments of our time, the solar thermonuclear neutrino detection scheme now being carried out far below ground in South Dakota, has for the first time tested the predictions of this elaborate chain. The detailed predictions fail, for the neutrino flux is at least a factor of 2 lower. Where the fault lies is not known. But the fact is clear. The long set of arguments somewhere hides a flaw, by a factor of 2 or more, in an important, though not dominant, part of the whole story. On the other hand, the particular reactions that produce the detectable neutrinos (beryllium-7 + proton → boron-8 → 2 helium-4 + positron + neutrino) are extremely sensitive to the central temperature of the sun; the neutrino flux varies as the twentieth power of the temperature. A change of 0.5×10^6 K from the calculated 15×10^6 K will lower the neutrino flux by a factor of 2. The question posed is, then: What can be changed in the calculation to have this large an effect? Few other external tests exist. Most of the subject consists of an increasingly sophisticated set of consistencies, where only a few observed parameters, like the temperature and radius of the stellar surface, connect the whole story with reality.

Higher thermonuclear temperatures, fitting stars of greater mass and higher luminosity than the sun, imply reactions with heavier nuclei. The most famous example is, of course, the set of cyclical reaction chains that overall involve (carbon-12 + 4p → carbon-12 + helium-4 + 2 positrons + 2 neutrinos + energy). These have very small cross sections at the tempera-

tures of stars dominated by the process. It is necessary to measure the re-
action with as much precision as possible and extrapolate to the relevant
low energy using the best phenomenological theories that take into account
both resonance and potential phenomena. When the necessity of careful
study of the individual haphazard character and position of all the relevant
resonances is added in, some feeling can be reached of how taxing is the
subject. Each individual nuclei has a unique role to play. Should any reso-
nance be moved a bit, our sun or ourselves might well not be here! The
full range of nuclear-structure theory is needed—and more. Every analogue,
mirror, symmetry, and regularity must be exploited. Semiempirical studies
are about the best that can be done as yet.

But there is more to come. In the hotter stars, helium then carbon, oxy-
gen, and neon are burned in short-lived but still not necessarily explosive
stages. But to know about these processes, the reactions of ^{12}C with ^{12}C
to form ^{24}Mg or (^{20}Ne + α) or (^{16}O + 2α) must be known at very low
energies when the nuclei have far too little energy to penetrate the repul-
sive Coulomb barrier with strength. Factors of 2 or 3 are about what we
estimate for the error today in our understanding of these important pro-
cesses for giant stars.

Beyond such reactions there is a free-for-all, a regime where the heavier
ions react in a many-branched network of reactions, each one feeding ions
or photons to many others. Here the rough concept of chemical equilib-
rium serves as a zeroth approximation, but nowadays the computers are
better than mere two-parameter thermodynamics. Here lie the secrets of
truly unstable stars, the supernovae, whose explosions—perhaps nuclear and
perhaps not—are still of uncertain origin. Again, the attractive semiempiri-
cal regularities of nuclear-reaction and nuclear-structure theory are good
guides, but guides that do not quite know all the paths. It will be an excit-
ing road to follow.

4.1.3   *Element Abundances*

So far we have addressed ourselves to energy production in the successive
stages of higher exciting temperatures during stellar evolution. But it is not
only starlight that we want to understand in detail. It is also star stuff, the
nuclide distribution of ourselves, our earth, the meteors, the sun, and stars.
For all of it is processed and given form in the context of the stars. Accord-
ing to present theories of cosmology, only hydrogen, and perhaps helium,
can predate the stars. True, the galaxy is made of technical grade hydro-
gen–helium mixture: 10 percent atomic helium, together with impurities
on the order of 1 percent of the first row elements; iron and silicon; and a
trace of all the rest. The acquisition of so much detailed knowledge of so

many samples leads us to try to account for every variation of every iso-
tope. Even those that all but vanish in any overall quantitative look—gold,
lead, deuterium, uranium—are decisive for human history. Such attempts
directly imply an interest in a variety of reactions—in summary:

1. Multiple, linked chains of charged-particle reactions at energies near
the 0.1- to 1.0-MeV range, representing the quasi-equilibrium state of hot,
dense stellar interiors. Reactants as heavy as the iron group are important.
    2. Photo-induced reactions.
    3. Neutron-induced reactions, in the same region. Here a direct sign of
such element-producing processes appears in the peaks of the abundance
curve near magic numbers. It is clear that systematics and overall surveys
are only the first steps toward unraveling the whole problem. The famous
competition between successive neutron captures and the intervening beta
decays, which is responsible for a split peak in observed abundances, re-
quires a study of both decay systematics and the level densities of nuclei.
    4. Fission processes are implicated in the case of rapid multiple ele-
ment-building captures, even to the point of recycling via capture and fis-
sion sequences.
    5. Not all events are found at high density. For example, there is a
clear sign that atmospheric processes in expanding shock waves, and pro-
cesses deep in space between cosmic-ray particles, may make decisive con-
tributions to the abundances of rare isotopes.

We have to inquire, then, into a very wide set of reactions. Usually only
the lightest nuclei are involved, but over an enormous energy range, cer-
tainly, to the meson-producing spallations of relativistic nuclei.

4.1.4 *Nuclear Matter*

Within the galactic neighborhood of our sun there now appears almost
surely to exist a number of chunks of nuclear matter, the size of large
mountains with the mass of small stars. They are held together both by the
large cohesive forces of nuclear interaction and the long-range forces of
their own gravity. They are abnormally compressed beyond normal nuclear
density, and their proton component has been squeezed into neutrons to
save the electron Fermi energy. Details are still lacking, but speculation
has not shrunk from imputing to these groups of 10^{56} nucleons structures
such as a stabilized Coulomb lattice, superfluidity, neutron ferro-magne-
tism, and superconductivity of the residual protons.
    However the details turn out, the discovery of pulsars is sure to set a
permanent challenge to the many-body theorists of nuclear matter. They

are given a wholly new domain for their theory. Their methods and parameters, adjusted to normal nuclear matter, will be severely tested. Once again there is an unexpected widening of the domain of nuclear physics.

Indeed, it must be admitted that somewhat higher densities will firmly push the normal meson clouds back into the nucleons, producing a fluid of interacting hyperons. The theory of strange particles is surely quite far from giving us a workable equation of state for that sort of matter.

The extension of these views of nucleon statics—as, in fact, of the whole picture of nuclear-reaction dynamics—into an evolutionary cosmology, is well known. On general grounds, most experts hold that long ago the big bang was an expansion of nucleons and antinucleons together with their particle ancestors. This expansion progressed from a stage of all but infinite density, through the rise of ordinary nucleon matter, into a stage of super-stellar nuclear reactions that began the galaxies with a legacy of hydrogen, helium, and not much else. So much remains uncertain that the nuclear uncertainties are not yet significant. It is interesting that the thermonuclear reactions of the lightest nuclei, first studied as long ago as three decades, dominate the early stages of element formation, pre-stars, and pre-cosmic rays. The abundance of helium has become a key test for cosmologies, since it depends so strongly upon the density—through the course of the pregalactic universe. Almost no other parameter remains from those times. The blackbody radiation has too fundamental an origin to fix details; as we now think, it serves, rather, to determine the correctness of the whole evolutionary picture. It is premature to urge more than watchful interest in this entire problem on the part of the nuclear physicists.

4.1.5 Neutrinos and Gamma Rays

Neutrinos belong to the domain of nuclear physics as well as to particle physics. The solar thermal neutrinos are already a matter of critical attention. Almost as close to the test are the neutrinos that parallel electromagnetic processes in the interaction of electrons. As an example, consider electron–positron annihilation into two gamma rays ($e^+ + e^- \rightarrow 2\gamma$). While the branch into a neutrino, ν, and antineutrino, $\bar{\nu}$, pair ($e^+ + e^- \rightarrow \nu + \bar{\nu}$) is very rare, it may be very significant. The photons remain deep in a star, but the rare neutrinos leak out carrying off energy. The course of the evolution of hot stars may well be determined by such rare and exotic energy robbery, which depends on interactions that, while easy to postulate on symmetry grounds, have never been observed. Once again, an alert view, based on electromagnetic interaction studies at nuclear energies, is implied.

Gamma rays of more normal origin, from nuclear level decay, are expected in principle. So far they have not been found, but it is still believed

that rare and perhaps unstable stars carry out nuclear reactions in plain view of the surface of the star. Perhaps every supernova remnant is a source of long-lived nuclear gamma decay. Here, too, is a direct connection between astrophysics and the normal gamma and beta spectroscopy of nuclear states. The topic has not yet had directly tested consequences.

The breadth and detail of the relations between nuclear theory and experiment and understanding the stars and the cosmos are a striking story. Its development is in the hands of a relatively small group of people devoted to this specialized field between nuclear physics and astrophysics. It is, however, a very important application of nuclear science and nuclear physicists.

4.2   NUCLEAR PHYSICS AND OTHER SCIENCES

The impact of nuclear physics and its powerful methods and concepts has been felt at the very core of a whole battery of other sciences. It is proper to summarize them briefly here and to draw attention to both the past and the potential for the future.

Rather than catalogue the sciences, and point to the nuclear contributions of each, we shall organize the effects of nuclear physics by their nature and list some of the salient consequences in a wide variety of sciences.

4.2.1   *Intrinsic Dating*

The unstable nuclei contained in any natural sample, or injected into a prepared sample, represent a form of intrinsic date, quite insensitive to the environment. Direct measures of active nuclei remaining, analysis of stable product nuclei, or counting internal atomic readjustments locally caused by decay interactions serve as clocks, given the half-life of the species concerned.

4.2.1.1   *Geology*   From the earliest days of radioactivity, it was realized that at last a dating method for geologists was at hand that did not depend upon unknowable past rates of geologic processes. By 1905, the first minerals were dated. Rutherford could display a sample and say, "I know this rock is eight hundred million years old."

Now, geochronology, using mass spectroscopy no less than counting techniques, is a fundamental part of the sciences of the earth. Uranium, radium, helium, lead, thorium, rubidium, samarium, potassium—these are by now classic isotopes and isotope chains for mineral dating. The whole domain of time, from a brief million years to the total span of five billion

years, has been studied. Improved accuracy and sensitivity, the selection of natural isotopes in proper lifetime ranges, and a greater sophistication in recognizing the chemical microenvironment and its effects—all have nourished the growth of geochronology. Though the work seems to have its own momentum and leadership, the nuclear physicist is a real resource for the geochronologist.

It seems almost unnecessary to extend the remarks to meteorites, lunar samples, newer sediments, and other specialties using special methods. Such applications are likely to grow continuously.

4.2.1.2   *Archaeology*   That special study that is man's past has a time scale for which a five- or ten-thousand-year half-life is quite suitable. The absence of any usable radioactivity of preterrestrial origin has been no barrier. The brilliant work of Libby and his colleagues made use of cosmic-ray-induced activity in carbon, that ubiquitous constituent of man's fuels, food, bones, and many of his artifacts. By now this, too, drives a burgeoning and mature science. In the last year or two arguments have emerged that suggest that certain photosynthesizers, like maize or sugar cane, are biochemical isotope-separators, compared to their general green plant cousins, and carbon samples of such an origin may give discordant dates, This is mentioned only to show the liveliness of so detailed a study, which focuses on processes, that a casual analysis would surely dismiss as unimportant. Probably not since the days of the radioactivity pioneers, who made the first scale of times for the earth, has nuclear physics passed so rich a gift to another science as radiocarbon dating.

Here, too, the work is mature, specialized, expert, and fed by the steadily improving techniques of the nuclear physicist. This good relationship can be expected to continue; it may even be that another method may arise to extend the dates beyond the few tens of millenia now almost commonplace.

4.2.2   *Tracing*

The isotope mixture, stable or unstable, of any element is little affected by processes of geological transport and only little affected by chemical processes. Radioactive or stable isotopes may be artifically added to laboratory samples, and their subsequent fate is then measurable. They are "labeled." The uses of this technique are too numerous to cite in any detail; modern chemical and biological research would be stultified without a battery of available isotopes, stable and unstable, and good detection schemes for all of them. This work is indeed the basis of a substantial industry, part government and part private.

The main elements of life—hydrogen, oxygen, nitrogen, carbon, sulfur,

phosphorus, sodium, and potassium—are all now traced with stable or unstable species. As a single example of the importance of tracers in biochemical studies, consider the experiment that proved that the single helix of the DNA molecule duplicates itself out of fresh components in order to rebuild the double helix. This experiment of a decade ago used the stable heavy isotope of nitrogen, nitrogen-15, followed by centrifugal separation. The fundamental step, detection of a nuclear isotope, is basically a nuclear experiment. At the physiological scale of "compartments" in kinetic models of living systems, tracers have risen to sophisticated use.

The chemistry of non-living matter is not lagging; there too, tracers are ubiquitous. Chemical industrial process research is worth recalling here.

Natural tracers are perhaps less well known. Yet every sample of water can be compared with standard average ocean water for its oxygen isotope ratios to parts per thousand; a systematic sequence can be built up, revealing the temperature at which water was condensed. This varies · smoothly with the latitude of the sample. Here, one suspects, much more remains to be found out; the whole subject of paleothermometry rests on such small but real differences in zero-point energy which give weak isotope separation in natural processes. The basic technique is mass spectrographic—an outgrowth of nuclear research and instrumentation. Isotope geochemistry is a booming young science.

Finally, natural and artificial (mostly bomb–induced) spiking of large natural masses of air, water, and natural gases has led to very considerable study of transport processes important to meteorology and oceanography. A knowledge of the slow mixing of the earth's atmosphere across the equator is one prime result of these measurements. Once again, the subject is young but vigorous and depends on a steady feed of nuclear instrumentation and results.

4.2.3   *Microreagents*

Under this heading is a great variety of scientific topics that depend on using nuclear properties deep within a crystal or molecule, as though one had a reagent that could be used on that tiny scale.

For instance, the recoil of a nuclear decay has long been used—the Szilard-Chalmers reaction—to separate out an isomeric state. The same idea, much enlarged and extended, gave rise to a whole new chemical kinetics of recoil fragments and the appearance of molecular radicals; this is sometimes called "hot" chemistry. In crystals, too, the method is useful, and the special anisotropies of travel, sometimes called "channeling," of high-velocity charged particles have been used both to study crystals and as a tool for nuclear physics itself.

The delicate interactions between nuclear electromagnetic moments

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and residual electric and magnetic fields in molecules and ions are, of course, a well-known part of fundamental chemistry. As the basis for nuclear magnetic resonance phenomena and their applications, it has been put to enormous use in solid-state physics and chemistry as well. The rotation of nuclei by the electric and magnetic fields affects the angular pattern of nuclear radiations, which provides still another nuclear technique for the microscopic study of solid-state physics.

The unique sharpness of particular, long-lived nuclear states is, of course, the fundamental reason for the Mössbauer effect and its applications for nuclear physics. It serves even more importantly in a constantly increasing number of ways as a rapid sensor of the atomic environment of a particular nuclear species. The present overwhelming emphasis of the hundreds of scientists who use the technique is on problems in solid-state physics, such as magnetic properties, critical phenomena, and valence states of atoms.

It is plain that nuclear results will always tend to suggest means of turning them logically inside out. What starts as a nuclear physicist's device for studying nuclei by a special interaction can be turned into a way of studying the external molecule for the purposes of chemistry. Measurements of nuclear interactions with their surrounding electrons sample the electron population at the nucleus and thereby the way the electrons dispose themselves. The nuclear instrumentation designed for electron spectroscopy has recently been put to use to analyze the bonding between atoms; the precise energy of the binding of electrons in these atoms measures in a direct way their bonding properties. A few years ago a nuclear group showed how to make such accurate measurements routinely thereby opening a new technique now used in a large number of chemistry and solid-state physics laboratories. Other nuclear techniques are also in use. The study of the radiation resulting from positron annihilation has recently become an important method for investigating the momentum distribution of electrons in solids.

Microanalysis itself is a powerful quasi-nuclear field of study. Consider the well-known neutron activation techniques that detect elements by first activating their instability through a neutron-induced reaction. Lately attention has been called to the analysis of samples by studying the elastic scattering of nuclear projectiles, accurately measuring the significant small changes of the energy of the recoil alpha or proton, in the laboratory frame. This was the basis of the clever device put on the moon surface in Surveyor missions: elementary analysis of chemical composition by nuclear and Coulomb scattering.

The detection of recoil decay fragments from fission and alpha emission by observing their etched tracks in insulators should be mentioned here.

Their uses as dating methods, as measures of particle flux, as analytical schemes, even as microdrills, are far from fully known. There is no better example of unexpected remote consequences of careful nuclear research than that shown by the growth of the etched track technique. We do not know what parallel case will be next.

The use of x rays for establishing the structure of molecules and crystals has been built into the very fabric of our modern view of matter. The use of neutrons and neutron diffractive study is central to solid-state physics, because the wavelength and the momentum and energy of neutrons are so well matched to the dimensions and excitation energies of condensed matter. Magnetic interactions between neutron and nucleus provide the crucial instrument in observing microscopic magnetic structure of solids. At all research reactors, both polarized and unpolarized neutron beams are shared between solid-state and nuclear applications, with the preponderance in solid-state research.

4.3 THE IMPACT OF NUCLEAR PHYSICS ON SPACE SCIENCE

4.3.1 *Introduction*

Illustrative of the broad impact of nuclear physics and its powerful techniques and concepts on other sciences is that made on space science. Since science and technology are so intimately connected in the space program, the impact that nuclear physics and nuclear physicists have made on both aspects of the program is sketched below.

Although the experimental and theoretical investigations of cosmic rays, solar activity, and aurora and airglow have been made for decades, the explosive growth of space science began with the availability of rockets and satellites for *in situ* measurements. The first geophysical rockets were launched in the late 1940's, and systematic rocket launches were made in the early and late 1950's. Artificial satellites were first used in 1958, and in the next five years, space activity increased rapidly in volume and sophistication. Thus, from a small semi-academic activity in 1955, the space program grew to a national effort with annual expenditures by the National Aeronautics and Space Administration of about $6 billion in 1965. Additional large sums are spent by the Department of Defense, industries, and universities.

Even before the use of rockets and satellites there had been a close connection between some of the subject matter of nuclear physics and space physics. Cosmic rays were recognized as the realm of both disciplines and were studied by groups interested both in the geophysical and astronomical aspects of cosmic rays as well as the nuclear information derivable from

high-energy nuclear collisions. The discovery of the radiation belts in 1958 clearly defined the space environment as a nuclear one and provided many problems that were tractable by available nuclear techniques. Nuclear reactors and isotope power sources are uniquely fitted for space applications, although their potential has not yet been fully exploited.

In accomplishing the national space program, nuclear physics and nuclear physicists have contributed in three essential ways: technology developed for nuclear-physics research has been applied almost without revision to solve space problems; scientific information derived in nuclear research has been important in clarifying the physical processes observed in space experiments; and scientists trained in nuclear physics have played a key role in manning the space effort. These contributions will be treated in more detail in the following sections.

4.3.2   *Applications of Nuclear Technology to the Space Program*

4.3.2.1   *Instrumentation* Perhaps the most direct application of nuclear-physics technology to space exploration has been the widespread use of nuclear instrumentation in space probes. Following the initial experiments, which showed that the earth's magnetosphere contained high-energy electrons, protons, and alpha particles, the measurement of these fluxes has progressed with a series of detectors, all developed originally for nuclear programs. The first measurements were made with Geiger-Müller counters, primarily because of the reliability of these detectors and the simplicity of the required electronics. Early measurements of protons from recoverable vehicles also were made with standard nuclear emulsions. In the years immediately following 1959, magnetic spectrometers were used, and scintillation counters became common in about 1962. Also in 1962, charged-particle spectrometers using solid-state detectors gained widespread application. Among the other useful techniques developed originally for nuclear physics are magnetic and electrostatic analysis of energetic particles, simultaneous measurement of dE/dx and total energy for particle identification, and various coincidence and anticoincidence shields for eliminating background. The electronic circuitry for these detectors has also been borrowed from nuclear physics, including multi-channel analyzers, linear amplifiers, and scalers.

The measurement of the space environment has also employed ground calibration of detectors using accelerators and radioactive sources, and many space instruments carry in-flight calibration sources consisting of small quantities of radioactive material.

4.3.2.2   *Spacecraft Survival in Nuclear Environment* Engineers and biologists have borrowed heavily from nuclear-physics data in designing space-

craft that will survive in the hostile space environment. Radiation-damage studies of sensitive components, such as solar cells and semiconductors, have used ground-based accelerators for testing and simulation. The extensive biological studies of the effects of radiation on living organisms formed a base for studies of astronaut protection. Nuclear data on the shielding of radiation has found extensive application in the engineering design of satellites.

4.3.2.3 *Nuclear Power and Propulsion*   The remote operation of instrumentation in space and on the moon and planets offers a unique opportunity for the use of reactor and isotope power sources. Instrumentation left on the moon by Apollos 11 and 12 is powered by isotope sources, and reactor power sources have been carried on satellites in development tests. These power sources will achieve their ultimate value on trips to the distant planets where the solar radiation is too small for conventional solar power supplies.

Nuclear rocket propulsion may be a practical means of lifting extremely large payloads, and the development of these engines is progressing satisfactorily. To date no nuclear-powered rocket engine has been used in space; however, for manned exploration of the distant planets, such a propulsion source may be necessary.

4.3.3   *Influence of Nuclear Physics on Space Science*

The scientific goal of the space program is to define the phenomena occurring in space and to understand the physical processes taking place. Inasmuch as many of the processes involve steps in which nuclear reactions take place, nuclear-physics information has been used extensively in the analysis and interpretation of space experiments. (The impact of nuclear physics on astrophysics was treated in Section 4.1.)

4.3.3.1   *Cosmic Rays*   Cosmic-ray studies have relied heavily on nuclear-physics instrumentation, and the interpretation of the measurements has utilized fundamental nuclear-physics data. The measurements made within the earth's atmosphere require an understanding of the mechanisms by which energy from a primary cosmic ray is propagated into the atmosphere by multiple collisions. This cascade phenomenon is understood in general, but the lack of detailed cross sections for the collision of 100-GeV protons and neutrons on nitrogen and oxygen still limits the usefulness of ground-based cosmic-ray studies.

Measurements of cosmic-ray primaries in space have recently achieved high accuracy in observing the population of various elements up to $Z = 28$. The observed abundances, when combined with nuclear data for

specific reactions, have led to information on the origin of the primaries, the possible acceleration mechanisms, the average lifetime of the cosmic rays, and the average density of material in the galaxy.

On a more restricted scale, the measurements of the relative abundances of various nuclides emitted by the sun, their time dependence, and their energy dependence lead to improved knowledge of the mechanisms of solar flares and of the interplanetary medium through which they propagate.

4.3.3.2 *Trapped Radiation Belts and Auroras* One of the principal features of the earth's magnetosphere is the presence of large fluxes of geomagnetically trapped particles. While the origin of these particles is not universally agreed upon, some of the principal theories for their origin and loss are based on nuclear processes. The most tenable theory for the origin of the high-energy protons trapped in the radiation belt holds that these protons result from the decay of energetic neutrons, which are produced by collisions of cosmic rays with nuclei in the earth's atmosphere. The origin of the low-energy protons and the electrons is more controversial. However, all particles trapped at low altitude (less than 1000 km) are strongly affected by energy-loss collisions with the earth's atmosphere, and range-energy data as well as scattering cross sections have been used in evaluating the loss rates.

The interaction of energetic particles with the atmosphere has also been used in studying the polar aurora, which is produced by the impact of electrons and protons on the atmosphere. Range-energy data originated for nuclear-physics purposes have been used extensively in studying this natural phenomenon.

4.3.3.3 *Nuclear Dating* Nuclear-dating techniques and activation methods for identifying trace elements have found wide application in studies of meteorites and, more recently, in analysis of lunar material recovered by Apollo flights. The ages of these materials can be measured by the relative abundances of nuclides in decay chains. Similarly, an analysis of the reaction products in meteorites indicates the history of their exposure to cosmic rays and indirectly measures the cosmic-ray flux during previous ages. Theories for the origin of the moon, the asteroid belts, and meteorites can be checked and revised by nuclear measurements of the ages, induced radioactivity, and nuclide abundances.

5 The Facilities and Instrumentation of Nuclear Physics

5.1 EXISTING ACCELERATOR AND REACTOR FACILITIES

5.1.1 Trends in the Nuclear Facilities Complex

5.1.1.1 *Introduction* As the capacity grew to initiate nuclear interactions with energetic particles and to detect and measure the nuclear results of the interactions, so nuclear physics grew. These capabilities have been enormously enlarged by the revolution in nuclear technology of this last decade, but it is a revolution that is by no means over. In this chapter are sketched the great events of the past that have led to our present capability and those developments now on the horizons that promise the means to attack the new nuclear problems. This chapter begins, following the sequence of a nuclear experiment, with a discussion of the accelerators that produce energetic particles and concludes with a description of the status of detectors and instrumentation.

5.1.1.2 *Accelerators—1930–1945* The first accelerators capable of producing energetic particles for the study of nuclear physics came into being in the 1930's. At once, they vastly increased the limited means that the nuclear physicist had had when only the natural alpha-particle sources were available to provide small bombarding intensities at a few million electron volts of energy. The Cockcroft-Walton voltage multiplier, the cyclotron, and the Van de Graaff electrostatic machines had their birth early in this decade and were eagerly put to use providing the means for the exciting evolvement of large-scale experimental nuclear science. By the end of the 1930's World War II has engulfed Europe and would soon involve the United States, but already some 20 of the leading universities had acquired these accelerators.

By today's standards the accelerators and instrumentation of the period were rudimentary, but they provided the means to establish the broad features of nuclear structure and nuclear dynamics. And the capabilities were rapidly expanded so that many kinds of projectile could be accelerated. The most common accelerated particles were protons, deuterons, and occasionally helium nuclei. The betatron, invented at the end of the 1930's, proved to be an efficient accelerator of electrons, which, converted into intense beams of high-energy photons, opened the way for an exploration of photonuclear reactions.

During World War II, physicists were occupied with the many projects vital to the nation's defense. Out of some of this work came the nuclear

reactor, whose importance on the national scene need not be again empha-
sized in this chapter. It had enormous impact on physics itself. A host of
new radionuclides could be manufactured and studied. In the near future
were to come the research reactors with intense external beams of neu-
trons for physics experimentation.

5.1.1.3 *Accelerators–1946–1960*   With the war's end, the way was
opened for the burst of development that was to extend to the present. A
flood of new accelerator concepts, matched by instrumentation advances,
enormously expanded the possibilities for probing the nucleus. With strong
federal support—stemming from the newly awakened interest in the po-
tentialities of nuclear science—these new possibilities were quickly trans-
lated into capabilities.

The accelerator developments can be characterized by the needs of the
nuclear investigations that they were designed to meet: higher energies,
higher intensities, the ability to accelerate numerous kinds of nuclear pro-
jectiles, and great precision and control for the fine scrutiny needed to ex-
amine nuclear excitations. Not all of these properties were available in any
one kind of machine. Each tended to open complementary areas of nu-
clear research.

The cyclotrons led the way into higher energies. The classical cyclotron
could reach energies of several tens of millions of electron volts, an energy
sufficient to allow nuclear projectiles to penetrate the Coulomb repulsion
of even the heaviest elements. Over a dozen such accelerators were built
to form the basis of nuclear laboratories.

However, the relativistic dynamics of fast-moving particles appeared to
be an intrinsic barrier to the extension of the ability of such machines to
accelerate particles to higher energies. This barrier was circumvented by
the invention of the synchrocyclotron by McMillan in the United States
and independently by Veksler in the Soviet Union; the basic idea is that
the frequencies of the oscillating electric accelerating fields are tuned to
match the relativistic increase of mass of the energized particles. With these
synchrocyclotrons, protons could be accelerated to energies of over 700
MeV, enough to break into the world of subnuclear phenomena with the
laboratory production of π-mesons. The study of elementary particles,
which these machines opened up, has, of course, expanded in its own nat-
ural directions; it is worth noting, however, that the achievement of the
higher energies needed was part of a larger effort of nuclear physicists
pushing back a frontier. The energetic beams from these machines were of
limited quality but were directly suitable for nuclear experiments that re-
quired only coarse resolution or made use of the secondary mesons.

In this period, another important accelerator concept, the Alvarez lin-

ear accelerator, was successfully developed. This development, started before 1946 (at the University of California, Berkeley) was culminated in a unique 32-MeV proton accelerator at Berkeley, which supported important nuclear research for many years. Machines that are outgrowths of this early development are now in the forefront of modern nuclear physics as will be discussed later.

The development of beams with the fine energy resolution needed to explore the fine structure of nuclear spectra lay with the Van de Graaff machines. Since the energies of these accelerators were low—up from the earliest models capable of 1 MeV or less to 7 MeV of energy—only the lighter nuclear species were open to this exploratory tool. However, the fine resolution already noted, together with the early choice of accelerated particles and the simplicity of their operation made them superb instruments for the investigation of detailed features of nuclei. A private company, the High Voltage Engineering Corporation, developed these machines for the nuclear laboratories. Some 20 were installed in the period after the war and before 1960, as very important components of the nuclear capability whose discoveries have been outlined in earlier chapters. The steady progress on increasing the limited energy range of these machines has been a continuous and successful effort, until in the 1960's—jumping ahead of the story—the whole periodic system was open to this fine resolution exploration but with an as yet limited number of kinds of projectiles.

Following the early development of the betatron for the acceleration of electrons, two additional kinds of machine were developed, the electron synchrotron and the electron linear accelerator or linac. The electron synchrotron was particularly useful for high-energy photonuclear work. However, from neither the betatron nor the electron synchrotron was it easy to extract an external beam that could be used directly in laboratory experiments. The linac provided a good solution in making intense external beams conveniently available. Important investigations of the distribution of charge and mass within the nucleus became possible; especially important were the high-energy, short-wavelength beams available from the Stanford University linac.

The nuclear-physics accelerators of the 1950's were rather modest in cost and were well suited in size and operability for full and vigorous use by small clusters of scientists. These accelerator characteristics when taken together with the multifaceted nature of nuclear research investigations made it natural for a dispersed network of such nuclear facilities to grow up in a highly decentralized manner.

The few large research reactors, used in part by the nuclear program, are, as will be seen, in the nature of big-scale facilities and have been national laboratory responsibilities. The overall picture of nuclear-research

activities is one of many programs each, for the most part, based on its own accelerator or reactor facility. In most cases a single such machine constituted a laboratory capability. Larger programs were built by putting together a number of kinds of complementary nuclear facilities. The tables shown later in this chapter present the details of this diffuse facilities network that has proved so successful for the nuclear program.

5.1.1.4 *Accelerators—1960–1970* During the 1960's the nuclear facilities followed this same set of general trends in meeting the needs of the nuclear program.

The lower-energy, fine-scrutiny work has been carried forward by the Van de Graaffs. Their energy range has been markedly expanded by the application of the "tandem" principle—first discovered by Bennett in 1938 but neglected and unused until it was rediscovered by Alvarez in 1948. The tandem machines take double advantage of a given electrical potential by changing the charge of the ion as it passes so that it is accelerated both coming and going. This energy-doubling principle when combined with the steadily increasing terminal voltages provided enough energy for protons, for example, to penetrate the Coulomb repulsion of even the heaviest nuclear targets. The variety of nuclear projectiles that could be accelerated to penetrating energies also expanded. The nuclear capability afforded by these machines was so great that over 20 nuclear laboratories based their programs on them. Three examples of such facilities are shown in Figures II.4–II.6. The lowest end of the scale, however, continued to hold problems of interest, and a comparable number of smaller Van de Graaffs were required for these programs. The range of the electrostatic accelerators in energy and particle choice appears to be still growing as we swing into the 1970's.

While the electrostatic machines were being developed in the lower-energy range to meet the needs of high resolution combined with great flexibility in energy or accelerated species, a new kind of cyclotron was meeting the complementary needs for higher energies with better energy resolution and improved beam characteristics than were available in the synchrocyclotrons. The isochronous cyclotron, first proposed by Thomas in 1938, but not realized until the late 1950's, overcame the problems with cunningly shaped magnetic fields. These isochronous cyclotrons provide tightly concentrated beams of particles accelerated to intermediate energies—protons, say, of 50 to 200 MeV—that are continuously variable. Nearly 20 such accelerators were constructed and integrated into nuclear programs during the 1960's.

The successful development of electron linear accelerators and their application in nuclear physics to the study of form factors, neutron cross
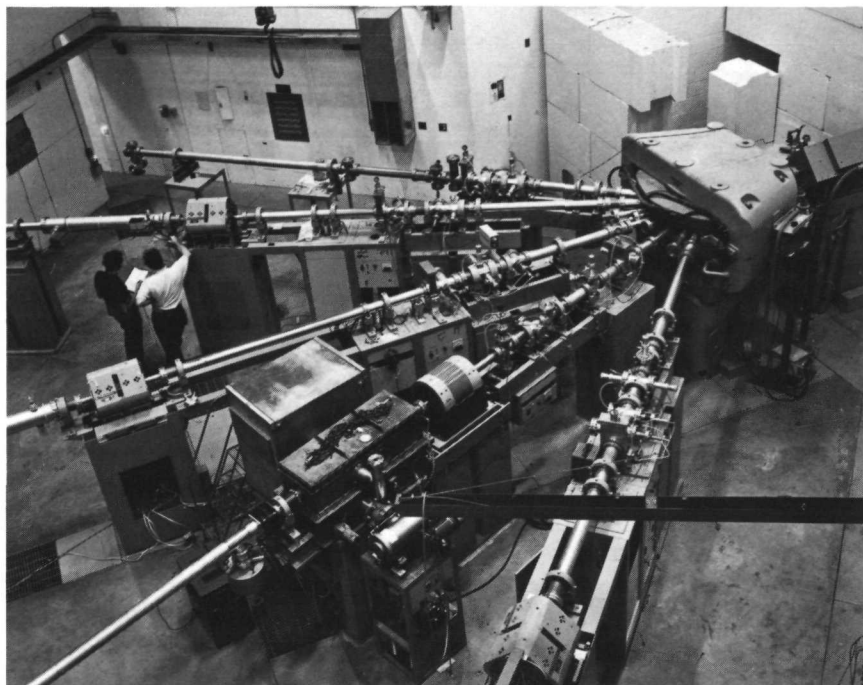
FIGURE II.4   Shown here is the switching magnet and the beam lines from the MP Tandem Van de Graaff accelerator in the Nuclear Structure Laboratory at the University of Rochester. Special features are a particle–gamma facility and an Enge split pole spectrometer. Also provided are a 21-in. scattering chamber and a neutron time-of-flight facility.

sections, and photonuclear reactions led to the need for machines with higher performance characteristics. This was met by a number of technological improvements that are described later when linac technology is reviewed. In response to the nuclear-physics potential of these accelerators, about a dozen new machines were constructed during this decade. The availability of higher-energy electron beams made it possible to study nuclear form factors with a shorter-wavelength probe and thus with more meaningful results. The applications to neutron studies will be discussed later.

5.1.2 *Distribution of Facilities*

Tables II.3 through II.7 and Figure II.7 present the nuclear facilities, their capabilities, costs, distribution across the country, and history in capsule form—as they appeared in 1969.
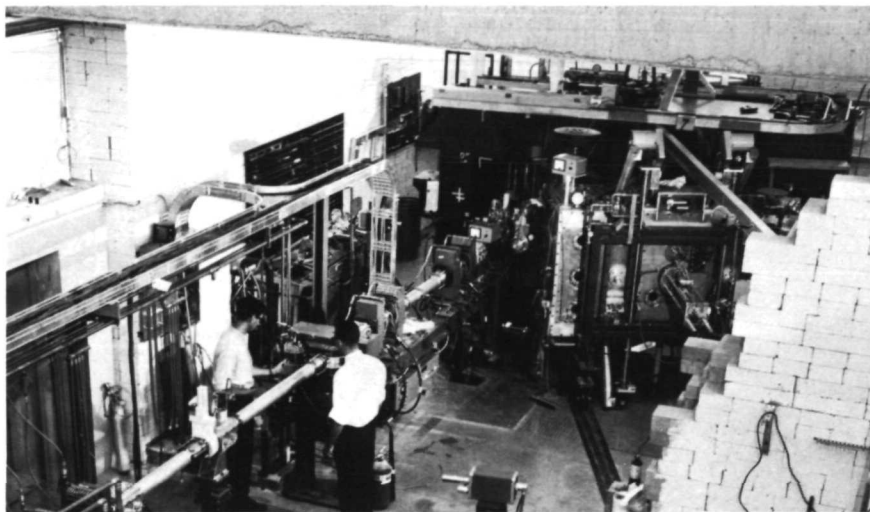
FIGURE II.5   Photograph of the 50-MeV variable energy cyclotron at Michigan State University. The beam line shown is for the unanalyzed beam with associate focusing magnets and beam viewer.

FIGURE II.6   The first of the Emperor class of electrostatic Van de Graaff accelerators, installed in the Arthur Williams Wright Nuclear Structure Laboratory at Yale University. This accelerator is the largest of the tandem configurations in research use and is the first to make available all the nuclear species to precision study. Terminal potentions in excess of 11 MeV have been obtained on the machine.

TABLE II.3   Accelerators and Reactors Identified with Basic Nuclear-Physics Research Programs—1969

Note: Under Positive-Ion Accelerators[a], the Potential-Drop Machines columns "Single → Tandem", "Tandem → Tandem", and "Cyclotron → Tandem" are grouped under Three-Stage[d].

Laboratories with	Single → Tandem	Tandem → Tandem	Cyclotron → Tandem	Two-Stage Tandem	Single-Stage ≥ 5 MeV	Single-Stage < 5 MeV	Synchrocyclotron (FM)	Isochronous Cyclotron (AVF)	Lawrence Cyclotron (FF)	Proton Linac	Heavy-Ion Linac[e]	Electron Linac > 150 MeV	Electron Linac < 150 MeV	Betatron	Synchrotron	Research Reactors
Positive-Ion Accelerators[a]	Potential-Drop Machines						Cyclotrons			Linacs		Electron Accelerators[b]				Reactors[c]
Large Multiprograms																
Argonne N L				18		4			23d				22			5
Brookhaven N L		(30)		(20)		4		36								40
U Cal., Berkeley							730	60			10/u					
U Cal., Livermore			(27)	(12)		0.5		15H⁻	14				100; 35			3
Columbia U					6		390 (550)									
Los Alamos S L	23			15	8	3			14d	(800)			30			8
Mass. Inst. of Tech.					8				16d[f]			(400)				
Naval Res. Lab.					6			50					65			1
Oak Ridge N L				13	6	3		66					140			30
Stanford U				19		3						1200; (2000)				
Yale U				22							10/u		75			
Intermediate Programs																
Cal. Inst. Tech																
U Cal, Davis				10		1;3		55								
U Cal, Los Angeles								50H⁻								
Carnegie-Mellon U							440									

273

TABLE II.3 (Continued)

Grouping of columns: **Positive-Ion Accelerators[a]** — Potential-Drop Machines (Three-Stage[d]: Single ← Tandem, Tandem ← Tandem, Cyclotron ← Tandem; Two-Stage Tandem; Single-Stage $\geq$ 5 MeV; Single-Stage $<$ 5 MeV), Cyclotrons (Synchrocyclotron (FM), Isochronous Cyclotron (AVF), Lawrence Cyclotron (FF)), Linacs (Proton Linac, Heavy-Ion Linac[e]); **Electron Accelerators[b]** (Electron Linac $>$ 150 MeV, Electron Linac $<$ 150 MeV, Betatron, Synchrotron); **Reactors[c]** (Research Reactors).

Laboratories with	Single ← Tandem	Tandem ← Tandem	Cyclotron ← Tandem	Two-Stage Tandem	Single-Stage $\geq$ 5 MeV	Single-Stage $<$ 5 MeV	Synchrocyclotron (FM)	Isochronous Cyclotron (AVF)	Lawrence Cyclotron (FF)	Proton Linac	Heavy-Ion Linac[e]	Electron Linac $>$ 150 MeV	Electron Linac $<$ 150 MeV	Betatron	Synchrotron	Research Reactors
U Chicago			32				460						50			
U Colorado								28								
Duke U				17 (18)				15 H^-								
Florida State U						3,4										
Idaho Nuclear Corp.																40
U Illinois													(30)	25		
U Indiana								(200)								
Iowa State U															70; 70	5
U Maryland					4			100 (140)								
U Michigan								35	9d							
Michigan State U								55								
U Minnesota				20												
NASA–Lewis									21d							
NASA–SREL							600									
Nat Bur. of Stand.					3	2f							10			
U Notre Dame				15	4											
U Pennsylvania				12									150			10
U Pittsburgh																
Princeton U	18							56								

274

Table (rotated 90°). Column headers do not appear on this page; values are aligned to each institution by position.

Institution									
Rice U		12	6						
U Rochester		20							
Rugers U		18							
U Texas	18	12		4					<1
Texas A and M	25								
U Washington		18		2	60				
U Wisconsin		13				22d			
Small Programs									
Aerospace Res. Lab.									
U Arizona		8	6	2					
Army Nuc. Eff. Lab.		15							
Bartol Res. Foundation									
Case Western R U			6	3					
U Iowa			6	2					
Johns Hopkins U				3					
Kansas State U		12							
U Kansas									
U Kentucky			6	4					
Lockheed, Sunnyvale				3					
Naval Postgrad.									
SUNY, Stony Brook		17							
Ohio State U				0.2					
U Oregon			6	4.5					
Purdue U		15							
Pennsylvania State U							100		
Rensselaer Poly I			6				100		
U Virginia			6						
Washington U			6		13				
Smallest Programs									
Brigham Young U				3; (4)					
Brown U				0.4					
Carnegie Inst.				4				12	
U Cincinnati									
Concordia College				0.4	8				
U Connecticut									
Cornell U									
U Florida				4					<1
U Georgia			(5)						

G

TABLE II.3 (Continued)

Laboratories with	Positive-Ion Accelerators[a]											Electron Accelerators[b]				Reactors[c]
	Potential-Drop Machines						Cyclotrons			Linacs						
	Three-Stage[d]			Two-Stage Tandem	Single-Stage ≥ 5 MeV	Single-Stage < 5 MeV	Synchrocyclotron (FM)	Isochronous Cyclotron (AVF)	Lawrence Cyclotron (FF)	Proton Linac	Heavy-Ion Linac[e]	Electron Linac > 150 MeV	Electron Linac ≤ 150 MeV	Betatron	Synchotron	Research Reactors
	Single → Tandem	Tandem → Tandem	Cyclotron → Tandem													
Georgetown U						0.4; 2										
Gulf Gen. Atomics													100			
Lowell T I					6											
W. Michigan U				12												
Montana S U						(3.5)										
U Montana						(4)							7			
SUNY, Alabama																
Ohio U				(11)												
U Oklahoma															70	
Oregon State U								18								
St. Louis U									4d							
U South Carolina						0.5										
Tulane U						3										
Texas Nuclear						3										
Virginia Poly. I						4										
Washington State U						2										
West Virginia U						2										
Worcester Poly. I						2										
U Wyoming						0.1										

TABLE II.3 ACCELERATORS AND REACTORS IDENTIFIED WITH BASIC NUCLEAR-PHYSICS RESEARCH PROGRAMS–1969 This table provides an analysis of the types of accelerators and reactors being used by 89 different institutions engaged in basic nuclear-physics investigations during 1969. In this table, the distribution among the laboratories of the 15 principal types of accelerator is shown in separate columns, as is the distribution of research reactors in the last column. A more extensive review of the properties of the accelerators is developed in Table II.4.

The analysis is divided into four categories determined by the total size of the nuclear-physics program at each institution. Large institutions that have many facilities and study many phases of nuclear physics concurrently are listed as "large multiprogram" laboratories. These institutions generally had operating and research budgets above $2 million per year. A second category is designated "intermediate programs." These institutions have broad and intensive nuclear-research programs often involving more than one accelerator but generally less comprehensive or smaller than the first category. Their annual operating and research costs were between $0.5 million and $2 million per year. The third category is labeled "small" programs. These programs generally are accomplished by a small team of investigators or research scientists. Very important work is often done by these programs, but in general their scope is considerably more limited than the first two categories. The operating and research costs for this category were between $150,000 and $500,000 per year. The fourth category is labeled "smallest" programs. These programs are generally accomplished with a single staff member or at most a few staff members and graduate students. In most cases a single accelerator is used in their research with the costs for research and operation generally below $150,000 per year.

These programs of various sizes have grown in response to the many ways of doing nuclear physics and to the great diversity of research interests. A nuclear laboratory is in general centered around an accelerator; the character of the laboratory depends, however, not only on this accelerator capability but also on the complementary instrumentation and analysis equipment. As one goes from the smallest to the largest laboratories, one goes from specialized programs based on small accelerators of delimited capabilities to wider and varied programs utilizing more powerful machines. The diversity of programs mirrors the diversity of opportunities in nuclear research.

The multiprogram laboratories have a variety of nuclear-research facilities and pursue research in a large number of directions corresponding to the broad and diverse nature of nuclear research. The various programs tend to interact and complement one another using, as needed, diverse beams, complex magnetic analysis equipment, large computers, and other specialized devices. These laboratories are easily able to undertake the operation of the large accelerators and reactors that require specialized crews and techniques.

In contrast the "small" and "smallest" programs may have one small accelerator, which is used for research by one or a few staff members. Each such laboratory in general is different, excelling in some special line of investigation and filling in some important piece of the nuclear pattern.

The large number of accelerators and reactors, 148 accelerators and 11 reactors, has served the many facets of the nuclear program. There is no more duplication of research programs than is needed for the competitive and checking processes that keep an experimental science at its peak performance.

TABLE II.4   Types of Accelerators in Basic Nuclear-Physics Research in 1969

	Energy Range (MeV)[a]	Cost Range[b] ($Millions)	Number of Given Type	Accelerators Placed In Operation				Distribution by Laboratory Type		
				Before 1950	1950–1959	1960–1964	1965–1969[c]	Government	University	Industry
Proton and Heavy Ion Accelerators										
High Voltage Machines										
Tandem										
Three Stage										
Single Stage Injectors	18-23	2.5–8.0	2	0	0	2	0	1	1	0
Double Tandems	18-30	3.5–12	3	0	0	0	3	1	2	0
Cyclotron Injectors	27-32	2–4	2	0	0	0	2	1	1	0
Two Stage	8-22	1.3–6.0	27[j]	0	0	8	19	7	20	0
Single Stage	≥5	0.4–1.5	15[j]	0	7	4	4	3	11	1
	<5	0.01–0.75	42	3	12	13	13	8	32	2
Cyclotrons										
Synchrocyclotrons, FM	≤730	1.5–14.3	5	1	3	0	1	2	3	0
Isochronous Cyclotrons, AVF	≤200	0.36–9.7	18[j]	0	0	5	13	5	13	0
Lawrence Cyclotrons, FF	≤23d	0.6–2	8	2	5	0	1	4	4	0
Linear Accelerators										
Proton Linacs	800	55	1	0	0	0	1	1	0	0
Heavy Ion Linacs	10/u	1.7	2	0	2	0	0	1	1	0
Electron Accelerators										
Linear Accelerators	>150	4–6	3	0	0	1	2	0	3	0
	≤150	0.3–8	15	0	4	3	8	9	5	1
Betatrons	12-25	0.05	2	1	0	0	1	0	2	0
Synchrotrons	70	0.06–0.6	3	1	1	0	1	0	3	0

[a] *Energy Range:* All energies are nominal proton energies, unless otherwise specified.
[b] *Cost Range:* These are generally facility costs.
[c] Includes 13 accelerators under construction.
[d] *Flexibility:* Simplicity and speed of change of projectile p, d, ^{3}He, ^{4}He, etc. Simplicity and speed of change of particle energy in small steps. Capability for change in beam microstructure. Capability for multiple external beams. *Excellent,* has most of the above; *Good,* has much of the above, but excessive time required; *Fair,* has some of the above; *Poor,* has little of the above.
[e] *Operational Demands: Simple,* has a few easily understood adjustments. Generally reliable with infrequent

Large Multiprogram	Intermediate	Small	Smallest	Flexibility[d]	Operational Demands[e]	Light Ions: p, d, ³He, α	Heavy Ions: Lithium and Heavier[f]	Typical Maximum Average External Beam Intensity[g]	Beam Quality[h]	Beam Energy Definition[i]
1	1	0	0	excel.	some complexity	yes	yes	med.	excel.	excel.
1	2	0	0	excel.	some complexity	yes	yes	med.	excel.	excel.
1	1	0	0	fair	some complexity	H⁻, D⁻ only	no	med.	excel.	good
7	13	5	2	excel.	some complexity	yes	yes	med.	excel.	excel.
5	1	7	2	excel.	simple	yes	yes	high	excel.	excel.
6	10	8	18	excel.	simple	yes	yes	very high	excel.	excel.
3	0	1	0	poor	moderate	usually p only	no	low[k]	poor	poor
5	10	1	2	good	generally complex	yes	yes	high	good-excel.	good-fair
4	3	0	1	poor	some complexity	rarely ³He	rarely	high	poor	poor
1	0	0	0	excel.	generally complex	p, H⁻	no	very high	good	good-fair
2	0	0	0	fair	generally complex	yes-10 MeV/u	yes	med.	fair	fair-poor
3	0	0	0	good-excel.	generally complex	not applicable	n.a.	high	good-excel.	fair-good
7	4	2	2	good-excel.	some complexity	n.a.	n.a.	high	good-excel.	fair-good
0	1	0	1	poor	simple	n.a.	n.a.	low	good-excel.	good-excel.
0	2	0	1	fair	some complexity	n.a.	n.a.	low		

maintenance. No staff required. *Moderate,* has several controls and systems. Brief operating training required. Small staff helpful. *Some Complexity,* More complex than *moderate,* with interacting controls and systems. Significant training required. Small staff necessary. *Generally Complex,* Has many interacting controls and complex electronics and mechanical systems. Generally requires substantial operating training. Staff required.

ƒ *Heavy Ions—Lithium or Heavier:* Many of these accelerators can accelerate heavy ions but to only very limited energies or with low intensities, especially for high-mass particles.

g *Typical Maximum Average External Beam Intensity: Very High,* ~1000 μA; *High,* ~100 μA; *Medium,* ~10 μA; *Low,* ~1 μA.

h Beam Quality: characterized by the degree to which the raw beam can be focused to a small spot with small angular divergence. Can be improved with loss of intensity.

i Beam Energy Definition: Excellent, less than 1 part in 2000; *Good,* 1 part in 1000; *Fair,* 1 part in 200; *Poor,* greater than 1 part in 100.

j This number includes machines that may also operate as part of a three-stage tandem installation.

k Can be used to provide very intense beam bursts for neutron time-of-flight studies.

TABLE II.4   TYPES OF ACCELERATOR IN BASIC NUCLEAR-PHYSICS RESEARCH IN 1969   This table provides an analysis of the distribution and characteristics of 15 different classes of accelerator that are currently in use for nuclear-physics research. For each type of accelerator there is a characteristic particle-energy range generally given in terms of the proton energy or the electron energy as is pertinent, except in the case of Lawrence cyclotrons where it is traditional to quote energies of deuterons and in the case of heavy-ion accelerators to list the energy in MeV per nucleon of heavy ions. In the next column the cost range for the different types of accelerator is given. The cost of an individual accelerator varies widely from about $50,000 to $55 million. Generally, the costs given are facility costs, but in a few cases they may be for machines only.

There are three separate analyses of the number and type of accelerators, first by the chronology of initial operation of each accelerator, second an analysis by distribution of laboratory type, and third an analysis by the type of laboratory with reference to the size and complexity of the research program (as defined in the caption of Table II.3). A fourth analysis is made comparing the beam and operational characteristics of the accelerators by type.

It may be noted that 148 accelerators are included in the analysis. Of these some 54 came into operation between 1965 and 1969. Some 16 additional machines are listed as under construction at the end of 1969. The fact that nuclear physics is not a static science but is continuously undergoing change is emphasized by the fact that while these new machines came into being during the last five years, many machines have also been shut down. As shown in Table II.7, some 60 machines have been shut down during the last two decades. In addition, nine other machines are listed as having been transferred from one laboratory to another.

From Table II.4 one can observe that there are large numbers of single-stage and tandem accelerators: 42 single-stage accelerators below 5 MV, 15 above 5 MV, and 27 two-stage tandems. Some reasons for the great popularity of these accelerators among nuclear-physics investigators may be gleaned from the study of the seven columns of table characterizing the beam and operating characteristics of the accelerators. Notice the excellent flexibility, beam quality, beam energy definition, the simple operational demands, the high beam intensity, and the capability of accelerating a variety of ions.

The seven columns defining beam and operational characteristics are as follows:

Flexibility   Flexibility implies that more types of nuclear problem can be investigated on the accelerator with a high degree of flexibility compared with one poor in flexibility. Also a flexible machine can be used more efficiently as particles and energies can be changed quickly and easily. The detailed definition of flexibility as we have used it is contained in footnote *d* of the table.

Operational Demands   The purpose of this column was to get at the requirements with respect to trained operating crews. The simplest machines can be operated by a graduate student with a small amount of training. The most complex machines require a highly trained crew with a significant training period. The details of this are amplified in footnote *e* of the table.

Light-Ion Capability and Heavy-Ion Capability   Many accelerators can accelerate a wide range of particles. Some accelerators are designed, however, for one or possibly two particles. The accelerator capability in this regard again determines the spectrum of nuclear problems that can be investigated or the breadth with which they can be investigated. The various classes of accelerator are characterized in this regard in columns 3 and 4.

Average External Beam Intensity   The machine with high beam intensity is able to complete experiments at a higher rate than one with low intensity and also may do certain types of experiment that are completely unfeasible at low intensity. A scale defining beam intensity is listed in footnote *g*.

Beam Quality   It is characterized by the degree that the beam can be focused into a small spot with small angular divergence. Many experiments require very-high-quality beam, and machines that rate "poor" in this area generally have had a difficult time doing competitive "on-the-frontier" research. An exception is the synchro-cyclotron, which can do very-high-quality neutron time-of-flight work in spite of generally poor beam quality.

Beam Energy Definition   This characterizes the homogeneity in energy of the accelerator beam. Many experiments undertaken nowadays demand good energy resolution. Consequently, machines with poor energy resolution again are in a relatively difficult competitive position. In some cases this can be overcome by special energy analysis equipment external to the accelerator. Beam energy definition classifications are contained in footnote *i*.

TABLE II.5 Census of Accelerators in Basic Nuclear-Physics Research—1969

	Energy[a] (MeV)	Other[b] Ions	Operating[c] Since	Identification[d]
Tandem, Three-Stage				
Brookhaven N L	(30)	(d to ^{37}Cl)	(1970)	MP + MP[e]
U Cal., Livermore	(27)	(d)	(1971)	AVF + EN[e]
Duke U	32	d	1968	AVF + FN[e]
Los Alamos S L	23	d to ^{16}O	1964	Single + FN[e]
U Pittsburgh	18	d to ^{16}O	1967	EN + EN[e]
U Texas	17.5	d to α	1963	CN + EN[e]
U Washington	24.6	d	1967	FN + FN[e]
Tandem, Two-Stage				
Aerospace Res. Lab.	8	d to α	1967	T-8
Argonne N L	18	d to ^{37}Cl	1967	FN
Army Nuc. Eff. Lab.	15	d to α	1969	FN
Brookhaven N L	(20)	(d to ^{37}Cl)	(1970)	MP[f]
U Cal., Livermore	(12)	(d to ^{12}C)	(1970)	EN[f,g]
Cal. Inst. of Tech.	10	d to ^{19}F	1961	EN
Duke U	17	d to ^{7}Li	1968	FN[f]
Florida State U	(18)	(d to ^{16}O)	(1970)	FN
Kansas State U	12	d to α	1969	EN
Los Alamos S L	15*	d and t	1964	FN[f]
W Michigan U	12	d to ^{16}O	1969	EN[g]
U Minnesota	20	d to α	1966	MP
SUNY, Stony Brook	17	d to α	1968	FN
U Notre Dame	15*	e; d to ^{16}O	1968	FN
Oak Ridge N L	13	d to ^{238}U	1962	EN
Ohio U	(11)	(d to α)	(1970)	T-11
U Pennsylvania	12	d to ^{16}O	1962	EN
U Pittsburgh	12	d to ^{16}O	1967	EN[f]
Purdue U	15	d to ^{32}S	1969	FN
Rice U	12	d to ^{16}O	1961	EN
U Rochester	20	d to ^{32}S	1966	MP
Rutgers	18*	d to ^{32}S	1964	FN
Stanford U	19	d to ^{37}Cl	1965	FN
U Texas	12	d to α	1963	EN[f]
U Washington	18	d to ^{16}O	1965	FN[f]
U Wisconsin	13*	d to ^{32}S	1960	EN
Yale U	22	d to ^{127}I	1966	MP
High-Voltage, Single-Stage, $\geqslant 5$ MeV				
U Arizona	5.5	d to α	1968	CN
Bartol Res. Found.	5.5	d; also e	1952	CN
Columbia U	5.5	d to α	1955	CN
U Georgia	(5)	(d to α)	(1970)	VdGraaff[g]
U Iowa	6	d to ^{7}Li	1964	CN
U Kentucky	6	d to α	1964	CN
Los Alamos S L	8	d to α	1950	Home Made VdGraaff[f]
Lowell Tech. I	6	d to α	1969	CN
Mass. Inst. of Tech.	8	d to α	1951	Home Made VdGraaff
Naval Res. Lab.	5.5	d to ^{129}Xe	1953	CN
Oak Ridge N L	6	d to α	1950	CN

TABLE II.5 *(Continued)*

	Energy[a] (MeV)	Other[b] Ions	Operating[c] Since	Identification[d]
Ohio State U	6*	d to α	1963	CN
Pennsylvania State U	5.5	d to α	1963	CN
Rice U	5.5	d to α	1953	CN
U Virginia	6	d to α	1966	CN
High-Voltage, Single-Stage, $<$5 MeV				
Aerospace Res. Lab.	2	+ ions	1953	VdGraaff
Argonne N L	4	d to α	1969	Dynamitron
Brigham Young U	2.5	d to α	1965	VdGraaff
Brigham Young U	(4)	(d to α)	(1970)	VdGraaff[g]
Brookhaven N L	4	d to α	1954	VdGraaff
Brown U	0.4	+ ions	1963	Cockcroft-W
U Cal., Livermore	0.5	d	1965	ICT
Cal. Inst. of Tech.	0.6	d to α	1952	Home Made VdG
Cal. Inst. of Tech.	2.8	d to α	1949	Home Made VdG
Carnegie Inst.	4*	d to ^{40}Ar	1938	VdGraaff
Case Western R U	3	d to α	1958	VdGraaff
Concordia College	0.4	d	1960	Cockcroft-W[g]
Duke U	3.3	d to α	1961	VdGraaff
Duke U	4.2	d to α	1952	VdGraaff
U Florida	4.2	d to α	1964	VdGraaff
Georgetown U	0.4	d to α	1964	VdGraaff
Georgetown U	2	d to α	1966	VdGraaff
U Iowa	2	none	1961	VdGraaff
Johns Hopkins U	3	d to α	1963	VdGraaff
U Kansas	4	d to α	1963	VdGraaff
Lockheed, Palo Alto	3	d to α	1958	VdGraaff
Los Alamos, S. L	3	d to α	1969	VdGraaff
U Maryland	3.5	d to α	1958	VdGraaff
Montana State U	(3.5)	d	(1971)	VdGraaff[g]
NASA–SREL	3e	none	1966	Dynamitron
Nat. Bur. Stand.	2	d to α	1955	VdGraaff†
SUNY, Albany	(4)	(e; d to α)	(1970)	Dynamitron
U Notre Dame	4	e; d to α	1956	VdGraaff
Oak Ridge N L	3	d to α	1948	VdGraaff
Ohio State U	0.2d	p	1961	Cockcroft-W
U Oregon	4.5	d to α	1966	VdGraaff
U South Carolina	0.5	d to α	1962	VdGraaff
Stanford U	3	d to α	1960	VdGraaff
U Texas	4	d to α	1963	VdGraaff
Texas Nuclear	3.2	e; d to α	1956	VdGraaff
Tulane U	3	d to α	1967	VdGraaff
Virginia Poly. I	4	d to ^{14}N	1968	VdGraaff
Washington State U	2	d to α	1965	VdGraaff
West Virginia U	2d	p	1964	VdGraaff
U Wisconsin	2	d to α	1951	VdGraaff
Worcester Poly. I	2	d	1959	VdGraaff
U Wyoming	0.1d	p	1968	Cockcroft-W
Synchrocyclotrons (FM)[h]				
U Cal., Berkeley	730	d, ^{3}He, α	1946	184 in.
Carnegie-Mellon	440	none	1952	141 in.†

TABLE II.5 *(Continued)*

	Energy[a] (MeV)	Other[b] Ions	Operating[c] Since	Identification[d]
Columbia U	390	none	1950	164 in.
Conversion to AVF-FM	(550)	(none)	(1971)	170 in.
NASA–SREL	660	none	1967	197 in.
U Chicago	460	none	1950	170 in.
Isochronous Cyclotrons (AVF)				
Brookhaven N L	36	d to α	1968	60 in.
U Cal., Berkeley	60*	d to ^{20}Ne	1962	88 in.
U Cal., Davis	55	d to α	1965	76 in[g]
U Cal., Livermore	15H$^-$	p, d, α	1969	30 in[f]
U Cal., Los Angeles	50H$^-$	none	1962	50 in.
U Colorado	28	d to α	1962	52 in.
U Connecticut	8	d to ^{7}Li	1967	28 in[g]
Duke U	15H$^-$	^{2}H	1968	31 in[f]
U Indiana	(200)	(many)	(1972)	260 in.
U Maryland	100(140)	(many)	1969	105 in.
U Michigan	35	d to α	1964	83 in.
Michigan State U	55	d to α	1965	67 in.
Naval Res. Lab.	50	d and α	1967	76 in.
Oak Ridge N L	66	d to ^{20}Ne	1964	76 in.
Oregon State U	18	d and α	1969	37 in.
Princeton U	56	d to ^{12}C	1968	69 in.
Texas A & M	60	d to ^{16}O	1967	88 in.
Washington U	13	d to α	1965	54 in.
Lawrence Cyclotrons (FF)				
Argonne N L	23d	p to α	1952	60 in.
U Cal., Livermore	14	d to α	1955	90 in.
Los Alamos S L	14d	p to α	1955	42 in.
Mass. Inst. of Tech.	15.5d	p and α	1940	42 in.†
U Michigan	9d	p to α	1936	50 in.
NASA–Lewis	21d	p and α	1952	60 in.
St. Louis U	3.5d	none	1966	27 in[g]
U Washington	21.5d	p and α	1951	60 in.
Positive-Ion Linacs				
U Cal., Berkeley	10/u	p to ^{40}Ar	1957	HILAC
Conversion	(8.5/u)	(heaviest)	(1971)	Super-HILAC
Los Alamos S L	(800)	(H$^-$)	(1972)	LAMPF
Yale U	10/u	p to ^{40}Ar	1957	HILAC
Electron Linacs, >150 MeV				
Mass. Inst. of Tech.	(400d)		(1972)	High duty factor
Stanford U[h]	1200e		1960	Mark II
Stanford U[h]	2000e		1972	Supercond.
Electron Linacs, ≤150 MeV				
Argonne N L	22e		1969	
U Cal., Livermore	35e		1959	
U Cal., Livermore	(100e)		(1970)	
U Chicago	50e		1957	
Gulf Gen. Atomics	100e		1958	
U Illinois	(30e)		(1970)	Supercond.
Los Alamos S L	30e		1969	LAMPF model
U Montana	7e		1955	e-linac[g]

283

TABLE II.5 *(Continued)*

	Energy[a] (MeV)	Other[b] Ions	Operating[c] Since	Identification[d]
NASA−SREL	10e		1966	
Nat. Bur. of Stand.	150e		1966	
Naval Postgrad.	100e		1965	
Naval Res. Lab.	65e		1964	
Oak Ridge N L	140e		1969	
Rensselaer Poly. I	100e		1961	
Yale U	75e		1961	
Betatrons				
U Cincinnati	12e		1966	
U Illinois	25e		<1950	
Electron Synchrotrons				
Iowa State U	70e		1949	
Iowa State U	70e		1954	
U Oklahoma	70e		1968	

[a] Maximum energy, proton unless otherwise indicated; design goals in parentheses.
[b] Symbols used throughout: d for deuteron, e for electron, p for proton, α for ionized helium-4, and t for ionized tritium; energy and intensity of heavier ions may be quite limited; proposed "other ions" in parentheses.
[c] Projected date of operation in parentheses.
[d] Commercial accelerator models, cyclotron pole diameters, etc.
[e] Can also be operated as two-stage, see below.
[f] Can also be coupled for three-stage operation, *see above.*
[g] Transferred from another laboratory, see Table II.7.
[h] Appreciable fraction used for basic nuclear physics.
* Also polarized protons, and deuterons.
† *Shut down or discontinued nuclear-physics program during 1969, see Table II.7.*

TABLE II.5 CENSUS OF ACCELERATORS IN BASIC NUCLEAR PHYSICS−1969 This table, provided for reference, is a catalog of the 148 accelerators analyzed in Table II.4 which were used in various aspects of the basic nuclear-physics program during 1969.

This table is arranged starting with potential-drop machines followed by circular machines, then, in sequence, heavy-ion linacs, electron linacs, and, finally, betatrons and electron synchrotrons. The information that follows is listed in order to clarify the terminology used in the column labeled "identification." Many of the symbols are High Voltage Engineering Corporation model numbers, which classify the accelerators according to terminal voltage and type as follows: EN, 6-MV tandem; FN, 7.5-MV tandem; MP, 10-MV tandem; CN, 5.5-MV *single stage. Models T-8 and T-11 are high-current tandems with 4-MV and 5.5-MV terminals,* respectively. ICT stands for insulated core transformer. This is a cascade transformer rectifier unit with a single three-phase primary and multi-insulated cascaded secondaries. The Dynamitron is a parallel-feed cascade generator available up to approximately 6-MV terminal potential. Cockcroft-Walton is a voltage multiplier circuit and is widely used as an injector voltage supply. This system is in use generally below 1 MV. The pole diameters quoted in the cyclotron section are a traditional way of specifying the size of cyclotron magnets. Column 1 gives the maximum proton energy unless otherwise noted. The expected energy of machines not yet in operation, but that were under construction during 1969 and are intended for nuclear physics, is indicated in parentheses.

Available information on other ions that could be accelerated or have been accelerated on these machines is listed in column 2. Acceleration of tritium presents special problems and is not included under table heading "Other ions" unless so specified. The approximate operating date is specified in column 3. A few machines are included that were operated during 1969 and then shut down in the same year; they are indicated by a dagger.

TABLE II.6   Census of Reactors Identified with Basic Nuclear-Physics
Research Programs—1969

Laboratories with	Reactor Identification		Operating Since	Power (MW)	Thermal Neutron Flux (n/cm² sec)
Large Multiprograms					
Argonne N L	CP-5	D₂O Tank	1954	5	6×10^{13}
Brookhaven N L	HFBR	D₂O Tank	1965	40	7×10^{14}
U Cal., Livermore	LPTR	Sw. Pool	1957	3	10^{14}
Los Alamos S L	Omega W	D₂O Tank	1956	8	5×10^{13}
Naval Res. Lab.	NRR[a]	Sw. Pool	1956	1	10^{13}
Oak Ridge N L[b]	ORR	H₂O Tank	1958	30	10^{14}
Intermediate Programs					
Idaho N. C.	MTR[a]	H₂O Tank	1952	40	2×10^{14}
Iowa State U	ALRR	D₂O Tank	1965	5	5×10^{13}
Nat. Bur. of Stand.	NBSR	D₂O Tank	1967	10	10^{14}
U Texas	Triga I	U–Zr Hydride	1963	<1	
Small Programs					
None					
Smallest Programs					
Cornell U	Triga II	U–Zr Hydride	1962	<1	

[a] Shut down in 1970.
[b] HFIR, the important target-producing high-flux reactor, is not included on this list as *in situ* basic nuclear-physics studies are not ordinarily undertaken there.

TABLE II.6   CENSUS OF REACTORS IDENTIFIED WITH BASIC NUCLEAR-PHYSICS RESEARCH PROGRAMS—1969   The 11 reactors listed are those with appreciable basic nuclear-physics programs that were under way during 1969. Reactors having only a small fraction of their research program devoted to basic nuclear physics (less than 10 percent) were not included. The census of reactors is arranged in the style of Table II.3 in which laboratories of various sizes are grouped together. The large multiprogram laboratories are listed together, followed sequentially by groupings of intermediate, small, and smallest programs. The criteria for the classification of programs is discussed in the caption for Table II.3. The reactors are listed by general type, by power, and by available neutron flux. It can be observed that most of the reactors are concentrated in the intermediate and large multiprogram laboratories. This trend occurs because most reactors, particularly the more powerful ones, require large installations, highly trained crews, and generally must run on a continuous basis.

TABLE II.7   Accelerators Shut Down from Basic Nuclear-Physics Research

Institution	Identification	Disposition
1969		
Carnegie-Mellon	141-in. FM cyclotron	Stored
Mass. Inst. of Tech.	42-in. FF cyclotron	To isotope prod.
Nat. Bur. of Stand.	2-MV Van de Graaff	To neutron physics
1968		
Argonne N L	4-MV Van de Graaff[a]	To Brigham Young U
U Cal., Livermore	2-MV Van de Graaff	To U Montana

TABLE II.7 (*Continued*)

Institution	Identification	Disposition
U Chicago	4-MV Van de Graaff[a]	To U Georgia
U Georgia	2-MV Van de Graaff	To Rutgers U
Harvard U	95-in. FM cyclotron	To biomed. research
U Illinois	300-MeV betatron	Dismantled
U Illinois	50-in. FF cyclotron	Dismantled
U Indiana	45-in. FF cyclotron	Dismantled
Mass. Inst. of Tech.	17-MeV electron linac	Dismantled
U Minnesota	68-MeV proton linac	Dismantled
Nat. Bur. of Stand.	180-MeV e-synchrotron	To atomic physics
Northwestern U	4.5-MV Van de Graaff	Stored
Purdue U	37-in. FF cyclotron	Dismantled
Rensselaer P I	250-kV Cockcroft-W	Stored
Rensselaer P I	30-MeV betatron	Dismantled
U Rochester	130-in. FM cyclotron	Dismantled
U Rochester	240-MeV e-synchrotron	Stored
U Virginia	70-MeV e-synchrotron	Stored
1967		
Argonne N L	EN tandem[a]	To W. Michigan U
U So. California	31-MeV p-linac	Dismantled
Naval Res. Lab.	2-MV Van de Graaff	To C of Holy Cross
Princeton U	35-in. FM cyclotron	Stored
U Pittsburgh	45-in. FF cyclotron	Dismantled
1966		
U Cal., Davis	22-in. FF cyclotron	To Santiago, Chile
Cal. Inst. Tech.	1.8-MV Van de Graaff	To atomic physics
Louisiana State U	1-MV Van de Graaff	Dismantled
Los Alamos S L	450-kV Cockcroft-W	To Kansas State U
U Minnesota	4-MV Van de Graaff	Dismantled
Naval Res. Lab.	2-MV Van de Graaff	Dismantled
U Wisconsin	4-MV Van de Graaff[a]	To Montana S U
1965		
Boston College	400-kV Van de Graaff	To solid-state res.
Brigham Young U	100-kV Cockcroft-W	
Columbia U	37-in. FF cyclotron	Dismantled
U Florida	1-MV Van de Graaff	Dismantled
Los Alamos S L	2.5-MV Van de Graaff	Dismantled
Nat. Bur. of Stand.	50-MeV betatron	
U Rochester	27-in. FF cyclotron	To India
1964		
U Cal., Berkeley	60-in. FF cyclotron[a]	To U Cal., Davis
U Chicago	700-kV Cockcroft-W	To Lake Forest C
Iowa U	4-MV Van de Graaff	Dismantled
1963		
Brookhaven N L	18-in. FF cyclotron	Dismantled
Los Alamos S L	2.5-MeV Van de Graaff	Dismantled
Oak Ridge N L	86-in. FF cyclotron	To isotope prod.
Yale U	28-in. FF cyclotron[a]	To U Connecticut
1962		
Johns Hopkins U	1-MV Van de Graaff	Dismantled
U Kansas	2-MV Van de Graaff	Dismantled
Naval Res. Lab.	21-MeV betatron	To U Illinois
Oak Ridge N L	63-in. FF cyclotron	To museum
Ohio State	47-in. FF cyclotron	To other uses
Ohio State	2-MV Van de Graaff	Dismantled

TABLE II.7 *(Continued)*

Institution	Identification	Disposition
1961		
U Cal., Los Angeles	41-in. FM cyclotron	Dismantled
Purdue U	300-MeV e-synchrotron	Dismantled
1960		
U Cal., Berkeley	340-MeV e-synchrotron	To Smithsonian
Iowa State	400-keV Cockcroft-W[a]	To Concordia C
Mass. Inst. of Tech.	300-MeV e-synchrotron	Dismantled
Naval Res. Lab.	250-kV Cockcroft-W	To MURA
U Michigan	100-MeV e-synchrotron	Dismantled
1959		
Stanford U	27-in. FF cyclotron[a]	To St. Louis U
1958		
Oak Ridge N L	625-kV cascade	To thermonuclear research; accelerator development
U Cal., Berkeley	32-MeV p-linac	To U So. Cal.
1956		
Naval Res. Lab.	500-kV Cockcroft-W	To Howard U
1955		
U Chicago	100-MeV betatron	Dismantled
U Cal., Livermore	100-MeV e-linac[a]	To U Montana
Carnegie Inst.	60-in. FF cyclotron	Stored
1950		
Mass. Inst. of Tech.	2-MV Van de Graaff	To museum
194-		
U Chicago	32-in. FF cyclotron	To MHD study; dismantled
1941		
U Cal., Berkeley	37-in. FF cyclotron	To calutron research; later FM and to UCLA; now Lawrence museum

Summary[b]

Cockcroft-Waltons and Cascades	7	Betatrons	5
Van de Graaffs	18	Electron synchrotrons	7
Electron linacs	1	FF cyclotrons	15
Proton linacs	2	FM cyclotrons	5
TOTAL 60			

[a] Transferred to another laboratory in the United States and still operating in basic nuclear physics—1969, see Table II.5.
[b] Does not include nine accelerators transferred to other laboratories in the United States but still in basic nuclear physics.

TABLE II.7 ACCELERATORS SHUT DOWN FROM BASIC NUCLEAR-PHYSICS RESEARCH This table is arranged chronologically showing the number of machines that have been taken out of nuclear-physics research for the last two decades. Nine of the 69 machines listed have been transferred to different U.S. laboratories and are still operating in basic nuclear physics. They are indicated by [a] and are included in this table to account for the machines that have disappeared from certain laboratories. The total number of machines shown on the table, as removed from nuclear-physics research, is 60. Sixteen of these were shut down in 1968, three in 1969, and two to seven per year from 1960 to 1967. Some of these machines, as is noted, have been transferred to other areas of physics, some to isotope production, some to medical research, and some have been dismantled.

The evaluation of the kinds of accelerator facilities in the program has necessarily been complex, responding to the complex set of needs of the whole field of nuclear physics. Gradually, older less capable accelerators fade from the scene as the questions these facilities are designed to answer are understood, and deeper problems thereby exposed pre-empt higher priorities. As accelerators of lesser value are discarded, they should be replaced with instruments of fresh capability closer to the research frontiers. This process of renewal is a necessary characteristic of all mature science; the continued viability of basic nuclear-physics research is uniquely dependent on it.

In choosing priorities, it is important to note that because of the large range of nuclear species and problems, a particular machine can remain at the forefront of part of the field even when more advanced machines are tackling new areas previously inaccessible. For this reason one cannot correctly speak of the obsolescence of a class of accelerators or require their closing-down at any given time. In making judgments, one must weigh the capabilities of the accelerator, its supporting research equipment, and its program in relation to the overall progress of the field.

5.1.3 The "Proliferation" of Van de Graaffs

The material presented above displays the creation of a large number of facilities based on similar accelerators; in particular numerous Van de Graaffs have been added to the nuclear research facilities during the last two decades. The nuclear-physics community has been criticized for this development of decentralized research, being charged with a proliferation of identical facilities and, therefore, by implication identical research programs. Nothing could be further from the truth.

A nuclear research laboratory is based in some large part on its accel-

LABORATORIES WITH:

O	Large Multi-Programs of Nuclear Research	11
◊	Intermediate Programs of Nuclear Research	30
△	Small Programs of Nuclear Research	20
☆	Smallest Programs of Nuclear Research	28
		89
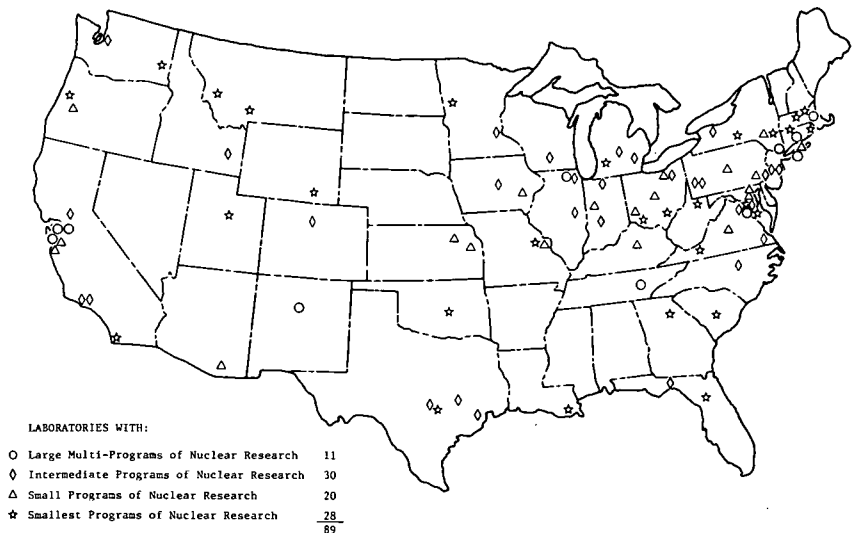
FIGURE II.7 Geographic distribution of laboratories having accelerators and reactors with nuclear-physics research programs in 1969 (148 accelerators and 11 reactors).

erator. The Van de Graaff has provided a machine easy to operate, with superlative beam characteristics, and capable of supporting a flexible and varied program. A Van de Graaff is fully and efficiently utilized by a relatively small scientific staff—from just a few at the smaller machines to some 35 at the newest and largest. To cover all the many nuclear problems within the purview of these machines, a great many groups and, therefore, accelerators were needed. They have been the basis of a large and important part of the nuclear research program.

The requirements of nuclear physics led directly to the need for a large number of Van de Graaff laboratories. It was possible for the country to mount this effort because a Van de Graaff is a relatively inexpensive machine. The great good fortune of being able to proceed efficiently with small units was seized upon to create a strong effort without the intricate complexities of large highly centralized operations. The sameness of accelerators does not, however, in any way imply or require sameness of scientific research. The difference in associated instrumentation and analyzing equipment makes an important difference in a laboratory's capabilities. Even more important is the specialization of different laboratories on different nuclear problems, on different kinds of nuclear species, and on different branches of nuclear physics. In fact, there has been only the overlap required of a healthy competitive scrutiny to validate the science. The marketplace criteria maintained by the refereed literature and the critical opinion of the scientific community serve as guarantors of uniqueness and quality. A less formal but stronger warranty against overlap is the fact that there are too many challenging questions open in the field to provide any serious temptation to repeat research that has already been done.

There now are being planned at least one and perhaps ultimately a few very large facilities that will require a centralized mode of operation and research. User-group participation in such facilities will be the style of much of the research work with these machines. However, the many unsolved problems within the province of smaller machines should still continue to be studied in the decentralized way. That this decentralized approach with smaller accelerators has important advantages for educational opportunities, for competitive decisions of the best research opportunities, and for encouragement of daring and speculative research possibilities cannot be denied. Therefore, the decentralized approach should not be discarded lightly.

5.2 NUCLEAR REACTORS AND NEUTRON SOURCES

Much of our information on nuclear physics comes from nuclear reaction studies within which the neutron-induced reactions hold an important

place. The production of intense well-defined beams of neutrons is there-
fore one of the necessities of a full nuclear research program. The electric
neutrality of the neutron—one of the reasons for its importance in nuclear
science—means also that the acceleration and detection techniques appro-
priate to all other nuclear projectiles cannot be directly applied, and there-
fore neutron-based nuclear physics has grown by a somewhat special tech-
nology. This special technology is itself divided according to the means of
production of the neutrons: accelerators, reactors, and nuclear explosions.

5.2.1   *Accelerator Neutron Sources*

Energetic neutrons—those with energies greater than several hundreds of
kiloelectron volts—are largely the province of accelerator-induced nuclear
reactions. Two approaches have been used and still form the basic founda-
tion: reactions in which a single group or a very few groups of neutrons
are emitted, the remaining nucleus being left in its ground state or one of
those nearby, and those in which a continuous or "white" spectrum of
neutrons is ejected, energy selection being brought off by auxiliary tech-
niques such as the time-of-flight devices to be discussed later. Both meth-
ods had their genesis in the early days, the late 1930's and early 1940's.
Much of the neutron work in the late 1940's and early 1950's was based
on the fast, single-group neutron source. By adroitly choosing the reac-
tions, good supplies of neutrons in various energy regions can be manu-
factured. For example, the reaction $(p + {}^7Li \rightarrow {}^7Be + n)$ has been a useful
source for neutrons below a million electron volts, while the strongly
energy-adding reaction $(d + {}^3H \rightarrow {}^4He + n)$ has supplied neutrons up to
20 MeV of energy. By varying the energy of the incoming charged particle,
or the direction from which the emitted neutrons are taken, neutrons of
different energies are selected.

   Although intense beams of nearly monoenergetic neutrons are available
from those accelerator-induced reaction sources, another technique known
as the time-of-flight method, provides a more versatile source of neutrons
over the broad energy range up to some 20 MeV. The accelerator is used
to produce a pulse of secondary neutrons whose energies are sorted out
electronically by measuring off the time taken to traverse a measured
path. As timing techniques have been developed and refined and as intense
pulsed sources have been developed, the energy resolution has been stead-
ily sharpened to pick apart and use separately narrow pieces of the "white"
spectrum of neutrons. From the early 1950's to the mid-1960's, resolu-
tions have been bettered by a factor of 1000 at neutron energies above
0.1 MeV, and a factor of 20 at lower energies—opening more of the cross-
section structures to investigation.

   A variety of accelerators is useful to produce the neutrons. In the late

1950's, the electrons from betatrons were used to produce the electromagnetic radiation that, in turn, ejected neutrons from heavy-element targets. Later, linear electron accelerators were developed, and one of the most powerful pulsed neutron-producing facilities has at its heart the high-intensity bursts of electron-induced bremsstrahlung radiation from a linac. At higher neutron energies the electron-based sources tend to become too weak. There cyclotron-accelerated protons take over, using very effectively the high-repetition rate and the very short size of the pulses that are possible with these machines, combined with the large intensities that come as the result of the many neutrons that pour out of a nucleus when hit with an energetic projectile. Such "spallation" sources are especially important as sources of energetic neutrons. The extension to a still higher order of intensities is one of the possibilities of the "intense neutron generators" that have been suggested over the years but that may only be realized with extensions of present accelerator technology; this will be discussed again later on.

5.2.2 *Nuclear Reactor Sources*

The fission reactor has had a very special place in the development of nuclear physics, both as a source of neutrons for direct studies of interactions with nuclei and to produce the radionuclides that enlarged the explored nuclear domain. The importance of copious supplies of such radioactive species for applications in industry, technology, medicine, and other sciences has been touched upon in previous chapters. Power reactors have, as already seen, grown into a great industry and are part of the discussion in Chapter 3 on the applications of nuclear physics. The numerous training and engineering reactors on college campuses and industrial sites are summarized there. This chapter is concerned with the few research reactors that mount sizable nuclear programs.

Within a few years after the construction of the first fission chain reactor, nuclear physicists began to use neutrons from such reactors in fundamental experiments on neutron interactions with nuclei and on the properties of isolated neutrons. At first only neutrons inside the reactor were available. Later, as reactors with high neutron fluxes became available, beams of neutrons, drawn out of the reactors, were strong enough so that experiments could be carried on outside the reactor shield in the laboratory. This increase in the intensity of the extracted beams has been one of the central goals of the development of the research reactor complex. The available neutron intensities have increased by a factor of 10^7 in the 30-year history of the field. The convenience and selectivity of the neutron beams too has defined the refinements required of the reactor. The modern high-flux research reactor has beams with special geometric prop-

erties, beams that tap different parts of the reactor core to emphasize different energies, beams filtered to enhance particular components of the neutron energy spectrum—all tailored to the experiments to be performed (Figure II.8). The improvements and specializations in reactors have been less the results of particular breakthroughs than from improved engineering and technology and growing expertness in customizing the facility. It is interesting that the increased intensities and specializations have been accompanied by steadily decreasing costs.

The neutrons in the external beams from a high-powered reactor range in energy from a few thousandths of an electron volt all the way to a few million electron volts. The very slow, lowest-energy neutrons are particularly useful in studying the fundamental properties of the neutron itself—its lifetime, its decay. The most intensive output occurs for neutrons near thermal equilibrium with the surroundings—fractions of an electron volt. Such neutrons exhibit high cross sections for initiating nuclear reactions and are much used, for example, for the study of neutron capture by nu-

FIGURE II.8   The Brookhaven High Flux Beam Research Reactor. This heavy water, 40-MW reactor has been in operation since 1965. The thermal neutron flux is 7×10^{14} $(n/cm^2/sec)$.

clei. At higher energies, energy selection can be made by time-of-flight techniques following the pulsing of the emergent beams by mechanical chopping. At the lower end, crystal diffraction spectrometers act as sharp monochromators.

The experiments with the very slowest neutrons, as well as those with energies of less than some hundred electron volts, are chiefly the province of the reactor. They complement the accelerator-based neutron sources, which are more efficient at higher energies. An experiment over a broad range of neutron energies combines data from both kinds of sources.

The modern high-flux research reactor is an expensive and complex installation. It is a large facility; extensive and exacting high-rate cooling systems and complex, carefully optimized shielding systems are vital. It must be operated and maintained by a professional crew that strictly adheres to the necessarily stringent regulations required. It is, then, natural that there are only a few of these reactors, and these are parts of either national or other large laboratory efforts. The universities have built and operated numerous small reactors, serving a variety of educational purposes, but supporting only minor basic nuclear-physics research programs. Tables II.3 and II.6 list the reactor facilities mounting substantial basic nuclear-physics research efforts.

In discussing research programs based at large reactors, it is misleading to think of them as totally or even mostly nuclear in nature. Solid-state, chemistry, and applied nuclear research play major roles in the overall programs. In determining the priorities to be accorded to reactor-based programs of any one field, this multidisciplinary nature must be kept in mind, for reorderings in one field may lead to serious imbalances in the others.

What are the future developments that appear likely? It is clear, from practical considerations of heat removal and of shielding, that further advances in neutron fluxes are achievable only by radical departures in design. A promising approach is to pulse the reactivity of the reactor so that a high intensity is produced for a short time, leaving a longer cooling-off period. The Soviet Union has operated such a pulsed facility since 1960 and is planning increases in power levels. Fluxes 10 to 100 times current levels can be achieved in this pulsed mode and applied to the many experiments that can take advantage of this time distribution. The pulsing mechanism itself may be a mechanical motion of components or the skillfully injected beam of neutrons from an auxiliary pulsed accelerator.

5.2.3 *Neutrons from Nuclear Explosives*

The underground nuclear explosion tests, regularly conducted by the U.S. Atomic Energy Commission, constitute an important national resource

and a uniquely powerful neutron source. A typical nuclear explosive device, as used in these tests, produces roughly a gram of neutrons, that is, about 10^{24} neutrons in approximately a ten-millionth of a second. This quantity is equivalent to the total neutron production of a high-intensity accelerator neutron source running for several years. In the last few years, the neutron source capabilities of underground nuclear test explosions have begun to be used for neutron cross-section measurements, which are important in the application of nuclear energy to power production. Several of the explosions have produced a variety of excellent data on the fission and capture cross sections of the transplutonium isotopes. The large flux renders insignificant the background problems brought about by the intrinsic activity of such targets, and, for highly radioactive targets, the explosion source technique is the only feasible alternative.

Up to now these sources have not been used extensively for problems in basic nuclear physics, partly because of difficulties arising from national security requirements.

5.3 RECENT CONSTRUCTION AND POSSIBLE NEW FACILITIES

5.3.1 *Near Horizons*

During the middle and late 1960's, plans were made and authorizations were obtained to design and construct a number of new accelerators for nuclear physics. These projects were conceived to enable physicists to penetrate the frontiers of nuclear science. Some of the projects were small and were motivated in order to provide suitable facilities and atmosphere for the teaching of nuclear physics in new or emerging institutions.

This new construction of accelerator facilities, in the United States and abroad, is summarized in Table II.8. Among the new areas of physics opened by the new accelerators are high-energy proton and meson nuclear probes generated by the meson facility being constructed at Los Alamos; heavy-ion physics investigations and the production of new superheavy nuclear species in newly hypothesized islands of stability using the super-HILAC; new high-energy electron probes with very much improved duty factor for a wide range of coincidence-type experiments in new electron linacs at MIT and Stanford; and a new separated-sector isochronous cyclotron opening the door for improved precision physics up to the 200-MeV proton range. The importance of the nuclear physics that can be undertaken on these facilities has been discussed in Chapter 2. It seems clear that this new construction of facilities will make possible very important and very exciting investigations during the coming years. (See Figures II.9-II.14.)

TABLE II.8 New Accelerators for Basic Nuclear Physics under Construction in 1969

	First Operation Expected	Principal Characteristics	Cost ($Millions)	
			Accel.	Total
UNITED STATES				
Accelerators for Short-Wavelength, High-Momentum Transfer Particles				
Proton Linear Accelerator (LAMPF) Los Alamos Scientific Laboratory	1972	Features 1-mA proton beam with 6% duty factor at 800 MeV, plus simultaneously either 100-μA H$^-$ at 800 MeV or 1-μA polarized H$^-$ at 800 MeV. Energy resolution $\Delta E/E = 0.4\%$. Beams of protons, neutrons, pions, muons, and neutrinos will be available for simultaneous use in a multidisciplinary program. This facility is designed to accommodate large numbers of users from all sections of the country	20	56
Synchrocyclotron Improvement Project Nevis Laboratory, Columbia University	1971	Conversion of 390-MeV Nevis proton synchrocyclotron to sector focusing with high-dee voltage and rep rate. Expected energy 550 MeV and extracted beam current of 40 μA, with 50% macroscopic duty factor. Will be especially useful for proton and meson experiments requiring intermediate intensity and energy		3.9
Electron Linear Accelerator High Energy Physics Laboratory, Stanford University	>1972	Superconducting electron linac with continuous 2-GeV, 100-μA beam and especially good energy definition, $\Delta E/E$ ~ 0.01%	3.5	8.15
Accelerators for Medium-Wavelength Particles				
Isochronous Cyclotron Indiana University	>1972	Open sector design permits high-energy injection with very-high-quality beams, easy energy variability (20–200 MeV for protons), and special provisions for high-energy resolution, $\Delta E/E$ ~ 0.1%. Heavy-ion energies to 280 q^2/A, for example, ^{12}C^{4+} to 375 MeV (31.2 MeV/u)	4.7	10

TABLE II.8 (Continued)

	First Operation Expected	Principal Characteristics	Cost ($Millions)	
			Accel.	Total
Isochronous Cyclotron (MUSIC) Maryland University	1969	Largest variable-energy isochronous cyclotron in operation. Conventional design with four-sector magnet and with two in-dependently driven 90° dees. Designed to provide 145-MeV protons, 95-MeV deuterons. Energy of other ions approximately 185 q^2/A	3.3	4.1
Electron Linear Accelerator Massachusetts Institute of Technology	1972	Designed especially for large macroscopic duty factor 150-μA beam. Duty factor 1.8% at 400 MeV, 5.8% at 200 MeV. Energy spread $\Delta E/E = \sim 0.2\%$	4.3	6.3
Accelerators for Low-Energy Particles				
Double Tandem Brookhaven National Laboratory	1970	Two HVEC Model MP tandems, 10-MV terminal rating. For high-resolution nuclear spectroscopy and for research with heavy ions. Will provide 30-MeV protons and deuterons, 40-MeV alpha particles, and $\sim$100-MeV $^{16}O^{8+}$ ions. Also 220-MeV sulfur ions (6.9 MeV/u) with experiment in terminal. Energy resolution, $\Delta E/E \sim 0.01 - 0.5\%$	6.5	12
Tandem Van de Graaff with Cyclotron Injector Lawrence Livermore Laboratory	1971	High Voltage Eng. Corp. Model EN Tandem with 15-MeV H⁻ isochronous cyclotron as injector. $\Delta E/E \sim 0.4\%$ at 27 MeV. Features easy energy variability		2.2
Tandem Van de Graaff Florida State University, Tallahassee	1970	HVEC Super-FN tandem. Terminal voltage, 9 MV. For high-resolution nuclear spectroscopy	1.3	2.35
Isochronous Cyclotron Oregon State University	1969	This is a conversion of 37-in. Lawrence-type cyclotron to a four-sector isochronous machine. The cyclotron will provide protons and alpha particles to 20 MeV, deuterons to 10 MeV, and ^{3}He ions of 27 MeV	0.35	0.45

296

Accelerator / Institution	Year	Description		
Tandem Van de Graaff U.S. Army Nuclear Effects Lab. Aberdeen, Md.	1969	HVEC Model FN tandem. 7.5-MV terminal voltage. 15-MeV protons and deuterons. 22.5-MeV ^{3}He and ^{4}He		~4
Tandem Van de Graaff Purdue University	1969	HVEC Model FN tandem. 7.5-MV terminal voltage. Gives protons and deuterons to 15 MeV and ^{3}He and ^{4}He ions to 22.5 MeV		4.2
Tandem Van de Graaff Kansas State University	1969	HVEC Model EN tandem. 6-MV terminal voltage. Provides protons and deuterons to 12 MeV and ^{3}He and ^{4}He ions to 18 MeV.		1.5
Tandem Van de Graaff Western Michigan University	1969	HVEC Model EN tandem. 6-MV terminal voltage rating. Gives protons and deuterons to 12 MeV, ^{3}He and ^{4}He to 18 MeV	0.76	1.26
High Current Tandem Van de Graaff Ohio University	1970	HVEC Model T-11 tandem equipped with nanosecond beam-pulsing equipment for high-resolution time-of-flight energy analysis of reaction products	1.0	1.33
Single-Stage Van de Graaff Lowell Technological Institute	1969	HVEC Model CN Van de Graaff. Single-stage accelerator for 6-MeV protons and deuterons, 12-MeV ^{3}He and ^{4}He ions. Nanosecond pulsing with Mobley beam bunching magnet		0.53
High Current Single-Stage Dynamitron Argonne National Laboratory	1969	4-MV Dynamitron, Radiation Dynamics, Inc. Positive ions or electrons to 5 mA. Especially useful for neutron production and, with nanosecond pulsing capability, for neutron time-of-flight work	0.40	
Single-Stage DC Accelerator State University of New York, Albany	1971	RDI 4-MeV Dynamitron. Hydrogen ion currents up to 2 mA, electron currents to 5 mA. Equipped for nanosecond beam pulsing for time-of-flight energy analysis of neutrons or other emitted particles	0.5	1.5
Single-Stage Van de Graaff Los Alamos Scientific Laboratory	1969	3-MeV Van de Graaff; HVEC Model KN-3000. Used for proton, deuteron, and alpha particle acceleration. Used mostly for neutron production	0.175	0.75
Electron Linear Accelerator Lawrence Livermore Laboratory	1970	This dual-mode 150-MeV linac provides either 3-μsec pulses at 1000 pps or high-current nanosecond pulses at variable 100–1000 pps. Used for production of monochromatic photons ($\Delta E/E = 0.1\%$) and for neutron time-of-flight work		4.5

TABLE II.8 (Continued)

	First Operation Expected	Principal Characteristics	Cost ($Millions)	
			Accel.	Total
Electron Linear Accelerator Oak Ridge National Laboratory	1969	140-MeV electron linac designed for production of short neutron bursts for time-of-flight neutron cross section measurements. Pulse width variable from 2.5 nsec to 1 μsec at rep rates up to 1000 pps. Peak current 15 A for pulses <24 nsec in length	1.74	4.8
Electron Linear Accelerator University of Illinois	1971	Superconducting electron linac with continuous beam, 10 μA at 30 MeV. Features high resolution, $\Delta E/E \sim 0.03\%$. Will be used in monochromatic photon experiments, also as accelerator for 600-MeV microtron, to be added later	0.5	0.6
Electron Linear Accelerator Los Alamos Scientific Laboratory	1969	This 30-MeV Electron Prototype Accelerator (EPA) was built to evaluate side-coupled 800-MHz cavities for the 800-MeV LAMPF proton linac. It provides duty factor to 6%. Used for engineering studies and some photonuclear research		0.45
Electron Linear Accelerator Argonne National Laboratory	1969	22-MeV electron linac designed for short, high-current pulses. Provides 20 A at 10 nsec, 2 A at 2 μsec. Used almost entirely for radiation chemistry research. A small fraction of beam time is available for high-resolution photoneutron cross-section measurements		1.1
Accelerators for Heavy Ions				
Heavy-Ion Linear Accelerator (Super-HILAC) Lawrence Berkeley Laboratory	1971	Conversion of HILAC ($A \leqslant 40$) to accelerate all ions to maximum energy of 8.5 MeV/u. Intensity ranges from 10^{11} ions/sec for uranium to $\sim 10^{12}$ ions/sec for light ions. $\Delta E/E \sim 0.5\%$	1.7	2.65

FOREIGN COUNTRIES

Accelerators for Short-Wavelength, High-Momentum Transfer Particles

Conventional Synchrocyclotron Ioffe Physico-Technical Institute Gatchina (near Leningrad), U.S.S.R.	1969	World's largest synchrocyclotron–1-GeV protons. Has 8000-ton magnet with 7-m pole diameter. First beam, 1967. Extracted beam of 6×10^{11} particles/sec (25% extraction efficiency) obtained August 1969	8
Synchrocyclotron Modification Project Laboratory for Nuclear Problems Joint Institute for Nuclear Research Dubna, U.S.S.R.	~1975	Conversion of Dubna 680-MeV synchrocyclotron to sector focusing with high-dee voltage and high rep rate. Expected energy 700 MeV and extracted beam currents up to 25 μA	3
Synchrocyclotron Modification Project European Organization for Nuclear Research (CERN) Meyrin (Geneva), Switzerland	1972	Program for improvement includes new radio-frequency system for higher rep rate and dee voltage, improved ion source, better magnetic focusing at center, and new extraction system. Beam current expected to be at least 10 μA	23
Isochronous Ring Cyclotron Swiss Institute for Nuclear Research (SIN) Villigen (near Zurich), Switzerland	1973	Will provide continuous 100-μA beam of protons at 585 MeV, $\Delta E/E = 0.3\%$. Injection at 70 MeV from a four-sector injector cyclotron, which can also provide 10–75-MeV protons, 10–65-MeV deuterons, and other ions at $E \sim 130\, q^2/A$	19
H⁻ Isochronous Cyclotron Tri-Universities Meson Facility University of British Columbia Vancouver, B.C., Canada	1973	To accelerate H⁻ ions to 550 MeV. Extraction by stripping will permit two simultaneous external beams, later six, independently variable in energy 150–500 MeV; beam currents to 120 μA at 550 MeV; $\Delta E/E < 0.2\%$	10

Accelerators for Medium Wavelength Particles

Isochronous Cyclotron University of Louvain Ottignies, Belgium	1973	Conventional Isochronous Cyclotron. Will provide 100-MeV protons, 52-MeV deuterons, 104-MeV alpha particles, and 104 q^2/A MeV for other ions. Similar in design to University of Maryland and Grenoble cyclotrons. Being built by Thompson-CSF	2
			6
			1

TABLE 11.8 (Continued)

| | First Operation Expected | Principal Characteristics | Cost ($Millions) | |
			Accel.	Total
Isochronous Cyclotron Institute for Nuclear Problems Joint Institutes for Nuclear Research Dubna, U.S.S.R.	Proposed	A large, low-field, separated-sector cyclotron of new design, "The Monoenergy Cyclotron," will deliver 80-MeV protons, 60-MeV deuterons, and other particle with energy $60q$ MeV (120 MeV for $^3He^{2+}$, $^4He^{2+}$; 180 MeV for $^6Li^{3+}$; etc.). Energy spread of extracted beam is planned to be $\leq 0.01\%$. Ion injection is from dc accelerator. The design is generally similar to that of the Lebedev Institute Spectrometric Cyclotron		24
Isochronous Cyclotron Kiev Physical Institute Kiev, U.S.S.R.	~1971	Three-sector isochronous cyclotron. Similar in design to ORIC. Energy is 100 MeV for protons, $120\,q^2/A$ MeV for other ions. Built in Leningrad at the Research Institute for Electrophysical Apparatus. Facility is provided with large, elaborate, shielded research areas		
Isochronous Cyclotron Kurchatov Institute Moscow, U.S.S.R.	~1973	Second model of Kiev-type cyclotron with improvements to give better energy resolution. Experimental areas reflect emphasis on high-resolution research. Novel time-of-flight beam preparation system proposed to provide 0.01% energy resolution		
Isochronous Cyclotron Lebedev Physical Institute Plakha (near Moscow), U.S.S.R.	Proposed	Separated-sector isochronous cyclotron. "The Spectrometric Cyclotron" features very low average magnetic field, 5000 G. Will provide protons in 10–100-MeV range and other ions. Energy spread, $\Delta E/E$, expected to be $\leq 0.01\%$. Research Institute for Electrophysical Apparatus, Leningrad, is cooperating in the design of the cyclotron		

Accelerator / Location	Year	Description		
Isochronous Cyclotron, University of Groningen, Groningen, The Netherlands	1970	Three-sector cyclotron of conventional design will accelerate protons to 70 MeV, deuterons to 65 MeV, alpha particles to 130 MeV, and other particles to 130 q^2/A MeV. Energy resolution of raw beam will be ~0.2%. Being built by Philips	3.5	~6.5
Isochronous Cyclotron, Bhabha Atomic Research Center, Calcutta, India	1972	Three-sector cyclotron based closely on the Lawrence Radiation Laboratory and Texas A & M University 88-in. cyclotrons. Will provide protons to 60 MeV, deuterons to 65 MeV, and other particles to energy of 130 q^2/A MeV	2.25	6.5
Isochronous Cyclotron, Institute for Nuclear Physics, Jülich, Germany	1969	Conventional isochronous cyclotron with three sectors. Provides protons to 45 MeV, deuterons to 90 MeV, alpha particles to 180 MeV, and other particles with energy of 180 q^2/A MeV. Built by AEG	3.5	6
Tandem Van de Graaff, Daresbury Nuclear Physics Laboratory, Daresbury, Lancashire, England	Proposed	The British Science Research Council (SRC) has been considering the authorization of a supervoltage tandem. The accelerator would be vertical with a terminal voltage of 30 MV. It would provide 60-MeV protons and deuterons, and 90-MeV alpha particles. With foil stripper at the terminal and at 2/3 V in the second tube the heavy ion energies would be 4 MeV/u for uranium and 6.5 MeV/u for iodine		
Electron Linear Accelerator, Center for Nuclear Studies, Saclay, France	1969	Accelerator was especially designed for high stability and large duty factor; accelerates electrons to 640 MeV at 1% duty factor or to 450 MeV at 2% duty factor with peak currents of 20 and 15 mA, respectively. Energy resolution and stability ~0.4%	8.5	22

Accelerators for Low-Energy Particles

Accelerator / Location	Year	Description		
Isochronous Cyclotron, Institute for Nuclear Study, Tokyo University, Tokyo, Japan	1972	Conventional three-sector cyclotron. It is expected to provide protons to 40 MeV, deuterons to 25 MeV, and other particles to energy of 50 q^2/A MeV	0.8	1.5

301

TABLE II.8 (*Continued*)

	First Operation Expected	Principal Characteristics	Cost ($Millions) Accel.	Total
Accelerators for Low Energy Particles				
Tandem Van de Graaff with Cyclotron Injector Australian National University Canberra, Australia	1971	Fixed energy 25 MeV H⁻ cyclotron being built by Cyclotron Corp. will inject beam into existing HVEC Corp. Model EN tandem. Will provide protons and deuterons to 37 MeV. Cost given is for cyclotron only	0.5	
3-stage Tandem Van de Graaff University of São Paulo São Paulo, Brazil	1971	Vertical tandem accelerator designed and built by National Electrostatics Corporation (NEC). Model 4 U "Pelletron" (4 MV) injector delivers negative ions to Model 8UD "Pelletron" tandem (9 MV terminal). Will provide 22 MeV protons and deuterons	2.0	
Tandem Van de Graaff Institute of Nuclear Physics Orsay, France	1971	HVEC Model MP tandem – 10 MV terminal voltage. Will provide 20 MeV protons and deuterons; 30 MeV ³He and ⁴ He. For heavy ions will give ≥ 4 MeV/μ up to A = 20 and > 2 MeV/μ up to A = 45	3.52	
Tandem Van de Graaff Center for Nuclear Research Strasbourg, France	1971	HVEC Model MP tandem – 10 MV terminal voltage. Will be similar to the Orsay tandem	3.52	
Tandem Van de Graaff University of Munich Munich, Germany	1970	HVEC Model MP tandem – 10 MV terminal voltage. Will be similar to the Orsay tandem	3.12	
Tandem Van de Graaff Atomic Physics Institute Bucharest, Rumania	1972	HVEC Model FN tandem – 7.5 MV terminal voltage. Will provide 15 MeV protons and deuterons, 22.5 MeV ³He and ⁴He ions	1.75	

Name / Location	Year	Description		
Isochronous Cyclotron Institute for Radiation and Nuclear Physics University of Bonn Bonn, Germany	1969	This is a conversion of the 70 MeV alpha particle synchrocyclotron completed in 1958. Converted accelerator will give 28 MeV deuterons, 14 MeV protons, alpha particles to 56 MeV, and 42 MeV ^{3}He ions	1.25	1.75
High-Current Tandem Van de Graaff Democritus Nuclear Center Athens, Greece	1971	HVEC Model T-11 Tandem. Rated terminal voltage 5.5 MV. Proton and deuteron energy 11 MeV. Guaranteed proton current 25 µA (designed for 100 µA). Especially useful for neutron work	1.0	
High-Current Tandem dc Accelerator University of Bochum Bochum, Germany	1971	9-MeV (4.5-MV terminal) high-current tandem Dynamitron (Radiation Dynamics, Inc.). Will be provided with pulsed injector (widths <1 nsec), a negative helium ion injector system, and a terminal ion source capable of 1 mA for single-stage operation	1.04	
Tandem Van de Graaff Center for Information Studies and Experiments Milan, Italy	1970	Vertical tandem Van de Graaff, 4-MV terminal voltage. Expected to deliver 8-MeV protons and deuterons and 12-MeV ^{3}He and ^{4}He		
Single-Stage Van de Graaff Center for Nuclear Physics University of Louvain, Louvain, Belgium	1971	Single-ended Van de Graaff HVEC Model KN4000. Will be used for proton, deuteron, ^{3}He, and ^{4}He acceleration at currents up to 50 µA. Will be provided with nanosecond pulsing capability		0.4
High-Current Single-Stage dc Accelerator University of Montreal Montreal, Canada	1971	4-MeV RDI Dynamitron. Will deliver proton and deuteron currents up to 2 mA and electron beams to 5 mA. Provided with nanosecond pulsing capability. Especially useful for research requiring time-of-flight analysis of nuclear reaction product energies	0.5	
High-Current Single-Stage dc Accelerator University of Birmingham Birmingham, England	1971	3-MeV RDI Dynamitron will provide proton, deuteron, and electron currents up to 2 mA. Unit is equipped with beam pulsing and mass analysis capabilities in terminal	0.35	

TABLE II.8 (*Continued*)

	First Operation Expected	Principal Characteristics	Cost ($Millions)	
			Accel.	Total
Single-Stage Van de Graaff Laboratory d'Analyses par Reéactions Nucléaires (LARN) Namur, Belgium	1970	HVEC Model AN2500, 2.5 MV. To be used for proton, deuteron, ^{3}He, and ^{4}He acceleration. Energy spread ~1%. Can provide up to 50-µA output		0.3
Electron Linear Accelerator University of Gent Gent, Belgium	1969	Two-section accelerator. Provides 80-MeV electron beam with average currents up to 100 µA. Energy spread, 1.5% at 25 MeV. Pulse duration, 2 µsec; beam spot size, 1.1 cm	0.9	1.3
Accelerators for Heavy Ions				
Heavy Ion Linear Accelerator Gesellschaft fur Schwerionenforschung (GSI) Darmstadt (Wixhausen), Germany	1974	Will accelerate heavy ions to energies ranging from 18 MeV/u for oxygen to 8.6–10.2 MeV/u for uranium depending on whether gas or foil strippers are used. Will provide protons to 30 MeV. Energy resolution ~0.5%. High-energy section of linac will use 20 individually driven cavities to permit wide-range variation of energy	8	19
Isochronous Cyclotron Institute for Nuclear Reactions Joint Institute for Nuclear Research (JINR) Dubna, U.S.S.R.	1972	Conversion of 3.1-m Lawrence-type cyclotron to 4-m diameter with sector focusing. Heavy-ion energies $E = 625 \, q^2/A$, for example, 5.2 MeV/µ for Xe^{12+} ions at internal intensity of ~5 × 10^{11} particles/sec. Energy resolution, $\Delta E/E = 0.5$–1.0%		
Isochronous Cyclotron with Linear Accelerator Injector (ALICE) Institute for Nuclear Physics Orsay, France	1969	A Sloan-Lawrence-type linear accelerator injects heavy ions at an energy of 1.16 MeV/u into a three-sector cyclotron giving an energy of $70 \, q^2/A$. The linear accelerator is designed to accept ions of $q/A \geqslant 0.1$. Has accelerated 10^8 particle/sec of Kr^{21+} to 400 MeV. Higher currents are expected	1.8	4.2

304

| Cyclotron with Tandem Van de Graaff Injector (NORDAC) Niels Bohr Institute Risø, Denmark | Proposed | Cooperative program of Norway, Sweden, and Denmark for accelerator to provide ions in mass range 100–130 to 8–10 MeV/u. Plan proposes use of the existing super-FN tandem at Risø to inject ions into a four-sector cyclotron of 300–400 q^2/A MeV energy rating | 8 | 13.7 |
| Tandem Van de Graaff with Linear Accelerator Booster Max Planck Institute Heidelberg, Germany | Proposed | Variable Energy Linear Accelerator to be added to the existing Heidelberg MP (10 MV). Two versions are considered: Version 1 with 16 1-MV helix linac sections would give 6.2 MeV/u for bromine; Version 2 with 23 linac sections would give 6.25 MeV/u for iodine. Cost is for Version 2 | 2.3 | |

TABLE 11.8 NEW ACCELERATORS FOR BASIC NUCLEAR PHYSICS UNDER CONSTRUCTION IN 1969   This table describes the new and emerging scene of accelerators for basic nuclear physics. It lists all accelerators under construction in 1969, including those that first obtained a beam in that year. Accelerators in foreign countries are listed so that the effort in the United States can be seen in the context of the worldwide activity.

For each accelerator in these tables we have given the year in which first operation is expected, a brief description of the accelerator noting special features, and finally, where known, the cost of the accelerator and the cost of the whole facility.

The accelerators are arranged in four categories according to the momentum transfer or wavelength of the particles. "Accelerators for Short-Wavelength, High-Momentum Transfer Particles" includes the energy range from 500 MeV to 1 GeV for protons and above 1 GeV for electrons. The second category, "Accelerators for Medium Wavelength Particles" includes the energy range from 50 to 500 MeV for protons and 400 MeV to 1 GeV for electrons. The class "Accelerators for Low-Energy Particles" includes all electron and proton accelerators below these energies. Finally, "Accelerators for Heavy Ions" are listed.

Accelerators for Short-Wavelength, High-Momentum Transfer Particles In this.category the scene in the United States is dominated by the powerful Los Alamos Meson Physics Facility. Out of the many projects that were proposed in the early 1960's, this is the only one that was funded. In other countries, accelerators in the meson factory category include the large isochronous cyclotrons at Villigen near Zurich in Switzerland and at Vancouver, British Columbia, in Canada. There are also synchrocyclotron conversion programs under way. In this country the only such project is the one at Columbia University. Finally, it seems worth noting that a very large but otherwise conventional synchrocyclotron has recently been brought into operation at Gatchina near Leningrad in the Soviet Union.

The electron accelerator being built in this high-energy category is the Stanford University superconducting linear accelerator which is expected to provide a continuous beam of high-energy electrons with superb energy resolution. Moreover, the technological developments from this program are expected to have a sharp impact on the accelerators of the future.

Table II-8 (*continued*)

Accelerators for Medium Wavelength Particles Looking now at accelerators for medium wavelength particles, we see that in the United States in the present scene there is the 140-MeV isochronous cyclotron at the University of Maryland. While not yet at full energy, the machine has already surpassed the maximum energy of existing cyclotrons in the United States; with its good energy resolution and the excellent research facilities it should be able to support an important program of nuclear-structure studies for many years. The next addition to the medium-wavelength capability of the United States will be a new cyclotron being built at Indiana University and scheduled for operation in 1973. Its unique open-sector design gives plenty of space for special radio-frequency systems and is expected to be able to achieve extremely high-energy resolution, perhaps approaching 10^{-4}. In addition, its very large magnet gives it exceptional heavy-ion capability. Later on, with an MP tandem as an injector, it might be able to provide 6 MeV per nucleon heavy ions up to the xenon region.

In other countries the new accelerators in this range are mostly conventional isochronous cyclotrons, but there are two important exceptions. In the Soviet Union a class of machines called "Spectrometric" or "monoenergy" cyclotrons is being developed. These are very large low-field separated-sector cyclotrons which use powerful multicavity radio-frequency systems to achieve discrete turns in the accelerated beam. These machines are predicted to be capable of very-high-energy resolution approaching 10^{-4}. It is suggested that they are being developed to give the Soviet Union a high-resolution nuclear spectroscopy capability that they have never had because of the lack of development of dc accelerators. A second exceptional overseas project is the plan being developed at Daresbury in England for a tandem Van de Graaff with a 30-MV terminal voltage. This would bring tandem Van de Graaff capabilities to the energy range that has been solely the province of fairly large cyclotrons.

The two new medium-energy electron linear accelerators have important new capabilities. The Saclay electron linear accelerator has a maximum energy of 600 MeV with a duty factor of 2%; the MIT linac scheduled to be completed in 1972 will have an energy of 400 MeV with a duty factor of 1.8% or 200 MeV at a duty factor of 5.8%. The importance of these two new machines rests on the fact that their duty factor is more than an order of magnitude larger than has been typical for past linear accelerators. This will make practical coincidence measurements that were formerly not possible. The new facilities are designed with heavy emphasis on high-resolution experiments. The MIT facility is provided with a novel very-high-resolution spectrometer for elastic and inelastic electron scattering utilizing the energy-loss design concept to gain substantial improvements in resolution and usable beam intensity.

Accelerators for Low-Energy Particles The new accelerators for low-energy particles encompass a wide variety of accelerator types and sizes ranging from small accelerators of a few MeV for protons to a 40-MeV isochronous cyclotron planned for the University of Tokyo and a new double MP tandem installation at Brookhaven National Laboratory. The double MP tandem will provide superb proton, deuteron, and alpha particle capability and can provide very good beams of the lighter heavy ions as well. The Cyclo-Graaff scheduled for operation in 1971 at Lawrence Livermore Laboratory (LLL), is essentially identical to the one at Duke University. It combines an FN tandem with a commercially built 15-MeV H⁻ cyclotron. This interesting combination of machines provides a useful blending of the characteristics of both accelerator types. At lower cost it provides nearly the same total energy as the double MP tandem with the same smooth energy variation, but it suffers from a slightly lower energy resolution and lack of heavy-ion capability.

Most of the smaller accelerators in this class represent extensions of existing programs or small new programs at degree-granting institutions in recognition of the fact that some research activity is necessary to maintain a competent staff. A few of the machines listed are directed primarily toward neutron programs. These are the T-11 high-current tandems at Ohio University and in Athens, Greece, and the Dynamitron installations at Argonne National Lab-

306

oratory and at the State University of New York in Albany. These machines will be equipped with nanosecond pulsing capability for time-of-flight measurement of the energies of particles emitted in nuclear reactions.

The electron accelerators listed here are in response to two principal uses. The ORNL linac, ORELA, is optimized for the production of short neutron bursts of very high intensity; both the burst width and the repetition rate of the burst are adjustable over a wide range. The electron linac at LLL is a dual-function machine. It is provided with a short-pulse, variable-repetition-rate capability for neutron work and can also give rather long bursts of electrons for research with electron or photon beams. The lower-energy machines in this class are used mainly for the production of secondary photon beams. The 30-MeV superconducting linac at the University of Illinois is especially noteworthy because it may later become the accelerating unit for a 600-MeV cw microtron. The 30-MeV electron linac at the Los Alamos Scientific Laboratory is actually a prototype test section for the 800-MeV proton linear accelerator. But because it can operate at very large duty factor, in excess of 6%, it has found application in important experiments.

Accelerators for Heavy Ions. Finally, in reviewing the category of accelerators for heavy ions, we see that in the United States there is a single project under way scheduled for completion in 1971. This is the Lawrence Berkeley Laboratory super-HILAC, which is actually a conversion of the existing heavy-ion linear accelerator now limited in energy to 10 MeV per nucleon for ions below mass 40. The conversion will yield 8.5 MeV/u throughout the periodic table. Unless one or more of the pending proposals for new heavy-ion accelerators are approved, the super-HILAC will be the only accelerator in the United States capable of participating in the search for super-heavy elements.

In other countries there is intense interest in this field. In Germany two projects are under way. The smaller of these will provide a linear accelerator booster for the MP tandem at the Max Planck Institute, Heidelberg, to produce useful beams of ions as heavy as xenon. A much more elaborate project, the UNILAC, is to be located at Darmstadt, Germany. It will provide over 10 MeV per nucleon for the full range of ion masses through uranium. As an indication of the seriousness of the German effort, a new institute has been formed to build the accelerator and direct the research programs. A number of the neighboring universities will be participants.

At Orsay in France a cyclotron with linear accelerator injector came into operation in 1969. So far, the system has accelerated krypton ions to an energy of approximately 5 MeV/u. The beam intensity is low, but eventually the machine will have important capabilities. In the Soviet Union a 3.1-m Lawrence-type cyclotron is being converted to a 4-m isochronous machine. It is large enough to accelerate heavy ions to fairly high energies directly from the ion source. For example, it will accelerate krypton 7^+ ions to 4.2 MeV per nucleon. The Dubna laboratory also maintains a very large heavy-ion source development program. In Scandinavia, a joint facility is being planned to be located at the Niels Bohr Institute, Risø, Denmark. The accelerator, called NORDAC, is to be an isochronous cyclotron with a tandem injector. The use of the existing Riso tandem with a large four-sector cyclotron seems to be the preferred plan.

307

FIGURE II.9   Aerial view of the Los Alamos Meson Physics Facility. Features 1-mA proton beam at 800 MeV, plus simultaneously either 100-μA hydrogen ions at 800 MeV or 1-μA polarized hydrogen ions at 800 MeV. Beams of protons, neutrons, muons, and neutrinos will be available for simultaneous use in a multidisciplinary program.

After the completion of these U.S. machines in 1972 to 1973, there are at present no authorizations for any additional frontier-opening accelerators in this country. Nevertheless there are urgent needs in the nuclear-science community. Some of these are analyzed in the next sections.

5.3.1.1 *Heavy-Ion Accelerators*   Heavy-ion studies form one of the important frontiers of nuclear physics. To plunge into these problems, the capability of accelerating heavy nuclei to energies high enough to penetrate the Coulomb repulsion of even the heaviest target nuclei is needed. Further, this must be done so that the important parameters can be varied with delicacy and flexibility if the new nuclear physics is to be systematically explored.

The super-HILAC now under construction is a rebuilt, much improved
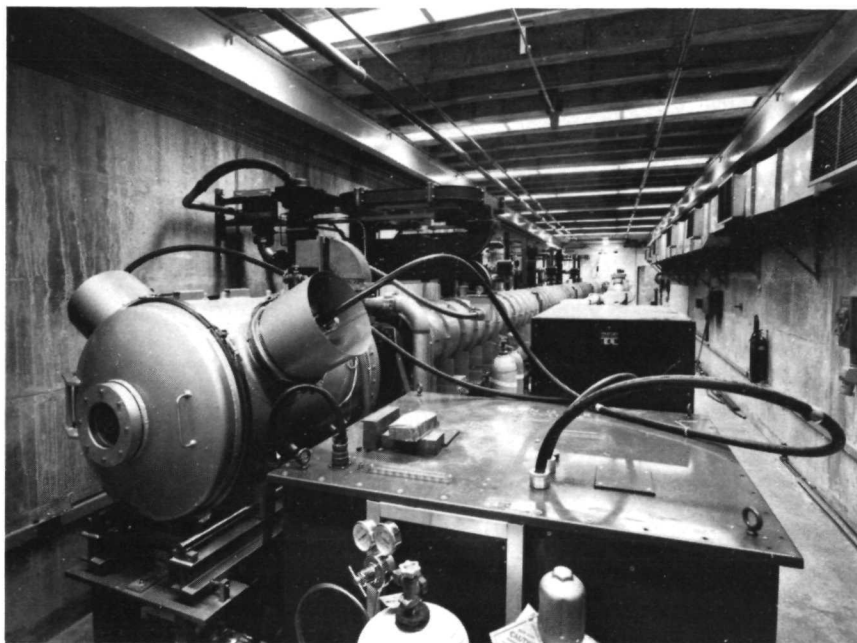
FIGURE II.10   Photograph of the 75-ft-long electron linear accelerator at the Oak Ridge National Laboratory. High current (>15 A), short (3–30 nsec) pulses from the electron gun in the left foreground are accelerated by radio-frequency traveling waves in the accelerator to 140 MeV and strike a tantalum target to produce short intense bursts of neutrons in a heavily shielded room in the background.

linear accelerator with heavy-ion beam capability throughout the periodic table. It seems clear for at least two principal reasons that one or more additional machines are urgently required. The number of people anxious to work with heavy ions greatly exceeds the capacity of a single machine, and the beam characteristics and instrumentation required for many experiments are different than those needed and planned for the super-HILAC.

There are, at the present time, two well-established methods capable of accelerating the full range of heavy ions to energies above the Coulomb barrier for all masses: the isochronous cyclotron with suitable injector and the linear accelerator. During the last three years a large number of groups have submitted proposals to the various U.S. Government agencies for funds to make possible the design and construction of a cyclotron injected by a tandem Van de Graaff. Such a combination machine can be designed
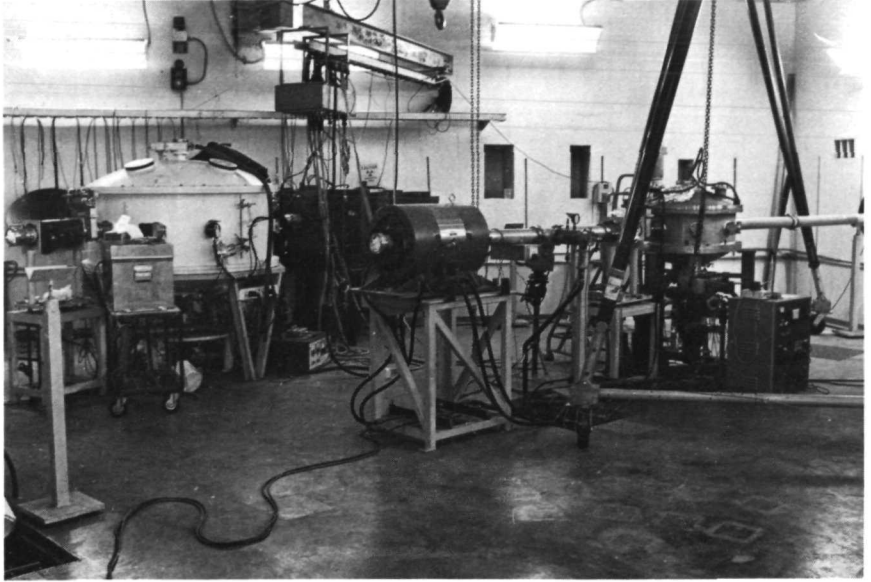
FIGURE II.11 View of the experimental area of the University of Maryland iso-chronous cyclotron showing one of the major physics scattering chambers and the smaller scattering chamber for nuclear chemistry. This cyclotron is designed to provide 145-MeV protons, 95-MeV deuterons. The energy of other ions is approximately 185 q^2/A.

to give virtually all the desired and needed beam characteristics including large intensity, precision energy definition, capability of changing energy in suitably small steps, and capability of accelerating all existing ions to needed energies. The possibility of accelerating heavy ions by the linear accelerator system is well established. Ways of using this method for the production of precisely controlled and varied beams are being actively explored, mainly in West Germany.

A third possibility requires extrapolation of the present technology. This is the "super-voltage" tandem electrostatic accelerator. As the potential of a tandem is raised, the accelerator becomes rapidly more effective for the acceleration of heavy ions, since the efficiency of electron stripping rises very sharply with ion energy. As the potential rises above 10 MV, the stripping efficiency takes a large leap upward. At 30 MV it has increased many times, and at 50 MV enough electrons have been removed to solve the whole problem of heavy-ion acceleration throughout the entire periodic table.
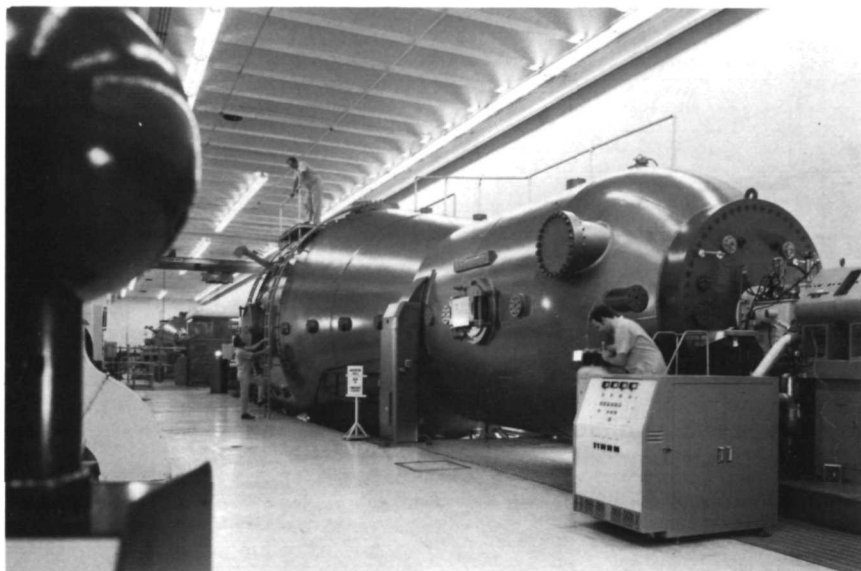
FIGURE II.12   The double tandem Van de Graaff at Brookhaven National Laboratory for high-resolution spectroscopy and for research with heavy ions. This accelerator will provide 30-MeV protons and deuterons, 40-MeV alpha particles, and ~100-MeV $^{16}O^{8+}$ ions.

5.3.1.2 *Super-Voltage Electrostatic Tandems*   Analysis of the technology indicates that a 30-MV electrostatic accelerator could probably be built at the present time. It is reasonable that with additional work in development the potential might be extended to 40 MV or perhaps even 50 MV, although the latter is not yet certain. As indicated above, a 50-MV tandem would bring the whole of heavy-ion work within its capability. Further, such an accelerator would yield protons of 100 MeV and alpha particles of 150 MeV with the superb energy resolution and voltage variability traditional with Van de Graaffs. This would then make possible precision measurements of high-lying nuclear states not now visible with the lower-energy resolution now available at these energies.

A novel and promising development of electrostatic accelerators is the Pellatron, which incorporates a completely new charge transport system. One machine based on this principle (9-MeV terminal voltage) has already been built for Brazil, and others are on order for other countries.

5.3.1.3 *Ion Sources*   An ion source is the basic starting point of all particle accelerators. The source is a device for converting molecules of any
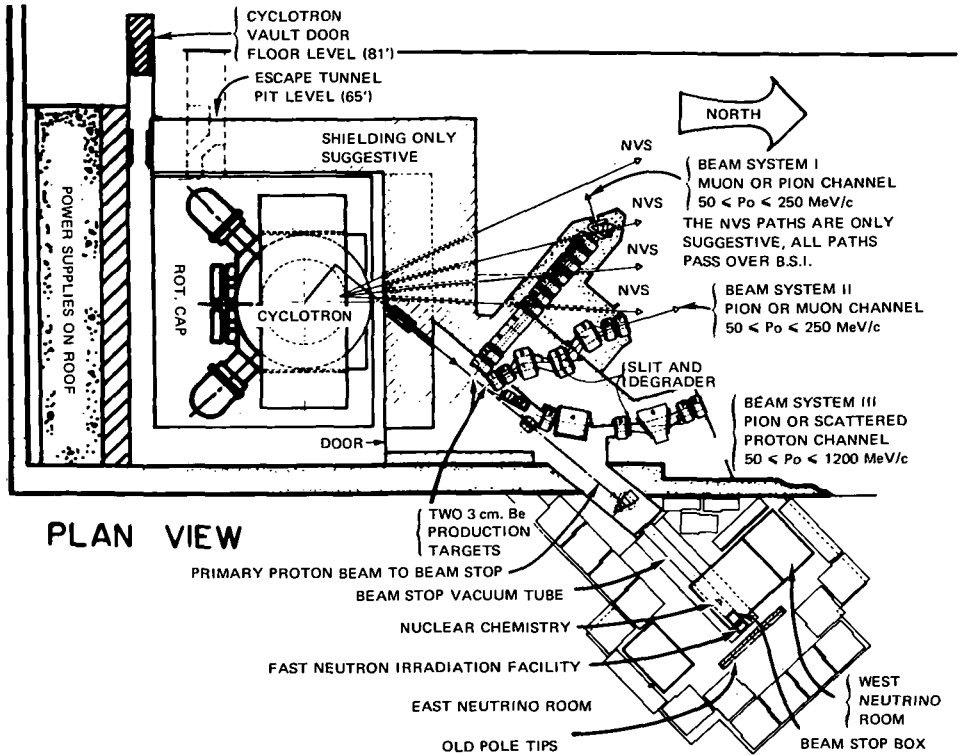
FIGURE II.13   Schematic drawing of the Columbia University Nevis Synchrocyclotron showing the overall layout of the cyclotron plus external beam path to an external underground beam stop. The secondary (pion–muon–scattered proton charged particle) beam lines through the main shielding to the experimental area are shown. An alternate pulsed neutron time-of-flight spectroscopy operation uses several paths. This facility will be especially useful for proton and meson experiments requiring intermediate intensity and energy.

desired element into ionized atoms by stripping electrons, usually in an intense electrical discharge. In the case of heavy-ion acceleration it has long been realized that enormous simplification would result in the acceleration system if a method could be developed for stripping very large numbers of electrons from atoms. Of course, the inner electrons of atoms are tightly bound and require substantial energy and time for removal. Also the ionizing process must in principle be accomplished in very high vacuum lest the ions, once stripped, quickly recombine. In view of the high stakes,
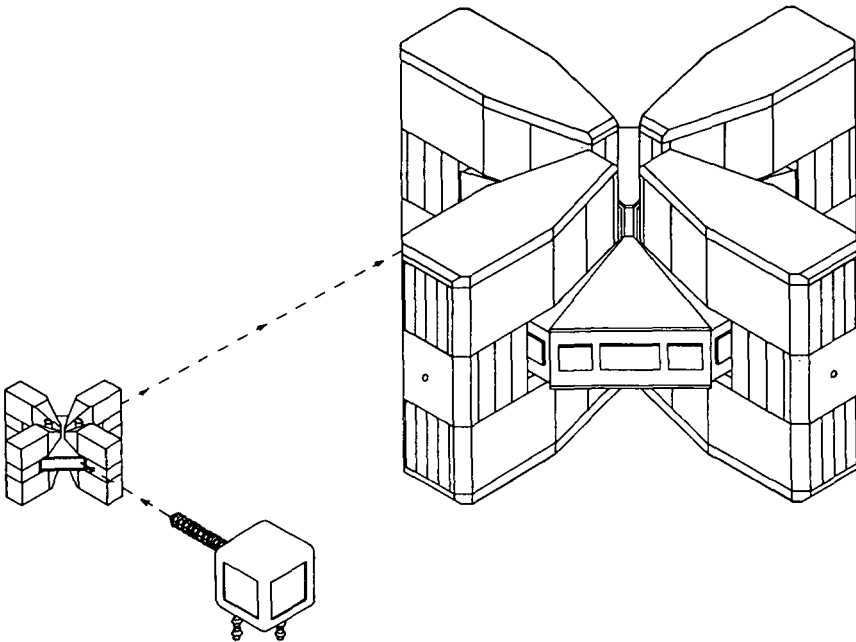
FIGURE II.14   Schematic layout of the Indiana University cyclotron facility. The
cubical box represents a potential-drop preinjector containing the source of ions. The
dashed arrows indicate the beam path. Beams are injected into the small injector cy-
clotron stage where their energies are increased by a factor of 30. The extracted beams
are then injected into the final stage for acceleration to their final energies. This facil-
ity will be capable of accelerating protons to energies up to 200 MeV. In the initial in-
stallation, heavy ions ($Q/M = 1/4$ or less) can be accelerated to maximum energies of
approximately 240 (Q^2/M) MeV.

heavy-ion-source research seems worth a substantial effort in spite of these
difficulties.

In light elements it is fairly easy with conventional high-performance
sources to strip enough electrons so that the charge-to-mass ratio of the
ion is of the order of 0.3. If additional research would reveal how to ex-
tend this performance to very heavy ions we might obtain, for example,
uranium 80+ (charge-to-mass = 0.3) or even 60+ (charge-to-mass of 0.25).
Then, any ordinary high-quality cyclotron would be able to accelerate all
or nearly all of the elements in the periodic table. The economies in the
construction of future heavy-ion accelerators would be enormous.

A careful search and study need to be made of all conceivable methods

of imparting high energy to molecules in a small region and in a high vacuum. Techniques used by the thermonuclear power groups should be carefully explored. Some advanced ion-source work is now under way in the Soviet Union on an electron-beam ion source; in this particular area it is clear that the Soviets are substantially in the lead. The only substantial recent effort in the United States, which went by the name HIPAC (high-potential positive-ion containment device), was discontinued for lack of funds. What seems to be needed in this country is a coherent, unified, and carefully planned ion-source program.

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5.3.2   *Far Horizons*

One can perceive several developments still in early stages that require substantial additional efforts but that may be of great importance in coming years.

5.3.2.1   *Superconducting Linacs*   The electron linac, developed in the early 1950's, made it possible to bring intense beams out of the machine into the laboratory. The push to extend them to higher energies and duty factors was blocked by a formidable set of technical problems. The intense radio-frequency electric fields required for acceleration were accompanied by very large power dissipation in the structures (radio-frequency cavities) producing these fields. In the last decade new, powerful generators of radio-frequency energy were devised—for the benefit not only of nuclear-research capabilities but, as already noted, to the technological applications they made possible. The intricate problems of stabilizing the linear array of radio-frequency fields and the dynamics of acceleration too were solved. In spite of these important developments, the problems of generation and removal of the radio-frequency power could only be practically managed by operating the linacs with small duty factor, that is by turning the power on in short intervals for a small fraction of the total time. Duty factor is the fraction of the time in which the machine is activated.

To advance by straightforward extensions of these methods to higher energies and higher duty factors would involve prohibitive power generation and dissipation problems and the aggravation of the associated problems. Even when these could be handled, the costs clearly put it out of the question.

Superconducting structures, kept at the very low temperatures of liquid helium, $-271°C$, exhibit dramatically low resistivity and thus appear to provide a way out. An electron machine based on this concept was undertaken; tests on the prototype systems have shown that the radio-frequency

power can be held to extremely low levels—of the order of a few watts—an improvement of radio-frequency handling efficiency of a factor of a million. The superconducting technology, using lead or niobium structures and circulating liquid helium as the coolant, is a stretch of the current art but one whose feasibility appears promising. That the design of a superconducting linac will not be dominated by power-dissipation problems, as is the room-temperature technology, is an important technical dividend; it also appears that the accelerating electric fields may be much higher than are ordinarily used. This means that the scale of the machine can be correspondingly decreased with the pleasant consequence of scaling down of costs to an attractive level. Because the power-dissipation problem has disappeared, the duty factor, or the fraction of the time the power is on, can be brought to the long-dreamed-of 100 percent. A panoply of interesting nuclear experiments then becomes possible because the beam is distributed uniformly, and therefore efficiently, in time.

Development of the superconducting electron machine is now under way. The success of such a device would open the interesting possibility of superconducting acceleration of protons and other light and heavy nuclei. The electron is most amenable to this technique, involving high-frequency accelerating fields. It is light and so reaches the constant velocity of light soon in the acceleration process. The necessity to tailor the structure to match variations of velocities is obviated. At the high velocity of light the sizes of the structures and the radio frequencies needed turn out to be convenient to our present technology. Because protons or other nuclei are heavy, their velocities will be low and will vary sharply in the energy range of importance. The acceleration of nuclear projectiles has, then, new developmental problems beyond those faced in the electron machine—but problems that ingenuity and application are likely to surmount.

5.3.2.2 *Electron Ring Accelerator*   Studies are actively under way in several laboratories to explore a very different kind of accelerator, based on an idea first put forth by Veksler in 1955. The basic idea is to trap nuclei into a stable cloud of electrons and then to accelerate the electrons. Since acceleration of electrons to a velocity near that of light is relatively easy, and since the nuclei are inescapably dragged along, a simple way of obtaining high-velocity and, therefore, high-energy projectiles seems to be offered. Calculations support the feasibility of this accelerating mechanism.

Realization of this collective acceleration idea in a practicable scheme is in the developmental stage. The stable cloud of electrons may be provided in the form of a ring generated by the injection of electrons into a focus-

ing magnetic field. The acceleration of this ring of electrons by the proposed innovation of expanding the ring into weakening magnetic fields or by the more conventional one of using a sequence of electric fields are both being developed.

The Dubna group has recently announced that it has achieved the acceleration of alpha particles to 29 MeV at its collective-method test facility. This is an impressive developmental step forward and should greatly encourage continued vigorous development in the Soviet Union and in this country.

If this is all carried off, nuclei will be accelerated at the low price of accelerating electrons. It would make for a vastly increased nuclear capability at a small fraction of present costs. The consequences would be enormous.

5.3.2.3   *Kaon Factory*   If it becomes possible to build very large proton accelerators with reasonable cost it may be economically feasible to construct a very-high-intensity 5–10-billion-volt accelerator. Such a high-energy, high-intensity machine would be a copious producer of the heavier mesons—kaons. This might be termed a "kaon factory" in the same sense that the Los Alamos 800-MeV proton machine is now called a "meson factory." With present technology, this is a project of great difficulty and expense; its full promise lies with the new technologies. The usefulness for nuclear physics of such probes, as has been discussed earlier, is potentially great. The use of present high-energy accelerators to carry out pilot experiments of this kind appears feasible and should go forward immediately.

5.3.2.4   *The Intense Neutron Generator or Neutron Factory*   Another concept somewhat beyond today's technology but of possible great importance both for nuclear research and for technology is the concept of the neutron generator. It is possible by impinging extremely intense beams of protons of an energy near 1 GeV on heavy-element targets to produce the most intense fluxes of neutrons ever generated. These fluxes themselves would be valuable for investigation of low-cross-section processes. They will also make possible the speeded up testing of components for power reactors that must survive the high fluxes inside such devices. The secondary particles—mesons and neutrinos—would also be invaluable nuclear probes.

The main long-range importance of such a machine could lie in its nuclear power production uses. The hope is for "electronuclear breeding" by generating enough neutrons to breed fissionable material. If this can be done efficiently with a net gain, a new power-generating method may exist

whose long-range importance requires careful investigation. The basic idea is not new. The new question is whether the new accelerator technologies will now make it feasible.

5.4 INSTRUMENTATION

5.4.1 *Introduction*

Accelerators and reactors provide bombarding particles that enable us to cause nuclear reactions at our command, but we could learn little or nothing from these reactions without instrumentation to detect and measure the radiations they produce. The development in the 1960's of a new kind of radiation detector and the spectacular advances during the same decade of electronics and computer technology have made it possible to learn things about nuclei that could not have been discovered before. Chapter 3, on applications of nuclear physics, touches on how some of this work affects our society; here we shall outline how advances in detectors, electronics, and computers blend together in the creation by nuclear physicists of a dramatically enlarged capability both for nuclear physics and for the whole technology.

5.4.2 *Detectors*

The detection and measurement of nuclear particles is most fundamental to all nuclear experiments and, therefore, to basic nuclear instrumentation. Detection of electrically charged particles has, since the beginning of nuclear physics, depended on the ionization that these particles produce in traversing gaseous, liquid, or solid material. The improvement in capability of detection devices from the early Geiger counter to the semiconductor detector of the 1960's is indeed spectacular when one remembers that the Geiger tube provided only an indication that a particle had passed through, whereas the semiconductor device measures the energy to better than a tenth of a percent. Energy resolution is only part of the advance; the time resolution of a thousandth of a second available to the Geiger counter has been shortened to a billionth of a second for the semiconductor—and even fractions of that billionth for plastic scintillation counters.

Although Geiger published a description of a radiation counter in 1913, it was not until the 1930's that gas ionization counters were developed to the point of being useful for energy measurements. Scintillation counters were developed in the late 1940's and the 1950's. The semiconductor detector that has again revolutionized nuclear science is the child of the

1960's. Each of these devices still has its special utility, but enormous changes have taken place in the quality with which routine energy measurements can be made. Nothing illustrates this more simply than a comparison of nuclear spectra obtained with scintillation detectors—themselves a great advance—and those with a modern semiconductor device (see Figure II.15).

The usefulness of the new detectors can readily be understood. Their energy resolution—from 2 kV for 1-MV gamma rays to 50 kV for 50-MV protons—matches that needed to separate the components of a nuclear spectrum. Gamma rays, protons, alpha particles, or heavy ions are within their capabilities. The time for detection of charged particles is very short—within a billionth of a second for protons—so that they can be used for measuring short decay times and can be used in high-counting-rate experiments. They can be put together into particle-telescope arrays or used in parallel to determine the time coincidence of two nuclear radiations. Their expense is relatively low—a few hundred dollars for the detector itself and several thousands for the associated electronics. As a result they have almost taken over in nuclear spectroscopy.

The first semiconductor detectors used for nuclear purposes were made from transistor grade germanium and silicon. Even these very pure materials have high enough impurity contents to render them unsuitable for making counters of sufficient thickness for many nuclear applications. A technique called lithium drifting has been used to compensate for the impurities, and, indeed, many of the counters now in use have been produced by this technique. Unfortunately, lithium-drifted counters are very sensitive to radiation damage, and the number of particles they can count be-
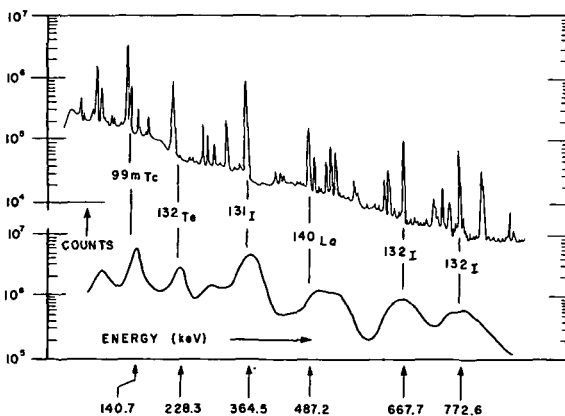
FIGURE II.15 Comparison between gamma-ray spectrum of gross fission products obtained with scintillation (lower curve) and germanium detector (upper curve).

fore they become useless is severely limited. Furthermore, lithium-drifted germanium counters must be used and stored at liquid nitrogen temperature. Recently, germanium crystals of purity nearly sufficient to eliminate the need for lithium drifting have been grown. Also experiments with new materials—cadmium telluride, for example—show promise of developing counters that can be used at room temperature. The scintillation detector has been eclipsed where good energy resolution is important by the semiconductors with their tenfold improvement. However, when good energy resolution can be given up for high detection efficiency or when higher timing accuracy is important, the scintillation detector comes into its own. New, ultra-fast scintillators have been developed, and improvements in phototubes have been made in the past decade.

5.4.3 *Magnetic Spectrographs*

We cannot cover all the detector work of instrumentation possibilities in this brief report. It would perhaps give some feeling for the range of facilities needed and the opportunities opened in nuclear research to review recent developments on magnetic spectrographs—a system used since the early days. The magnetic method of analysis has in every period been capable of the most accurate energy measurements. For example, the present generation of spectrographs is capable of from five to ten times the energy resolution of the semiconductor detectors. They are, however, complementary rather than competitive devices. The magnetic spectrograph is a massive, expensive facility and hence is used only when direct use of semiconductor detectors is precluded because of special conditions. Two such conditions are not uncommon in nuclear experiments. One occurs when even the good resolution of semiconductor detectors is still not adequate, and the other occurs when a very-low-intensity radiation must be detected in the presence of a very-high-intensity radiation of another kind or another energy. In that case nuclear reactions produced in the detector itself by the unwanted high-intensity component can produce a background that obscures the low-intensity radiation, or the unwanted component may saturate the electronic system used with the detector. Magnetic analysis can be used effectively in these situations.

Substantial advances in magnetic analysis systems have taken place in the last ten years. The resolving powers, aberrations, and precision of the magnetic characteristics have been improved. Broad-range and wide-angle spectrographs permit wide acceptances without sacrifice of resolution. Multispectrometers have been built that permit measurement of an entire angular distribution in one exposure. But most dramatic has been the

automation of the read-out of the spectrograph record. In the end, the charged particles, analyzed by the magnetic fields, must be counted by some device. The method most often used is to count the tracks that the particles leave in nuclear emulsion plates; this slow and inefficient read-out by observers peering through microscopes is giving way to automatic plate-scanning. However, the great advance, now in the making, is the development of highly accurate and reliable "position-sensitive detectors," which translate the position of a particle traversal into an electrical output that can be counted quickly and accurately by the electronic techniques developed over the years. Semiconductor detectors, the proportional counters of earlier days, and spark counters first developed in high-energy particle-physics work are being adapted for these uses. When fully developed they will revolutionize the magnetic spectrograph. They will also be used in a wide range of nuclear applications.

5.4.4 *Electronics*

Detectors are only part of a complex system used in a nuclear experiment. In fact, when one visits a nuclear laboratory one is hard pressed to find the detector under the towering dominance of banks of amplifiers, discriminators, pulse-sorting devices, and computing equipment. Detectors are used singly; in pairs to form telescopes; and in arrays to provide energy measurements, particle identification, and spatial and time distribution information. The gathering and recording of this information for complex nuclear measurements is routinely reliable only because of the availability of precise and stable electronics.

A dramatic improvement in one component of a system can be useless if the rest of the system is not of commensurate quality. Thus the exciting new possibilities for nuclear measurements opened up by the semiconductor counter would not be possible were it not for spectacular advances in electronics and data-handling techniques that parallel the advances from the Geiger counter to the semiconductor detector.

The electrical signal from a semiconductor detector contains vastly more information than the yes or no output from a Geiger tube; the amplitude of the electrical pulse measures the energy deposited in the detector. Thus the goal of the electronic equipment is to measure and record the pulse amplitude, keeping track of the fine gradations that are meaningful because of the high resolution of the detector.

The essential backup equipment of a Geiger tube could consist merely of a mechanical register to retain a tally of the number of times the tube fires. By extension, a semiconductor detector would require about ten

thousand registers to keep track of the ten thousand or so gradations in pulse height that a single detector is capable of resolving. To accommodate these new demands, new developments were required along three lines: the amplification of the pulses, the measurement of their amplitudes, and the recording of the amplitude information. We shall now comment on these three lines of development.

The amplifier that strengthens the feeble impulse from the detector must be of sufficiently good quality that the fine gradations in pulse height are not smeared into each other by amplifier noise. Pulse amplifiers that introduce so little noise as to produce only insignificant smearing of the pulse-height spectrum are now available. These owe their existence in no small measure to the development of the field-effect transistor and to the results of sophisticated mathematical research into the problems of pulse shaping carried out by specialists in the design of nuclear amplifiers.

A sophisticated sorting device is needed to determine which of the ten thousand or so sorting bins the amplified impulse belongs in. The "bins" clearly cannot be mechanical registers, not only because of the required speed of response but even more because the scope of the subsequent data handling clearly transcends the realm of pencil and paper bookkeeping. The bins must be components of computer memory capable of being manipulated in millionths of a second.

Advances in electronic sorting of the amplified pulses have kept pace with the other dramatic improvements that we have pointed out. Pulse sorting is done with an analog-to-digital converter (ADC) that translates the pulse height into a number representing the "bin" in which the pulse belongs. In 1960, one could buy an ADC with 256 channels that took 260 μsec for the conversion. By 1970, the number of channels had gone up more than 32-fold, and the conversion time down by a factor of 50. This improvement in the last ten years came about through engineering advances and inventiveness on an international scale.

Perhaps the most important advance in data handling was the introduction (in 1956) of magnetic core memories into nuclear spectroscopy instrumentation. These memories replaced the mechanical registers for storing the tallies in each bin of the pulse-height spectrum, and their introduction marked the beginning of a trend that has made the electronic computer an integral part of nuclear-physics apparatus. This development has also had a far-reaching effect on the computer industry.

The introduction of magnetic core memories into nuclear pulse-height analyzers meant that the nuclear spectrum was stored in the same form as that used for computer data in general whether it was part of a scientific calculation or part of business bookkeeping. Recognition of the similari-

ties in the needs of nuclear experiments, scientific computation, and business bookkeeping led to the production of general-purpose small computers that could handle a variety of tasks needed in nuclear experiments. Industry was motivated to produce these computers at prices lower than would have been possible had their sole application been in nuclear research.

5.4.5   *Computers in Nuclear Research*

The use of computers as part of the nuclear-physics experimental apparatus has made possible experiments that could not have been done otherwise. The easiest problem to see is that of the volume of data. Each spectrum from a semiconductor counter involves, perhaps, 4000 datum points. An experiment lasting several hours might produce many such spectra. Merely plotting the points with pencil and paper would overwhelm the experimenter, yet he must also fit peaks to calculated curves and perform other analyses of the data. Clearly such data handling is impossible without the aid of computers (Figure II.16).

Even when the volume of data is not overwhelming, computers have increased the efficiency of experiments and reduced costs by decreasing the amount of accelerator time needed. An experimenter can with the aid of a computer reduce his data rapidly enough to be able to interpret an experiment while it is still in progress. He can scan a mass of data in a few minutes that might have taken weeks or months to scan by older methods, and thus he can come to an immediate decision of what experimental course to follow.

A new use of computers in nuclear physics is for accelerator control. Many existing accelerators were built before computer control was economically advantageous, but new accelerators are being built to take advantage of the recent advances in computer technology and include provisions for computer control and computer diagnostic analysis of faults.

Present trends suggest that computer techniques will be integrated more and more into nuclear experiments right up to the detector. We can expect that in the future the computer will not be a separate, identifiable instrument that goes on one end of the nuclear experiment. Rather, each part of the equipment will in itself include computer-like functions.

An estimate of the total investment for on-line data-acquisition systems, and a breakdown of the expenditures into several categories, is available from the results of a survey made at the end of 1968 of 23 different AEC- and NSF-sponsored laboratories. Four national laboratories—Brookhaven, Lawrence Radiation Laboratory (Berkeley), Oak Ridge, and Ar-

FIGURE II.16   View of the oscilloscope display of the on-line computer used with the Maryland cyclotron. The display illustrates typical nuclear data accumulated during experiments and the interaction of the computer through a light-pen.

gonne—reported a total of 25 systems. The other 19 institutions, all universities, reported a total of 23 systems. Of the 27 different types of computer employed in those systems, all but four were machines designed for on-line operation. It is interesting that only four of the machines in the on-line class (two each of two types) were manufactured by the largest computer manufacturer; the others were all products of the new companies specializing in small computers. It is estimated that these systems represent an investment of about $12 million. In addition, the survey disclosed plans for additional systems, and for modifications of existing systems, estimated the cost nearly $5 million. Since the survey covered only AEC- and NSF-supported laboratories, and since a few of those did not supply any information, $17 million must be regarded as a considerable underestimate of the total investment in all nuclear-structure laboratories in the United States. It is safe to say that the total now exceeds $20 million. (This does not include high-energy-physics applications, of course.) Of this, it is estimated that about 39 percent was spent for the computers, 22

percent for standard peripheral equipment such as card readers and line printers, 30 percent for special data-acquisition gear required to transform experimental information into digital form and to transmit it to the computer, and 9 percent for the preparation of special systems software, that is, programs required for routine control of the operation of the whole data-acquisition system.

It is of some interest to learn what fraction of the investment in laboratory equipment can be expected to go for the computer system. According to an analysis of expenditures at nine different modern university accelerator installations, the ratio of the cost of the total data-acquisition system to the cost of the bare accelerator was rather consistently about 0.22. (Obviously this figure cannot be applied universally in planning new facilities because laboratories vary greatly in type and purpose.)

5.4.6 Management Problems in Accelerator and Instrumentation Development

The advances of the past decades in nuclear instrumentation and accelerator development have forged an enormously strong research enterprise. The development work of the nuclear research laboratories has been fed, via the instrumentation and accelerator industry, into the mainstream of technological applications. As has been noted in Chapter 3, these industries are small but crucial to an advancing technology. However, the basic designs and major innovations have been created within the nuclear research laboratories. For the sake of the future of nuclear physics itself and its impact on society it is important to keep these efforts at the strength essential to ensure continued progress.

In response to funding cuts of the last few years, to be discussed more fully in Chapter 6, some of the research laboratories have tended to support only the most cogent and most urgent projects aimed directly at obtaining new nuclear knowledge. The instrumentation and accelerator development projects, being part of the longer-range developmental goals, therefore, have suffered disproportionate funding contractions. While this is a very natural, and perhaps even wise, short-term response to funding exigencies, we do not believe that it should be permitted to become a longer-term pattern. While a detailed assessment is needed, some conclusions are already clear.

The nuclear instrumentation and accelerator development groups function within and as a part of the major laboratories—many of them at the national laboratories. The objectives and resources of these groups are determined mainly by the nuclear research needs.

Since we believe that the nuclear instrumentation and accelerator development work is important for the long-range future, we conclude that the overall nuclear research funds should be allocated so that this development work can be carried on vigorously, consistent with the whole nuclear effort. We believe that these efforts will be most fruitful when they are tied to the goals and opportunities of the research program, and we strongly advocate the continuation of such close ties. However, at the same time, both the budget allocations and the ties to the research program should be managed so as to ensure a sustained program. Accelerator work especially tends to be conducted in crash programs when a new facility is in the immediate offing, but a more uniform support might offer a richer long-range reward.

6   Organization and Funding Policy

6.1   INTRODUCTION AND SUMMARY

In the years covered by this report, from 1964 through 1970, the nuclear-physics capability of the United States has shown marked growth, based on the strong support the science has enjoyed over many years. Advances in experimental technology and in fundamental concepts are being exploited in an increasing number of forefront facilities, and it can fairly be said that the United States is presently in a position of leadership in its contributions to nuclear physics, in applications to technology, and in the production of broadly educated nuclear scientists capable of operating effectively in many scientific and technological areas. The steady growth of the field and its manifold extensions into other areas are tributes to the wise management of the significant national investment in nuclear research over the past 25 years.

The early years of nuclear science were characterized by an almost explosive growth in federal support. Even as recently as 1958–1964, the annual increment in federal funding was nearly 14 percent. From 1964 through 1967 the increase was much smaller—about 8 percent per year—

but still adequate to permit a substantial growth in output and a significant strengthening of the nation's research and educational establishment.

Since 1967, however, the growth rate of funds for nuclear research has declined abruptly. In terms of actual purchasing power, federal support in 1969 was about 2.5 percent lower than it was in 1967.* Including both federal and nonfederal sources, the total funds for basic nuclear physics in 1969 were just equal in purchasing power to those available in 1967 and slightly less than those available in 1968. Estimates for 1970 and 1971 indicate further diminutions of several percent in each year.

The real effect of the changing funding over the past years is best measured in dollars of constant purchasing power. Actual operating funds, excluding major capital investments, for basic nuclear research from 1958 through 1970 are shown in Figure 11.17 expressed in 1969 dollars. For comparison, the trend of the Gross National Product expressed in the same base is also shown. Of particular concern here is the abrupt change in slope in 1967–1968 when the federal support of nuclear research not only failed to rise with the Gross National Product but actually started to decrease in absolute purchasing power.

The steady diminutions over the past few years, in the face of sharply increasing needs of the science, present severe constrictions. While support levels have been dropping, research has inexorably become more expensive. In part these increases reflect the escalation of labor and other costs shared by many components of the economy. In part they reflect the increased sophistication and difficulty of the scientific questions being asked. Further, new facilities designed to attack new areas of nuclear physics are large, complex, and expensive. These increased claims on the available funding have created severe stresses that have been met so far by austerity measures that cannot be long extended without deleterious effects. It seems clear to us that continuation of present trends in funding will seriously threaten the viability of nuclear physics in the United States and drastically reduce the contributions it can make to other sciences and to technology.

If a science is to live, it must grow in depth. As it grows, the questions asked become more difficult and the answers more expensive. To continue its present progress, nuclear physics must move forward on several well-defined fronts. At least two of these, exploitation of high-energy probes and studies with heavy ions, require facilities and operations substantially

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* The decline in support may actually be appreciably greater. The effects of inflation have been included only as they apply to the economy at large. Further, an indirect source of support is rapidly disappearing. The downward trend in federal support of students receiving fellowships and traineeships, which began in 1968–1969, has continued more sharply into fiscal year 1970. Budget estimates for fiscal year 1971 reflect a further sharp drop.
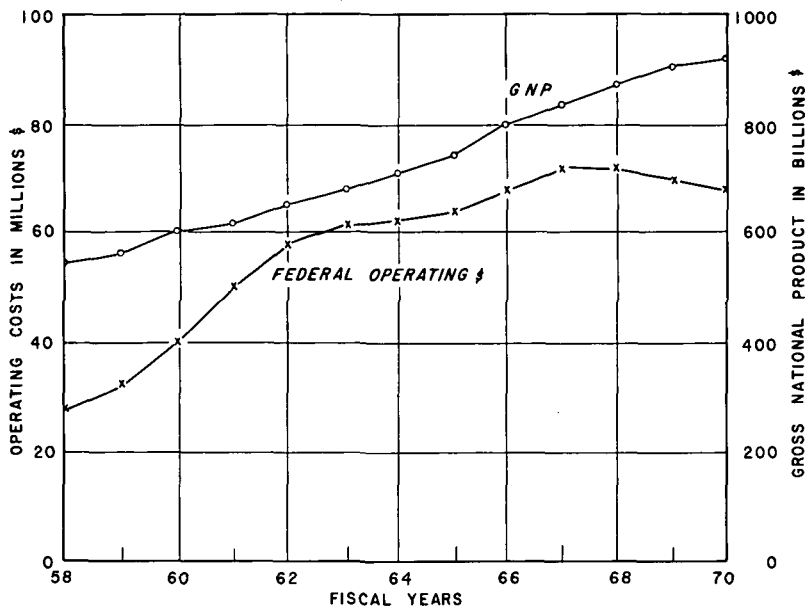
FIGURE II.17   The trend of federal support of basic nuclear-physics research oper-
ations for 1958–1970 compared with the Gross National Product, both in 1969 dollars.
Figures for 1964–1970 are from Table II.10; earlier values are from *Physics: Survey
and Outlook, Subfield Reports* (NAS-NRC, 1966), normalized and smoothed to match
in 1964–1965.

more costly than the average of those now comprising the nuclear-physics
program. The question is how to proceed along these new lines and still
support the already ongoing and still essential projects.

Quite evidently, the prospects for continued progress in nuclear physics
will depend sensitively on the level of funding and on the degree to which
the available resources are wisely distributed. In this chapter we present
analyses of several different levels of hypothetical future funding and dis-
cuss their consequences to the discipline of nuclear physics and to its con-
tributions to society. To make definite discussion possible we speak in
terms of specific distributions. The detailed choices of distributions are
controversial and debatable, and we do not put them forward as a blue-
print for the future of nuclear physics. We believe, however, that these
distributions are reasonable definite choices on which to form a discussion
of the effects of funding patterns on the health of the nuclear research
program.

We analyze first a budget that permits full exploitation of the oppor-

tunities of nuclear physics. Aggressive explorations in the use of high-energy nuclear probes, heavy-ion physics, and a prudent expansion of the present multifaceted program based on conventional accelerators form the major parts of this program. We find that there exists already an adequate base of facilities to support most of this ambitious effort, and that the rate at which the new fields can be explored is limited primarily by manpower. Such a program, actually requiring quite modest annual increases in funding levels, would, we believe, ensure an active and productive enterprise with the prospect of important new discoveries and many benefits to science and technology.

Although we believe that a full exploitation of the capacities of nuclear physics, and indeed of all science, will pay handsome dividends in the long run, we recognize that the budget increases required may not be available over the next several years. In order to help to answer the question of what is the minimum funding level that will keep nuclear physics productively viable in the United States, we have studied the consequences of a budget that maintains an approximately constant level of scientific effort. Such a budget, it develops, could adequately support the best of existing facilities and permit some exploration of the frontier fields of high-energy and heavy-ion research. While the scientific man-years of effort are constrained as approximately constant, with a turnover of personnel of a few percent per year, some hope is offered that places will be available to young scientists. There is, then, a reasonable expectation that the field will not only survive, but that new opportunities will not be overlooked. Entry into the new fields requires substantial incremental funding, starting in fiscal year 1973. Within the "classical" areas of nuclear physics, the budgets are held nearly constant. For this to be done in the face of increased sophistication of the scientific problems attacked and the consequent increased expensiveness of experiments, it is necessary to place emphasis on the most versatile facilities. Since this is an approximately constant scientific manpower budget, the increase in the new areas is obtained by a reduction in the other areas. The annual operating budget needed by 1977 would be of the order of 40–50 percent greater than that of the base year, 1969.

Over the past five years, the federal support of nuclear physics has been nearly constant (see Figure II.17). Since projecting this practice into the future requires either a level real budget, if inflation ceases, or one declining at 5 percent per year at current inflation rates, we have examined the consequences of both funding patterns. When analyzed in detail, the first possibility implies a highly uncertain prospect for productive viability, while the second requires abandonment of large segments of the subfield and may well disorganize it completely.

Under the constraint of a level budget—level in purchasing power—the

new and expensive ventures considered necessary for the viability of the field could be carried out only at the expense of drastic contraction of the rest of the program. When details are worked out in realistic terms, it develops that any reasonable distribution of the restricted resources among the various kinds of programs has the effect of forcing the closing down of many facilities and of forcing many talented people out of the subfield. The remaining program is heavily concentrated in relatively few facilities, with many presently highly productive groups becoming obliged to change to a user's mode of operation.

Finally, the effects of a budget declining at the rate of 5 percent over an eight-year period are studied. Much of the new ventures must be given up, and the multifaceted program is severely truncated. The number of institutions and scientists that can be supported is drastically cut.

We argue for the creation of a national program in nuclear physics that would best exploit the opportunities available—best for the field itself and its applications to technology and to society. We hope the analyses presented are useful data in the creation of that program.

6.2   REVIEW OF THE 1964-1969 FUNDING PATTERN

The pattern of expenditures for basic nuclear-physics research in the university and federal laboratories during the period from 1964 through 1969 is presented in Tables II.9 and II.10 and in Figure II.18.

Over the period from 1964 to 1967, federal annual operating expenditures for basic nuclear-physics research rose from $52 million to $66 million, an annual increase of about 8 percent per year. State and private contributions to university laboratories in this period for the same purpose increased from about $4 million to $6 million, from about 15 percent of their total operating budget to 18 percent.* The overall increase of federal and nonfederal funds of about 10 percent per year exceeded the 2.5 percent annual drop in purchasing power, to leave a growth rate of 7.5 percent to cover the expansion that took place in manpower and facilities and the increasing costs of the more sophisticated nuclear experimentation.

From 1967 through 1969, annual federal operating expenditures rose from $66 million to $72 million. Nonfederal support of the universities rose from about $6 million to $9 million, to 22 percent of their operational budgets. This diminished rate of growth—around 5 percent per year— has been canceled by inflation, leaving the total real purchasing power in

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* For definitions of operating expenditures see the second paragraph of the discussion of Table II.9.

TABLE II.9   Total Costs of Basic Nuclear-Physics Research in the United States 1964–1971[a]

Performer/Agency	Funds in $Millions, for Fiscal Year							
	1964	1965	1966	1967	1968	1969	1970	1971
Operations								
Laboratories								
AEC	27.6	28.0	32.3	33.8	34.4	35.7		
DOD	2.4	2.7	2.7	3.2	3.3	3.1		
NASA, NBS	1.0	1.0	1.0	1.3	2.0	2.3		
SUBTOTAL	31.0	31.7	36.0	38.3	39.7	41.1		
Universities								
AEC	13.2	13.7	15.8	17.7	19.0	19.0		
DOD	4.7	4.5	3.2	3.2	0.9	1.4		
NSF	3.2	3.3	6.0	7.0	8.4	10.3		
Nonfederal	(3.6)	(4.2)	(5.6)	(6.3)	(7.5)	(8.6)		
SUBTOTAL	24.7	25.7	30.6	34.2	35.8	39.3		
Total								
Federal	52.1	53.2	61.0	66.2	68.0	71.8	(∿71.4)	(<70.0)
Nonfederal	(3.6)	(4.2)	(5.6)	(6.3)	(7.5)	(8.6)		
TOTAL OPERATIONS	55.7	57.4	66.6	72.5	75.5	80.4		
Facilities								
Laboratories								
AEC	8.4	5.8	7.7	11.6	14.2	16.2		
DOD, NASA, NBS	(2.5)	(2.6)	(2.2)	0.1	0.4	0.3		
SUBTOTAL	10.9	8.4	9.9	11.7	14.6	16.5		
Universities								
AEC	5.7	4.1	5.3	6.6	5.7	6.6		
NSF	3.0	1.6	6.3	5.2	7.0	6.1		
SUBTOTAL	8.7	5.7	11.6	11.8	12.7	12.7		
Total								
Federal	19.6	14.1	21.5	23.5	27.3	29.2	(∿28.9)	(∿25.4)
Nonfederal	(1.7)	(0.7)	(2.9)	(3.3)	(3.4)	(3.1)		
TOTAL FACILITIES	21.3	14.8	24.4	26.8	30.7	32.3		
Scientific Man-Years[b]								
Laboratories	(498)	(480)	(507)	(525)	(520)	(520)		
Universities	(520)	(542)	(585)	(675)	(675)	(665)		
TOTAL	(1018)	(1022)	(1092)	(1200)	(1195)	(1185)		

[a] Numbers in parentheses are approximate.
[b] A scientific man-year (SMY) means a scientific staff member, faculty member, or postdoctoral fellow devoting full time to research for a 12-month period. For example, working at a local facility, dividing his time between research and his university responsibilities, a faculty member provides 0.5 to 0.7 of an SMY. In user programs at off-campus facilities, a particular scientist usually provides less then half an SMY.

Table II.9 exhibits the Panel's estimates of the total costs of basic nuclear-physics research in the United States for the fiscal years 1964–1969. To delimit the field of discourse as precisely as possible, we have adopted the following definitions:

1. Basic versus Applied Research*: "In basic research the investigator is concerned primarily with gaining a fuller knowledge or understanding of the subject under study." "In applied research the investigator is primarily interested in a practical use of the knowledge or understanding for the purpose of meeting a recognized need."

* *Federal Funds for Research and Development,* NSF 69-31, p. 95.

2. Nuclear Physics*: "Nuclear physics is here defined to include the study of nuclei, their structure, disintegration, interactions, and other properties: It includes also the study of the constituent parts of the nucleus, their interactions with one another and with nuclei."

The table distinguishes two categories or expenditures: "Operations" includes salaries, overhead, and expendable supplies necessary to an ongoing research program. "Facilities" includes minor capital equipment and major capital and construction items such as computers, accelerators, and buildings that permit expansion or redirection of a research program.

The two categories of performer considered are universities and laboratories, the latter including federally funded research and development centers (FFRDC), in-house government laboratories, and industrial laboratories.

Among the performers of basic nuclear-physics research are included certain high-energy (100- to 1000-MeV) installations, large synchrocyclotrons, and linear accelerators—most notably the Los Alamos Meson Physics Facility (LAMPF)—which engages in both nuclear-structure and meson research. Although the total costs of these facilities are included in our discussions it should be noted that about one half of the total budgets of these facilities is for nuclear-physics research. This estimate is subject to considerable uncertainty since particle-physics and nuclear-structure research have broad areas of overlap and mutual support. Generally speaking, we consider as primarily nuclear physics such studies as nucleon–nuclear scattering, meson–nuclear scattering, nuclear form factors, and mesic atoms. "Gray areas" include π-meson–nuclear scattering and nucleon form factors, while such experiments as decay properties of mesons we consider to lie in the realm of particle physics. The Chicago synchrocyclotron and the Stanford MkIII and superconducting linac projects are not included here, since they have been funded in the elementary-particle physics category. In addition to LAMPF, Nevis, Rochester, Carnegie-Mellon, LBL (184 in.), and part of SREL are included here.

Funding agencies supporting nuclear research include, within the AEC Division of Research: Chemistry, Low Energy Physics, and Medium Energy Physics; the Division of Reactor Development and Technology (RDT); and the Division of Military Applications (DMA). For the latter two, the separation between "basic" and "applied" research is necessarily somewhat arbitrary. We have chosen to base the distinction on the stated motivation—whether for devices or for new knowledge—rather than on the character of the work. Thus, for example, about $4 million in 1969 by RDT for reaction cross-section work has been excluded. On the other hand, some $9.5 million expended by DMA at Los Alamos and Livermore has been included as "basic," according to the judgment of the group leaders involved. In government laboratories, we have included only those programs clearly segregated from mission-oriented endeavors.

The funding figures of Table II.9 derive mainly from an informal survey conducted by the AEC, supplemented by numerous formal and informal reports from the AEC, DOD, NSF, and NASA, and further supplemented by a questionnaire distributed in February 1970 to nearly 150 research groups. For fiscal year 1969, this process led ultimately to a detailed listing of about 280 individual projects in 102 universities and 15 laboratories. Figures for earlier years were largely derived from the AEC report, with adjustments in categories based on the 1969 list. Nonfederal funding estimates came from the questionnaires, which were normalized to take account of incomplete response.

It should be noted, however, that because of some differences in definitions already alluded to, the present figures are in no sense official.

Our figures differ significantly from those of the annual NSF report *Federal Funds for Research and Development*, NSF 69-31. Detailed comparison with earlier editions in which nuclear-structure research is set forth separately indicates that the principal difference lies in the inclusion here of the DMA funds referred to above and funding by the AEC Chemistry Section of nuclear-physics projects: together these add about $19 million in fiscal year 1969. As compared with fiscal year 1964 figures given in the Pake report† our estimates of operating costs agree within $1 million if chemistry funds are subtracted. "Facilities" funds in the two reports are not comparable because the Pake report lists obligations, while we have used estimated actual costs. For large facilities, costs may lag behind obligations by several years.

The number of scientific man-years given in the table was obtained from the questionnaires. A "scientist" as the term is used here means a staff member, faculty member, or postdoctoral fellow. To normalize for incomplete coverage by the questionnaires, the reported numbers were normalized by the ratio of federal operating costs determined in the present study to federal operating costs reported in the questionnaires. For the government laboratories the questionnaire coverage was close to 100 percent for 1969. For universities the coverage for fiscal year 1969 was about 85 percent.

* By-laws of the Division of Nuclear Physics, American Physical Society.
† *Physics: Survey and Outlook. Reports on the Subfields of Physics* (National Academy of Sciences—National Research Council, Washington, D.C., 1966).

TABLE II.10   Total Federal Costs of Basic Nuclear-Physics Research by Supporting Agency

Agency	Funds in $Millions, for Fiscal Year							
	1964	1965	1966	1967	1968	1969	1970	1971
Operations								
AEC (Chem)	(8.9)	8.9	9.4	9.8	10.4	10.3	10.4	10.4
AEC (LENP)	(19.2)	20.0	21.6	22.5	23.4	24.3	24.0	22.5
AEC (MEP)	5.5	5.6	9.2	11.0	11.1	11.3	12.8	13.0
AEC (Res.) TOTAL	33.6	34.5	40.2	43.3	44.9	45.9	47.2	45.9
NSF (N.S.)[a]	2.7	2.8	4.7	5.0	6.4	8.0	6.5	9.3
(Theo.)[a]	0.5	0.5	0.8	0.8	0.8	0.9	1.0	0.6
(E.P.)[a]	0.0	0.0	0.5	1.2	1.2	1.4	1.1	0.0
NSF TOTAL	3.2	3.3	6.0	7.0	8.4	10.3	8.6	9.9
SUBTOTAL AEC AND NSF	36.8	37.8	46.2	50.3	53.3	56.2	55.8	55.8
AEC (DMA)	7.2	7.2	7.9	8.2	8.5	8.8	9.4	(9.6)
DOD	7.1	7.2	5.9	6.4	4.2	4.5	4.3	–
NASA, NBS	1.0	1.0	1.0	1.3	2.0	2.3	1.9	
TOTAL FEDERAL	52.1	53.2	61.0	66.2	68.0	71.8	∿71.4	<70.0
Facilities								
AEC (Chem)	(0.8)	0.8	0.8	0.9	1.1	1.3	1.9	3.3
AEC (LENP)	9.2	7.3	9.4	12.1	10.7	5.7	3.3	3.2
AEC (MEP)	(3.7)	1.3	2.3	4.4	5.7	13.1	20.8	18.7
AEC (Res.) TOTAL	13.7	9.4	12.5	17.4	17.5	20.1	26.0	25.2
AEC (DMA)	0.4	0.5	0.5	0.8	2.4	2.7	1.2	–
NSF	3.0	1.6	6.3	5.2	7.0	6.1	1.4	0.2
DOD, NASA, NBS	2.5	2.6	2.2	0.1	0.4	0.3	0.3	
TOTAL FEDERAL	19.6	14.1	21.5	23.5	27.3	29.2	28.9	(∿25.4)

[a] N.S., Nuclear structure; Theo., theory; E.P., elementary particles.

Table II.10 exhibits total costs of basic nuclear research sorted according to supporting agency, for laboratories and universities combined. The AEC Divisions involved are DMA: Military Application; Chem: Chemistry; LENP-MEP: Low- and medium-energy physics. DOD includes the Office of Naval Research, the Army Research Office, and the Air Force Office of Scientific Research. NASA and the National Bureau of Standards support some work in basic nuclear physics.

The medium-energy physics program of AEC involves work at energies above 50 MeV and less than 1000 MeV or, for complex projectiles, more than 10 MeV per nucleon.

It should be emphasized that certain agencies, e.g., AEC (Chem), here included as contributing to basic nuclear-physics research may have additional important reasons for their support that are beyond the scope of this Report.

Insofar as they are known to us, industrial contributions to support of basic research are included in the category "Nonfederal," in Table II.9.

1969 at a little below the level of 1967. The increase in state and private support has, in part, been in the nature of seed or rescue money. On the basis of the results of a survey of university research groups, it appears that continued growth of these sources is not likely in the future. Many univer-
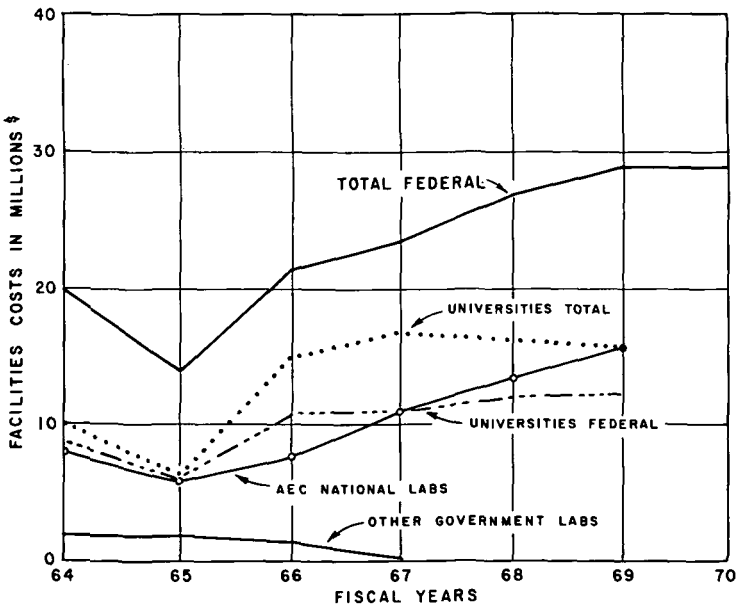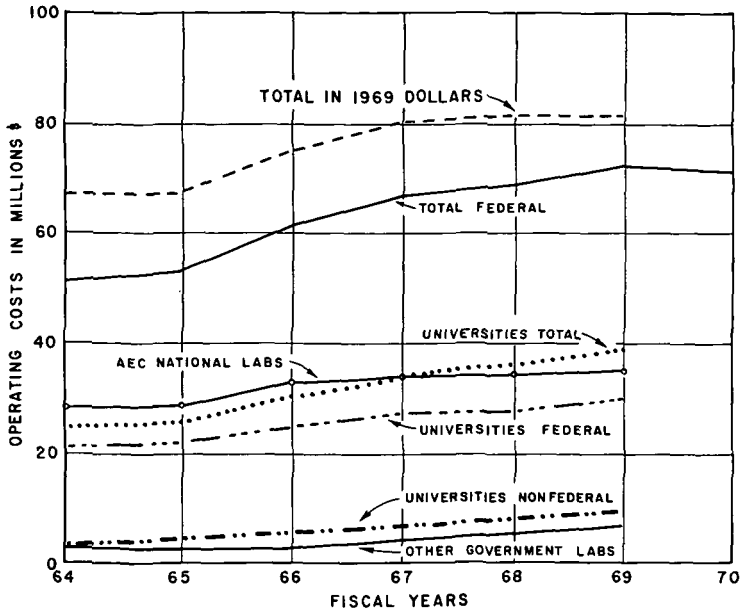
FIGURE II.18   Total costs of basic nuclear-physics research, 1964–1970.
Construction costs project to zero in about 1973, when presently author-
ized installations will be complete. No new authorizations are presently in
sight.

sities have indicated that they have reached the limit of possible nonfederal research support.

The total support for nuclear physics in the period 1964–1967, while increasing at a rate of 7 percent annually, fell well below the effective exploitation budgets for the period that had been developed in 1964 by the Panel on Nuclear Physics of the Pake Physics Survey; this growth rate, however, permitted the orderly development of a powerful and broad-ranging program of nuclear research.

The momentum thus developed has carried nuclear physics through the period of decreasing support since 1967, but there are already unmistakable signs of faltering. The momentum has been maintained by ever deeper retrenchment in instrumentation, in staff, and in the scope of research. This response can carry the field through short-range stringencies, but the longer it is maintained the heavier will be the toll exacted from the health and vitality of the field and the weaker will be the remaining response capability.

A questionnaire survey of some 150 university and federal laboratory groups revealed the changes that they have had to make. In order to retain trained staff, the lifeblood of research institutions, it has been necessary to reduce support of young postdoctoral scientists, to cut back on support staff, and thereby to waste the time and talents of highly trained physicists on routine support functions. Equally critical has been the need to give up the full exploitation of new developments in the experimental technology that keep facilities at their long-term scientific peak. In contrast to the small laboratories, the larger laboratories that maintain a variety of nuclear programs have been able to respond to funding constrictions by cutting off some areas of research and keeping the rest strong. This too can be carried on for only a limited time without lessening the advantages of multifaceted research efforts; the effect is already observable.

It is important not to be misled by apparently contradictory short-range consequences as has been pointed out occasionally in recent scientific journals. During initial periods of financial stricture in recent years it has been noted that the actual publication output from some groups has *increased*, and this has been adduced as evidence for greater efficiency and effectiveness in the field. Frequently this phenomenon is simply a reflection of general retrenchment in which a research group lives off its capital of instrumentation, ideas, and working progress, mortgaging its future against the short-range objectives of getting as much as possible of its output into print—often in less than its most effective or complete form—before possible worsening circumstances preclude this.

It is clear from the survey of the nuclear research community referred to above that further reductions in support levels would produce highly

amplified cuts in scientific productivity. The funds above fixed costs available to a laboratory are only a fraction of its operating budget, because there are sizable maintenance, material, and technical staff costs that must be met if the facility is to operate as intended. As these are cut away, scientific efficiency is lost. The community estimates that a further 20–30 percent cut in funds below 1969 levels would take most facilities, especially the largest and most advanced, past the "critical point" to a markedly lower level of scientific capability. This suggests that, from the viewpoint of research efficiency, further funding constrictions should not be met by across-the-board measures.

Of special concern for the future are the effects of the diminishing funding of the past few years. For both operations and facilities, expenditures lag behind obligations or appropriations by a year or more—in the case of facilities, often several years. Thus, while Figure II.18 shows continuing expenditures for construction of new facilities, these were mainly obligations undertaken several years before 1969. All the major facilities now under construction were started in 1966 or 1967; at this writing in fiscal year 1971, there is no immediate prospect of any comparable commitment for future facilities. Thus there is a lapse of three to four years, which implies an equivalent delay in access to new frontiers already recognized.

As one example, we cite the heavy-ion accelerator facility, for which a number of feasible designs have been in existence for several years. Taking into account the time required for administrative action on these proposals, the lag between authorization and actual funding, and the construction period, the most optimistic prognosis would not anticipate their entry into this field before 1975 or 1976. West Germany, France, and the Soviet Union are moving ahead aggressively. If we delay, we may well be accepting from the beginning an inferior status in areas of national consequence.

Before turning to estimates of future needs for research funds and possible responses to a set of assumed funding levels, it is useful to describe the structure of the nuclear research establishment.

6.3 THE INSTITUTIONAL STRUCTURE OF NUCLEAR RESEARCH

The institutional structure for research in basic nuclear physics has gradually developed over the past 25 years; this structure simultaneously encourages an intense and productive research effort, the training of young nuclear physicists, and fruitful ties to other scientific fields and to our technology. This section provides a brief summary of this structure and its institutions and sources of support.

6.3.1 *Sources of Funds*

The several sources of funds for basic nuclear research reflect the many applications of nuclear physics. One of the chief missions of the U.S. Atomic Energy Commission is to sponsor research into all aspects of nuclear science. The largest share of basic nuclear research funding comes from this source—about $55 million in operating funds in fiscal year 1969. The Low-Energy Physics, Medium-Energy Physics, and Chemistry Programs of the AEC Division of Research sponsor basic nuclear physics at universities and at the AEC's national laboratories. The Division of Military Applications sponsors within two of the AEC laboratories such basic nuclear research as is consonant with its special mission.

The National Science Foundation has dual missions—to support both basic research and education. Its operations budget for nuclear physics in university laboratories has grown gradually—from $3.2 million in fiscal year 1964 to $10.3 million in fiscal year 1969. It is the second largest source of funds for basic nuclear research.

The Department of Defense pioneered in the support of pure nuclear-physics research in the universities, but its support has been cut back over the years since 1965. It, however, has maintained a strong interest in its in-house facilities. The basic nuclear part of this interest has been included in the tables. The National Bureau of Standards and the NASA nuclear facilities carry the mission-motivated interest in nuclear physics of these agencies—a part of which is in basic nuclear research.

Basic nuclear-physics research receives support from a number of federal agencies and for reasons consonant with the mission of those agencies. The priorities that these agencies place on the field, and the various components of the field, are different. These differences may become increasingly important during funding exigencies.

Nonfederal support, as noted above, is vitally important to nuclear-physics research in the universities. Industry supports only a small basic nuclear-physics program.

6.3.2 *The Research Institutions*

University laboratories together with the AEC national laboratories constitute by far the greatest part of the basic nuclear research effort. The mission-oriented laboratories form the remaining research effort. The manner in which these laboratories function and the nature of their contributions to the objectives of the research program are somewhat different, although the fundamental motivation—to advance nuclear science—

is the same. In considering their separate functions it is natural to focus on the differences, but it is important to keep in mind that there are considerable overlaps between them. Both the differences and the overlaps are essential ingredients in a balanced overall program.

Both universities and research institutes have demonstrated that they can perform scientific research with success. In the massive federally supported nuclear research effort that grew up after World War II, extensive successful work was done in both types of institution. The national program has been characterized by a fairly even division of funds and scientific manpower between the two, and there have been no major changes in this division.

6.3.3 *University Programs*

In the case of university laboratories it has long been recognized that the educational and research aspects must go hand in hand. Without active and viable research, advanced education is stultified. Consequently, the approach to nuclear research in the universities has tended to emphasize projects in which graduate students could usefully participate, projects with time scales of a year or two, utilizing apparatus that could be managed by small teams. In general, the needs of many university laboratories were met by the smaller and more easily managed accelerators—mostly electrostatic generators that still provide a broad range of research opportunities. There are, however, a few university laboratories that run sophisticated and highly instrumented installations with large staffs and budgets.

The distribution of federal support for university programs in nuclear physics, in terms of federal operating funds expended in fiscal year 1969, as shown Tables II.11 and II.12, ranges from small projects to large programs comparable in size with those undertaken at the national laboratories. The number of universities with programs costing less than $100,000 per year is sizable; however, the funds in these programs are less than 4 percent of the total federal budget for universities, and they represent about 9 percent of the scientific man-years of effort going into basic nuclear physics nationally. About 70 percent of federal funds and 60 percent of the scientific manpower are used in the programs of 21 universities. The cost of maintaining a scientist in the larger programs is about double that in the smaller groups, reflecting the costs of larger and more complex facilities.

The funds for operation of the university laboratories, as shown in Table II.11, come mainly from the federal government, but there is considerable additional support from nonfederal sources, especially in the smaller pro-

TABLE II.11   Distribution of Basic Nuclear Research Funds and Manpower in Universities for Fiscal Year 1969 (All Projects at a Given University are Summed)

Range of Oper. Funds ($Millions)	No. of Univ.	Fed. Oper. Funds ($Millions)	Total Oper. Funds [a] ($Millions)	Scientific Man-Years			Total Cost Per Sci. Man-Year ($Thousands)
				Exp.	Theoret.	Total	
<0.1	33	1.0	2.0	35	20	55	35
0.1–0.3	21	3.5	6.0	85	30	115	50
0.3–0.5	9	4.0	5.5	90	10	100	55
0.5–0.8	9	5.5	7.0	100	25	125	55
0.8–1.2	8	8.0	10.0	110	35	145	70
1.2–2.1	5	8.0	8.5	105	20	125	70
TOTALS	85	30.0	39.0	525	140	665	60

[a] Numbers have been rounded to nearest half-million dollars and 5 SMY.

Table II.11 gives the distribution of operating funds and manpower for nuclear research among universities. The basic data were obtained from a census of all federal grants and contracts for 1969, supplemented by a questionnaire distributed to a large fraction of the active research groups. For this table, all the individual projects in a given university have been grouped together. In addition, projects in the immediate neighborhood of a major university facility have been included with the larger group. The resulting entries thus represent size distribution of more or less independent and self-sufficient research units. Particularly among the smaller units, there will be some who operate primarily as users of distant facilities; for the smallest class, with budgets <$0.1 million, about half are of this character.

Total federal operating funds here represent the total of grants and contracts, exclusive of facilities or construction funds. Nonfederal funds were obtained from the questionnaires. To correct for nonrespondents, the ratios (nonfederal/federal funds) were determined for representative members of each class, and the total federal funds were then multiplied by these ratios. The corrections required were about a factor of 2 for projects < $100 thousand, 20 percent for the class $100 thousand to $300 thousand, and negligible for larger projects.

The total federal operating funds given here differ by about $1 million from those of Table II.9 because the figures presented are based on the 1969 census of individual projects, whereas those in Table II.9 are based on adjusted summary data provided by the federal agencies.

The number of scientific man-years (SMY) was obtained from the questionnaires, with non-respondents dealt with by assuming a constant cost per SMY within each class. Again, for the two smallest classes, the corrections were about a factor of 2 and 1.2, respectively. Of the total 665 SMY, 598 represent actual questionnaire reports, so the total is probably correct to about 10 percent for projects having federal support in 1969. The indicated division between experimentalists and theorists is much less certain.

grams. In 1969, about 20 percent of operating funds were provided by nonfederal sources, but the largest programs are almost wholly federally supported.

6.3.4   The Atomic Energy Commission National Laboratories

The Atomic Energy Commission (AEC) national laboratories carry out basic and applied research in support of the agency's mission. Some of

TABLE II.12   Size Distribution of University Projects for Fiscal Year 1969

Range of Oper. Funds ($Thousands)	No. of Projects	Total Fed. Oper. Funds ($Millions)	Median Oper. Funds ($Thousands)
<100	95	4.0	35
100–300	29	4.6	130
300–500	13	5.3	420
500–800	12	7.6	600
800–1200	8	8.3	900
TOTAL	157	29.8	75

Table II.12 shows the distribution in size of university nuclear research projects, measured in terms of federal operating funds expended in fiscal year 1969. A "project" here means an individual grant or contract with a named principal investigator. In a few of the larger projects, significant particle physics is supported under nuclear physics funds.

The total operating funds given here differ by about $1 million from those of Table II.9 because the present figures are based on the 1969 census of individual projects, whereas those of Table II.9 are based on adjusted summary data provided by the federal agencies.

It is worth noting that while the project sizes in nuclear physics have a broad distribution, the number of small contracts is especially large. In fact, one half of the total number of university projects operate at less than $75 thousand per year, and their total expenditure amounts to $2.8 million, or less than 10 percent of the total for universities. However, we should also note that not all of these small projects operate independently. In fact, as shown by the distribution in Table II.11, by far the largest number are either associated with or at least contiguous with larger groups.

these laboratories are multidisciplinary in character, with broad activities in many branches of physics, chemistry, engineering, biology, and medicine. The Division of Research sponsors basic nuclear research at the Ames, Argonne, Brookhaven, Oak Ridge, and Lawrence (Berkeley) laboratories and at the new Los Alamos Meson Physics Facility. The Division of Military Applications, as part of its large nuclear programs at Los Alamos and Livermore, has sizable, mission-connected basic nuclear programs. The Division of Reactor Development and Technology has important projects at the national laboratories; these programs are not classified here as basic nuclear physics but are very closely related. The close relation between nuclear physics and its numerous applications in our society is reflected in this organizational arrangement.

The 1969 operating funds for basic nuclear research at the AEC national laboratories—about three quarters from the Division of Research— are just a little less than the total federal and nonfederal operating support of universities. The national laboratories are large and complex organizations made up of many sizable projects. The distribution of the sizes of these projects and the scientific manpower they require are given in Table

II.13. The tendency toward larger-sized projects at the national laboratories is consonant with their mission.

The AEC laboratories have taken on nuclear research programs that closely involve large costly instruments and complex or long-time-scale operations. Instrumentation and accelerator design and development work, as part of the forefront research effort, has been an integral part of the programs. The importance of ensconcing this work in a strong basic research program cannot be overemphasized. Similar considerations apply to another traditional mission of the national laboratories—the construction, management, and operation of special facilities that otherwise could not be available to the national community of scientists. For example, the Los Alamos Meson Physics Facility now under construction is beyond the capacity of any one university group. Such facilities are shared with outside scientists carrying on independent research—as user groups. The successful operation of such installations requires the sophisticated backup that can be provided only by an in-house research effort that is itself of the highest quality. The research activities are not divorced from the training of young

TABLE II.13   Size Distribution of National and Government Laboratory Projects for Fiscal Year 1969

Range of Oper. Funds ($Thousands)	No. of Projects	Total Oper. Funds ($Millions)	Median Oper. Funds ($Thousands)	SMY	Cost per SMY ($Thousands)
<100	32	1.7	50		
100–300	41	7.5	200		
300–500	26	10.1	380		
500–800	8	4.9	580		
800–1200	8	7.6	950		
1200–1800	5	7.7	1500		
Total of Above	120	39.5	200		
(Not classified:		5.3)			
TOTALS		44.8		520	86

Table II.13 shows the distribution in size of projects at national and government laboratories. A "project" is here somewhat loosely defined as the minimum unit easily distinguishable in the budget and having some degree of independent management within a major division of the laboratory. The DMA laboratories have been omitted from the listing because their organizational structures did not lend themselves easily to the present categorization.

Comparison with Table II.12 shows that the administrative groupings tend to be much larger in the laboratories than in universities, with a median size of about $200,000 as opposed to the universities' $75,000. A part of this difference comes about because many of the laboratory groups are involved in operating large facilities (see Table II.3, Chapter 5).

scientists, for postdoctoral training is part of the program of most of the national laboratories.

Special emphasis as noted for the national laboratories and, in the previous section, for the university groups, should not obscure the close coupling of purpose and scientific talent. In fact, the close working relations that have developed have greatly strengthened the role that both communities can play in the future of nuclear physics. The success of the new national facilities, which will loom large in the nuclear effort, is predicated on this cooperation.

6.3.5 *Mission-Oriented Government Laboratories and Industrial Laboratories*

The mission-oriented laboratories of the DOD, NASA, and the NBS play a small but significant role in the total basic nuclear research enterprise, one that permits rapid exploitation of ideas and results that are applicable to the mission-determined problems of the parent organizations. The industrial nuclear laboratories have similar aims. All these laboratories contribute to nuclear knowledge; most importantly they form a bridge between nuclear science and its applications. This overlap of interests is essential to ensure a vigorous interchange of people, ideas, and know-how between mission-oriented and basic activities, a feature that has been important to the success of the U.S. program. The importance of this coupling is developed in detail in Chapter 3.

6.4   USER-GROUP AND COLLABORATIVE PARTICIPATION

The description of nuclear-physics research in terms of separate laboratories and separate projects is especially appropriate because the prevalent pattern has been a research group with its own in-house accelerator and equipment. The reasons for this pattern are inherent in the nature of nuclear experimentation. The general situation is that the experiment is carried out as a unit, using the accelerator and auxiliary equipment until completion. This close coupling of accelerator time and experiment time is the basic reason for the large number of facilities—each utilized to near capacity by its own staff.

This prevalent pattern, however, is not the only one in nuclear research. A number of large facilities at the national laboratories and the largest of the university-connected laboratories have nuclear research programs initiated and carried out by outside scientists. As nuclear-physics research

moves toward the use of large facilities—the new Los Alamos facility or the Brookhaven tandem Van de Graaff—the user group arrangements appropriate to such large installations will become more the rule.

In discussing participation by outside groups, one may distinguish two different modes: as *collaborators* with the in-house staff directly cooperating on an experiment and not requiring any special arrangements; as a *user group* carrying out research independently of the in-house scientists and sharing only the accelerator and some major auxiliary equipment. Such users' arrangements are an important part of particle physics and optical- and radio-astronomy research. User groups require special arrangements if their activities are a sizable fraction of the work at a facility: logistic and technical support must be arranged; support staff to take care of the operation and maintenance of the facility are required so as not to burden the in-house staff with all the "housekeeping chores"; the physical layout of the laboratory must be such as to make it possible for visitors to set up their experiments independently, without interfering with ongoing runs. The new major facilities take these needs into account. Existing facilities, especially in the larger laboratories, although not designed for this purpose, could be restructured for user groups but at the expense of considerable money and effort. On the other hand, collaborative participation can be effected immediately since it would simply entail merging with ongoing efforts.

With the exception of certain unique facilities, there is only small usage in the current nuclear programs, about 10 percent of the time, of facilities by groups from other institutions. Even in these instances, a part of this small outsider usage is on a collaborative basis with the in-house staff.

A natural question that arises at this time of funding exigencies is whether a new pattern of research style is required. Except for the largest, nuclear facilities are keyed to utilization by a research group with a very few principal investigators. From the viewpoint of carrying out nuclear research at these moderate-sized facilities most efficiently, nothing is gained by splitting the manpower between institutions except increased travel and equipment duplication costs. There would be a gain in a user-group participation in a moderate-sized facility were it staffed well below its capacity. Were funding cuts administered to cut staffs in an across-the-board way, such a situation might be produced. From the standpoint of cost-effectiveness, it would then be more advantageous to keep some laboratories fully staffed and to drop others completely. There are, however, considerations beyond simple cost-effectiveness. Institutions and scientists cut off from active research possibilities lose touch with their fields in a way that is not easily reversible. Collaborative participation goes some of the way toward

a palliative solution if some additional funding for travel and salary is available; but if required on a massive scale, this might well prove too burdensome to be workable.

Special situations for user groups or collaborative participation might well offer special opportunities that must be measured against other needs and priorities: the opening of research facilities to emerging institutions that could not otherwise attract a staff, opening research opportunities to good younger scientists at smaller institutions, and encouraging industry–university interactions. Should funding constraints close off research opportunities to some universities, the educational opportunity that would be lost might be partially saved in this way. The new, major facilities are based on utilization by many separate groups each having one or several principal investigators. User-group participation is then the natural mode and, indeed, is predicted for these installations. The funding agencies would have to budget travel and other expenses for such user participation.

6.5   SCIENTIFIC REVIEW MECHANISMS–THE NEED FOR NUPAP

A variety of mechanisms operates to ensure high quality in the national research program. These all depend on the rigorous judgments of the informed research community marshaled in informal and formal ways.

It has been a useful custom to convene *ad hoc* committees to review at the national level the need for specific new large-scale programs and to help to define the form and direction of such programs. Other *ad hoc* groups have been asked from time to time for an overall assessment of the field of nuclear physics; this Panel is the latest of such groups. These direct uses of the nuclear community seem to us a necessary review mechanism for the whole program of nuclear research.

At the same time, this Panel believes that a longer perspective might be achieved if a continuing panel were formed to review and report on the field. In a time of funding constraints it is especially true that sizable new programs affect all the programs in the field, both future and ongoing; this makes it important to assess the whole field on a continuing basis. We concur, then, with the recommendation of the Pake Committee that a Nuclear Physics Advisory Panel be formed to advise all government agencies supporting the field. We do not specify the administrative arrangements for this body; however, it is essential that they be such as to permit direct and easy communication with the funding agencies and the nuclear community. We urge that this proposed panel have available an information-gathering

capability, in order to ensure a continuous expertise. It is vitally important that the membership of this Panel be appointed on a rotating basis and that it represent all aspects of the field and its practitioners.

6.6 FUTURE DIRECTIONS

The nation today has an impressive nuclear-physics establishment, one that has the intrinsic capacity to make further important contributions to the science and its applications. Viewed as a system for producing new nuclear knowledge and trained young scientists, we have a capital investment of several hundred million dollars and a cadre of scientists numbering over 2000. What this investment and staff have produced in the past and are producing now has been well documented; it can scarcely be doubted that it has much more to contribute.

Before discussing plans for the future of nuclear physics, we summarize the present program and the facilities now being constructed or devised, together with their budgetary needs.

6.6.1 *Present Program*

The program that has grown over the years is multifaceted and is based on a broad range of experimental facilities that provide the necessary complementary approaches to the study of nuclei. As discussed in Chapter 2 on the science of nuclear physics, such a many-sided program is an intrinsic necessity because of the very nature of nuclear studies.

In Chapter 2, we discussed how the different parts of the program combine toward the objective of building a full picture of nuclei. In reviewing the present program it is important to correlate this objective with the nuclear research activities and facilities.

Table II.14 shows the distribution of nuclear projects according to their main research activity or facility base.

The facilities and the physics are, of course, closely interrelated. Much of the precise and in-depth work on the structure and dynamics of low-lying nuclear excitations has come from the Van de Graaff facilities, which have the necessary precision and variability of energy and projectiles; exploration into the intermediate-energy regions beyond have used primarily cyclotrons. For example, very interesting experiments demonstrating high-lying excitations corresponding to removal of nucleons from deeply buried shells are mainly cyclotron work. Neutron facilities are actually a very diverse class including nuclear reactors, which with time-of-flight instrumentation provide neutrons with well-defined energy over a broad range.

TABLE II.14   Distribution of Basic Nuclear Research Projects by Activity for
Fiscal Year 1969

Project Type	No. of Projects	Operation and Research Cost, Federal ($Millions)	Scientific Man Years[a]
Primarily Accelerator-Centered			
1. Neutron facilities	11	7.0	80
2. Potential-drop machines	64	18.5	340
3. Cyclotrons	21	13.5	200
4. Heavy-ion accelerators	2	3.0	15
5. Electron accelerators	9	4.5	50
6. High-energy facilities[b]	5	8.0	75
Nonaccelerator-Centered			
7. Theory	48[c]	5.5	195
8. Nuclear spectroscopy[d]	25	3.5	65
9. Nuclear chemistry[d]	30	4.5	95
10. Accel. devel., inst.	4	1.0	15
11. Nuclear data	4	1.0	15
12. Other	15	2.0	40
TOTALS	238[e]	72.0	1185

[a] Scientific man-years are estimated totals, including both federally and nonfederally supported
scientists.
[b] It is estimated that about one half of the dollars and SMY's associated with these facilities is
devoted to elementary-particle physics as defined in the captions to Table II.9. However, the full
operating and research costs are included here.
[c] There are 48 separately funded theory groups with an operating budget of $4.0 million (federal)
and 150 scientific man-years; another 45 scientific man-years and ($1.0 million federal) are
theorists that are part of the accelerator-based groups.
[d] Nuclear chemistry and spectroscopy groups working directly with specific accelerators have been
included in the facilities category above.
[e] This total is smaller than the total number of "projects" as defined in Tables II.12 and II.13
because we have here grouped together all the projects attached to a given accelerator.

Table II.14 subdivides the field of nuclear-physics research according to kind of activity, as
determined by the general nature of facility employed. Such distribution is necessarily crude but is
useful insofar as different accelerator types represent distinct experimental capabilities and
different approaches to the nuclear problem. Thus neutron facilities are generally–but not always–
used to measure cross sections and to study details of energy level structure, while electron
accelerators are generally–but not always–used to study electromagnetic interactions. The
ordering of the various accelerator categories is very roughly by energy range, but it is also some-
what historical, in the sense that future programs will tend to place relatively more emphasis on the
higher-energy facilities.

Also listed are some general categories of nonaccelerator-centered activities: those for which
immediate access to an accelerator is not essential or for which the major part of the work is done
off-line.

Some more specific comments:

1. *Neutron Facilities.* Seven major reactors are included. However, three electron linear acceler-
ators are also considered to be engaged primarily in basic neutron research and are entered here.
The Los Alamos (Nevada) nuclear explosion project is included.

2. *Potential-Drop Machines.* Included here are one-, two-, and three-stage Van de Graaffs and
Dynamitrons. More than half of these are single-stage machines with terminal voltages ≤ 6 MeV,

accounting for about $2.6 million of the total funds. A good many small machines have been omitted here as either not funded or being used as injectors for larger installations.

3. *Cyclotrons.* The list includes seven active fixed-frequency machines, all 15 years or more old. The major portion of effort and cost is devoted to the AVF cyclotrons ranging from 50 to 260 inches in pole face diameter. The largest of these, the Indiana machine, is not yet in operation.

4. *Heavy-Ion Accelerators.* Although many other machines accelerate heavy ions, we have included here only the Yale and Berkeley HILAC's. The latter is presently undergoing an extensive conversion program.

5. *Electron Accelerators.* Included here are the linacs, ranging from 22 to 1200 MeV, and one synchrotron. Three of the linacs have been considered to be primarily neutron facilities. The Stanford operation is not included because it is funded as a high-energy particle-physics facility.

6. *High-Energy Facilities.* LAMPF and four synchrocyclotrons are included of which one is now retired.

7. *Theory.* To provide an assessment of the total effort devoted to theory, we have included not only separately funded groups but also an (admittedly rough) estimate of expenditures by theoretical groups operating within experimental contracts or grants. Included in the table are some 64 university groups, spending $3.0 million and seven groups in national laboratories, spending $2.0 million.

8. *Nuclear Spectroscopy.* These are groups working mainly with radioisotopes, produced in a variety of facilities.

9. *Nuclear Chemistry.* Nuclear-physics work, mainly on radioisotopes and fission, supported under AEC chemistry contracts. Chemistry groups working directly with specific accelerators have been listed in the facilities categories above.

10. *Accelerator Development and Instrumentation.* Four groups, the LAMPF and Indiana projects are included in the above categories.

11. *Nuclear-Data Compilation.* Four groups, spending about $900,000. The CINDA operation is not included.

12. *Other.* Astrophysics ($400,000), user groups ($400,000), and projects not otherwise categorized ($1.0 million).

This diversity corresponds to the wide utility of neutrons in nuclear experimentation, both basic and applied, ranging from simple production of new radionuclides to direct use in studies of nuclear dynamics.

Heavy-ion work, we have seen, is still in early stages of exploration. The interaction between massive amounts of nuclear substance is largely unknown; knowledge is increasing as the capability for accelerating heavier ions to higher energies is slowly being extended. Heavy ions can be applied to the measurement of nuclear properties and states that cannot be reached with other techniques. In the U.S. program the important special application to new super-heavy elements awaits completion of the upgrading of the Berkeley heavy-ion accelerator (HILAC).

Electron accelerators have been the means for probing the nucleus through the delicately informative electromagnetic interaction (see Chapter 2). High-energy electron facilities have been especially useful in providing the earliest information on the short-distance structure of nuclei. As beam energies have been extended, this distance scale has become finer and finer. The complementary short-wavelength probing with strongly interacting nucleons and nuclear studies with mesons comprise the nu-

clear research field of the present high-energy facilities. The Los Alamos facility now under construction will greatly extend the capabilities in this direction.

Theory groups, while listed separately because of their separate funding pattern, play an important role in all the research directions already described besides the purely theoretical problems. Of great significance is the fundamental work on nuclear models and on the basic theoretical problem of quantitatively connecting already well-established nuclear information with the primary force between nucleons. Theorists are especially important to a field as broad and diverse as nuclear physics in helping to provide the essential unifying component. In nuclear physics, as in other branches of physics, there is a partnership between theory and experiment, and this is reflected in the close interaction between theorists and experimentalists in developing the ideas and concepts that move the field.

The nonaccelerator-based activities round out the program in special ways.

This briefly outlines the scope and distribution of effort of the nuclear program as it now exists. Together with the facilities coming into operation (described in the following section) it illustrates the broadly based program that we consider essential for rapid progress in nuclear physics. It is necessary in planning future programs to appreciate the range in the quality of facilities within each of the categories, for there is a wide range in accelerator capability and instrumentation sophistication. Under the constricted budgets of the past several years, the necessary upgrading of equipment has been impossible in many laboratories. We believe that it is important to carry forward the present work as part of future programs; it seems necessary that these investigations exploit the most powerful instrumentation and facilities available. In our discussions we shall assume this point of view in responding both to the possibilities of increasing funding and the exigencies of decreasing budgets.

6.6.2 *New Facilities Coming into Operation*

Concomitant with the evolution of the program just described has been a recognition of an impending need for a reach into quite new and hitherto inaccessible regions of study. For some years now, more powerful and more versatile accelerators have been under construction in the United States and abroad, and the years 1969–1973 will see the completion of about $121 million worth of advanced facilities in the United States alone (Table II.15).

A review of the largest of these projects is necessary to illustrate their importance in future nuclear programs. The LAMPF (Los Alamos Meson

TABLE II.15   Budget Estimates for New Facilities under Construction in Fiscal Year 1969 (Operating Budgets Include the Total Cost of the User Programs)

	FY 1969			Full Operation	
	Construction Cost[a] ($Millions)	Operating Cost[a] ($Millions)	SMY	Estimated Operating Cost[a] ($Millions)	Estimated SMY
1. Neutron Facilities					
ORELA	4.8	0.9	7	1.8	20
2. Potential-Drop Machines					
BNL (3-stage)	12.0	1.8	9	2.7	35
Other Accel. (7)[b]	13.0	1.9	42	4.0	70
3. Cyclotrons					
Md. and Ind.	17.1	1.0	21	2.0	30
4. Heavy-Ion Facilities					
LRL HILAC	2.7	2.4	11	3.3	15
5. Electron Accel.					
Linacs (3)	11.1	1.9	25	4.2	55
6. High-Energy Facilities					
Nevis	3.9	1.6	10	2.4	15
LAMPF	56.0	3.7	30	15.0	40
LAMPF Users		0.2	2	5.0	75
TOTALS	120.6[c]	15.4	157	40.4	355

[a] Federal dollars, 1969 value.
[b] Two of these were state financed.
[c] About 10 percent is nonfederal.

Table II.15 presents the estimated costs, in fiscal year 1969 dollars, of operation and research for major new facilities that were under construction or just completed in fiscal year 1969. Costs listed include operation and maintenance of the facility and costs of the directly associated research program, including capital equipment. Expenditures in fiscal year 1969 represent development work, partial operation and research, or current research on a facility that the new equipment will replace.

Estimates for full operation assume some reasonable optimum level but not necessarily the maximum possible. For example, the LAMPF figures assume 15 shifts per week.* The estimates also assume adequate, but not luxurious, technical and materiel support—about on the 1967 scale of the larger laboratories.

For some of the facilities listed, full operation is possible almost immediately. For others, notably LAMPF, the programs will build up gradually, until 1975 or 1976. Within the accuracy of the estimates, a linear increase in annual funding to $40 million per year in 1976 would permit full utilization of these facilities.

* The full cost of operation and maintenance of LAMPF is assigned to basic nuclear-physics research, but approximately one half of the beam time will be used for basic particle physics and applied research.

Physics Facility) is a high-intensity proton linear accelerator designed to attack the high-energy or short-distance frontier with high-energy nucleons and mesonic probes. With its enormous beam intensity and copious production of secondary particles, LAMPF will provide a new capability for nu-

clear structure and meson-physics explorations. The Nevis cyclotron modification program is also designed to tackle these questions but in limited scope because of its more modest beam intensity.

The new electron accelerators will investigate short-distance nuclear phenomena with high-energy electron scattering, an approach that is complementary to the LAMPF and Nevis investigations. The converted Lawrence Berkeley Laboratory heavy-ion linear accelerator (super-HILAC) will make possible work with very heavy ions—mostly in a search for heavy and super-heavy elements. The physics of heavy-ion studies has excited the interest of the nuclear community, as is evidenced by the large number of proposals for new heavy-ion facilities. The new Brookhaven National Laboratory double-tandem Van de Graaff installation will extend the range of precision heavy-ion studies now possible. In addition this unique facility will extend to higher energies the flexible, high-precision investigations that have proved so profitable with the lower-energy tandem Van de Graaffs. The capability at energies intermediate between this region and the high-energy region will be enhanced by the Indiana and Maryland cyclotrons with beams of light and heavy ions. The Oak Ridge National Laboratory electron accelerator (ORELA) is already in action in high-intensity, high-quality neutron studies.

In summary, this complex of new facilities already approved and under construction constitutes a consistent effort to advance the field in new directions, in a deliberate and prudent way. However—and herein lies a major difficulty—these accelerators will be expensive to operate. As shown in Table II.15, it is expected that by 1975, about $40 million per year will be required for operations and nuclear research associated with just these instruments. This is some $25 million per year more than was spent in 1969 in program buildup at these facilities (or in the facilities they will replace). To put the matter another way, the cost per scientific man-year for the new facilities—including users—will be nearly $100,000, as compared with the approximately $60,000 per man-year average for the present program.

6.6.3   *Future Facilities**

There are well-defined and important directions in which nuclear physics should move that will require complex machines and large laboratories beyond those outlined so far. Among the most pressing and most fully designed are heavy-ion physics facilities, for which there have been about half a dozen proposals. Such facilities would greatly extend the capabilities of the super-HILAC or the other facilities now beginning exploration in this

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* See discussion in Chapter 5.

field. To maintain the momentum already built up in this field, a national installation (costing about $20 million to $25 million to construct and $8 million per year to operate its total research program) should be initiated as soon as possible. If it were to be funded in fiscal year 1973, it could be in partial operation by 1975 or 1976.

At about the same stage in design are large 30- and 40-MV tandem electrostatic accelerators, which could provide quite respectable heavy-ion performance and extend the precision and flexibility characteristics of these machines into a quite new and important energy region. Facilities incorporating such machines would cost about $15 million to build and some $3.5 million a year to operate.

New developments in superconducting accelerators are less advanced. The Stanford superconducting linear electron machine is planned to be in operation within the next five years. Studies to use such superconducting machines to accelerate positive ions, rather than electrons, are actively under way. If these promising possibilities are brought to fruition, numerous interesting directions for nuclear physics will be opened. These machines may prove to be the way to produce high fluxes of heavy mesons, such as kaons, for nuclear structure studies. They might be the means to extend high-energy and heavy-ion work. Collective, or electron-ring, accelerators also offer exciting new possibilities that should be exploited, as seen in the discussion in Chapter 5. By 1975, at least developmental prototypes of such accelerators should be under way.

The potentialities inherent in very intense neutron generators both for power production applications and for basic nuclear physics have aroused strong interest in the past. The exploratory work of E. O. Lawrence in the 1950's demonstrated that the technology of that time was not adequate for a practical device. The Canadian study of such a facility in the 1960's, under W. B. Lewis, foundered on funding considerations. Recent advances in accelerator technology would seem to justify a renewed study program to examine the practicality of such a facility in this decade. The results of such a study program would indicate the consequent development policy. Because the main thrust of the development effort work would be on power production applications, the expectation is that the funding should stem mainly from outside the basic science program sources. However, the nuclear research made possible by such a capability would be a natural part of the basic program.

6.7 FUNDING PATTERNS AND THEIR CONSEQUENCES

In the succeeding sections the consequences for nuclear physics of different funding patterns are outlined. In response to the various budget oppor-

tunities or constraints marked reorientations of the research areas and research patterns are considered necessary. The reorientations will take nuclear physics into a new set of frontiers with new facilities, while continuing to mount the multifaceted program basic to the science. In order to make this exercise as realistic as possible, a definite eight-year period, 1969–1977, is used for discussion. Although 1969 is three fiscal years ago, it still forms a sensible basis for discussion, since the problems of today are well stated in those terms and almost unchanged in scope. It is our estimate that this period of eight years will see massive realignments in the nuclear-physics establishment, realignments forced by changes in the field itself, superimposed on those determined by the various budget options. If the field is adequately supported, these changes will take place smoothly, and the character of the program will probably change only slowly in the succeeding period. If on the other hand, severe budget constraints are imposed for several years, a period of recovery and rebuilding will be required before nuclear physics can again press forward with full vigor.

In discussing budget analyses and realignments in nuclear-physics programs, it is important to clearly understand the framework within which these considerations operate. A significant fraction, about 20 percent, of the support for basic nuclear-physics research derives from applications-oriented organizations, such as the AEC's Division of Military Applications, the DOD, NBS, and NASA. These organizations have to consider criteria in addition to those stemming from the research programs. It seemed to us, therefore, most useful to carry out the analyses twice over, first, omitting these applications-oriented organizations from explicit consideration but implicitly relying on their continued interest and support to fill out the programs and aid in achieving a better balance; second, including them on the assumption that they would find it possible to freely reorient their basic nuclear-physics programs.

Beginning with the first approach, the budget under discussion amounts to $56 million in fiscal year 1969. The distribution among the various components of the nuclear program is shown in Table II.16.

6.7.1 *A Future Budget Matched to the Opportunities in Nuclear Physics*

Exploitation of the opportunities now available to nuclear physics requires that the field move in all the directions previously discussed: the extension and regrouping of the present program, vigorous exploitation of the new frontiers, and aggressive capitalization on the possibilities now in the early stages of development. What should the research program be in 1977 to achieve these goals?

In following the directions dictated by progress in the science, it is clear

TABLE II.16   Distribution of Basic Nuclear Research Projects by Activity for Fiscal Year 1969, Excluding DOD, NASA, NBS, and DMA Support

Project Type	No. of Projects	Operation and Research Cost, Federal ($ Millions)[a]	Scientific Man-Years[a,b]
Primarily Accelerator-Centered			
1. Neutron facilities	7	4.0	45
2. Potential-drop machines	55	14.5	300
3. Cyclotrons	17	10.5	150
4. Heavy-ion accelerators	2	3.0	15
5. Electron accelerators	5	2.0	25
6. High-energy facilities	4	7.0	60
Nonaccelerator-Centered			
7. Theory	40	4.0	170
8. Nuclear spectroscopy[c]	23	3.5	60
9. Nuclear chemistry[c]	28	4.0	85
10. Accelerator development and instrumentation	4	1.0	15
11. Nuclear data	4	1.0	15
12. Other	13	1.5	35
TOTALS	202	56.0	975

[a] Operation and research costs and scientific man-years (SMY) are rounded to the nearest half-million dollars and 5 SMY.
[b] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.
[c] Nuclear chemistry and spectroscopy groups working directly with specific accelerators have been included in the facilities categories above.

Table II.16 is the analogue to Table II.14 using the budgeting conventions adopted for the remainder of Chapter 6. As compared with Table II.14, the following change has been made: only basic nuclear research supported by the AEC Division of Research (part of Chem, all of MEP, part of LENP) and by NSF, Nuclear Physics is included.

that there will be a move to bigger and more expensive installations—expensive in dollars and in scientific effort. There are several limitations that must be considered if we are to proceed prudently. Among them is the limitation in number of scientists available; judging from the experience of past years, our highly qualified scientific manpower can be extended by as much as 8 percent a year. However, to accomplish the desired goals will require more than simple addition of new people. A considerable reorientation of the present program, with both cutbacks and extensions, as well as some change in the style of research, will be necessary. We outline below a 1977 research program that fulfills the goals and obeys the constraints. Such a program would require an increase in the purchasing power of the operating budgets over those in 1969 and about $80 million of new capital facilities.

As noted above, the broad-based effort of the present program must be continued but with facilities that incorporate the best of nuclear technology. Therefore the present program is to be maintained in part by funding the most productive and best laboratories in a way that will enable them to operate under optimal conditions; this immediately requires an increase over the present budgets for these groups on the order of 10 percent. At the same time, some of the laboratories, lacking forefront equipment but not scientific leaders, should be permitted to move into the forefront of research efforts with new accelerators and instrumentation now becoming available. The remaining installations should be dropped from basic nuclear-physics research or operated at a minimum level, their scientists being funded mainly as users at the new laboratories. It will be seen in the detailed tables that these new laboratories are quite sizable, with large annual budgets of dollars and scientists. They should be considered as regional or national centers with full user-group participation. The net results of this plan are that, by 1977, the present program will be extended—there will be bigger and fewer laboratories, each with more men and money; the number of separate and competing research groups, however, may not be significantly different than in the present program. The majority will be outside users rather than resident users.

The details are set out in Table II.17, but some illustrations might be useful. As an example, consider the most numerous of nuclear projects, the Van de Graaff-based groups. A new generation of electrostatic accelerators, capable of providing a significant extension in energy range and of the kinds of particles accelerated, is now becoming feasible. Three such installations by 1977 would provide the strength to exploit the opportunities offered by the new accelerators. Each of these would require a sizable laboratory with an estimated operating budget of $3.5 million and some 40 scientist man-years annually; about half of these scientist man-years would be contributed by users from other institutions. These installations, together with more than 30 of the most productive Van de Graaff laboratories, would form the national effort in this area of nuclear physics with an annual operating budget of about $31 million and 400 scientist man-years. This is to be compared with the present 55 projects funded at $14.5 million and supporting 300 scientist man-years annually, it being understood that the mission organization-based work noted above is not included here.

Similar considerations apply to other categories. The neutron work at some of the reactors and at the best of the linac laboratories should be fully supported. Work supported by the AEC (DMA) is not included in Table II.17. This work will fill out the program.

Heavy-ion work is presently being extended by the conversion of the

TABLE II.17 Elements of an Expanding Budget Excluding DOD, NASA, NBS, and DMA Support

	FY 1969			FY 1977[a]		
	No. of Facilities	Operation and Research Cost ($Millions)[b]	SMY[c]	Estimated No. of Facilities	Estimated Operation and Research Cost ($Millions)[b]	Estimated SMY[c]
Primarily Accelerator-Centered						
1. Neutron facilities	7	4.0	45	6	9.5	80
2. Potential-drop machines	55	14.5	300	36	31.0	400
3. Cyclotrons	17	10.5	150	12	12.0	120
4. Heavy-ion accelerators	2	3.0	15	2	12.0	100
5. Electron accelerators	5	2.0	25	3	4.0	40
6. High-energy facilities	4	7.0	60	2	22.5	125
7. Small-scale projects				30	4.0	70
Nonaccelerator-Centered						
8. Theory		4.0	170		13.0	400
9. Nuclear spectroscopy[d]		3.5	60		4.0	50
10. Nuclear chemistry[d]		4.0	85		7.0	100
11. Accelerator development and instrumentation		1.0	15		4.0	50
12. Nuclear data		1.0	15		2.0	30
13. Other		1.5	35		3.0	60
14. Future facilities					10.0	70
TOTALS	90	56.0	975	91	138.0	1695

[a] In the 1977 projections, the estimated dollars and scientific man-years (SMY) for a particular category are written for definiteness to the nearest half-million dollars and 5 SMY, but, by the very nature of this exercise, no such accuracy is implied.
[b] Federal dollars, 1969 value. Costs include total support of user groups.
[c] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.
[d] Nuclear chemistry and spectroscopy groups presumed to be working directly with specific accelerators are included in the facilities category above.

354

Table II.17 is designed to indicate how one might best exploit the nation's nuclear research capability to achieve the goals of advancement of knowledge, education, and technology outlined in the text.

This is a manpower-limited budget. The number of scientific (PhD-level) man-years devoted to basic nuclear research in 1969 was about 1185 (Table II.14). About 200 nuclear-physics PhD's were granted in this year, about 10 percent of the number of PhD's in the field. Assuming a comparable rate for the next several years, and noting that some will leave the field by choice and that others will teach part time, we estimate a net increase of 6 to 8 percent SMY per year, with a total reaching possibly 2000 in eight years. Assuming that the fraction of SMY's supported by the mission agencies will be the same as it was in 1969, about 1700 SMY's would be available for this program.

The priorities as we envisage them, for either a manpower-limited or a dollar-limited budget are the following:

To exploit recently constructed facilities that give access to high-energy interactions (500–1000 MeV) and to nuclei far outside the normal stability region.
To restore and strengthen the broad base of multifaceted programs that provide breadth to the attack on nuclear problems and represent the principal channels into education.
To follow up the entry already made into the intermediate-energy region of 50–200 MeV.
To anticipate the needs for new equipment to exploit the results of the present preliminary explorations.

In the table we present some rough but considered estimates of how these objectives might be met. Although we have again, as in Tables II.14 and II.16 characterized the nuclear-physics community mainly by its machines, it should be stressed that the considerations that enter the allocations are in fact oriented to quite specific research objectives, as discussed in some detail in Chapter 2. The broad change in emphasis that is illustrated in Table II.17 reflects our assessment of the changing needs and interests of the sub-field. The detailed allocations reflect also what we believe would be the probable response of the existing research groups to the opportunities available for exploitation.

Items 1, 2, and 3 in Table II.17, corresponding to the categories "neutron facilities," "potential-drop machines," and "cyclotrons" embrace a large part of the traditional multifaceted attack that yields precise and essential information on nuclear structure and the course of nuclear reactions. At the same time, the class contains a subset, characterized principally by the higher-energy cyclotrons, which represent a frontal attack on the newly opened intermediate-energy region. We expect both of these kinds of endeavors to prosper but probably with only a modest increase in the total manpower. Our projections are based on the assumption that a fairly large number of outmoded facilities will be abandoned and that perhaps three to five major groups will install new facilities in the class of the 30–40 MeV electrostatic accelerators or the more advanced cyclotrons. Anticipating the opening of new fields in neutron physics with the ORELA machine, we have included provision for another neutron facility on approximately the same budgetary scale. This would not necessarily be a new accelerator; it might very well take the form of a conversion of some existing facility.

Of the total operating budget of 52.5 million in 1977 for Items 1, 2, and 3 we estimate that approximately half would involve use of new facilities, including those now under construction (Table II.15), and the remainder would be devoted to a selection of the most versatile of the presently existing installations. The total cost of the new facilities would be about $50 million.

A redistribution of this kind, carried out over the next five years, would at once ensure a prudently intensified exploitation of the low-energy, high-precision field and make possible the systematic pursuit of problems in the intermediate-energy field, where exploration has just begun. Most of the proposed new installations will also contribute importantly in the heavy-ion field.

Implicit in the budget and manpower estimates is the recognition of a trend toward a number of well-supported comparatively large regional centers.

Such a policy is a new departure for nuclear research—and not without hazard; some ameliorating provisions can easily be made under the assumed constraints.

Items 4, 5, and 6 in Table II.17 "heavy ions," "electrons," and "high-energy facilities" provide the main thrust into the thus far almost untouched regions of high-energy interactions and of interactions involving very heavy ions. Because these fields are new, and because we expect that their exploration will be lucrative, we expect a sizable increase in manpower, from about 100 SMY in 1969 to 265 SMY in 1977, with a concomitant increase in annual operating expenditures from $14 million to $38.5 million.

In the heavy-ion field, we are already proceeding with the Berkeley super-HILAC; in addition, we believe there will be need for a complementary major heavy-ion facility with precise and easily varied energies and beams of the kind described in Chapter 5. This facility will cost about $20 million to $25 million to build, and the operation and research programs will amount to perhaps $8 million annually, including user programs.

We have included three high-energy facilities also capable of producing useful beams of mesons; one of these is the Los Alamos Meson Physics Facility (LAMPF), which should begin operation in 1973.

In Table II.17, the full operating cost of high-energy facilities has been attributed to the nuclear-physics budget.

Item 7, "Small-Scale Projects." We alluded earlier to some trepidation about the consequences of a general trend toward concentration of resources in the larger laboratories. Clearly such a trend is inevitable as the cost of equipment continues to mount, and surely the arrangement has advantages in facilitating close communication and mutual support among physicists. Still it is important for the vitality of the science that provision be made for some innovative small-group operations and for the support of talented young people in small institutions. Indeed, it is one of the attractive virtues of nuclear physics that trailblazing work can still be done with modest equipment, and the identification of people who can make contributions in this way will remain an important challenge for the funding agencies. These small-scale projects will generally be based on the smaller potential-drop machines and cyclotrons. An allocation of about $4 million—roughly 3 percent of the total budget—for this purpose in 1977 would hardly be excessive.

Items 8 through 13 of Table II.17 require little comment. We have assumed for most of these categories an increase in manpower and cost approximately proportional to the totals for 1977. Nuclear spectroscopy will probably be reduced somewhat as the focus shifts to heavy-ion reactions in which the relevant activities are more properly described as nuclear chemistry.

Item 14, "Future Facilities," represents an allowance for activities only seen in general outline at this time. With the LAMPF, the heavy-ion facilities, the several cyclotrons, electrostatic accelerators, and electron accelerators just discussed, qualitatively new kinds of phenomena will surely be observed. What new physics these ventures will lead to is hard to predict, but it is certain that new discoveries will bring new needs, needs for more powerful or more precise or more versatile accelerators than are presently designed. Also to be anticipated are needs for special installations designed to exploit applications of nuclear physics to technology or to other sciences. While in the spirit of this Report such activities would not be called "basic nuclear research," a realistic view would argue that new applications often need to be carried at least to the demonstration stage before funds from other sources can be commanded. In the recent past such demonstrations have included accelerators dedicated to work in atomic physics, astrophysics, and solid-state studies. The future may well bring even more interesting applications, and some monies should be set aside to permit their exploitation.

At the same time, we can also see the development of new accelerator technologies, some parts already well in hand, and others still in early stages. Current developments of superconducting accelerators for positive ions, and of collective electron-ring accelerators are examples that can be expected to reach fruition in a short time. With due apologies for the fact that firm predictions cannot be made five years ahead in a field just venturing into new territory, we suggest allocation of an operating budget of the order of $10 million per year by 1977 for advanced development and prototype construction of two new major machines, most probably to be in the higher-energy and heavy-ion fields. A study of the potentialities of intense neutron generators is included.

356

Berkeley HILAC machine. The nuclear community has expressed strong interest in a heavy-ion physics facility that would complement this machine, and we believe that such a new installation would adequately support the national effort when taken together with the limited but important capabilities in this field of the large electrostatic accelerators and cyclotrons. Such a machine would support about 85 scientific man-years of effort, of which roughly a quarter would represent user groups.

The high-energy facilities now being constructed at Los Alamos (LAMPF) and as part of the conversion at the Nevis Laboratory will provide partly complementary and partly competitive new capabilities. The 184-in. cyclotron at Berkeley, with its higher energy, can also contribute to this capability. The LAMPF especially is an intrinsically costly operation and will channel a large share of the nuclear effort into this high-energy direction. The LAMPF program in nuclear and elementary-particle research will require some 38 scientific man-years annually of in-house effort and 75 scientific man-years in user groups.*

As has been indicated by the examples above, a sizable fraction of the scientists utilizing the larger facilities will be users, in the sense that their home bases will be elsewhere and that they will make use of the facility for relatively brief periods. For many of the research teams in this category, it will be important to maintain some modest facility at home base, for checking out instrumentation, for training student participants, and possibly for pursuit of small-scale independent research programs. It seems likely that a large fraction of the presently existing small Van de Graaff and cyclotron facilities would be kept active in this way, albeit with very much smaller budgets than they presently require. Machines of the size of the smallest two-stage tandems can in principle be maintained and operated without large technical staffs and hence with small fixed costs. The normal budget allocation for an active user group might well include some allocation for home-based activities of this kind, which would significantly strengthen their user operation.

The nonaccelerator-based activities require little further comment. The growth envisaged in these activities fits well within their own potential and that of the whole program.

The development of prototypes of the superconducting linacs for the acceleration of a variety of nuclear projectiles and of the electron-ring, or collective, accelerator has been budgeted. Either or both of these may, by

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* It should be noted that although full costs of operation and maintenance are assigned to the medium-energy program, LAMPF is an interdisciplinary facility that will be utilized in many areas of immediate practical utility, such as biomedical applications, radiation-damage studies, and isotope production.

1977, be sufficiently developed to make possible their incorporation in nuclear facilities. These new possibilities together with those generated by the research discoveries of the next few years have been budgeted under future facilities in what must be considered a very rough estimate.

Also in this category of facilities not yet ready for detailed specification are possible new instruments specifically designed to exploit applications to other sciences. The uses of nuclear instrumentation for atomic physics, solid-state studies, and astrophysical work are obviously growing fields, which may well require further development on a relatively large scale and funds to carry them to the demonstration stage.

Finally, we have included a number of small projects, with a total annual operating budget of only $4 million and 70 scientific man-years, to continue in a deliberate way the small-group structure that has been the style of nuclear research in past years. These groups would be attacking nuclear problems that do not require the largest installations. While they could be attached to such laboratories, it seems worthwhile to keep them quite distinct for as long as possible, simply because nuclear physics grew well under such a regime in the past, and we should not discard the mode of past successes as we move into a more frenetic future. Such a dissemination of university research facilities is important also in maintaining the broad-scale character of education opportunities.

The program elements listed in Table II.17 add up to an annual operating budget of about $138 million in 1977.

A continuation of an annual growth of 11 percent in terms of 1969 dollars since 1969 would have produced the exploitation program that we have discussed here for fiscal year 1977. However, because the support level has fallen since 1969, were we to begin in fiscal year 1973 to build toward this capacity in fiscal year 1977, a growth rate in the intervening period of 22 percent would be required. Clearly such a growth rate must be regarded as a one-time catch-up phenomenon. Although the Panel has not attempted to carry its projections beyond 1977, it recognizes the importance of longer-range plans—both to the funding agencies and the nuclear community. These, however, can only be determined in the light of developments in the intervening period.

We emphasize again that the program that we have outlined is based on the potential of the field and on the projected availability of the necessary manpower. We recognize that the competition for federal funds will make it difficult to attain this growth rate, but we believe that the rewards of this full program justify the required funds. The expansion of the highly profitable nuclear enterprise, closely coupled to our technological and educational systems, seems an eminently prudent investment.

6.7.2   *A Nearly Constant Manpower Budget*

Nevertheless, the investment suggested above may not materialize. We have, therefore, studied the consequences of an intermediate budget—one intermediate between the expanding budget of Table II.17 and the current slowly declining budget.

Table II.18 presents a possible allocation of resources constrained by a condition of nearly constant total manpower. Although it is far from the bold approach that we believe would be most profitable, the intermediate budget preserves the national investment in scientists and facilities for nuclear physics and provides a reasonable base for progress on all presently identified major fronts in the field. By no means does the funding level here contemplated guarantee recovery of the dominant position formerly enjoyed by the United States in nuclear physics, but it should at least permit a highly competitive level of research.

The intermediate budget requires an increase in annual operating funds of about 50 percent by 1977, an average rate of 6 percent per year in the period 1969–1977. Since the actual funding level in 1972 is down by about 15 percent from 1969 (in dollars of constant purchasing power), a sizable increment is needed quickly if we are to achieve the goals of this plan. The single most important element that demands incremental funding is the impending completion of LAMPF in fiscal 1973. In that year, operation and research at LAMPF will require nearly $15 million, and by 1975, the need will be about $20 million.

As may be seen from Table II.18, almost all of the increased funding required is for the areas of high-energy and heavy-ion physics; we expect these increases to amount to $15.5 million and $7.5 million, respectively, as compared with 1969 expenditures in these fields. The remainder of the budget is held nearly constant; however, to do this in the face of upgrading needs dictated by the increasing sophistication of scientific questions requires reducing the number of accelerators and concentrating funds and manpower on the most versatile installations. In the first three categories of the table, for example, only 39 to 45 installations will be fully supported in 1977, as opposed to 79 in 1969. We anticipate that perhap 20 others may be partially supported, or supported at minimal levels in the category "Small-Scale Projects."

The only new installation contemplated in the intermediate budget is a national heavy-ion physics facility. As the single machine enjoying the widest support in the nuclear community, a precision accelerator for heavy ions represents a promise for the future that should not be postponed. This new facility is not entirely based on new manpower and new

TABLE II.18   Elements of A Nearly Constant Manpower Budget Excluding DOD, NASA, NBS, and DMA Support

	FY 1969			FY 1977[a]		
	No. of Facilities	Operating and Research Cost ($Millions)[b]	SMY[c]	Estimated No. of Facilities	Estimated Operating and Research Cost ($Millions)[b]	Estimated SMY[c]
Primarily Accelerator-Centered						
1. Neutron facilities	7	4.0	45	3	4.0	40
2. Potential-drop machines	55	14.5	300	27–33	17.0	220
3. Cyclotrons	17	10.5	150	9	9.5	95
4. Heavy-ion accelerators	2	3.0	15	2	10.5	100[d]
5. Electron accelerators	5	2.0	25	2	3.0	25
6. High-energy facilities	4	7.0	60	2	22.5	125
7. Small-scale projects				20	2.0	40
Nonaccelerator-Centered						
8. Theory		4.0	170		6.0	185
9. Nuclear spectroscopy		3.5	60		2.5[e]	40
10. Nuclear chemistry		4.0	85		2.5[e]	40
11. Accelerator development and instrumentation		1.0	15		2.0	20
12. Nuclear data		1.0	15		1.5	25
13. Other		1.5	35		1.5	30
TOTALS	90	56.0	975	65–71	84.5[f]	985

[a] In the 1977 projections the estimated dollars and scientific man-years (SMY) for a particular category are written for definiteness to the nearest half-million dollars and 5 SMY, but, by the very nature of this exercise, no such accuracy is implied.

[b] Federal dollars, 1969 value. Costs include total support of user groups.

[c] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.

[d] This estimate assumes that a new heavy-ion physics facility could be in full operation by FY 1977. If this project is delayed, an interim allocation of scientific manpower and operating funds to other projects on the list would be necessary.

[e] Nuclear chemistry and spectroscopy groups working directly with specific accelerators are included in the facilities category above. These estimates reflect the presumption that a relatively larger fraction of spectroscopic and nuclear chemistry work will be carried out at accelerator centers.

[f] In 1972 dollars, this number is $98 million.

360

Table II.18 presents a budget intermediate between the expanding budget of Table II.17 and the current declining trend. The intermediate budget is designed to keep the total manpower approximately constant while utilizing the best of existing facilities. Only one new facility, the heavy-ion accelerator, is contemplated. Again only funding by the AEC Division of Research and NSF Nuclear Physics is included: high-energy facilities <1 GeV in these programs are entered at full cost.

1. *Neutron Facilities* have been reduced to three in an effort to concentrate resources on the most powerful and versatile. The ongoing programs of the AEC Division of Military Applications will be important in balancing the total program.

2. *Potential-Drop Machines.* The 1969 list contains 21 accelerators capable of producing <10 MeV protons. The indicated allocation for 1977 assumes that a large fraction of these would be funded plus up to a dozen smaller machines with special capabilities. Of the remaining small machines, some would be funded as "small-scale projects" and others would be abandoned.

3. *Cyclotrons.* Of the 17 installations listed in 1969, roughly half a dozen are already approaching obsolescence. In absence of funds for reconstruction, these facilities probably would not be supported in 1977. Again, some might qualify for support as "small-scale projects."

4. *Heavy-ion Accelerators.* The proposed new havy-ion physics facility is included. By 1977 it should be in full operation, with an annual budget of about $8.0 million, accommodating perhaps 85 scientific man-years in-house and users. About half of the manpower and half of the costs of the new heavy-ion facility will have been gradually transferred from other projects. If this facility should be delayed beyond 1977, it is important that these transfers also be delayed to permit the changes to occur smoothly.

5. *Electron Accelerators.* Probably only two or three of the most versatile electron accelerators can be adequately funded in the present program. Several others will presumably continue to be supported by mission agencies, and these, together with the Stanford superconducting accelerator (not included in Table II.18) will provide good coverage of the fields.

6. *High-Energy Facilities.* LAMPF and one or two complementary smaller installations are included. Full operation of LAMPF is estimated to require about $20 million per year (including minor capital costs and some allowance for increasing costs of experiments) by 1977.

7. *Small-Scale Projects.* This is an allocation intended to provide some flexibility in the following categories: (a) particularly innovative new enterprises; (b) accelerator facilities in support of user's operations elsewhere—for training, checking instruments calibrations; (c) exploratory applications of nuclear techniques to other fields; (d) research enterprises with a strong educational content.

It is assumed that projects in this category will generally operate at funding levels adequate to support part-time operation but not in general permitting sizable expansion of facilities. Many such projects may be expected to enjoy supplementary support from nonfederal sources. According to Table II.11, $2 million in federal and nonfederal funds supported nuclear research in 33 universities in 1969.

8. *Theory.* The estimates include theorists working with accelerator groups as well as those working in independent projects. It is anticipated that the fraction of nuclear physicists working on theory will increase to about 20 percent by 1977.

9, 10. *Nuclear Spectroscopy, Chemistry.* Allocations here are for nonaccelerator-connected groups only. It is expected that these activities will grow, but mainly at accelerator sites.

361

operating funds; it is assumed that about half of the full operating costs and manpower have been gradually transferred from other projects to attain its projected funding.

Other important possibilities on the nuclear horizon have been put off. Thus, the new generation of electrostatic accelerators cannot be included. Much of the development work on new possibilities for nuclear accelerators has been postponed.

It is clear that the plan of Table II.18 involves many dislocations and implies an increasing dependence on large facilities serving numbers of extramural scientists. To some extent this emphasis is forced by the development of the field and will take place regardless of fiscal constraints. However, the constant manpower limitation forces the situation to a degree that involves some hazard. These drawbacks are very sharply exacerbated in the still more stringent budgets that follow below; we leave these points to be analyzed there, but it must be remembered that they apply to a lesser extent even in this intermediate-budget situation. It is obviously not the case that progress is guaranteed by the support of large facilities alone: if they do not attract skilled and imaginative scientists, they will fail. It must be clear that the actual allocation of support must rest, as it always has, on the continuing systematic evaluation of scientific quality.

6.7.3   *Consequences of a Level Budget*

From 1967 to 1972, the operating budgets for the categories here considered (AEC Division of Research, NSF Nuclear Physics) have remained essentially constant in current dollars. With inflation, this means an actual decline in purchasing power of several percent per year. If this trend were to continue into the next five years, the consequences for nuclear physics would be grave indeed, with or without further inflation. We examine here a possible allocation of constant funds—a "level budget"—and in the next section, a budget declining from 1969 to 1977 at 5 percent per year.

We assume that the realignments forced by budget constraints should take place in such a way as to maximize the prospects for survival of nuclear physics as a field of research. This means, we believe, that the new lines would be pursued, even at the cost of profound changes in the style of research of the nuclear-physics community as a whole.

The *level* budget, which we examine first, already illuminates the problems. As noted before, a number of new unique facilities designed to open new areas of nuclear physics are now under construction and will come into operation in the next several years. Their importance to the nuclear community can be assessed by the large number of experiments being proposed

for them. Further, heavy-ion facilities to make possible a full range of heavy-ion physics have been sought.

It is important that we go ahead in the new directions. It is also important for us to go ahead with the broad base of programs now under way. The question is to what extent both can be done with a level budget.

The new facilities and their research programs (listed in Table II.15) are estimated to need some $40 million annually in operating money by 1977, $25 million above the 1969 levels. These estimates will, of course, require adjustments in the light of actual experiences with the program. If, however, we accept them for now, and if these facilities are to be used in a reasonably efficient manner, $25 million must be taken from the operation of the present program if a level budget is to be maintained (Table II.19). This would be a reduction of more than 50 percent. If a new heavy-ion physics facility is to be included, the reduction in the other programs would be still more severe. Such a cut, especially coming on top of the stringent budgets of the last few years, cannot be faced without drastic

TABLE II.19   Consequences of a Level Budget[a]

	FY 1969			FY 1977		
	No. of Projects	Operating and Research Cost[a] ($Millions)	SMY	No. of Projects	Estimated Operation and Research Cost[b] ($Millions)	Estimated SMY
Existing new facilities	13	13.2	127	13	37.9	320
Other present programs	189	42.8	848	189	18.1	355
TOTALS	202	56.0	975	202	56.0	675

[a] Including full cost of high-energy facilities; AEC (Division of Research) and NSF (Nuclear Physics) only.
[b] Federal dollars, 1969 value.

Table II.19 illustrates the problem brought about by the prospect of a level budget in combination with the impending accession of major new facilities. About $121 million of new facilities are coming into operation in the period 1969–1972. Of these, $114 million worth are facilities supported by the AEC's Division of Research or the NSF's nuclear-physics programs. The new Livermore linac and tandem accelerator are excluded in the present table. In 1969, the operating and research costs associated with these facilities, or with the instruments they replace, was $13 million out of a total budget of $56 million. By 1977, the "new" facilities will require $38 million for what we have called "full operation," typically 15 shifts per week.

Excluding the projects identified in Table II.15, the rest of the AEC Division of Research and NSF nuclear-physics program–some 189 projects–spent $43.4 million in 1969. By 1977, under a level budget, this number will be reduced to $18 million, a loss of more than 50 percent.

The projections in this table make no allowance for future facilities, such as the urgently needed heavy-ion physics accelerator, which would cost an additional $8 million per year in full operation.

consequences. This, then, is the outline of the problem imposed by the level budget.

If the new ventures are passed by, the quality of the nuclear-physics research program will surely not survive. A science can only maintain its vigor and make its contributions if it moves forward to meet the opportunities it has uncovered. But the present programs, too, contain complementary opportunities important to the development of the field. Can the more than 50 percent reduction that a level budget enforces be contained and still leave these programs viable? Our survey of the nuclear community indicates that were a cut of as much as 30 percent distributed uniformly, the groups based on the more major facilities would move beyond a "critical" point to a drastically reduced level of research productivity. A new philosophy is clearly required. The constraints outlined seem to force us inexorably to the idea of maintaining the essentials of the present program with a smaller number of laboratories—but funding them as fully as efficiency demands. Since it is clear that, if only a fraction of the laboratories in each of the major areas can be supported, they must be the best ones, the criteria for the level budgets are determined. The results of such an alignment are presented in Table II.20, which is to be compared with the present distribution, as well as with the possibilities offered by the full budget of Table II.17.

Many of the problems of balance in the field under this level budget, and even more so in the more constricted budgets, are intensified by the very large expenditures required for the LAMPF facility. It is to be noted that the Bethe Panel* that recommended the construction of LAMPF included in its recommendations the following:

We recommend that for the next few years approximately 30% of the new construction budget in nuclear structure physics, and later on about 20 to 25% of the operating budget, be used for a Meson Factory. We feel, however, that this should not reduce the support of nuclear structure laboratories now in existence or under construction, and we, therefore, recommend an increase of the total support for nuclear structure physics. We feel especially justified in making this recommendation because part of the use of a Meson Factory is for high-energy physics, to replace (at least in part) the facilities which would otherwise have been provided by MURA.

A marked retreat from the possibilities offered by the full, or even the intermediate, budget is obviously necessary. Some 54 facilities have been deprived of federal support; one third fewer scientists can be funded. The high-energy nuclear projects are budgeted adequately but at somewhat reduced levels compared to efficient operation. The advanced electron facilities now under construction are planned for, but it has been necesary to drop some on-going effort to make budget room available. Heavy-ion work

* *Meson Factories,* Report of Ad Hoc Panel to the Office of Science and Technology, H. A. Bethe, Chairman (March 1964).

TABLE II.20 Elements of a Level Dollar Budget Excluding DOD, NASA, NBS, and DMA Support (Such a Level Dollar Budget Implies a Decreasing Scientific Man-Year Effort)

	FY 1969			FY 1977[a]		
	No. of Facilities	Operating and Research Cost ($Million)[b]	SMY[c]	Estimated No. of Facilities	Estimated Operating and Research Cost ($Millions)[b]	Estimated SMY[c]
Primarily Accelerator-Centered						
1. Neutron facilities	7	4.0	45	2	3.5	35
2. Potential-drop machines	55	14.5	300	12–16	9.0	120
3. Cyclotrons	17	10.5	150	5	6.5	70
4. Heavy-ion accelerators	2	3.0	15	2	7.5	65
5. Electron accelerators	5	2.0	25	2	2.5	20
6. High-energy facilities	4	7.0	60	1	14.5	75
7. Small-scale projects				10	1.0	20
Nonaccelerator-Centered						
8. Theory		4.0	170		4.5	145
9. Nuclear spectroscopy		3.5	60		2.5[d]	40
10. Nuclear chemistry		4.0	85		2.5[d]	40
11. Accelerator development and instrumentation		1.0	15		1.0	15
12. Nuclear data		1.0	15		1.0	15
13. Other		1.5	35		0.5	15
TOTALS	90	56.0	975	34–38	57.0	675

[a] In the 1977 projections the estimated dollars and scientific man-years (SMY) for a particular category are written for definiteness to the nearest half-million dollars and 5 SMY, but, by the very nature of this exercise, no such accuracy is implied.

[b] Federal dollars, 1969 value. Costs include total support of user groups.

[c] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.

[d] Nuclear chemistry and spectroscopy groups working directly with specific accelerators are included in the facilities categories above. These estimates reflect the presumption that a relatively larger fraction of spectroscopic and nuclear chemistry work will be carried out at accelerator centers.

365

Table II.20 presents a possible strategy for dealing with a constant funding level, while still pressing forward with exploitation of the new facilities. Rather than accepting a uniform attrition of 50 percent or more in all the other programs, we have chosen drastic curtailment of the number of projects. For the survivors, we have allocated moderately comfortable operating levels, in the hope that they will thus be able to operate efficiently and possibly accommodate a fair number of outside collaborators. It is to be emphasized that no magic has been performed: we have still lost many good facilities and highly trained scientists. As compared with Table II.19, in fact we have not only many fewer facilities but fewer man-years, because the facilities chosen here are the relatively more expensive ones.

As in Tables II.17 and II.14, we again present the budget in terms of specific classes of facilities. Again we caution, however, that it is a research program and not a complex of facilities that is under discussion. Thus, for example, the indicated allocations to potential-drop machines and cyclotrons include several groups who are clearly pointing to the heavy-ion field, so the character of the program proposed has changed more than would be indicated by a casual comparison of the figures with those of 1969. In fact, most of the new facilities of Table II.15, which are included in Table II.20 will introduce profound changes in the meaning of our somewhat arbitrarily chosen categories.

1. *Neutron Facilities.* In addition to one of the more versatile reactors, provision is made for ORELA and some modest synchrocyclotron operations to provide moderate-energy neutrons.

2. *Potential-Drop Machines.* Allowance is made for 12–16 facilities to span the diversity necessary for the broad multifaceted attack necessary to the nuclear program including some work with intermediate-mass ions. A range in the number of facilities is given; the exact number will depend on the size of the actual laboratories selected.

3. *Cyclotrons.* Again, as with the potential-drop machines, some variety in capability is afforded by the multiplicity of installations; again some will enter the heavy-ion field.

4. *Heavy-Ion Accelerators.* The only new major facility that it seems possible to press for under a level budget is a single heavy-ion accelerator, estimated to cost $20 million to $25 million to construct and $8 million per year for full operation and research, including users. While we indicate the operation of two facilities, it has been necessary to budget for less than full operation of both. An optimal funding division within the constraints must be left to decision based on the interim discoveries of this just emerging field. About half of the costs and half of the manpower of the new facility will have been gradually transferred from other projects. If this facility should be delayed beyond 1977, it is important that these transfers also be delayed to permit the changes to occur smoothly.

5. *Electron Accelerators.* Only two machines are budgeted here; however, machines in the mission-oriented sector would partially complement the program, as will the Stanford superconducting linac.

6. *High-Energy Facilities.* LAMPF is included. The scale of operation has been reduced to an estimated minimum, with the assumption of a cutback of one third in research use.

7. *Small-Scale Projects.* The allowance for flexibility in this category is sharply reduced.

366

would be expanded above the present program with the converted Berkeley HILAC and with one new national heavy-ion physics facility. However, exploitation of other directions in the nuclear program has been curtailed. The electrostatic or potential-drop accelerator program is continued with only a fraction of the most productive and best of those laboratories presently operating or just coming into operation. Similar cuts are necessary in all other parts of the nuclear program.

It is important to note again that when we speak of the "best" laboratories, we mean the best in quality and not necessarily the largest. In the budget presentations that follow, this point is emphasized by giving, in some categories, a range in the number of facilities supported. The exact number would depend on the size of the actual laboratories selected.

By these drastic measures involving the closing off of many facilities, uprooting many scientists, and depriving the science itself of full potential, it is possible, in principle, to achieve a level budget. This budget attempts to open the areas that progress in nuclear physics requires, while maintaining the necessary multifaceted base; but we have given up depth and diversification to a degree the dangers of which are hard to estimate. We have tried to hold to a minimum the heavy price that has been paid. With only one new facility, that for heavy-ion physics studies, to be started in the eight-year period, we cannot say that an adequate base has been established for the future we confidently should expect from the new field now being opened.

Problems that were glimpsed in the previous budgets become very severe here. Compared to the present program, an increased portion of funding has gone into the new fields of higher energies and heavy ions—a change in direction imposed by the needs of the science itself. However, as a result there has been drastic reduction in the number of facilities being supported in nuclear physics. The high-energy and heavy-ion facilities are large, expensive installations that will involve large in-house efforts, as well as many user groups. The remaining diversified program is mounted with only a small fraction of the present laboratories. While this drastic reduction in number of facilities does not necessarily mean a proportional reduction in independent and competitive research groups, it should be recognized that a new pattern is thereby arising. There has been a move from almost total in-house research toward a large user-group effort at major facilities. However, the fact that the number of facilities has been cut to less than half, with the removal of federal support for more than 50 facilities, has serious long-term implications. The many negative aspects will be discussed later. It is certainly very serious that the scientific manpower that could be maintained by the program is sharply reduced—by some 30 percent below the

number in the present one—since, in general, it is the smaller facilities, costing less per scientist, that have been dropped.

The effect on scientific talent is only partly reflected by the fact that a third fewer scientists could be funded. In the intervening years, several hundred additional nuclear physicists will be trained, thus increasing the number forced out of the field. Putting aside the personal heartaches and the fierce morale problems, one can ask cold questions. To what extent can a large transfer into other scientific fields be effected? Will economy measures in other areas, in fact, permit these transfers? At best, a large re-orientation and relearning effort would be required, and in the massive transfer of people the efficiency of the scientific enterprise as a whole would surely suffer. We can only conclude that the level budget would bring with it the danger of dissipating an important national resource.

Great precautions would have to be taken so that, in spite of the con-traction of the overall basic nuclear effort, its vitality is maintained with the continuing recruitment and support of bright young scientists. Post-doctoral positions must be maintained in suitable numbers. Permanent positions and funding must be devoted to new people as well as to new lines of research. The maintenance of opportunities for young nuclear scientists in the face of overall contractions would not be an easy matter for either the younger or the older practitioners and is a problem for which we have no special prescriptions. As a minimum, it is vital that positions for temporary appointments—postdoctorals, research associates, and junior staff—be maintained.

The changes in the pattern of nuclear research efforts toward the style of big physics and away from the smaller-scale in-house way of study will have other profound if less dramatic effects. The rich experience afforded by in-house graduate training will be greatly diminished. Students will not have the full benefit of pursuing all aspects of an experiment from start to finish. The need for programming every move in advance means the loss of some of the speculative daring that has characterized nuclear-physics re-search.

The loss to less specialized scientific education can also be profound. The close presence of an active, accessible scientific effort is an important ingredient in the education of a modern man—for whom scientific literacy should be one of the basic skills. The continuing education of working scientists too would suffer. No small part of the cross-fertilization between sciences would be removed as the number of research facilities is sharply contracted.

But the most severe loss for the country is simply the slowdown in the rate of progress of the science of nuclear physics—leading to an inevitable

reduction in the direct and indirect discoveries and applications that have
been touched upon earlier in this report. The bare bones of the losses are
to be seen in the data of Table II.20—the reduction in the scientific man-
power that can be devoted to many of the areas of nuclear physics. The
loss in scientific productivity is almost certain to be much greater than
simply proportional to the manpower decrease. Scientific investigations
must be carried out at a steady pace if their active pursuit is to be credible.
For example, an important discovery may take on the order of five years
to explore fully; the decision of the working scientist to persist in this
exploration is often influenced by the rate at which new developments ap-
pear. If this stream is slowed by a factor of 2, say, the five years would at
least double and become a large fraction of a scientific lifetime. Even
worse, the dragging pace will destroy the cohesion and drive necessary if
an exploration into the unknown is to be successfully sustained. Many new
ideas, new facts, new insights must be brought together before a new
substantial forward step is made; this is especially true in nuclear physics,
which is heavily dependent on a base of systematic studies of nuclear
phenomena over a wide range of nuclear species and parameters. The drop
in real scientific productivity, as measured finally in major accomplish-
ments, is then likely to be the product of the decreases of the separate
contributing factors and many times more serious than just proportionate
with the manpower cuts. Wise management and judicious choices of the
areas likely to be fruitful would undoubtedly help toward minimizing the
losses. But there would be severe losses.

Let us squarely face the fact that should these budgets come to pass, an
enormous gamble would have to be taken. In the allocative scheme out-
lined here, the gamble is based on following the new directions as fully as
possible and thinning out the rest of the program to the best that can be
mounted with the remaining resources. The gamble is that more will be
gained in the unknown areas already partly explored. It has been pointed
out repeatedly that, by the very nature of nuclear physics, to cut off the
multifaceted diverse program is to kill the subfield. To cut off new fron-
tiers is, by the very nature of science, also guaranteed to kill the subfield.
We have tried to tread the middle ground, to minimize the risks and the
losses necessitated by the constrictions that such budgets would mean,
but there are still considerable risks and losses remaining.

This analysis has been predicated on the assumption that these stringent
budget conditions will apply to the indefinite future beyond the 1969–
1977 analysis period. If we knew that an expanding situation were to fol-
low this time period, another course of action might be well advised: hold
to the scientific manpower at all costs and plan for an expansion in the

future. Our charge here has been to analyze the less optimistic prospects of a steady state.

6.7.4 Consequences of a Declining Budget

In order to portray the effects on basic nuclear research of a declining level of support—measured in dollars of constant purchasing power—a possible response to a budget decrease of 5 percent per year from 1969 through 1977 is examined here. This extreme situation is quite conceivable; for example, a budget constant in dollars but subjected to a 5 percent inflationary rate would lead to such a result. In fact, the "declining" budget is very nearly the simple extrapolation of the 1968–1972 trend.

The problems that must be faced are sharp amplifications of those posed by the level budget. The nature of these problems can be briefly recapitulated. New facilities designed to attack new opportunities in nuclear physics are under construction; further, a heavy-ion physics facility is being sought to exploit the subfield. These are expensive ventures, requiring a sizable share of the budget. At the same time, it is important to continue the exploration of the areas for which the present multifaceted program was designed. Under the conditions of the level budget it was seen that if the new ventures were to be funded, the cuts inflicted on the multifaceted program would be so severe that a new philosophy would appear advisable. Instead of an across-the-board contraction, the multifaceted program was to be based on only a fraction of the laboratories now involved.

This declining budget aggravates the problems, removes more of the flexibility, and cuts deeper into the life of the subfield. There can be no alternative to basing the multifaceted program on a very much smaller fraction of the best laboratories. In addition, we must cut back on the new ventures with significant loss of strength in both the high-energy and heavy-ion fields. In the budget outlined in Table II.21, the new ventures are attacked on a minimal level and the remaining program distributed as prudently as possible. The multifaceted program is barely preserved; it is certainly perilously close to extinguishment. Even with the drastic measures indicated, including the abandonment of the heavy-ion physics facility, there seems to be no possibility of significant support of LAMPF in the present budget. By 1977, the entire budget would be only twice that required for LAMPF alone; to support LAMPF from this budget and leave only $20 million for the rest of nuclear physics would quite simply destroy the subfield.

There can hardly be serious consideration within the entire national budget picture of actually abandoning LAMPF; a facility that has cost $56 million to build and one whose completion is eagerly anticipated by scien-

TABLE II.21 Elements of a Declining Budget Excluding DOD, NASA, NBS, and DMA Support

	FY 1969			FY 1977[a]		
	No. of Facilities	Operating and Research Cost ($Millions)[b]	SMY[c]	Estimated No. of Facilities	Estimated Operating and Research Cost ($Millions)[b]	Estimated SMY[c]
Primarily Accelerator-Centered						
1. Neutron facilities	7	4.0	45	2	3.5	25
2. Potential-drop machines	55	14.5	300	11–16	9.0	115
3. Cyclotrons	17	10.5	150	5	6.5	70
4. Heavy-ion accelerators	2	3.0	15	1	3.0	15
5. Electron accelerators	5	2.0	25	1	2.0	15
6. High-energy facilities	4	7.0	60	1	2.5	15
7. Small-scale projects				10	1.0	20
Nonaccelerator-Centered						
8. Theory		4.0	170		4.5	140
9. Nuclear spectroscopy		3.5	60		2.5[d]	40
10. Nuclear chemistry		4.0	85		2.5[d]	35
11. Accelerator development and instrumentation		1.0	15		1.0	10
12. Nuclear data		1.0	15		1.0	15
13. Other		1.5	35		0.5	10
TOTALS	90	56.0	975	31–36	38.5	535

[a] In the 1977 projections the estimated dollars and scientific man-years (SMY) for a particular category are written for definiteness to the nearest half-million dollars and 5 SMY, but, by the very nature of this exercise, no such accuracy is implied.

[b] Federal dollars, 1969 value. Costs include total support of user groups.

[c] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.

[d] Nuclear chemistry and spectroscopy groups working directly with specific accelerators are included in the facilities categories above. These estimates reflect the assumption that a relatively larger fraction of the spectroscopic and nuclear chemistry work will be carried out at accelerator centers.

Table II.21 paints the lugubrious picture of a declining budget, decreasing at 5 percent per year in purchasing power, to a level of $38.5 million (1969 dollars) in 1977

Even with a minimal exploration of the frontiers of nuclear physics, a declining budget forces an extreme centralization of facilities. Just how extreme this process needs to be is indicated by the treatment of individual categories outlined below.

1. *Neutron Facilities.* Only two survive, probably including ORELA.

2, 3. *Potential-Drop Machines, Cyclotrons.* 11–16 installations selected from the best and most versatile of the existing laboratories. Several of these will stress the heavy-ion field.

4. *Heavy-Ion Accelerator.* Under this budget there is no possibility of the full-scale new facility discussed previously. Because of the great importance of heavy-ion nuclear physics and the need to go further into this field, we press for, as a minimum, a facility of intermediate capability. We have not investigated the precise design that would meet these intermediate needs. It might be speculated that an intermediate design would involve conversion of an existing facility.

5. *Electron Accelerators.* One installation is budgeted to provide some coverage of the regions of high-energy, high-intensity electrons, and, it is hoped, monochromatic photons.

6. *High-Energy Facilities.* As noted, funding of LAMPF would bring the subfield to the point of serious unbalance; and if no other sources of funding for LAMPF become available, there would be no possibility of significant support in this budget. We have therefore omitted LAMPF from this table and maintain an entry into this field with a smaller facility.

The degree of centralization forced by this budget may be appreciated by noting that of the 90 accelerator installations listed in Table II.16, only 31–36 remain. Of the others, most will have to be entirely or very nearly abandoned, although some few may be kept in operation on nonfederal funds.

tists in many fields besides nuclear physics, cannot simply be turned off. The point at issue is: How shall it be financed? Our considerations here show that it cannot be supported from a level nuclear-physics budget without severe damage to the subfield; and in a budget steadily declining by 5 percent per year, it cannot be supported at all. At the very least, supplementary funds must be found to support the operation of LAMPF, starting in fiscal year 1973. Even so, the general health of nuclear physics will be seriously impaired unless an overall funding level approximating that of Table II.18 is achieved.

If LAMPF were to be separately funded and the rest of the budget were allowed to decline by 5 percent per year, we should still face a grim prospect, as Table II.21 shows. We would be able to support only 31–36 facilities, instead of the present 90, and only about half of the scientists now working in basic nuclear research.

The strong network of active nuclear laboratories would be dismembered. Even more important, the resource of a strong group of active nuclear scientists would be dissipated. These are acts whose effects would be felt over a very long term. It would not be easy to recreate the institutions, for a laboratory closed temporarily is not simply reawakened. Scientists are created only by a long educational process. The situation that would be brought about by such a declining budget is far worse than the already bad one of the level budget. It would bring the field to near disaster and cut the contributions nuclear physics now makes to the many areas of our society to a comparative trickle.

The disastrous effects on education, both specialized and general, would be far worse than those judged by any extrapolation from the level budgets. In-house research facilities would be very few and far between. The nuclear research remaining would have moved very largely into a user-group pattern at relatively few facilities. The nearness to an ongoing scientific research effort that is an important ingredient to the intellectual background of modern society and a modern university would almost disappear.

Far worse for the national welfare is the effect on the field of nuclear physics and, consequently, on its contribution to science and technology. The attempt has been to allocate the resources available so as to leave the field in the most viable form possible within the constraints. The attacks on the new frontiers of the science are the *sine qua non* of such attempts. But how thinly spread are the resources! The new directions can only be barely entered. Further, the multifaceted base program outside the new directions has been cut in scientific manpower support by nearly a factor of 2. As we have argued in the level-budget case, the effect on research productivity is much greater than simply proportionate. The cut in the number of independent facilities is in part made up by the formation of user

groups directed toward the utilization of the new facilities; nevertheless, so great is the cut necessary that one must expect a very great loss in vitality that cannot now be estimated.

The question of still further cuts in funding support would have to be faced in an entirely new way. It would no longer be possible to keep a multifaceted effort in nuclear physics. Instead it would appear wiser to opt for a decreasing reach, drop whole categories of nuclear research, and bargain with the world for a smaller place in the overall international effort.

6.7.5   *The Total Federally Supported Program in Basic Nuclear Physics*

To this point, in our discussions of various budgetary contingencies, we have considered only the basic nuclear-physics research supported by the AEC Division of Research (part of Chemistry, all of Medium Energy Physics, part of Low Energy Nuclear Physics) and by the NSF, Nuclear Physics Program. As noted earlier, a significant fraction, about 20 percent, of the support for basic nuclear-physics research derives from applications-oriented organizations such as the DOD, NASA, NBS, and the AEC's Division of Military Applications. In Tables II.22–II.25 we assume that the total program could be realigned in response to the possible budget situations discussed in the preceding sections of the chapter.

TABLE II.22 Elements of an Expanding Budget Including All Federally Supported Basic Nuclear-Physics Research

	FY 1969			FY 1977[a]		
	No. of Facilities	Operating and Research Cost ($Millions)[b]	SMY[c]	Estimated No. of Facilities	Estimated Operating and Research Cost ($Millions)[b]	Estimated SMY[c]
Primarily Accelerator-Centered						
1. Neutron facilities	11	7.0	80	8	12.0	110
2. Potential-drop machines	64	18.5	340	40	35.0	460
3. Cyclotrons	21	13.5	200	14	14.5	160
4. Heavy-ion accelerators	2	3.0	15	2	12.0	100
5. Electron accelerators	9	4.5	50	6	7.5	85
6. High-energy facilities	5	8.0	75	2	22.5	125
7. Small-scale projects				30	4.0	70
Nonaccelerator-Centered						
8. Theory		5.5	195		14.0	450
9. Nuclear spectroscopy		3.5	65		4.5	60
10. Nuclear chemistry		4.5	95		8.0	120
11. Accelerator development and instrumentation		1.0	15		4.0	50
12. Nuclear data		1.0	15		2.0	30
13. Other		2.0	40		3.0	60
14. Future facilities					10.0	70
TOTALS	112	72.0	1185	102	153.0	1970

[a] In the 1977 projections the estimated dollars and scientific man-years (SMY) for a particular category are written for definiteness to the nearest half-million dollars and 5 SMY, respectively; but, by the very nature of this exercise no such accuracy is implied.

[b] Federal dollars, 1969 value. Costs include total support of user groups.

[c] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.

TABLE II.23 Elements of a Nearly Constant Manpower Budget Including All Federally Supported Basic Nuclear-Physics Research

	FY 1969			FY 1977[a]		
	No. of Facilities	Operating and Research Cost ($Millions)[b]	SMY[c]	Estimated No. of Facilities	Estimated Operating and Research Cost ($Millions)[b]	Estimated SMY[c]
Primarily Accelerator-Centered						
1. Neutron facilities	11	7.0	80	5	7.0	70
2. Potential-drop machines	64	18.5	340	31–37	20.5	275
3. Cyclotrons	21	13.5	200	11	11.5	125
4. Heavy-ion accelerators	2	3.0	15	2	10.5	100
5. Electron accelerators	9	4.5	50	4	5.5	55
6. High-energy facilities	5	8.0	75	2	22.5	125
7. Small-scale projects				20	2.5	40
Nonaccelerator-Centered						
8. Theory		5.5	195		7.0	220
9. Nuclear spectroscopy		3.5	65		3.5[d]	55
10. Nuclear chemistry		4.5	95		2.5[d]	40
11. Accelerator development and instrumentation		1.0	15		2.5	20
12. Nuclear data		1.0	15		1.5	25
13. Other		2.0	40		2.0	40
TOTALS	112	72.0	1185	75–81	99.0[e]	1190

[a] In the 1977 projections the estimated dollars and scientific man-years (SMY) for a particular category are written for definiteness to the nearest half-million dollars and 5 SMY, respectively; but, by the very nature of this exercise, no such accuracy is implied.

[b] Federal dollars, 1969 value. Costs include total support of user groups.

[c] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.

[d] Nuclear chemistry and spectroscopy groups working directly with specific accelerators are included in the facilities category above. These estimates reflect the presumption that a relatively larger fraction of spectroscopic and nuclear chemistry work will be carried out at accelerator centers.

[e] In 1972 dollars this number is $105 million.

376

TABLE II.24 Elements of a Level Dollar Budget Including All Federally Supported Basic Nuclear-Physics Research

	FY 1969			FY 1977[a]		
	No. of Facilities	Operating and Research Cost ($Millions)[b]	SMY[c]	Estimated No. of Facilities	Estimated Operating and Research Cost ($Millions)[b]	Estimated SMY[c]
Primarily Accelerator-Centered						
1. Neutron facilities	11	7.0	80	4	4.5	60
2. Potential-drop machines	64	18.5	340	18–24	17.0	225
3. Cyclotrons	21	13.5	200	7	8.5	100
4. Heavy-ion accelerators	2	3.0	15	2	7.5	65
5. Electron accelerators	9	4.5	50	4	5.0	50
6. High-energy facilities	5	8.0	75	1	14.5	75
7. Small-scale projects				10	1.0	20
Nonaccelerator-Centered						
8. Theory		5.5	195		5.5	185
9. Nuclear spectroscopy		3.5	65		2.5[d]	45
10. Nuclear chemistry		4.5	95		3.0[d]	45
11. Accelerator development and instrumentation		1.0	15		1.0	15
12. Nuclear data		1.0	15		1.0	15
13. Other		2.0	40		0.5	15
TOTALS	112	72.0	1185	46–52	72.5	915

[a] In the 1977 projections the estimated dollars and scientific man-years (SMY) for a particular category are written for definiteness to the nearest half-million dollars and 5 SMY; but, by the very nature of this exercise, no such accuracy is implied.

[b] Federal dollars, 1969 value. Costs include total support of user groups.

[c] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.

[d] These estimates reflect the presumption that a relatively larger fraction of spectroscopic and nuclear chemistry work will be carried out at accelerator centers.

377

TABLE II.25 Elements of a Declining Budget Including All Federally Supported Basic Nuclear-Physics Research

	FY 1969			FY 1977[a]		
	No. of Facilities	Operating and Research Cost ($Millions)[b]	SMY[c]	Estimated No. of Facilities	Estimated Operating and Research Cost ($Millions)[b]	Estimated SMY[c]
Primarily Accelerator-Centered						
1. Neutron facilities	11	7.0	80	3	3.5	30
2. Potential-drop machines	64	18.5	340	15–20	14.0	180
3. Cyclotrons	21	13.5	200	7	8.5	100
4. Heavy-ion accelerators	2	3.0	15	1	3.0	15
5. Electron accelerators	9	4.5	50	2	3.0	30
6. High-energy facilities	5	8.0	75	1	2.5	15
7. Small-scale projects				10	1.0	20
Nonaccelerator-Centered						
8. Theory		5.5	195		5.0	150
9. Nuclear spectroscopy		3.5	65		2.5[d]	45
10. Nuclear chemistry		4.5	95		3.0[d]	45
11. Accelerator development and instrumentation		1.0	15		1.5	15
12. Nuclear data		1.0	15		1.0	15
13. Other		2.0	40		0.5	15
TOTALS	112	72.0	1185	39–44	48.0	675

[a] In the 1977 projections the estimated dollars and scientific man-years (SMY) for a particular category are written for definiteness to the nearest half-million dollars and 5 SMY; but, by the very nature of this exercise, no such accuracy is implied.

[b] Federal dollars, 1969 value. Costs include total support of user groups

[c] Scientific man-years (SMY) are estimated totals, including both federally and nonfederally supported scientists.

[d] These estimates reflect the presumption that a relatively larger fraction of spectroscopic and nuclear chemistry work will be carried out at accelerator centers.

378

7 Manpower — Supply, Demand, Education, and Utilization

7.1 INTRODUCTION

People are at the heart of any scientific enterprise—and so it is with nuclear physics. The discoveries in the basic science and the applications to practical ends are the result of the interaction of many scientifically trained minds and a complex of nuclear machines and instrumentation. The enormous impact of nuclear physics has come about because of the ingenuity, insights, and work of its practitioners both within and outside their fields of specialization. The contributions nuclear physics has made in so many areas are largely a result of the ingenuity and insight of scientists and engineers, coupled with generous financial support from the federal government.

The United States leads the world in nuclear technology. To maintain this position, we must continue to train and to introduce into the field appropriate numbers of people in the science of nuclear physics. In this chapter we review the current education and employment of nuclear physicists and attempt to assess future requirements.

7.2 THE ROLE OF NUCLEAR PHYSICISTS IN SOCIETY

The nuclear-physics enterprise encompasses much more than a basic research program. In Chapter 3 we drew attention to the importance of the nuclear physicist in endeavors such as power development, instrumentation work, medical applications, and the national defense and disarmament programs. It hardly need be added that he plays an important role in the education of our scientifically based society. The training of young nuclear physicists through their doctoral degree is, of course, the duty of the university physicists; the national laboratories, too, participate in the training of graduate students mainly through the user-group mechanism. Both the universities and government laboratories participate in postdoctoral training. The need for scientific education is, however, broader than the training of specialists. There is the important need for the teaching of physics in the general university and college community, and here the nuclear physicist plays his proportionate part. The requirement of research training as part of the qualification of teachers is clear in the university and increasing in the two- and four-year colleges. Extension into the secondary schools

TABLE II.26   Distribution of Physicists According to Institutions and Primary Activity[a]

Primary Activity	Total (%)	Univ. or College (%)	Industry (%)	Govt. (%)	Research Center (%)	Other (%)
Total Physicists (PhD)						
Basic research	38.6	20.10	5.41	4.95	6.55	1.60
Applied research	14.9	1.33	7.95	1.23	3.50	0.91
Development and design	1.4	0.10	1.00	0.06	0.21	0.07
Management R&D	12.9	1.33	6.00	2.23	2.55	0.75
Management other	3.8	2.10	0.97	0.29	0.08	0.38
Teaching	23.5	23.10	0.02	0.02	0.03	0.39
Other	2.0	0.25	0.75	0.28	0.24	0.52
No response	2.8	1.60	0.59	0.22	0.21	0.13
TOTAL	100	49.9	22.7	9.3	13.4	4.8
Total Nuclear Physicists (PhD)						
Basic research	42.3	23.96	1.67	3.90	11.11	1.68
Applied research	13.8	0.94	3.56	0.67	7.54	1.12
Development and design	1.4	0.10	0.33	–	0.94	0.06
Management R&D	14.6	1.19	3.85	2.01	6.93	0.67
Management other	3.5	1.94	0.39	0.45	0.28	0.39
Teaching	20.2	19.75	0.05	–	0.06	0.33
Other	2.0	0.30	0.39	0.39	0.55	0.39
No response	2.2	1.54	0.17	0.28	0.11	0.06
TOTAL	100	49.7	10.4	7.7	27.5	4.7

[a] From replies to the 1968 National Register of Scientific and Technical Personnel.

appears natural; however, no immediate movement in this direction is now evident.

The distribution of nuclear physicists in the nuclear enterprise is presented in Table II.26 and Figure II.19; data were taken from the National Register of Scientific and Technical Personnel for 1968. For purposes of this report, we define a nuclear physicist as a respondent to the Register who lists a nuclear-physics specialty as his employment. For comparison, the distribution of the total physics community is included. The data show that nuclear physicists are mostly academically based; the nuclear research center is a close competitor for people. The nuclear physicist, to a greater extent than his colleagues, staffs the research center rather than industrial laboratories. Nuclear physicists participate directly in industry to a larger extent than do the elementary-particle physicists but much less than those specializing in the more classical subfields.

The sizable group of nuclear physicists without doctoral training have quite a different distribution of institutional population. A higher percentage is in industrial laboratories and research centers.

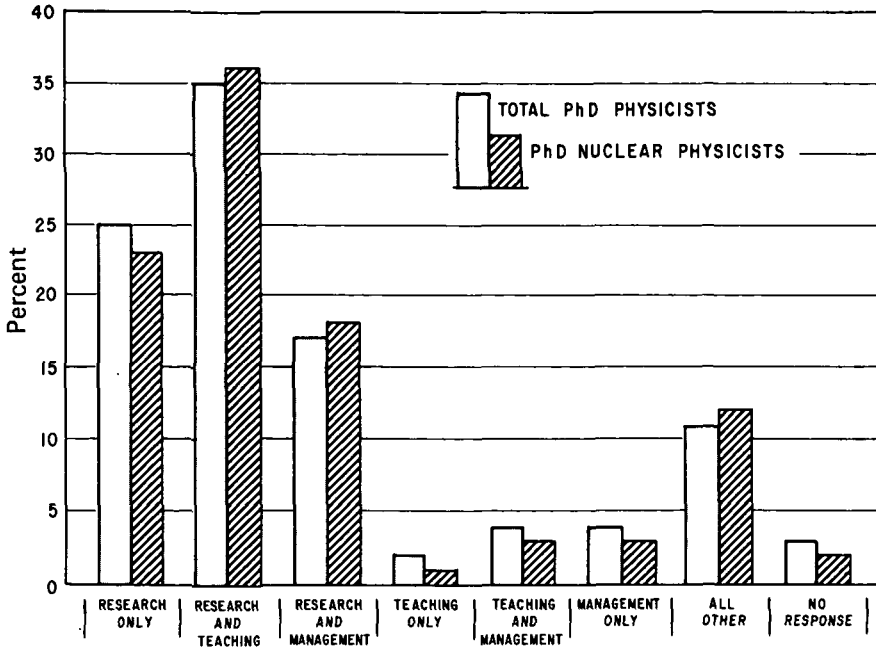The nuclear physicist is primarily a researcher and a teacher as can be

FIGURE II.19   Distribution of total PhD physicists and PhD nuclear physicists among major areas of activity. (Taken from the 1968 National Register of Scientific and Technical Personnel, combination of first and second scientific specialty.)

seen from Table II.26 and Figure II.19. Nuclear physicists carry a slightly larger proportion of management responsibilities in industry together with the research center than is the case for all physicists.

The data collected by the National Register of Scientific and Technical Personnel show that in 1968 there were about 3700 nuclear physicists of whom 2000 had PhD's. The number of nuclear physicists is exceeded only by that of those specializing in work with condensed matter. This distribution of physicists in subfields of physics is given in Table II.27. Other scientists are also engaged in nuclear-physics research: nuclear chemists and accelerator and instrumentation specialists constitute approximately 10 percent of the scientific manpower.

The kind of work a nuclear physicist does depends very much on his age or, more accurately, the length of time since graduation. Analysis of the responses to the 1968 National Register of Scientific and Technical Personnel indicates that an expected pattern does occur. The proportion of recent graduates in basic research is high (64 percent), and this proportion drops as the professional age increases, to 18 percent for those in the pro-

TABLE II.27   Distribution among Subfields of Physics[a]

Subfield	Number of PhD's	Number of non-PhD's	Total Number of Scientists in Subfield
Nuclear physics	1,804	1,530	3,334
Condensed matter	4,112	3,838	7,950
Optics	752	2,072	2,824
Astronomy, astrophysics, relativity, space and planetary physics	1,408	1,243	2,651
Elementary-particle physics	1,297	683	1,980
Atomic, molecular, and electron physics	1,006	684	1,690
Plasmas, fluid physics	935	701	1,636
Acoustics	298	852	1,150
Physics in biology	206	145	351
Other	2,493	6,432	8.925
TOTAL	14,311	18,180	32,491

[a] The distribution among the subfields shown in the table was arrived at from replies to the 1968 National Register of Scientific and Technical Personnel. It has been roughly estimated that there was a 90 percent overall response, and no survey has yet been made to sample the relative response rate in the different subfields. If the reasonable assumption of a 90 percent response is made, the number of nuclear physicists is determined as about 3700, of whom 2000 hold the doctoral degree.

fession for 30 years. Management, mainly of research, increases with professional age to 46 percent for the oldest group. Teaching peaks to 30 percent after five years, while applied work is fairly constant at 14 percent. (While these data represent only the 1968 picture, they seem quite naturally to follow the changes expected if one were to follow the careers of some graduating class.) The concentration on basic research, with many young physicists in postdoctoral positions, breaks early, as permanent positions are found. There is a sizable outflow to teaching jobs. Of those who remain in research, promotion into research management positions—as project leaders—can be counted a change in category. The median age of those in basic research is 34 years, while the research managers average some 10 years older. This picture is not much different from that for physics as a whole, except that there is a larger concentration of nuclear physicists in basic research soon after graduation and a sharper decrease with increasing professional age as they increasingly shift to teaching and management.

The age distribution of nuclear physicists, as it was in 1968, is shown in Figure II.20. The peaking at the youthful 30–40-year age groups reflects the interest of young people in entering the profession and the expanding nature of the nuclear establishment up to 1968. It is a healthy distribu-
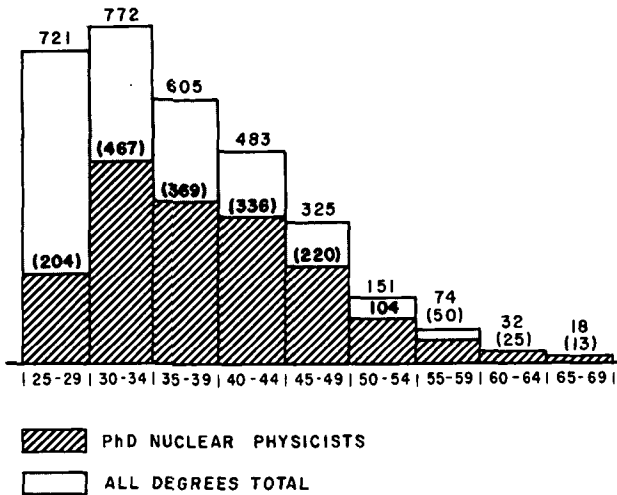
FIGURE II.20   Distribution of nuclear physicists by age.
(Data from 1968 National Register of Technical and Scientific
Personnel.)

tion, for it indicates a blend that mixes young enthusiasm with a continu-
ing and mature contingent of practitioners. The maintenance of such a bal-
ance is critically important; this is certain to become a problem if funding
exigencies force a cutdown in the existing overall effort.

7.3 MOBILITY OF NUCLEAR PHYSICISTS

This picture of the nuclear community is only a snapshot in a developing
pattern. The extent to which physicists move into and out of nuclear phys-
ics is an important factor. Since little in the way of migrational data is
available, the Panel undertook a small pilot survey of the graduating
classes of 1961 and 1965 to spot trends that might be useful in projecting
future manpower requirements. The results are presented in Table II.28.
Fully half of the class of 1961 has migrated into other fields, primarily
into elementary-particle, solid-state, and plasma physics, although a sig-
nificant number moved into health-oriented sciences. The class of 1965
has not migrated so much, perhaps because there has not been time, but
migration appears to have taken place into basic sciences with which nu-
clear physics has close ties as well as into applied fields. This successful
mobility supports the view that a nuclear-physics education produces flex-

TABLE II.28 Mobility of Nuclear Physicists

	Present Distribution of Those Who Received Nuclear-Physics PhD's in 1961		Present Distribution of Those Who Received Nuclear-Physics PhD's in 1965	
	Stayed in Nuclear Physics	Moved to Other Fields	Stayed in Nuclear Physics	Moved to Other Fields
Basic research	35%	15%	30%	10%
Teaching	10%	10%	25%	5%
Applied research }	5%	15%	15%	10%
Management }		10%	5%	—

ible and versatile scientists capable of switching into new endeavors. The statistics are too scanty to be conclusive, but it is interesting to note that the class of 1965 appears to be moving more rapidly into areas of applied research. As the country finds need for scientists in new callings for which regularized study disciplines have not yet been established, such as pollution control and abatement, the other fields of science must be tapped for manpower. The indications are that the training given nuclear physicists enables them to move quickly in new directions.

7.4 EMPLOYMENT PROSPECTS

Given this versatile enterprise, what can we forecast about the future needs of manpower? In addition to the usual difficulties that greet the soothsayer, the current situation of rapidly changing conditions of science funding requires that any attempt at prediction be treated with the greatest

TABLE II.29 Number of Nuclear-Physics and Chemistry PhD's Granted in Recent Years[a]

Year	Nuclear Physics		Nuclear Chemistry[b]	Total	Total Physics and Astronomy
	Exp.	Theoret.			
1963	138	17	36	191	858
1967	172	27	26	225	1233
1969	158	30	29	217	1325

[a] From the National Research Council, Office of Scientific Personnel, Doctorate Records File.

[b] Nuclear chemistry is included in this table because a large fraction of the doctoral theses in this field are subjects that fall within the domain of nuclear-physics interests.

TABLE II.30   Institutions That Have Granted More Than 10 PhD's in Nuclear Physics in 1960–1969[a]

Institution	Average No. of Nuclear-Physics PhD's Granted per Year		Institution	Average No. of Nuclear-Physics PhD's Granted per Year	
	1960–1969	1965–1969		1960–1969	1965–1969
MIT	7.3	8.2	Purdue	2.6	3.6
Yale	6.1	7.0	Vanderbilt	2.6	2.8
Wisconsin	5.8	6.0	Rensselaer	2.5	4.0
Cal Tech	5.5	5.8	Rochester	2.4	2.4
Florida State	4.5	5.2	Pennsylvania	2.3	2.6
Indiana	4.3	3.6	Iowa State	2.2	3.0
Ohio State	4.1	3.6	Johns Hopkins	2.1	2.2
Rice	4.0	3.6	Kansas	2.1	2.2
California, Berkeley	3.8	2.4	Colorado	1.9	2.6
Duke	3.7	3.8	Harvard	1.9	0.8
Michigan	3.6	3.2	Tennessee	1.8	2.4
Columbia	3.5	3.4	Cornell	1.6	1.0
Stanford	3.5	4.2	Iowa	1.6	2.4
Texas	3.4	5.0	Michigan State	1.6	3.2
Notre Dame	3.3	3.2	Carnegie Tech	1.3	1.6
Washington	3.3	3.6	LSU	1.3	1.2
Illinois	3.2	3.0	Rutgers	1.3	2.6
Minnesota	3.2	4.2	VPI	1.3	2.2
Princeton	3.2	3.6	Chicago	1.2	0.6
Virginia	3.2	2.6	Kentucky	1.2	2.0
UCLA	3.1	3.6	Oregon	1.2	1.8
Maryland	2.9	4.0	Wash. U., St. Louis	1.2	1.2
Penn State	2.8	2.0	Catholic	1.1	1.8
Pittsburgh	2.8	3.6	California, Davis	1.0	2.0
Case Western Reserve	2.6	3.2	Oregon State	1.0	1.2

[a] From the National Research Council, Office of Scientific Personnel, Doctorate Records File.

of caution. Expertness can only be achieved through a continuing effort, year after year, to track and predict the manpower pattern in each of the subfields of physics.

In Table II.29 we see that during the 1963–1969 period an average of about 200 PhD nuclear physicists and nuclear chemists were graduated per year.

Table II.30 lists the universities that have awarded at least 10 PhD degrees in nuclear physics in the 10-year period 1960–1969.

A relatively small fraction of the PhD's in nuclear physics is granted to nonimmigrant aliens. For example, only 107 nuclear-physics PhD's were awarded to this group in 1965–1969, 106 in 1960–1964, compared with the total number of 1580 during 1960–1969. Further, of these 213 PhD's

only 99 planned to take even their first position in the United States. Foreign students, therefore, do not appear to have much to do with the main unemployment problems. They do form a small but significant area of international influence.

During 1960–1969, support of nuclear research increased in 1967 and was accompanied by a sharp growth in the size and number of university research groups. College physics teaching also expanded. Retirement because of age opened a few positions. Migration out of the field, as noted below, was sizable. From the age distributions, we conclude that, until 1969, about half of the graduating PhD's were able to find jobs in basic nuclear physics. The rest, some perhaps unwillingly, found employment in other areas.

Any prognostication of manpower needs depends strongly on the staffing of the university and research laboratories, which in turn are closely tied to the national funding patterns. If the full exploitation of nuclear physics, outlined in Chapter 6, is realized in an expanding budget, then the rate of production of doctorates in nuclear physics will barely suffice for these and other needs of the whole nuclear enterprise.

If, however, the nuclear community is faced with a sharply reduced budget over the next period, a very few positions in universities and national laboratories will still be available, although the reduction of the overall scientific staffs will result in sizable unemployment among older physicists, especially those finishing postdoctoral appointments. Financial constraints may force a slowdown of the growth of the general educational establishment and lead to smaller needs than in the past. Retirement because of age will open up only a very few positions. A limited survey of the types of industrial organizations that now have nuclear physicists on their staffs indicated that their rate of employment would not increase in the next few years. Some graduates will take advantage of the opportunities to work in other areas and in new multidisciplinary programs—such as environmental pollution, transportation, and urban problems. Our best guess at the net outcome is that, even under a level budget of constant purchasing power, there will be a drop of some 50 percent in the number of new nuclear PhD's needed to go into the channels of previous years—just over 100 new graduates. On this same reasoning less stringent budgets lead to larger recruitment.

In order to check these extremely crude estimates, another calculation of employment prospects was made. Following the National Science Foundation report on *Science and Engineering Doctorate Supply and Utilization 1968–1980* (published in 1969) and the paper by W. R. Gruner, "Why There is a Job Shortage," *Physics Today* (June 1970, page 21), an estimate of 20 positions per year for nuclear physicists at two- and four-

C5

year colleges results. A rough estimate of positions in research and development made on the same basis leads to 50 positions per year. On the assumptions of level budget and no reorientation in the field, our survey indicates that the basic research institutions—the universities and national laboratories—will have some 30 more. The total of 130 is slightly larger than the requirement of 100 PhD's noted above, but we must bear in mind that even a level budget results in a decrease of total manpower by about 200 man-years (see Chapter 6).

We caution against using these estimates as a justification for slashing admissions to graduate study in nuclear physics should funding exigencies arise. It is, however, clear to us that under conditions of level or reduced funding a sizably larger number of young nuclear physicists will have to find employment in fields other than nuclear-physics research than have done so in the past.

The extreme difficulty in making accurate predictions points up the need for a continuing appraisal and a more detailed gathering of manpower statistics. The requirement for careful monitoring arises from two characteristics of the nuclear-physics program. A doctoral education is expensive in time, money, and the use of the research facilities involved; it is a large investment to be made only with prudent foresight. On the other hand, the importance and expanding utility of nuclear physics demands a steady stream of highly trained young people. The Bureau of Labor Statistics* estimates that by 1980 there will be a sizable shortage of trained physicists for the needs of the economy. Whether these needs will be matched with funding required for utilization of such people can only be conjectured. It is clear that continuation of the present trend will result in a future shortage.

There have been funding constraints since 1967, and recent graduates are experiencing difficulties in finding positions. While almost all have finally succeeded, many have had to go rather far from the field in which they have been educated.

This has produced a feeling of crisis, both in the graduates and those looking toward graduation in the next few years. The academic pipelines are now full, and for the next three years we can expect the current rate of new nuclear PhD's to hold. If funding constraints continue, the need to look far afield from the time-honored situations may well be necessary. A great concern is that some graduates will be forced out of the area where their education can be used with real profit. If this comes to pass,

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*"College Educated Workers 1968–1980," Bulletin 1676, U.S. Department of Labor, Bureau of Labor Statistics (U.S. Govt. Printing Office, Washington, D.C., 1970).

conservation measures would be necessary, and these are touched upon in the next section.

7.5 HOW SHOULD THE SUPPLY AND DEMAND BE MATCHED?

Wisdom and care must be exercised in placing artificial restrictions on the number of students given training in nuclear physics, since the effects that follow have a long time delay. The average time for graduate training through the nuclear-physics doctorate is about five and a half years. The first two years of graduate study are generally applicable to all of physics, but a specialization time of over three years is involved. Shifts from one subfield to another then have this latter delay time built in, and the more serious shifts in or out of physics have still longer periods. The long-range demands for nuclear physicists must be known before constraints on admissions are undertaken.

It is recognized that prudent use of the resources of the university research community requires that the number of new graduates be matched rather closely with needs. However, any system devised toward this end must be flexible in order to make possible the necessary expansions that would appear to be the inevitable long-term pattern dictated by the rich potential of nuclear science. Should it become necessary to cut down on PhD output, we believe that there should be an appropriate shift in the funding in the organization of research in nuclear physics from research by graduate students to increased numbers of postdoctoral appointments. The funding shift away from or toward graduate-student participation should, in the future, be contingent on the monitored needs for graduates trained in this field. In the immediate future, an increase in postdoctoral positions would have the additional benefit of permitting the inclusion in the nuclear research program of young PhD's excluded by present funding limitations.

If constraints on the number of students to be trained in nuclear physics are required, care must be taken that it is the best laboratories that do the training. Graduate training should, then, be carried on at those universities that share in forefront research.

7.6 SUMMARY OF MANPOWER SITUATION

Nuclear physics has before it expanding horizons toward which it should move. If funding restrictions are such as to brake the move into new directions, the need for new people will be restricted. We believe that it is

important to maintain a balance between the numbers trained and the numbers needed, and that this must be done in a way that will not discourage the talented young people, who will be important in future national needs for scientific manpower, from entering the field.

7.7   COMMENTS ON THE NATURE OF GRADUATE EDUCATION

Graduate education is an essential part of the overall nuclear establishment. The program is usually made up of several years of formal courses and lectures that cover the broad range of all physics, together with some specialized courses and seminars. As already noted, the research effort that forms the heart of graduate education is on the average of three years' duration, so that the total time to a doctorate is about five and a half years. Two additional years are usually spent in postdoctoral work, so that a total of seven or eight years beyond the bachelor's degree is required to educate a nuclear physicist.

The importance of direct research experience in the education of a scientist cannot be overstated. If teaching is to be based on a deep understanding of the field, it is best done by people who have shared the agonies and the pleasures of truly finding something that no one has known before. It is becoming the fashion to suppose that college teaching can be done well by people whose training stops short of the research dissertation. So much is learned and first understood during this period of training as to make this conjecture highly questionable.

This is true of nuclear physics. The field itself is intimately connected with a large number of other sciences—astrophysics, solid-state physics, elementary-particle physics—and the exploration of some of these is a natural part of the graduate student's exposure. Nuclear-physics experiments are of sufficiently small scale so that a student can play a central role in the design of the experiment, in the operation of the apparatus, and in taking and analyzing the data. He is, however, working with large enough units so that professional staffs and technicians are important in the maintenance and construction of the apparatus, parts of which are commercially built. His research mode is today intermediate between that of big physics in which the student can play only a peripheral part in a large group effort and that of the solitary scientist working with a single piece of equipment or technique. The experimental student has the rich experience of personally organizing large units for an end result that he has sought. The net outcome is an acquaintance with many kinds of experimental equipment and experimental procedures and a competence in many techniques and procedures.

Two kinds of development are tending to erode this breadth of training. One is the natural desire to streamline and increase the efficiency of research by highly sophisticated apparatus professionally built and operated. Second, certain types of nuclear research are moving into a big-physics mode of operation at new nationally based facilities; the user-group participation can leave the student even more divorced from the reality of the experiment. These can both be mitigated by deliberate educational efforts aimed at making the student an active participant. Special apparatus can be made available to him to learn procedures and techniques without the need to disrupt any on-going measurements. The remoteness of user-group arrangement can be partially dispelled by integrating the student into the scientific community of the facility at which the experiment goes on. These and additional efforts are necessary if the double objectives of full student participation and research at the forefront of nuclear physics are both to be realized.

No special comments seem necessary for the theoretically oriented students, except for the general sentiment that the theorist is best educated in a milieu where both experimental and theoretical work go on with some close rapport. In the face of funding difficulties, the temptation to maintain a presence in the field via the theorists, who cost less to support than the experimentalists, is understandable. However, special efforts are then called for in order that the student receive the full benefit of both aspects of nuclear-physics research.

Graduate students in nuclear physics achieve a broad competence in a variety of techniques and procedures and a basic preparedness for participation in industrial and applied work. However, modern graduates seem most reluctant to do so for reasons that, we believe, derive from failure to understand the challenges and opportunities. Nuclear physics is closely connected to applications in industry, technology, medicine, and other sciences. Special efforts are needed to interest students in the important applications of their science. We believe that the universities and federal agencies should encourage multidisciplinary-oriented research, and that students should be encouraged to participate in such research. Ties between nuclear physics and its neighboring applied fields should be strengthened.

Does nuclear physics capture its share of the best students? It is difficult to answer this question completely, but strong indication that it does is given by the distribution of National Science Foundation (NSF) predoctoral fellows among the different subfields of physics, since these fellowships are offered before a choice of subfield is made. The statistics available at this time show that 15 percent of the students who earned a doctorate in nuclear physics were at one time predoctoral NSF fellows, about

the average in physics and astronomy. This indicates that nuclear physics is as attractive to the bright, keen student as are the other areas of physics.

7.8 COMMENTS ON POSTDOCTORAL TRAINING

The postdoctoral period of training has become an important part of the education of young physicists who intend to pursue basic research—and nuclear physics shares this common need. Approximately half of the recent nuclear-physics doctoral graduates take on a period of postdoctoral work (Physics Manpower Studies—1969 by the Committee on Physics and Society, American Institute of Physics). The active competition for these posts ensures that it is the abler half that is admitted to these opportunities. The primary purpose of a postdoctoral appointment is to provide further training in research under the guidance of a mature scientist. Postdoctoral fellows are able to devote their full time and energy to new research problems during a period of very high creativity. For them the user mode is less of a hardship than for faculty who carry campus-based responsibilities. These appointments have the additional benefit of linking the lines of thought of the research center in which they have been educated with those of the institution to which they have come. The postdoctoral period is only rarely spent in another subfield of physics; it is very much a time for acquiring more expertise in nuclear physics, although frequently in an area different from the doctoral dissertation. Such appointments are usually held for one to two years, although as the availability of new permanent positions has decreased there has been a tendency to take a second postdoctoral position at another institution.

While predoctoral education is chiefly the province of the university, postdoctoral training is a function of both the universities and the national government laboratories. Of the approximately 250 postdoctoral positions, the universities maintain about 150. Support for the postdoctoral program is primarily from federal funding; nonfederal support accounts for only 15 percent of such university positions.

The nuclear-physics research community firmly believes in the importance and value of all aspects of the postdoctoral program. Nevertheless, this program is clearly very vulnerable to funding constrictions. Under a budget cut, the tendency of the universities is to maintain the permanent staff and their graduate students. In spite of reluctance to cut back on the postdoctoral appointees, it is this transient research population that is most easily eliminated as part of the short-term response. Some government and national laboratories have responded in the same way. As discussed in Chapter 6, if the funding constriction is to continue on a long-

term basis, it may be necessary to rearrange the research efforts in a funda-
mental way. Part of this rearrangement must be such as to include a bal-
anced postdoctoral program.

Because of the strength of U.S. nuclear physics over the past decades,
there has been a much greater flow of postdoctorals into this country than
have gone abroad. Since the total foreign nuclear-physics effort is ap-
proaching parity with this country's, a parity on postdoctoral appoint-
ments should be sought for a variety of reasons that range from the need
to husband more limited resources for our own manpower to the great
importance of cross-fertilization of ideas and techniques between different
research centers.

8   U.S. Position in Nuclear Physics on World Scale — Relative Strengths of U.S. and Foreign Programs

Capitalizing on its strong thrust into nuclear physics in the 1930's and its
World War II experience, the United States developed a vigorous program
of exceptional quality beginning in the late 1940's. American, British, and
Canadian pre-eminence in the postwar years amounted almost to a mo-
nopoly. Since that time, the international balance of the nuclear research
enterprise has changed markedly and is still changing. Our present research
establishment is comparable with but still larger than that of Western
Europe and much larger than that of the Soviet Union and the East Euro-
pean nations. The Canadian program and the smaller efforts in Israel and
Japan complete the main parts of the picture.

The Western European countries have, as part of postwar rebuilding,
devoted considerable resources to achieve a nuclear research capability that
is approaching that of the United States. The Soviet Union has moved
quickly into a sizable but much more specialized nuclear research program.
In 1970, the United States had about half of the world's nuclear-particle
accelerators and a third of its larger research reactors. Both here and
abroad, the vast majority of these accelerators were built in the 1960's fol-
lowing the introduction of commercially produced tandem Van de Graaffs
and the successful operation of isochronous cyclotrons shortly thereafter.
In recent years, the rate of construction of that centrally important tool of
nuclear physics—accelerators—has been about the same in the United States

as in the rest of the world combined. The flow of research papers gives still another feeling for the relative programs. About one third of the publications in nuclear physics are of U.S. origin, a quarter from Western Europe, an eighth from the Soviet Union; the principal other contributors are Canada and Japan.

In the fields of nuclear instrumentation and engineering (accelerators and reactors) the United States continues to hold a particularly strong position. The exports of radiation detection and monitoring instruments alone are estimated to be over $30 million in 1969,* about a quarter of the total market. Many other technical developments of the U.S. research programs and the instrumentation accelerator industries have enriched both nuclear physics and high technology industry around the world. This outflow of technology is perhaps the most basic measure of the country's nuclear expertise. As the European nuclear establishment continues to grow, the technology transfer is becoming more two-way. As examples, a Scandinavian company is selling us excellent isotope separators; three European companies are prepared to sell cyclotrons that may win a large fraction of the expected big market for machines capable of producing isotopes for industrial and medical purposes; a French company has supplied a large part of an American university's cyclotron. If this country is to maintain its technological position in world commerce it must maintain both its instrumentation development programs and the basic research program on which these programs are based.

8.1 THE INTERNATIONAL NATURE OF NUCLEAR PHYSICS AND WHAT IT MEANS TO US

The health of any science requires that its ideas and developments be allowed to flow freely between all workers in the field. U.S. nuclear physics, as part of the total world nuclear research program, proceeds most profitably in active two-way interaction with the wider community. The dynamics of this interaction is best seen in retrospect of past developments.

The school of theoretical nuclear physics built around the Niels Bohr Institute in Copenhagen has had a profound influence on the entire subject. This school has pioneered the discovery and description of nuclear collective phenomena. Much of the important experimental work that led to and from their ideas was carried out in the United States. In turn, the modern complex shell model was developed by international efforts in

* *USAEC, The Nuclear Industry—1969* (U.S. Govt. Printing Office, Washington, D.C., 1969).

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which American theorists played a major role. How to establish the connection between these models and the basic forces between nucleons came about from the work of U.S. theoretists; their ideas gave rise to a worldwide effort. The theoretical ideas on direct reactions, originated by an Australian visiting Great Britain, were developed mainly through U.S. theoretical and experimental work to form one of the most important sources of nuclear information. The experiments and analysis that gave us the optical model description of nuclear reactions was largely that of U.S. physicists. The ways in which a marriage of optical model, shell model, and reaction theory could be arranged were proposed in this country, and groups here, in West Germany, and in still other places, have been active in developing their practical realization.

The ability of lithium-drifted germanium counters to detect gamma rays with high-energy resolution also has had an enormous impact on nuclear structure physics. The story of the development of these counters illustrates perfectly the benefits of information flow from one country to another, from one scientific discipline to another, and from research to industry and back again. Research in a U.S. industrial laboratory showed (in 1959) that trace impurities in silicon would be compensated precisely and automatically by drifting lithium ions into the material. This work had nothing to do with nuclear physics, but nuclear physicists in several U.S. laboratories immediately recognized that the compensated silicon might be suitable material for counters. By late 1960, they had succeeded in making counters that outperformed all existing types. Several U.S. and foreign companies began routine production of these devices. In 1964, an instrumentation group at Chalk River in Canada, one of whose members was an Australian visitor, succeeded in applying the same lithium-drifting process to high-purity germanium crystals that are commercially available from the U.S. and foreign solid-state electronics industry. The Canadians showed that their counters could detect gamma rays with an energy resolution far surpassing other types of counter. Production and further research were eagerly taken up by companies and laboratories in the United States and other countries, and today it would be hard to find a nuclear-physics laboratory anywhere in the world where lithium-drifted germanium counters are not in frequent use in the study of nuclear energy levels. Almost universally, germanium raw material especially suitable for the purpose is bought from a Belgian company.

Other examples can be drawn from the field of accelerator design work. The principle of phase stability, which makes possible the design of very-high-energy accelerators, was discovered independently and almost simultaneously in the Soviet Union and the United States. The U.S. effort in Van de Graaffs has led to a versatile nuclear physics tool; cyclotron development in this country and Western Europe is the central part of the effort

in intermediate-energy nuclear physics. Recent highly promising work in this country on superconducting linear accelerators may well orient the future of nuclear experimental programs. The latest Russian contribution, the concept of collective particle acceleration noted above, promises the acceleration of light and heavy nuclei at a greater rate than is possible by conventional means—and with smaller and cheaper equipment than would otherwise be possible. This concept is being actively developed in this country as well.

The discovery of nuclear fission and the developments and applications of this discovery are, of course, the classical example of the international character of scientific work. The paradoxical results of Italian workers were explained in prewar Germany by Hahn and Strassman uncovering the phenomenon of the fissioning nucleus. When transmitted to the United States during the visit of the Danish physicist, Niels Bohr, the correctness of the basic idea was very soon confirmed in every major nuclear laboratory. The enormous first efforts that put these physical discoveries to work in war, and then in peace, were very largely American—to be followed by all the great powers. Both our national defense and future power resources are intimately connected now with these nuclear phenomena. Anything that we can learn about them is interesting and important. And much has been learned over the years by the nuclear physicists of this and other countries. Very recently something quite new and unexpected was discovered about the process of nuclear fission: "fission isomers" or nuclear states that fission in quite different ways than do the normal ones. The original Russian find was soon shown, mostly by Danish experiments, to be a general phenomenon. The consequences can hardly be assessed as yet, but they are clearly important to all who care about nuclear ideas and nuclear practicalities.

U. S. nuclear physics lives and grows in this exciting milieu of give and take. In order to be able to seize on new ideas in any area, an expertness is required that can be achieved only by doing forefront research in that same area. Only in this way is the open literature of nuclear physics, as of any science, quickly understood, interpreted, and taught. If the United States is to maintain an expert awareness of the whole of nuclear physics, it cannot restrict itself to a spectator's view of some parts of the subject.

The importance of direct contact between physicists to the understanding and appreciation of new developments must be stressed; however, such contacts are fruitful only if between equally competent people. Successful interaction with the full international effort can be favored in no small measure by a wise policy with respect to the scientists themselves—our own, as well as visitors, and both mature experts and young people just starting out.

The migration of foreign nuclear physicists over the years has done

much to enrich our laboratories. In addition, the prestige of U.S. nuclear physics attracts large numbers of foreign visitors and postdoctoral researchers. At present, about half* of the temporary research workers in nuclear physics and chemistry come from abroad. They are an important means of communication with the large segment of nuclear physics done elsewhere—especially in this time of rapid growth of foreign laboratories. It is also important for there to be opportunities for our young scientists to have postdoctoral study in the outstanding foreign institutions.

Travel by nuclear physicists to laboratories and conferences abroad is as valuable as domestic travel, if not more so. The literature alone is often a poor substitute for direct meeting with the scientists responsible for an important development; seeing and working with a piece of equipment and talking to its designers is an infinitely richer experience than can be extracted by reading the published research paper that rarely explains the many alternatives discarded along the way. However, foreign travel funds have been reduced in recent years, while foreign laboratories have been growing in importance. It is particularly unfortunate that Canadian laboratories, which interact so strongly with U.S. nuclear physics, should be treated as foreign for travel purposes. We recommend to the Physics Survey Committee that foreign and domestic travel be put more nearly on a par and that funds be disbursed according to the scientific value of the travel.

The visits of foreign scientists for short or long working periods can provide an extremely useful interaction. To make this as unencumbered of visa problems as possible, we recommend to the Physics Survey Committee as a general policy that any unclassified laboratory be able to invite for a visit or working period any foreigner who can be admitted to the country.

8.2   SCOPE OF NUCLEAR PHYSICS ABROAD

The scope of the combined programs in nuclear physics of the Western Europeans and the Soviets parallels that of the U.S. effort. The programs of the various countries engaged in nuclear research are patterned along different lines; some have constructed broad efforts, while others have concentrated on rather specific areas of nuclear physics. Some highlights of frontier developments in their programs will suffice to give the flavor of the enterprise.

Theoretical nuclear physics is strong in many places in Europe, but

* *The Invisible University: Postdoctoral Education in the United States,* Report of a Study Conducted under the Auspices of the National Research Council (National Academy of Sciences. Washington, D.C., 1969), Appendix Table B-1, p. 288. (Data obtained in 1966–1967.)

among the outstanding leaders is the Copenhagen group. This institute, inheriting and enhancing the mantle of Niels Bohr, has long functioned as a center important for its own ideas and discoveries and as a place for the world's nuclear physicists to meet, discuss the important problems, and work with each other. France, Britain, Germany, Israel, and the Soviet Union all now have strong schools. The old structure of specialized institutes is dissolving, and while some of the theorists are still concentrated in such centers, the practice of building laboratories made up of both experimental and theoretical groups is taking hold.

There is a great upsurge of interest in the use of heavy-ion beams to study interactions between massive nuclei. West Germany is building two machines for the acceleration of very heavy ions. The Scandinavian countries collectively and Britain are each considering different entries into the heavy-ion sweepstakes. In France and the Soviet Union, machines will soon begin bombarding heavy elements with energetic heavy ions primarily to attempt the synthesis of the as yet unknown superheavy elements. The Russians in fact, have devoted a large fraction of their entire basic nuclear research program to the discovery of new elements without building the multifaceted program that we consider essential. A large group in the Dubna laboratory is preparing to make a major thrust toward the discovery of superheavy nuclei and is channeling its heavy-ion program for this purpose.

France and West Germany have developed broad nuclear-physics programs. Accelerators and research reactors covering a wide range of particle types and energies in well-equipped laboratories have been the centers of excellent work at the forefront of the field. The French were among the first to use a high-energy electron machine in studies of nuclear structure. The new Franco-German high-flux reactor at Grenoble will have the world's highest neutron intensities and is expected to be the basis of extremely important programs in neutron physics.

Laboratories abroad will be strengthening themselves in the emerging field of intermediate-energy nuclear physics roughly characterized by bombarding proton beams of about 100 MeV of energy. Strong groups are working in this direction in France, England, and Sweden and at the CERN laboratory. "Meson factories," aimed at goals similar to the United States' unique Los Alamos facility now nearing completion, are under construction in Canada and Switzerland.

This extremely active thrust toward the new opportunities that have appeared in nuclear physics coupled with a broad-based overall program speaks for a healthy future for the science. The ability of this country to profit by the discoveries and applications that lie ahead depends greatly on whether our own nuclear program keeps us at the forefront of all the important areas.

Appendix A: Subpanel Reports

The following Subpanel Reports are available from the Divison of Physical Sciences, National Research Council, 2101 Constitution Avenue, Washington, D.C. 20418, for the cost of reproduction, handling, and postage:

Report of the Subpanel on the Benefits for Biology and Medicine
Report of the Subpanel on the Benefits for Industry
Report of the Subpanel on the Impact on National Security
Report of the Subpanel on Nuclear Facilities (Accelerators and Auxiliary Equipment)
Report of the Subpanel on Nuclear Facilities (Instrumentation)
Report of the Subpanel on the Reactor and Fusion Devices Industries, Including Nuclear Safeguards

III
Atomic, Molecular, and Electron Physics

PANEL MEMBERS

N. BLOEMBERGEN, Harvard University, Chairman
P. L. BENDER, Joint Institute for Laboratory Astrophysics
R. BERNSTEIN, University of Wisconsin
RONALD GEBALLE, University of Washington
J. A. GIORDMAINE, Bell Telephone Laboratories, Inc.
D. A. KLEPPNER, Massachusetts Institute of Technology
M. O. SCULLY, University of Arizona
R. W. TERHUNE, Ford Motor Company Research Laboratory

LIAISON REPRESENTATIVES*

HAROLD GLASER, National Aeronautics and Space Administration (alternate)
HARRY HARRISON, National Aeronautics and Space Administration
RICHARD J. KANDEL, Atomic Energy Commission
GOETZ OERTEL, National Aeronautics and Space Administration
ROLF M. SINCLAIR, National Science Foundation

READERS

BENJAMIN BEDERSON, New York University
R. STEPHEN BERRY, University of Chicago
WADE L. FITE, University of Pittsburgh
HENRY M. FOLEY, Columbia University
KEITH B. JEFFERTS, Bell Telephone Laboratories, Inc.
J. V. PHELPS, Joint Institute for Laboratory Astrophysics
FRANCIS PICHANICK, University of Massachusetts
FRANCIS M. PIPKIN, Harvard University
T. N. RHODIN, Cornell University
FELIX T. SMITH, Stanford Research Institute
G. KING WALTERS, National Bureau of Standards

* In general, no agency was represented on the Panel by more than one person at a given time.

Contents

III

ATOMIC, MOLECULAR, AND
ELECTRON PHYSICS

1   The Nature of the Field

The concept of the world as composed of basic building blocks dates back
to the ancient Greek philosopher Democritus who coined the word ατομος,
or atom—indivisible particle. However, the development of physics in the
seventeenth and eighteenth centuries following the Renaissance concen-
trated chiefly on more readily observable physical properties such as
light (optics), heat, and motion (mechanics) that did not emphasize the
atomistic concept. With the development of chemistry and spectroscopy
during the nineteenth century, the study of atoms, molecules, electrons,
and ions became a most influential part of physics and led to the investi-
gations that culminated in the formulation of modern quantum theory in
the 1920's.

The atom proved to be not indivisible but composed of a small, heavy
nucleus and a cloud of orbiting electrons. And the nucleus consisted, in
turn, of elementary particles, neutrons, and protons. Further, the laws of
quantum mechanics were applicable to and valid in the domain of nuclear
structure. During the 1930's and 1940's, as physicists continued to probe
deeper into the structure of the nucleus, nuclear physics held the center
stage in physics. However, after about 1950, high-energy physics, which
deals with the structural relationships of elementary particles, came to the
forefront in the search for the intrinsic structure of matter, though atomic
and nuclear physics still retained lively frontiers of their own. These sub-
fields deepen and broaden our understanding of the relationships of the
basic laws of quantum mechanics and electromagnetic interactions. In

doing so they develop ever-closer ties between physics and other fields of science such as chemistry, biology, and astrophysics. The refinement of techniques and instrumentation resulting from atomic and nuclear physics research also has led to new applications in technology and new inter-actions with engineering. One measure of the vitality of atomic physics is the number of recent Nobel Prizes awarded to physicists in this subfield—four in each of the past two decades.

The evolution of physics, of course, has not progressed uniformly along a line but rather across a plane as illustrated in the schematic representa-tion in Figure III.1. The word "intensive" has been used to describe the ef-fort to formulate new laws of the physical universe, and the word "exten-sive," to denote the task of enlarging and deepening our understanding of generally accepted basic physical principles. Thus, Newtonian mechanics, thermodynamics, fluid mechanics, optics and electromagnetic theory, special relativity, quantum mechanics, and atomic and nuclear physics have changed in turn from a purely intensive to a more extensive character.

Because subfields of a more intensive nature have enjoyed a higher priority in the physics community in recent decades, as discussed in Volume I, the report of the Physics Survey Committee, subjects such as
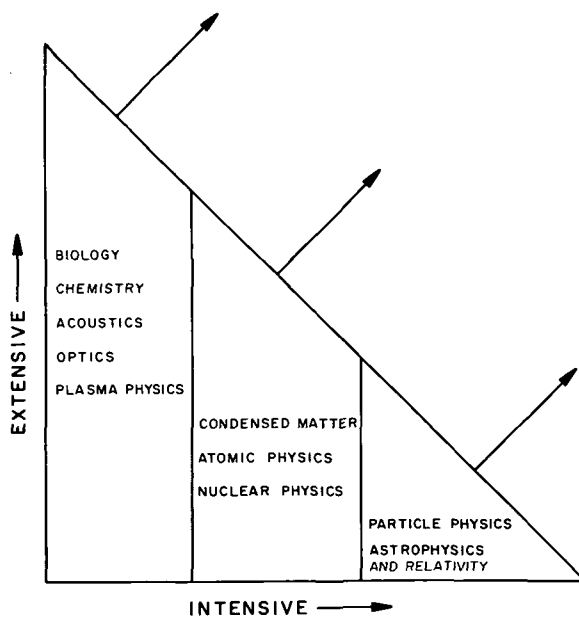

FIGURE III.1   Intensive and extensive physics.

optics, thermodynamics, and fluid dynamics have all but disappeared from the physics curricula. In many physics departments atomic and molecular physics, solid-state physics, and plasma physics also receive less attention than was previously the case. These subjects frequently are delegated to chemistry, engineering, and applied science departments. Such departments have long taught these subjects in their own right but typically without emphasizing fundamental physical aspects. This approach is unfortunate since these subjects present many challenging problems that can be solved only by a thorough understanding of the physical principles of quantum and statistical mechanics and electromagnetism. Furthermore, they constitute a bridge between physics and the human environment about which we are currently so greatly concerned. Electromagnetic quantum-mechanical interaction between electrons and nuclei, on which the structure of atoms, molecules, fluids, and solids depends, determines our immediate environment. And in the study of this interaction and these structures physics relates directly to chemistry, biology, materials science, and earth and atmospheric sciences. Consequently, atomic, molecular, and electron physics, together with solid-state and plasma physics, should occupy a central position not only in scientific and technical endeavor but also in physics itself. It is important to re-emphasize that physics has both an intensive and extensive character. The subfield of atomic, molecular, and electron physics (referred to hereafter as AME physics) has close ties and relevance to the problems of society yet retains also a substantial intensive character. Examples of this last statement are provided by new tests of quantum electrodynamics and new progress in radio astronomy made possible by frequency time and length measurements of unprecedented accuracy.

1.1 COLLISION PHYSICS, SPECTROSCOPY, AND ELECTRONICS

Broadly speaking, the field of AME physics is concerned with the interactions between electrons, atoms, molecules, and ions and with the interactions of these particles with electromagnetic radiation. The former major division may be called collision physics, the latter may be called spectroscopy in the broadest sense, including the electromagnetic spectrum from very long radio waves, through the microwave, infrared visible, and ultraviolet region of the spectrum to the ultrashort wavelengths of x rays and gamma rays. The histories of optical spectroscopy and electron and collision physics all date back to the nineteenth century. The collision phenomena in gas discharge tubes led to the discovery of the electron. Thermionic emission and the study of electron beams in vacuum led to the devel-

opment of vacuum tubes and the world-encompassing technology of radio communications. The gas discharge tube itself became ubiquitous in neon signs and fluorescent lighting. It would be very interesting to trace the development of vacuum and gas discharge tubes, the mass spectrometer, the electron microscope, and other widely used technological instruments from their origin in AME physics, but this would carry us too far afield.

Optical spectroscopy, broadly defined to include ultraviolet, x-ray, and infrared spectroscopy, provided most of the early information on atomic and molecular structure, but its popularity and use declined during the 1930's. The work of Maxwell, Hertz, and Lorentz in the nineteenth century established the unity of electromagnetic phenomena. As spectroscopy at optical frequencies, that is, near 0.5×10^{15} Hz (cycles per second), declined, a new spectroscopy at very much lower frequencies emerged. The study of the behavior of electrons in vacuum tubes and plasmas advanced the art of electronics to the point at which radio-frequency spectroscopy could emerge as a new field of AME physics. Beams of atoms and molecules were subjected to electromagnetic waves with frequencies eight or more orders of magnitude lower than the visible waves. The unifying principles of Maxwell's electromagnetic theory and quantum mechanics were thus established from audio frequencies at about 10^2 Hz to x rays at 10^{17} Hz.

The study of collisions between electrons, atoms, ions, and molecules leads, of course, also to a great deal of spectroscopic information. Studies of ionization potentials and resonances in collisional processes are important in their own right and are fundamental to our understanding of processes occurring in the atmosphere, the ionosphere, the planets, and the stars, and all of plasma physics. These aspects of AME physics will be covered in more detail in the panel reports on space and planetary physics, astrophysics and relativity, and plasma physics and the physics of fluids.

The work of the present panel has also been considerably lightened by the concurrent availability of the Report of the Committee on Atomic and Molecular Physics of the Division of Physical Sciences of the National Research Council.* That report contains a comprehensive description of the whole field of AME physics, and we quote (pp. 70–72) from that report the following paragraphs on collision physics.

Basically, a collision between two atomic systems (or between an atom and an elementary particle such as an electron, positron, or proton) can be described by the Schrödinger equation or its relativistic counterpart, although the interaction is much more involved than that which takes place within a single atom. The dynamic nature of this problem generally requires a time-dependent description. A collision by its

* *Atomic and Molecular Physics* (National Academy of Sciences, Washington, D.C., 1971).

nature describes the temporal evolution of the state of two interacting systems; therefore, collision studies differ from those of isolated atoms and molecules, for which a time-independent description usually is adequate. The computational ability of the physicist is challenged to the utmost by the demands of this problem. Only the simplest of collision problems can be computed without the use of gross approximations. A close and continuing interplay of theory and experiment is essential for progress in this field. Its appeal for the quantum many-body physicist lies in this intimate and mutually stimulating relationship between theory and experiment. Each advance in one stimulates new and more sophisticated approaches in the other. Some examples of the importance of electron-atom collisions follow.

Studies of electron–atom collisions provided much of the early knowledge of the structure of atoms and molecules. In recent years, such collision work has been the most fruitful testing ground of collision theory. Among the more dramatic contributions was the discovery of resonances attributable to compound structure effects in electron–atom and electron–molecular scattering. These compound states have their analogues in other branches of physics. However, in atomic and molecular physics, it is possible to calculate resonances with much greater computational sophistication than usually is possible in other fields. Further, experimental studies of electron resonances in atoms have led to an essentially new method of performing atomic structure studies, since these resonances are associated with specific states of the compound (electron + atom) system.

The development of high-energy-resolution electron guns has been the significant technological advance that made resonance studies possible. Such devices are also applicable to energy-loss experiments, which contribute an essentially new and very rich complement to ordinary spectroscopy. Electron energy-loss spectroscopy, like ordinary optical spectroscopy, gives the energy levels of excited atoms relative to the ground state. Such spectrographic studies with monoenergetic electrons possess resolution comparable to, and possibly soon to exceed, that of optical spectroscopy and have the additional advantage of permitting energy determinations of states that do not emit light.

Compound states exist not only in atoms but also in molecules. In the latter, the compound state, once formed, can emit an electron, leaving the molecule in various vibrational states. Alternatively, a negative ion plus a neutral fragment may result. The cross section for the excitation of vibrational levels via compound states is very large. In fact, a molecular gas laser, such as the CO_2 laser, would not be nearly so efficient if the compound state did not provide the vibrational excitation that results in population inversion.

The postulate of the compound state supplied the first real understanding of negative ion formation. For example, there have been recent and realistic calculations for dissociative attachment and three-body attachment in molecules such as H_2 and O_2. A wealth of phenomena has been discovered in compound-state studies.

Soon it may be possible to conduct experiments using polarized beams of electrons and atoms. A great increase in the knowledge of atomic and molecular structure and understanding of the effects of the exclusion principle—one of the basic postulates of quantum mechanics—can be anticipated as a result of this development.

Experiments, in which essentially monoenergetic fast electrons are scattered by

target gases, are complemented by multiple collision or swarm experiments, which involve the study of the passage of charged particles through gases. Such experiments yield transport coefficients, such as diffusion, mobility, and attachment coefficients or relaxation coefficients, such as temperature decay times and recombination coefficients. These transport and relaxation coefficients characterize the interaction of electrons, ions, and molecules under gas-kinetic conditions and, as such, are of direct interest to the aerodynamicist, plasma physicist, student of the stellar atmosphere and interstellar space, fluid-mechanics physicist, and others.

The transport and relaxation coefficients are related to the cross sections of single collision experiments through averages over scattering angle and over the appropriate distribution of relative energies of collision. The measurements are analyzed by solving the Boltzmann equation, which governs the motion of electrons in gases and plasmas; therefore, they are of great value in effecting a comparison of theory and experiment in low-density gas dynamics and plasma physics.

In order to avoid needless duplication with the complete and balanced description of AME physics given in the above-quoted report, the status and the relation of AME physics to other parts of physics, to other sciences, and to technology in the remainder of this report will be illustrated almost exclusively with examples taken from laser physics. Although this choice cannot possibly do justice to the work of all AME physicists, it will enable the nonspecialist to get a feel for AME physics without making undue demands on his reading time.

It is believed that the relation of AME physics to the total physics endeavor can be made clear in a rather concise manner by concentrating on a short history of, and the current activity in, laser physics. Before proceeding with this illustrative example, it should be made abundantly clear that many other examples could have been chosen and that the relative importance of the different aspects of AME physics cannot be assessed from this one example. In fact, one of the important conclusions of this report will be that no priority ratings can be assigned to the different subsections of AME physics, including collision physics, electron physics, spectroscopy, and laser physics. The conclusions and recommendations of the Panel, which are summarized in Chapter 5, are in full agreement with the conclusions and recommendations contained in the Report of the Committee on Atomic and Molecular Physics of the Division of Physical Sciences of the National Research Council. The two studies, carried out independently, reinforce each other.

1.2   AME PHYSICS AND THE LASER

To illustrate the nature of AME physics and its role in our technological society, the development of masers and lasers will be briefly sketched,

starting with that important technological offshoot of electron physics—
microwave radar. The availability of microwave equipment at the end of
World War II spurred the development of radio and microwave spectros-
copy. The extension of spectroscopic techniques to the frequency range
from 10^6 to 10^{11} Hz greatly enhanced the knowledge of molecular struc-
ture and the structure of magnetic ions. Precision microwave spectroscopy,
in combination with the concept of stimulated emission of radiation, led
to the realization of the cesium beam frequency standard and the atomic
hydrogen maser. These are the most accurate clocks yet devised and have
helped to create a new time standard. With the present definition of a sec,
the hyperfine interaction of atomic hydrogen, equal to 1,420,405,751.77
Hz, is the most accurately known physical quantity. In addition to atomic
and molecular spectroscopy of unprecedented accuracy, the atomic clocks
have made possible timekeeping with an accuracy of one part in 10^{12}, an
error of but 1 sec in 30,000 years. Improvements in navigational systems
and astronomy also are by-products of this development.

Immediately after World War II, the techniques of magnetic resonance
were extended from molecular beams to fluids and solids and developed
into a widely used analytical tool in organic chemistry and biochemistry
and in biomedical laboratories. Molecular radio-frequency spectroscopy
also afforded much insight into the kinetics of chemical reactions. Com-
bining magnetic resonance techniques with optical spectroscopy initiated
the field of optical pumping. One application of optical pumping is a
magnetometer that can be flown on a spacecraft and will measure the mag-
netic field in interplanetary space. During the last few years the domain
of magnetic-field measurement has been extended downward by three
orders of magnitude so that fields equal to less than one billionth of the
earth's magnetic field can be measured. Fields created by currents in the
human body also are detectable.

Magnetic resonance, again in combination with the concept of stimu-
lated emission of radiation, led to the multilevel solid-state maser, the
most nearly noise-free microwave receiver. It is used in early warning radar
defenses, deep-space tracking facilities, and radio astronomy. The receiving
stations in Maine, England, and France for the trans-Atlantic television link
via satellite also employ these masers.

Developments in microwave spectroscopy rejuvenated the study of
optics. The stimulated emission of light was achieved in many forms of
lasers (see Figures III.2 and III.3). And lasers in turn, have, revolutionized
atomic and molecular spectroscopy. Raman spectroscopy has been revived
and now is becoming a practical analytic tool in chemistry. Light-scattering
techniques are widely used in solid-state and plasma physics as probes for
elementary excitations and in the study of phase transitions. Literally and
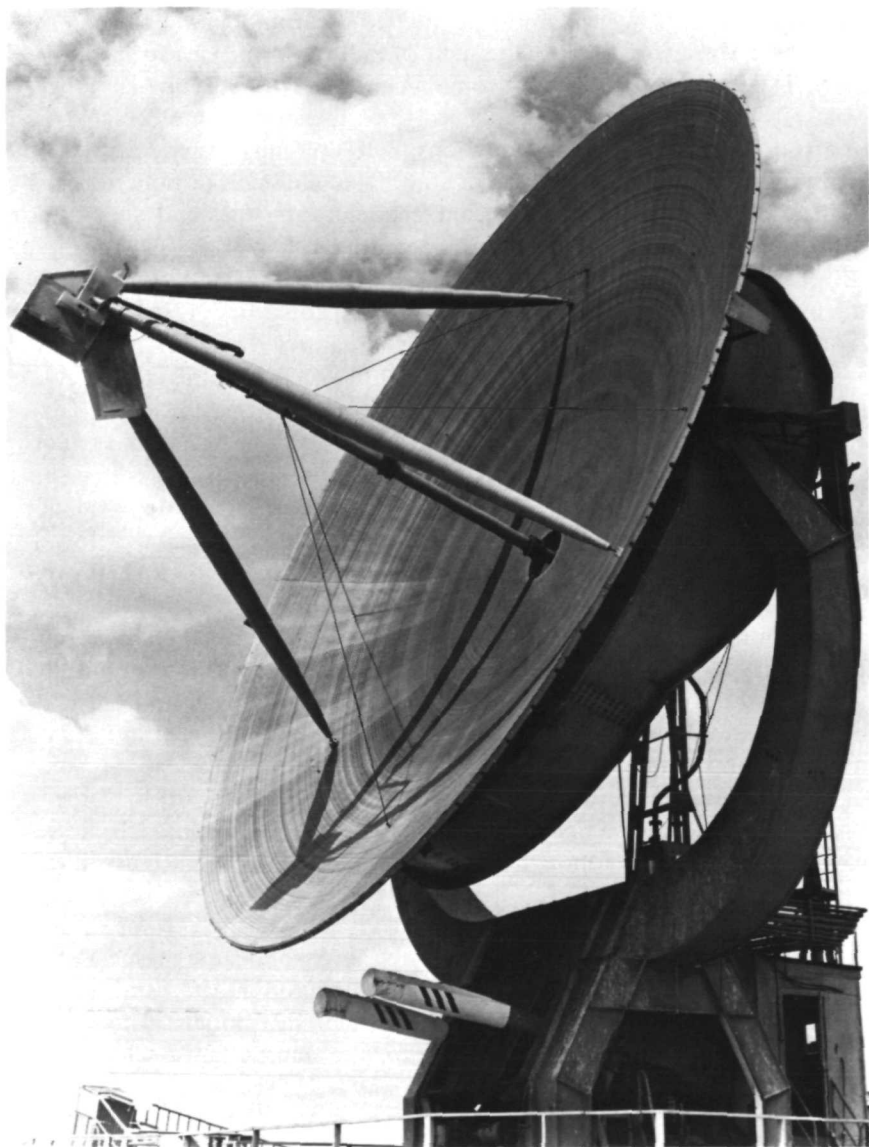
FIGURE III.2   Three-level maser is mounted at the focus of the 50-ft radio telescope of the Naval Research Laboratory in Washington, D.C. The "noise-free" operation enables the telescope to detect very faint signals. (Naval Research Laboratory Photograph.)
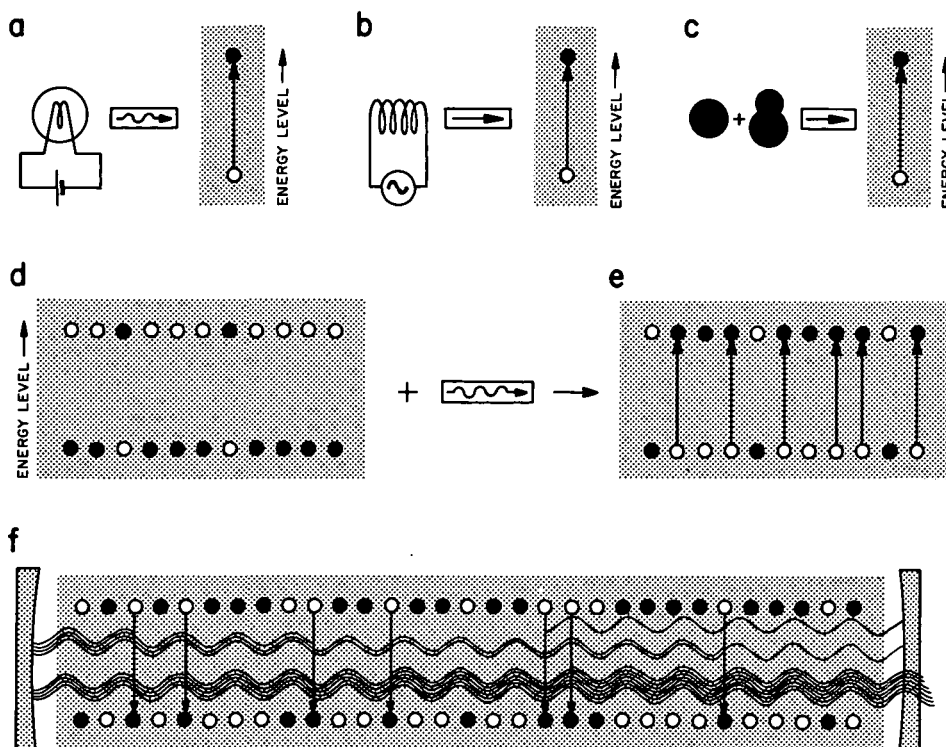
FIGURE III.3   Essential components of a laser are a pumping system, a system of atomic or molecular energy levels, and an optical resonator. Pumping excites the atoms or molecules preferentially into a particular state. It can be done by: (a) light, (b) electron bombardment, (c) energy released in a chemical reaction, (d) the molecules raised to the higher state, and (e) and (f) each molecule is stimulated by the light wave between the mirrors to add more photons to the light beam, so that a very intense directional, monochromatic laser beam emerges through the partially transparent mirror to the right. (From "Chemical Laser," George C. Pimentel. © 1966 by Scientific American, Inc. All rights reserved.)

figuratively, such techniques throw new light on the motion and structure of fluids and have many applications in biophysics.

As optical, radiowave, and microwave techniques developed, physics could progress further. Conversely, physics provided basic principles on which further improvements of the technology could be based. Throughout this brief history, a continual interaction between physics and technology is apparent. The most recent example, laser technology, has made possible the following new trends in atomic physics.

Spectroscopy in the far infrared—that most difficult frequency range of 10^{11}–10^{13} Hz, between the microwaves and the near infrared—can now

be investigated with a precision hitherto unattainable. The frequency of rotational states of molecules has been measured with a precision of one part in 10^6. This is an order of magnitude better than could previously be achieved in the far infrared and offers one of several new possibilities for a more precise determination of the speed of light by using lasers.

The centuries-old laws of optical reflection and refraction have been extended as part of a new field of investigation called nonlinear optics. Numerous new nonlinear optical phenomena, such as parametric up-and-down frequency conversion; self-focusing of light; and stimulated Raman, Brillouin, and Rayleigh scattering, have been demonstrated. Nonlinear laser techniques have led to tunable lasers and to light pulses that last 10^{-12} sec, or 1 psec. These pulses may contain 1 J of light energy; therefore, the power flux is 10^{12} W. This enormous power, about equal to the generating capacity of all electric power stations on earth, is packed into a light pulse of pancake shape, 0.3 mm thick, with a diameter of a few millimeters. The light-field amplitude can be as high as 10^8 V/cm. Any material subjected to this field strength for about 10^{-10} sec will vaporize and form a very dense plasma at thermonuclear temperature. Neutrons from such a laser-generated plasma have been observed. New domains of energy and time measurement are open to investigation; very short lifetimes of excited states of fluorescent molecules can be measured; and the behavior of matter under extremely high electromagnetic energy densities can be studied. Laser spectroscopic techniques will be extended to the ultraviolet and perhaps even to the x-ray region. Therefore, atomic and molecular spectroscopy undoubtedly will remain a very active and challenging field of research during the next decade.

1.3   THE LASER AND TECHNOLOGY

The laser is having an increasing impact on fields of science and technology other than AME physics. Extreme directionality of the light beam and extreme monochromaticity make it unique as a length standard and length-measuring instrument. Currently, lasers are being used to measure the distance to the moon from different points on earth, a procedure that not only yields new information about the solar system but also about the distance between continents, which can be measured with an accuracy of a few inches. A laser interferometer across a geological fault line may give early warning of earthquakes. (Appendix A presents a more detailed discussion of this application.) Laser interferometers also are integrated with high-precision milling machines and other tools to improve machining accuracy.

Since fundamental constants are essential building blocks in physics,

high-precision determinations by means of the new standards are of interest in their own right. Such measurements of fundamental constants often involve, directly or indirectly, fundamental questions of physical theory, for example, the validity of quantum electrodynamics. Advances in high-precision measurements frequently suggest important applications in metrology. The recent successes of long-baseline interferometry in radio astronomy made it possible to determine the angular position of very distant stellar objects with an accuracy of 0.0003 sec of arc. This is the angle that would be subtended by a letter of about the size of the type on this page put at a distance of 1500 miles. A necessary condition for this success was the utilization of atomic clocks.

Other applications of lasers include routine use of alignment procedures, both in the laboratory and for boresighting of tunnels and laying of sewer pipes; potential use as gyrosensors; use of laser pulses for delicate welding and trimming operations in microelectronic integrated circuitry production; and in holography (Appendix B describes an application of holography that could result in a new type of computer memory).

Recently, a purely chemical laser has been demonstrated. By mixing the flow from several bottled gases between a set of mirrors, an intense light beam is created in which 4 percent of the stored chemical energy is converted to infrared light. Other gas lasers with a continuous output in excess of 60 kW also have been constructed.

These examples show that AME physics is a stimulating field of scientific endeavor with a variety of applications. It challenges the physicist by opening new realms in the orders of magnitude of parameters occurring in the classical subfields of physics, and it also impinges on and interacts with other fields of science as well as electronic, optical, and instrumentation technology. Scientists working in AME physics are conscious of the relevance of their work to society and are stimulated by the continual interplay between pure and applied aspects of this work. The following chapter explores more fully the present status of this subfield and its interaction with other physics subfields and other sciences.

One of the characteristics of AME physics is that it has very little need for large and expensive centralized facilities. Work generally is performed by individual workers or small groups in all institutions in which physics flourishes—universities, colleges, industrial laboratories, and government laboratories. A good balance and a close relation exist between theory and experiment. In fact, because of the well-established and precise formulation of electromagnetic and quantum theory, there are many AME physicists who combine theoretical and experimental work. One student or research worker can become intimately involved with a variety of techniques, for example, laser, high-vacuum, optical, electronic, and spectroscopic. No large work team is essential, although contacts with scientists representing

a broad range of activities are beneficial and should be encouraged. AME physics, of course, will benefit from larger facilities if these are available, for example, powerful magnets for magnetic spectroscopy, high-voltage accelerators for beam-foil spectroscopy and collision studies, synchrotron radiation for ultraviolet x-ray generation, and high-power laser installations for plasma production.

2   The Status of AME Physics and Its Interaction with Other Physics Subfields and Other Sciences

The purpose of this chapter is to describe the central position occupied by AME physics among the sciences. The examples given in this chapter are not intended to be exhaustive. Although many other examples that could be cited are indeed described in the Report of the Committee on Atomic and Molecular Physics of the National Research Council referred to in Chapter 1, it is believed that the abbreviated account in this chapter conveys the important fact that AME physics continues to be a fountain that refreshes not only other areas of physics but other sciences as well.

2.1   INTERACTION WITH PLASMA PHYSICS, ASTROPHYSICS, AND ATMOSPHERIC PHYSICS

As was already mentioned in Chapter 1, atmospheric physics is determined by collisional rate processes involving electrons, atoms, ions, molecules, and electromagnetic radiation. So is the study of the earth's ionosphere, radiation belts, solar and stellar atmospheres, nuclear fireballs, combustion in rockets, supersonic flight, shock waves, space vehicle re-entry, and the like. To draw a line between AME physics and plasma physics or space and planetary physics is difficult. Laboratory studies of relatively simple, prototype systems probably should be classed as AME physics. However, a rigid definition of boundaries is neither useful nor meaningful. For example, the Joint Institute for Laboratory Astrophysics does much work in AME physics, as do plasma institutes working on controlled thermonuclear fusion. The distinction probably relates more to the motivation of the individual than to the nature of his studies; that is to say, is he principally interested

in the characteristics of atoms and molecules or in the earth's atmospheres and in the achievement of controlled thermonuclear plasmas? Clearly, the emerging field of molecular astrophysics benefits a great deal from laboratory studies of molecular spectroscopy that originally were conducted for purposes far removed from astronomy. The less sharp the distinction, the healthier the development of science.

The amount of basic data available on highly ionized atoms and collisions between more energetic particles (with energies in the thousand electron volt, or keV, range) is rapidly expanding. These data obviously are of interest to the subfields mentioned above. Beam-foil spectroscopy, a convenient and informative method that has emerged in the last five years, is proving useful in this context. Ions from a Van de Graaff accelerator are stripped in passage through a thin foil and the outcoming ions selected according to charge and energy. Significant fractions are in excited electronic states, and their spectra are observed and analyzed.

2.2   INTERACTION WITH SOLID-STATE PHYSICS

Electron beams are used in more refined ways to obtain information about surfaces. Low-energy electron diffraction (LEED) is stimulating increased interest and activity. The energy loss of scattered electrons, as well as the emitted x-ray spectra from solid surfaces, gives detailed information about impurities and chemical bonding. Cold-field electron emission and field-ion microscopy have been developed to the point at which the diffusion of individual atoms along the surface and the influence of atomic configuration along different crystallographic planes can be followed visually. Spectroscopic relaxation techniques also are useful in the study of solid surface interactions of atoms. Electron- and ion-beam sputtering and ion implantation, and especially scanning-beam electron microscopy, provide other examples of the interaction of AME physics with solid-state surface physics and high-vacuum technology.

Another interface with the physics of condensed matter is the study of molecular fluids. As the density of molecules in a gas is increased and condensation sets in, especially near the critical point of a fluid, it is difficult to say where molecular physics ends and the physics of condensed matter begins. Phase transitions have become the focus of much work in the last five years. Laser beam light-scattering techniques are contributing much more precise information about the mechanics and molecular arrangements in liquid-crystal transitions and in mixtures of fluids. Here we have a research area on which AME physics, solid-state physics, chemical physics, and biophysics impinge.

2.3   INTERACTION WITH NUCLEAR AND PARTICLE PHYSICS

The study of the hyperfine structure in positronium and muonium, which resulted in more stringent tests of quantum electrodynamics, provides an example of the contact of AME physics with elementary-particle physics. The magnetic moments of these particles have been measured with techniques related to magnetic resonance. The moments of many nuclides have been determined by high-resolution, atomic radio spectroscopy. At present, more stringent upper limits to the electric dipole moment of elementary particles are being sought to test the law of time-reversal invariance. The study of the spectra of mu-mesonic atoms is another point of contact between AME and nuclear physics. Somewhat unexpectedly, the study of positronium annihilation in gaseous and condensed media paved the way for useful advances in chemistry and solid-state physics. The interaction of a polarized laser beam with an electron beam to obtain polarized high-energy gamma rays is another example of the interaction with high-energy physics. Optical and radio-frequency pumping are also used to obtain polarized targets in nuclear and particle physics, an investigative area exciting much current activity.

2.4   INTERACTION WITH OPTICS AND ACOUSTICS

The traditional physics subfields of acoustics, fluid mechanics, and optics have close ties with AME physics. Brillouin, Rayleigh, and concentration scattering have yielded new information about damping and kinetics in fluids, and the venerable science of optics has been rejuvenated by its contact with AME physics. Holography, photon quantum statistics, and the study of the concept of coherence have progressed rapidly in the past five years. The statistical properties of laser light and the fundamental distinction between coherent and incoherent light were largely clarified by laser experiments during this period. Ultrahigh-resolution spectroscopy by means of correlations in photon arrival times has been developed. Nonlinear optics, which deals with the interaction between light waves, has produced data on nonlinear or hyperpolarizabilities of atoms, molecules, and crystals. This field is approaching a peak in activity, and many new industrial applications appear likely. The large task of collecting nonlinear spectroscopic data lies ahead.

Research areas as yet almost untouched are high-precision, far-infrared spectroscopy and nonlinear ultraviolet spectroscopy. A difficult to reach but alluring goal would be x-ray holography, which would permit direct

visual observation of atoms and molecules. Nonlinear interactions between light waves and x rays have been detected very recently.

2.5 INTERACTION WITH CHEMISTRY

Colliding-beam techniques have greatly advanced the study of low-energy atomic and molecular collisions. It is possible to study a chemical reaction, not as a statistical thermodynamic average but with details about individual rotational and vibrational states. Individual reaction channels also can be studied. Experiments involving elastic, inelastic, and reactive scattering of atoms and simple molecules have led to an evaluation of interatomic forces, knowledge of the various modes of transfer of translational to internal energy, and even to the angular distribution of the products of elementary chemical reactions. The existence of relatively long-lived "complexes" or reaction intermediates in certain systems has been demonstrated. And evidence of the importance of the relative orientation of the colliding partners in a chemical reaction has been acquired. Appendix C describes such work in greater detail.

AME physics benefits from the techniques developed in analyzing nuclear reactions and, in turn, deepens the understanding of chemical reactions in a revolutionary way. Obviously, AME physics substantially overlaps chemical physics and physical chemistry; a healthy exchange between physicists and chemists has resulted. Such interaction across conventional boundaries between scientific disciplines is highly beneficial and helps to remove the stigma of overspecialization that recently has been attached to physics.

3   The Relationship of AME Physics to Industry, Society, and National Security

As indicated in the opening chapter, AME physics is closely related to technological effort in the United States; laser techniques provide some of the most striking recent examples of this linkage. Traditionally, AME physics also is closely linked with electronics and has many ties with electrical engineering and communications. Society's continuing need for improved

channels of communication that offer ever-increasing capacity is one of its most obvious and pressing problems.

3.1 COMMUNICATIONS

Improved individual communication media could alleviate travel problems in commerce, industry, government, and other enterprises in addition to providing entertainment, education, and recreation. Efficient three-dimensional displays should broaden the scope and enhance the effectiveness of communication. Much industrial effort currently is devoted to electrooptical displays, light switching, and elements of communication and information processing involving light beams. Clearly, laser beams and nonlinear optics have stimulated this activity and will continue to play an important role. The picturephone has come into being and probably will be widely accepted, thus necessitating a large increase in communication-channel capacity that can be provided by laser beams (see Appendix D). Three-dimensional television, employing holographic techniques is more distant. However, holography has important implications for information storage (see Appendix B) and is used in industry for the detection of small mechanical deformations of large objects.

More immediate applications of light sensing and optoelectronics are in short-range control and guidance; light radar and range finding are already in use and could become important in aerial reconnaissance and obstacle avoidance. Public transportation, an increasingly critical problem facing society in the coming decades, also would benefit from better monitoring and control devices.

3.2 POLLUTION AND CONTROL OF THE ENVIRONMENT

Although the conservation of our resources and protection of our environment are primarily matters of economics and priorities in public policy, A M E physics can contribute to their solution through better instrumentation, which is essential for the development of the effective control devices that will inevitably be needed to implement an antipollution program. For example, laser-beam probing of the atmosphere and of smokestack exhausts is a new method for remote monitoring of individual chemical constituents by high-resolution absorption and Raman spectroscopy and by Rayleigh scattering from dust particles.

Basic questions of chemical kinetics in reactions that cause smog also need further clarification. To be able to predict the effect of releasing

various gases in the atmosphere, for example, from the exhaust of supersonic transport (SST) jet engines, we must know what end products and processes will result and the nature of the process that determines the duration of such gases in the atmosphere. To obtain this information requires the determination of electronic ionization and recombination cross sections, photoionization cross sections, and many two- and three-body collisional reaction rates for neutral–neutral and ion–molecular reactions. The techniques for accurate crossed-beam experiments near thermal energies that have been developed in recent years will be highly important in making the necessary laboratory measurements. Extensive theoretical research now under way on methods for calculating cross sections and reaction rates at low energies also is likely to make major contributions to the solution of the technical problems involved in controlling atmospheric pollution. The situation in the *D*-region of the ionosphere, in which many ions involving clusters with water molecules recently have been found to be important, could well be indicative of the complex problems apt to be encountered in attempting to understand pollution.

3.3 MEDICINE AND PUBLIC HEALTH

Better instrumentation developed by, and through, AME physics contributes new analytic methods to the biomedical and health fields. Magnetic resonance and laser spectroscopic techniques are finding increasing use in medical research areas for analytic purposes. More important may well be the ever-widening applications of mass-spectrometric and electron-microscopic techniques, which in turn lean heavily on AME physics for further improvement. The interaction of strongly ionizing beam of heavy ions with living tissues is studied for treatment of certain types of cancer. Intense laser beams are used for retina welding, and laser light may be guided by fibers via blood vessels into internal organs for visual observation and for internal cauterization. There are enough further possibilities to encourage increased interaction of health research with AME physics.

3.4 NATIONAL SECURITY

Lasers and masers play an important part in radar defense, ranging, and navigation. In addition, lasers are used for nighttime visual surveillance, bomb sighting, satellite tracking, and the like. Molecular physics and infrared spectroscopy are employed in the detection and tracking of rockets and in studying space-vehicle and re-entry problems. The use of high-

power laser beams for energy transmission in space and for antiballistic missile (A B M) defense purposes also has received much attention. Realizing the relevance of A M E physics to questions of national security, the Department of Defense (D O D) and the Atomic Energy Commission (A EC) have supported a large fraction of this type of research. It is ironic that in the present climate of cutbacks in research supported by the D O D, the A EC, and the National Aeronautics and Space Administration (N A S A), A M E physics at academic institutions is likely to be affected more severely than other areas of science, an unintended penalty for its relevance. Other government sources of funding, such as the National Science Foundation (N S F) and the National Institutes of Health (N I H) have left the initiative to the D O D and the A EC and have not assumed a proportionate share of the support of A M E physics. The Panel believes that this situation merits careful attention and correction in the next four years.

3.5   EDUCATIONAL AND CULTURAL ASPECTS

A M E physics shares with solid-state physics some of the most desirable characteristics for the training of students in physics. Such training brings the student into direct and meaningful contact with many aspects of physics. His PhD thesis is likely to involve him in the subfields of electromagnetic radiation, statistical mechanics, quantum mechanics, and fluid mechanics, as well as in a variety of electronic, spectroscopic, and vacuum techniques. The training is well matched to many future jobs in industry or government in which his ability to move freely among several disciplines or subfields and to operate independent projects is of great value.

The cost to train a PhD candidate is lower than in many other physics subfields. At the time of the Pake report,* this cost was about $27,000, as compared with $45,000 in nuclear physics and $130,000 in particle physics. The ratio between these numbers has changed little in subsequent years. The cost per finished research paper also is lower in A M E physics than in some other subfields, a point discussed more fully in a later section on manpower and funding.

The scientific heritage of completed physical theories, such as Maxwell's theory of the electromagnetic field and the quantum theory of atomic structure, should be safeguarded and described in sufficient detail to succeeding generations of science students. Training in A M E physics in sev-

*Physics Survey Committee (G. E. Pake, chairman), Division of Physical Sciences, National Research Council, *Physics: Survey and Outlook* (National Academy of Sciences–National Research Council, Washington, D.C., 1966).

eral institutions has shifted from the physics departments to applied phys-
ics and electrical engineering departments. It is not wise, however, to rely
wholly on other departments to do the job, since the emphasis on basic
physical principles—the major source of fresh ideas—would be diluted or
lost with time. A M E physics occupies a central and basic position in a gen-
eral science curriculum and offers an excellent opportunity for physicists
to interact constructively with chemists, astronomers, and electronic en-
gineers in a joint educational enterprise and to cross the conventional
boundaries between academic departments. Such an interdisciplinary pro-
gram may be needed to achieve the versatility and adaptability that in-
dustry finds lacking in many recent physics PhD's.

3.6 PUBLICATIONS AND CONFERENCES

A profusion of summer schools, symposia, conferences, extension courses,
and the like characterize A M E physics. Many of these occasions are ef-
fective in training young researchers and retraining older ones. The need
for updating is great. One exceptional program to meet this need is de-
scribed in Appendix E.

A limited number of national and international conferences clearly is
necessary to preserve contact between advanced researchers. In the opin-
ion of this Panel, the number of conferences probably could be reduced
through better coordination and cooperation among different societies.
More nearly adequate funding of travel to a smaller number of selected
conferences seems a feasible and desirable goal.

The publication of many conference proceedings is superfluous. In-
formative abstracts, available at the time of the conference, should suffice
in most cases. However, proceedings of some of the well-established
summer schools are particularly valuable.

A problem peculiar to A M E physics is its contact with so many other
subfields and disciplines. As a result, conferences organized by chemical,
optical, and electronics engineering societies frequently are of interest to
the A M E physicist. The establishment of the Joint Council of Quantum
Electronics by the American Physical Society, the Optical Society of
America, and the Institute of Electrical and Electronics Engineers is a step
in the direction of better coordination.

The need for authoritative critical reviews of less ephemeral value is
great. Several careful critical reviews of work on particular subjects in
A M E physics have been issued in the last few years; however, the available
reviews cover only a small fraction of the subjects in which they are
sorely needed. The need for critical reviews and data compilations rather

than just collections of unevaluated references is particularly strong in
AME physics because of the widespread use of such data in other subfields
and subject areas. Users in other subfields and disciplines frequently are
unable to review the references and decide which data are reliable and
which are not. Scientists who will devote the amount of time required to
investigate thoroughly the many kinds of systematic errors that may be
present in experiments and evaluate their overall merit are few. Many ap-
parently feel that the one or two experiments or calculations that could
be done in the same amount of time would have a more beneficial effect
on their professional reputation. The Panel believes that some kind of
formal recognition of the contribution made by individuals who do out-
standing critical reviews is urgently needed.

3.7   POSTDOCTORAL STUDY AND INTERNATIONAL RELATIONS

Postdoctoral fellows in AME physics often play a significant role in the
educational process by initiating new students into research. Their pres-
ence also provides continuity in the transmission of laboratory know-how
to successive generations of students. Postdoctoral training should remain
an integral part of the educational process for those who aspire to careers
in research. It is desirable to maintain a reasonable ratio between the
number of postdoctoral fellows and graduate students engaged in PhD re-
search; the Panel suggests a ratio of about one in three or one in four.

Undoubtedly, the postdoctoral program has contributed greatly to
international understanding. Not only do large numbers of postdoctoral
fellows come to the United States from Canada, Europe, and Asia, but
U.S. physicists hold postdoctoral positions abroad. Under official exchange
agreements between the respective governments, a significant exchange of
postdoctoral fellows with East European countries also has occurred.

European nations, by design, have spent a much larger fraction of their
physics research budgets on AME physics than has the United States. West
Germany is about to take the lead in colliding-beam spectroscopy (see
Appendix C), while France and England are leading in the development
and application of optical pumping techniques. On balance, the center of
gravity of the AME physics effort probably lies somewhere in the mid-
Atlantic but appears to be drifting away from U.S. shores. For example,
although practically all types of lasers were first developed in the United
States, the effort of the Soviet Union in quantum electronics is compa-
rable with ours. The Soviets were the first to produce a thermonuclear
plasma by a short laser pulse, and they had a lead, with France, in the pro-
duction of high-power, solid-state lasers.

In general, such competition has a healthy effect on the efforts in the United States. A M E physics is a subfield in which smaller countries can and do make important contributions. A good international exchange of persons and information exists; therefore, a special international organization or separate institute seems unnecessary. However, an increase in foreign travel grants from diverse U.S. sources would be beneficial and the Panel recommends this to the Physics Survey Committee.

4   Manpower, Productivity, and Funding

4.1   MANPOWER AND PRODUCTIVITY

The overall effort in A M E physics is difficult to assess because of its considerable overlap with other subfields of physics and with chemistry and electrical and optical engineering. However, the amount of activity in this subfield clearly is much larger than was estimated in the earlier Pake report,* which disregarded most of the extensive industrial research effort in A M E physics.

Data from the National Register of Scientific and Technical Personnel and from special surveys and studies conducted by the Physics Survey Panel on Statistical Data provide some background material on the characteristics of A M E physicists and on productivity and funding in this subfield as compared with other subfields and with physics as a whole.

Physicists who identified with the A M E subfield in the 1970 National Register of Scientific and Technical Personnel constituted 5.4 percent of the total number of physics participants, as shown in Table III.1. This table also shows that 6.6 percent of all PhD physicists indicated this subfield as compared with 4.4 percent of the non-PhD's.

The median age of A M E PhD physicists was 35.5 years in 1970, slightly less than that of PhD's in all physics subfields taken together, which was 37.4. Physicists associated with the elementary-particles subfield were somewhat younger on the average than A M E physicists, 34.2 years being the median age for that group, and those identified with the subfields of optics and acoustics were somewhat older, median ages for the latter groups being 38.7 and 40.1 years, respectively.

*Physics Survey Committee (G. E. Pake, chairman), Division of Physical Sciences, National Research Council, *Physics: Survey and Outlook. Reports on the Subfields of Physics* (National Academy of Sciences–National Research Council, Washington, D.C., 1966).

TABLE III.1   Percentage of the Physics Population Identified with
Atomic, Molecular, and Electron Physics

Degree Level	Physics Population	Percent Identified with AME
PhD	16,631	6.6
Non-PhD	19,705	4.4
PhD and Non-PhD	36,336	5.4

More than three fourths (77.8 percent) of the PhD AME physicists had
obtained their degrees in physics. Thirteen percent indicated doctoral
degrees in chemistry, and 6.5 percent in engineering.

Roughly half (53.7 percent) the AME physicists were employed by col-
leges and universities, and approximately one fourth (22.0 percent) by in-
dustry, as Table III.2 shows. The employment pattern for PhD and non-
PhD AME physicists was much the same and much like that characterizing
doctorates in all physics subfields taken together. The nondoctorate AME
physicists were more frequently employed in academic institutions and
less often in industry than was true of the overall non-PhD physics pop-
ulation.

The principal work activities of AME physicists, depicted in Table III.3,
were basic research, teaching, and applied research, with the doctorate
group being more heavily involved in basic research and teaching than the
non-PhD's. The latter were somewhat more often engaged in applied re-
search than were the PhD's. The percentage of physicists ranking basic re-
search first or second was substantially higher in the AME group than was
true of physics as a whole, as Table III.3 shows. Teaching and applied re-

TABLE III.2   Distribution of AME Physicists by Employing Institution

Employing Institutions	AME PhD $N = 1065$ (%)	Physics PhD $N = 16,248$ (%)	AME Non-PhD $N = 755$ (%)	Physics Non-PhD $N = 17,679$ (%)	All AME $N = 1820$ (%)	All Physics $N = 33,927$ (%)
College and university	54.1	50.6	53.1	30.0	53.7	39.9
Industry	22.7	23.4	21.1	30.2	22.0	27.0
Government	9.5	9.0	14.4	14.4	11.5	11.8
Research center	9.7	11.8	4.0	5.0	7.2	8.2
Other	4.0	5.2	7.4	20.4	5.4	13.1

TABLE III.3 Principal Work Activities of AME Physicists Compared with Those of All Physicists

Work Activity	AME PhD $N = 1277$ (%)	Physics PhD $N = 16,017$ (%)	AME Non-PhD $N = 882$ (%)	Physics Non-PhD $N = 17,001$ (%)	All AME $N = 2159$ (%)	All Physics $N = 30,018$ (%)
Basic research						
Primary	42.4	34.7	40.7	20.7	41.7	27.5
Secondary	26.5	24.9	11.3	7.2	20.3	15.8
Applied research						
Primary	15.2	16.8	17.9	20.8	16.3	18.9
Secondary	17.9	18.1	19.5	19.3	18.6	18.7
Design and development						
Primary	1.9	2.3	6.3	9.3	3.7	5.9
Secondary	5.9	6.6	11.0	14.9	8.0	10.8
Management						
Primary	13.2	16.0	11.7	16.0	12.6	16.0
Secondary	9.4	11.4	5.6	10.4	7.9	10.8
Teaching						
Primary	23.3	25.7	16.2	23.1	20.4	24.4
Secondary	20.5	18.2	14.7	7.6	18.2	12.8
Other						
Primary	1.9	2.4	5.0	8.0	3.1	5.3
Secondary	7.0	8.8	12.7	17.3	9.4	13.1
No response						
Primary	2.3	2.1	2.2	2.1	2.2	2.1
Secondary	12.8	12.1	25.1	23.3	17.8	17.9

search were indicated as principal work activities by equivalent percentages of AME physicists and all physicists.

Undoubtedly, many scientists working in subfields with a strongly applied orientation, such as plasma physics, optics, or acoustics, also received their basic training in AME physics. The changing climate of public funding probably will stimulate a shift from the present emphasis on intensive or basic research areas in nearly all physics subfields to more extensive or applied areas. If the growth rate of physics manpower must be reduced, as appears almost inevitable, the many applied areas in which training and experience in AME physics are fundamental and the many potential applications of AME physics suggest that this subfield should not be subjected to as severe a reduction in growth rate as a number of others. To incorporate a sense of relevance and flexibility in the training of AME physicists is characteristic of this subfield. Possibly for this reason, students educated in AME physics were, until recently, less aware of and less affected by the recent shortage of jobs requiring advanced training in physics. In the opinion of this Panel, there has been no overproduction of

AME physicists in terms of the long-range needs of our technological society. Data on productivity, as measured by published work, in relation to manpower and funding tend to substantiate this view.

AME physics is a highly productive subfield in terms of publishable results. Table III.4 compares selected subfields in regard to population and number of published papers in a random sample taken from entries in 1969 issues of *Physics Abstracts*. AME physics, with 5 percent of the physics population, produced 12 percent of the published papers in this sample, a percentage higher than that of nuclear physics or optics, each of which has a larger number of physicists associated with it. Condensed matter, the largest of the physics subfields, also is the most productive.

Examination of PhD theses appearing in 1970 issues of *Dissertation Abstracts* showed that 9 percent of these dealt with AME physics. This percentage was substantially higher than that found for optics and less than that for nuclear physics. Table III.4 also presents these data.

Theses listed in the Engineering Section of *Dissertation Abstracts* that dealt with the subject matter of physics and could have appeared in the Physics Section also were examined. Forty-five of these 870 engineering theses were concerned at least in part with AME physics, which was the same number found for nuclear physics as well as for optics.

Nearly three fourths (72.5 percent) of the work reported in the AME physics publications took place in academic institutions. Twelve percent

TABLE III.4 Comparison of Selected Subfields in Regard to Population, Publication, and PhD Thesis Production

Selected Subfields	Percentage of Physics Population $N = 36,336$ (No.)	Percentage of Total Published Papers in *Physics Abstracts* Sample[a] $N = 1181$ (No.)	Percentage of PhD Theses from *Dissertation Abstracts*[b] $N = 1400$ (No.)
AME	5.4% (1964)	12.0% (142)	9.0% (3280)
Nuclear Physics	9.5% (3435)	7.4% (87)	16.0% (225)
Condensed Matter	21.5% (7818)	41.4% (489)	41.2% (577)
Optics	9.0% (3280)	5.0% (58)	2.9% (40)

[a] A random sample of papers from 1969 entries in *Physics Abstracts*.
[b] Theses appearing in the Physics Section of *Dissertation Abstracts* in 1970.

was conducted in industrial laboratories. Two thirds of the papers from academic institutions were theoretical, as Table III.5 shows.

Academic institutions also produced the highest percentage of papers in nuclear physics, condensed matter, and optics. About one third of the work reported in condensed matter and optics was performed for industry, and 25 percent of that in nuclear physics took place in federally funded research centers. Most of the published work in these other subfields was experimental. This finding was especially true of condensed matter.

The emphasis on theoretical work in published papers in AME physics was somewhat unexpected in that only 19.6 percent of the PhD's and 14.8 percent of the non-PhD AME physicists who participated in the National Register survey described themselves as theoreticians. (An additional 18.8 percent of the doctorates and 22.4 percent of the nondoctorates described themselves as both theoretical and experimental physicists.)

A second sample of papers from *Physics Abstracts* included institutions throughout the world. The total number of papers in this sample was 1296. Thirteen percent (167) were concerned with AME physics. The United States and Western Europe (including the United Kingdom) produced

TABLE III.5 Distribution of Published Papers by Producing Institution and Nature of Content

Type of Institution	AME $N = 142$	Nuclear Physics $N = 87$	Condensed Matter $N = 489$	Optics $N = 59$
Academic institution				
Experimental	36	23	147	15
Theoretical	67	30	89	11
Total papers	103(72.5%)	53(60.9%)	236(48.1%)	26(44.0%)
Industry				
Experimental	11	2	113	11
Theoretical	6	3	39	9
Total papers	17(12.0%)	5(5.7%)	152(31.0%)	20(33.9%)
Government laboratory				
Experimental	5	5	30	3
Theoretical	5	1	9	1
Total papers	10(7.0%)	6(6.9%)	39(8.0%)	4(6.8%)
Research center				
Experimental	4	17	48	5
Theoretical	4	5	4	2
Total papers	8(5.6%)	22(25.3%)	52(10.6%)	7(11.6%)
Other				
Experimental	1	1	7	1
Theoretical	3	0	3	1
Total papers	4(2.8%)	1(1.1%)	10(2.0%)	2(3.4%)

equivalent percentages of these papers (United States, 34 percent; Western Europe, 36 percent). *Eleven percent came from institutions in the Soviet Union.*

All the data on the production of papers in AME physics indicate a high level of research activity for a relatively small subfield. Data on principal work activities of AME physicists reinforce this finding. However, the support of AME physics research by the federal government has been relatively low compared with that in several other physics subfields in spite of high productivity and the potentially broad application of research in this subfield.

4.2 FUNDING

Figure III.4 shows the level of funding from four government agencies for AME physics during the years 1965–1970, as collected by the Physics Survey Data Panel. The major source of support for the subfield is probably the Department of Defense (DOD). There is evidence that the figures reported by the Atomic Energy Commission (AEC) to the Data Panel as atomic physics and shown as such in Figure III.5 are in actual fact largely expended in areas outside AME physics as defined within the scope of this report. The curve shown includes expenditures on, for example, work in radiological research at Oak Ridge National Laboratory, work on magnetic resonance in polarized solid targets, work on superconductivity, and work on plasma physics. It is believed that the curve for AEC contributions in Figure III.5 should be reduced by a factor of 2.5 or 3. Such a reduction would bring this curve also in closer correspondence to the AEC expenditures quoted in the report of the Committee on Atomic and Molecular Physics. Furthermore, AEC funding has recently been reduced in areas not directly related to atomic-energy physics. This discussion points up the difficulties in matching the budgetary categories used by various agencies with the definition of subfields used by the Physics Survey Committee. It is gratifying that, on balance, there are no other major discrepancies between the estimates found in this panel study and the report quoted above.

Funds allocated by DOD fluctuated but showed a marked decline from 1969 to 1970. Funds from the National Aeronautics and Space Administration (NASA) for AME physics research have been steadily declining, and funds from the National Science Foundation (NSF) are roughly equivalent to those received from NASA during this period. Only about one half of the NSF budget category Atomic Physics is allocated to AME physics as defined in this report. The other half is assigned to plasma physics and the physics of fluids, optics, and other miscellaneous small-scale ex-
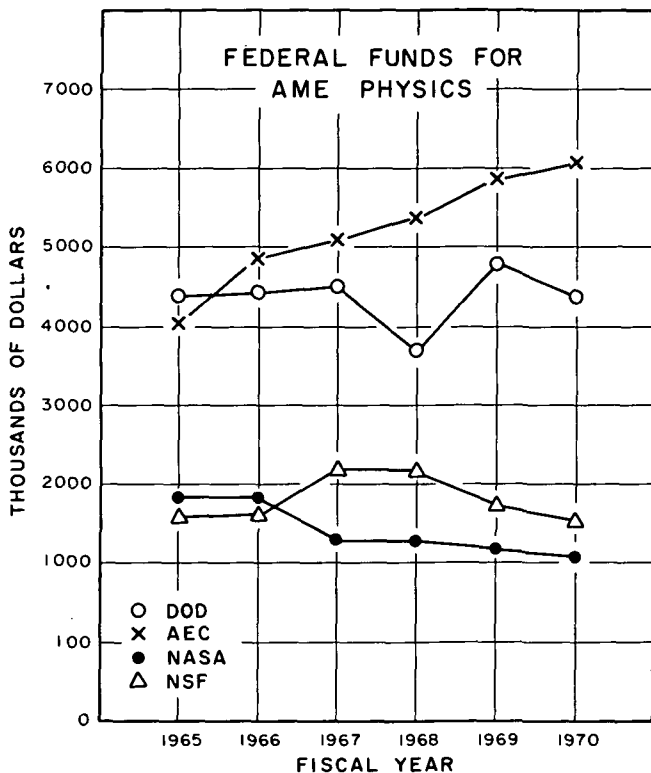
FIGURE III.4   Allocation of funds to work in AME physics by
four federal government agencies, 1965–1970. The curve labeled
AEC represents an overestimation as explained in the text.

perimental work in physics. The NSF spends about 5 percent of its funds
for the support of physics on AME physics ($1.5 million of a total of $28
million for physics in 1970). Condensed matter, nuclear physics, and
elementary-particle physics received 80 percent of the physics funds. If
the NSF is going to take over the support of fundamental research in AME
physics from other government agencies, its funding of this subfield must
be increased drastically.

The total support from all four government agencies for AME physics
in 1970 is about $13 million, compared with $407 million (in 1970 dol-
lars) for physics as a whole. Thus AME physics consumed only 3.4 per-
cent of the total budget, a finding that contrasts sharply with the pattern
of high research productivity in this subfield. Figure III.5 depicts the man-
power–productivity–funding situation in AME physics. If the independent

MANPOWER   PUBLICATIONS   FUNDING

5 %   13 %   4 %

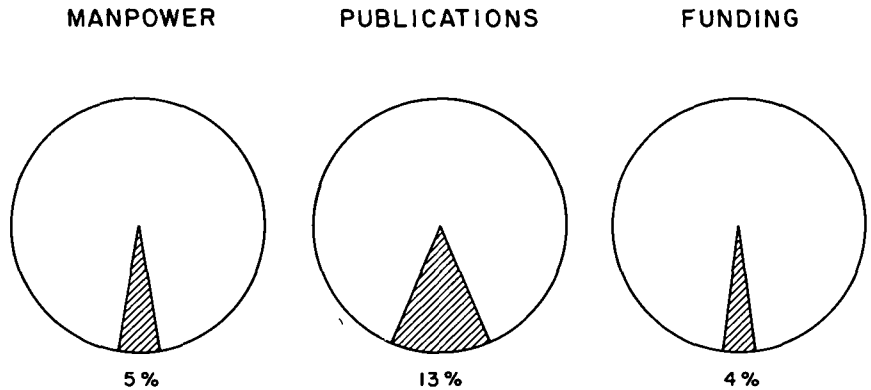FIGURE III.5   Comparison of manpower, productivity, and funding in AME physics.

support of industry to AME physics is taken into account, the fraction spent on AME physics research increases slightly. Industry employs about one fourth of the AME physicists and accounts for slightly more than one tenth of the published papers.

That AME physics has received a large share of its funding from the DOD and private industrial sources is undoubtedly the result of its relevance to the DOD mission and technology. Even though the support of relevant research should be less susceptible to cuts, a general and rather rapid decline in DOD and private industrial funding of all types of physics research seems to be under way. This trend could greatly affect the conduct of AME physics in university settings. Support from the NSF in this subfield, especially at the larger academic institutions, has been low. Although some correction of the distribution of NSF funds over physics subfields has occurred in recent years, it is not sufficient to offset the drastic reductions now taking place in the support of AME physics in other agencies. Federal agencies concerned with pollution, health, and transportation, which would reap long-range benefits from a vigorous program in AME physics, should assume some responsibility for its support.

Since the subfield significantly overlaps other subfields of physics and other scientific disciplines, it is difficult to assess precisely the amount contributed to the support of AME physics by, for example, chemistry or electrical engineering. In general, it is reasonable to assume that the effects of this overlap are not unidirectional. Undoubtedly some of the funds earmarked for AME physics in the graphs of Figure III.5 were used to support work in electronics. However, this figure gives a fair estimate of the actual amount spent. The central position of AME physics and the large degree of overlap with many other disciplines, shown schematically in Figure
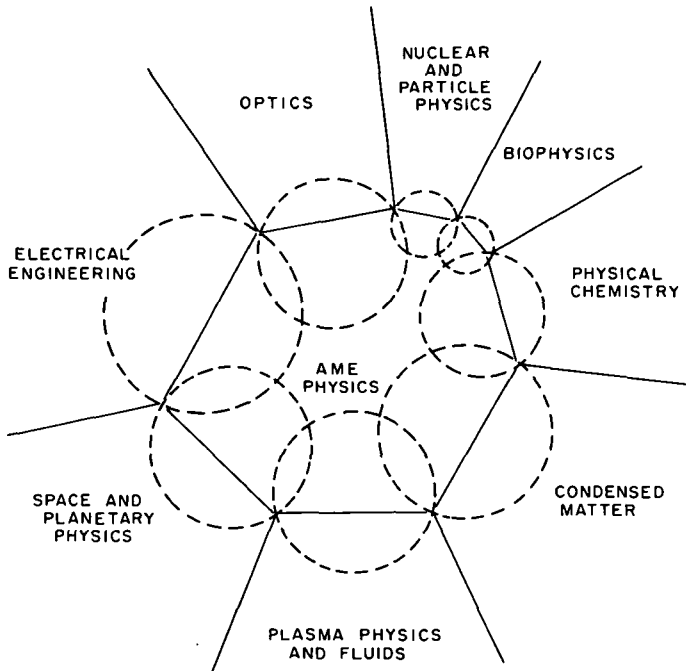
FIGURE III.6   Overlap of AME with other physics subfields, sciences, and engineering.

III.6, should receive careful consideration in the development of a funding policy for this subfield.

4.3   COST OF AME PHYSICS EXPERIMENTS

A survey of the cost of laser experiments, corrected for inflation, during the last five years shows no significant upward trend. In part, this finding results from the relatively modest instrumentation requirements of AME physics. In addition, increasingly sophisticated equipment needs are offset by improvements in technological production in the rapidly advancing laser field. However, this counterbalance is not typical. There has been an increasing need for sophisticated data-processing equipment. Molecular-beam apparatus now requires a mass spectrograph as a standard detection instrument, and optical spectrographs, costing $2000, have been replaced by integrated spectrometers that cost ten times as much. The need for up-dating equipment is felt most severely at the universities, especially among

the smaller institutions. Antiquated equipment prevents the university-based researcher from making important contributions. An annual average of $10,000 for equipment per experiment appears necessary. The typical cost of AME physics research per experimental graduate student in an academic institution is $15,000 per year; for a postdoctoral fellow it is $25,000 per year. The corresponding figures for theoreticians can be as much as $5000 to $10,000 lower. However, if expensive computer use is required, then costs for the theoretician and experimentalist are about equivalent.

To these figures the capital outlay for new equipment must be added. We thus arrive at a basic minimum annual expenditure of $40,000 for an experimental program involving a faculty member (summer salary only) and two graduate students at a university. When a postdoctoral fellow is added, the annual cost rises to about $70,000. We can assume that programs supported by less than $40,000 per year cannot keep the required equipment up to date. This situation has prevailed during the past several years for AME research at most universities. Unless a substantial increase in NSF support for AME physics at educational institutions is forthcoming, most of the equipment in these institutions will become obsolete within five years.

A typical senior PhD AME physicist in industry represents an annual cost of about $70,000 per year, a figure that includes technical assistance and overhead. This cost has increased during the past decade at a rate of about 3 percent per year, which is consistent with inflation during this interval.

Extensive use of computer techniques has added as much as 10 percent to the cost of some experiments and increased the cost of some theoretical investigations much more. The primary application of computer technology has been to the understanding of the structure and energy levels of atoms, ions, and molecules as well as their interactions during collisions. With the computer, accurate calculations of atomic orbitals and energy levels for two-electron atoms are now possible, as well as a number of approximations useful in the study of more complicated atoms and ions in their ground and excited states. This technology also has led to an improved understanding of the cross sections for numerous scattering and collision processes, chemical bonding in simple molecules, and the dynamics of simple chemical reactions. These processes are relevant to the technologies of spaceflight and re-entry, electrical discharges, high vacuum, air pollution control, plasma and fusion research, and gas lasers and also have intrinsic interest.

Automation of data acquisition has just begun. Large amounts of spectroscopic data and data on collision processes will have to be pro-

cessed in the next decade. New methods such as Fourier transform spectroscopy make use of the computer as an integral part of data collection. Without such methods, studies of solid-state and molecular vibrational spectra in the far infrared would not have been possible.

4.4 FUNDING POLICY AND OPTIONS

In this section we shall attempt to describe the consequences for AME physics of: (a) an increase in the present level of support of 7.5 percent per annum during the next five years, (b) constant (in real dollar value) level of support, and (c) a decrease, at an annual rate of 7.5 percent per year, in the present level of support. Throughout the discussion of these three hypothetical cases, the reader should recall that the AME physics budget represents only 3.4 percent of the total expenditure for physics and that the subfield is characterized by the absence of large fixed installations. It is not organized around accelerators, radio telescopes, rocket-launch installations, high-magnetic-field laboratories, or meson factories. However, some important new experiments in AME physics could be performed at these facilities, if they were available for this purpose. We assume that experiments on mesic atoms would be budgeted under nuclear physics, as AME physics in the narrower sense of its presently defined budget could not justify the construction of a meson factory or a plasma containment device.

4.4.1 *The Exploitation Budget*

A growth rate of 7.5 percent per year in the support of AME physics would increase the present budget of $13 million to $20 million by 1976. The production of manpower in the subfield is sufficient to exploit an annual growth rate of 7.5 percent. Such a budget would provide opportunities for the most promising young research workers to follow up their creative ideas and explore new problems and questions. Some high-risk experiments, with uncertain outcome but potentially high payoff, would be possible. For example, laser and nonlinear optical techniques are available to extend investigation into the vacuum ultraviolet region. Recently the operation of a molecular hydrogen laser at 1600 Å has been reported. The general availability of such devices could provide important information on photochemical reactions and the influence of ultraviolet radiation on molecular processes in the atmosphere and extend our knowledge of solid-state electronic processes. The devices presumably would require the

installation of large capacitor banks and the further development of optical crystals in the ultraviolet. The spectroscopic laser techniques in the far infrared also could be exploited more rapidly. The existing laboratories with colliding molecular beams could be refined and additional facilities built. These will be needed to study the myriad reaction processes in chemistry and atmospheric physics. The detailed study of the collision processes between highly excited atoms and molecules and the extension of nonlinear interactions and coherence questions in the x-ray region would be possible. Additional funds available with increasing levels of support probably would be allocated to some of the larger institutions, which already have a sizable capability in atomic physics and quantum electronics. At the same time, the exploitation budget would allow much of the effort at small institutions to continue. Although most of this work would not open new avenues of exploration, it would fill the gaps in our present knowledge of many phenomena. Most important, however, it would boost the morale of many physics teachers and vastly improve the general level of education in physics. Such small-scale research stimulates student interest and plays a vital part in the training of the scientific manpower that our technological society will need in the next decade.

4.4.2 *Level Funding*

If the budget were to remain at its present level of $13 million (at a 1970 dollar value), a choice between continuing the support of most small-scale efforts and sacrificing many of them to allow the conduct of a few more-expensive major experiments would be necessary. Since the subfield has had essentially level support during the past four years, the disposition of research proposals during this interval gives an indication of the effect of a continued policy of level funding. Figure III.7 shows the proposals granted and declined by the NSF in 1967, and Figure III.8 shows those in 1970. It is clear that during those years the competition for available funds has increased in most areas of AME physics. An important exception is research on lasers, which has continued to receive adequate support from the DOD because of its relevance to that agency's mission. Even so, the level budget would not permit as rigorous an effort to develop and apply ultraviolet (and soft x-ray) lasers. Molecular spectroscopy in the infrared would proceed at a slower pace. No single research area in AME physics would deteriorate entirely, but general progress would be sluggish. Equipment could not be updated and a general erosion of experimental facilities would begin.

This Panel believes that the best way to proceed under a level budget is to rely more consistently on the present system of competitive evaluation

FIGURE III.7   New research proposals declined and research proposals granted
by the National Science Foundation in atomic, molecular, and plasma physics
in 1967. (The Reviewers' Rating is a simple average of other opinions. It is not
the only selection criterion and is used here only to generate an abscissa.)

of new research proposals. The "survival of the fittest" would produce the
least damage to the scientific endeavor. However, many projects would
have to terminate. This step would lead to underuse of laboratory space
and instrumentation, especially at the newer institutions. The morale of
physics teaching in many colleges would be seriously impaired. Atomic
physics is one of the few fields of physics research that can still be con-
ducted on a small scale; it also keeps the teaching of physics in many
smaller institutions alive and relevant. Perhaps the pursuit of some more
ambitious, high-power laser programs or complex molecular beam and

FY 1970

NEW RESEARCH PROPOSALS—DECLINED

(chart: bars by reviewers' rating)

(24)   (24) $1,000,000

Annual Rate (requested)

(17)

(1)

$500,000

(3)

(7)   (5)

(1)   (3)

Poor   Fair   Good   Very Good   Excellent

(REVIEWERS' RATING)

FY 1970

RESEARCH PROPOSALS GRANTED: New ▨   Renewal ☐

(chart: bars by reviewers' rating)

$1,000,000

Annual Rate (granted)

(1)

(18)   (8) $500,000

(10)

Poor   Fair   Good   Very Good   Excellent
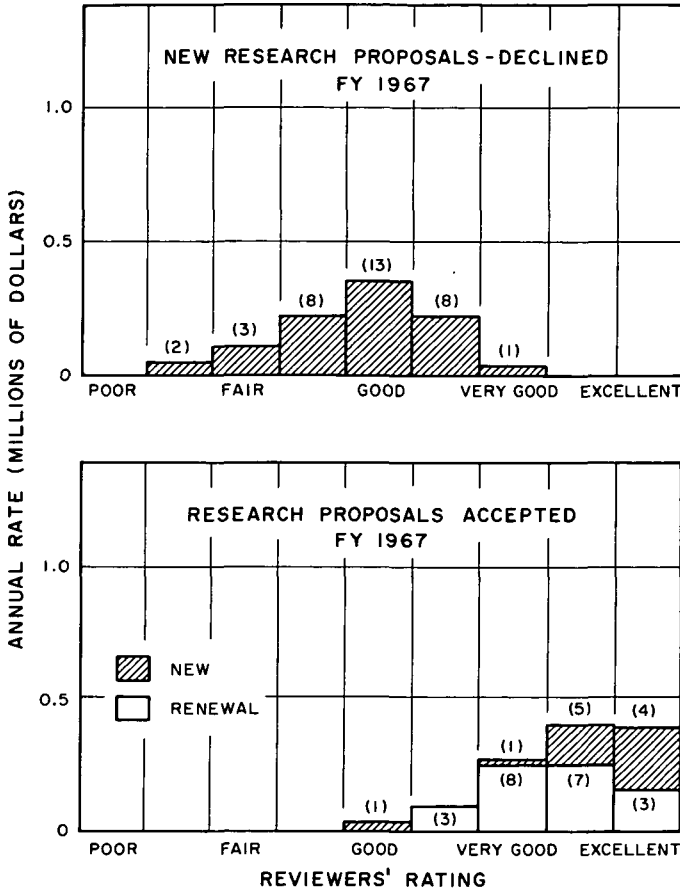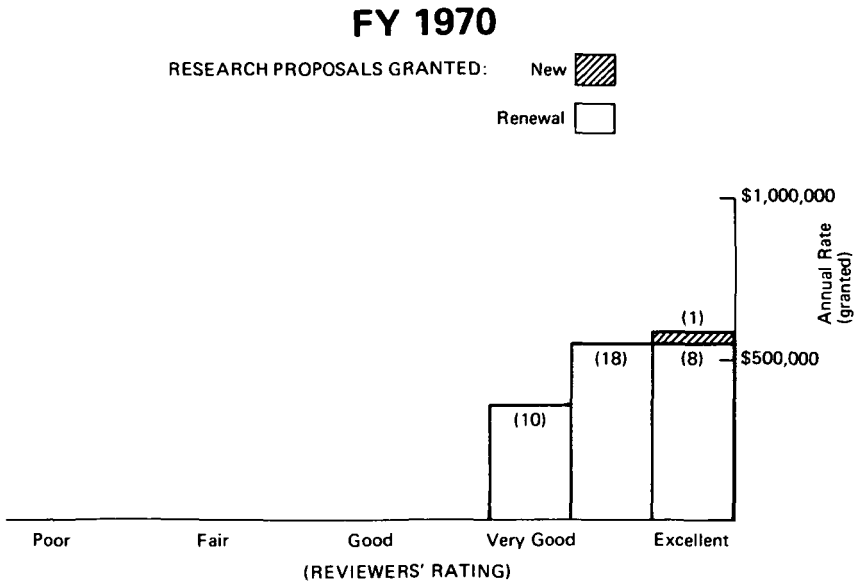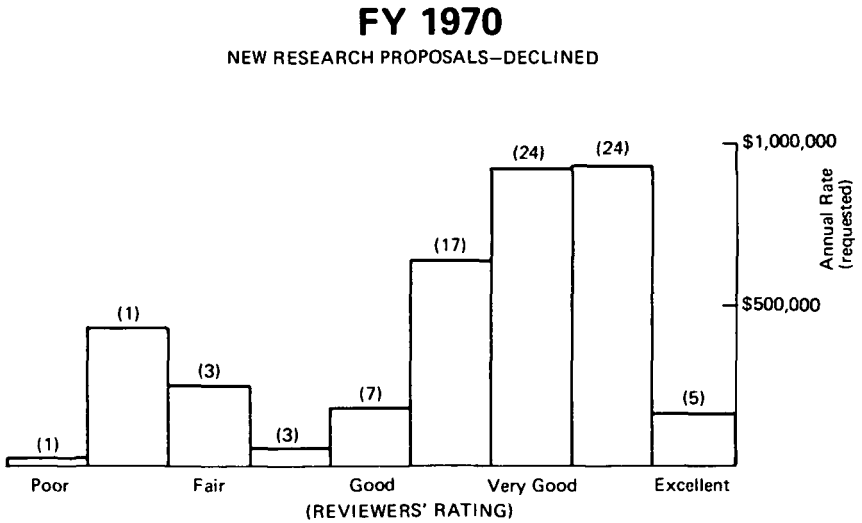
(REVIEWERS' RATING)

FIGURE III.8   New research proposals declined and research proposals granted by the National Science Foundation in atomic, molecular, and plasma physics in 1970. (The Reviewers' Rating is a simple average of their opinions. It is not the only selection criterion and is used here only to generate an abscissa.)

computer facilities should be sacrificed to keep the spirit of physics alive in many parts of the country.

4.4.3 *The Declining Budget*

With an annual decrease of 7.5 percent per annum, the budget for AME physics in 1975 would be reduced to $7 million. In this case there would be no choice but to discontinue the large majority of small-scale efforts. The total research output would be reduced to about 70 percent in 1975. It would be concentrated at institutions that currently have large and healthy ongoing efforts in this subfield. No single field of AME physics should be abandoned completely; the reduction should be spread rather evenly over all AME activities. It would not be possible for bright young people to start their own research programs. Damage to morale at this rate of retrenchment would be severe, especially at academic institutions, for in them the rate of reduction probably would be much larger than in mission-oriented industrial and government laboratories. This development would have long-range damaging effects on a future generation of physicists. The international position of the United States in AME physics research also would be seriously challenged. At the present level of support, our position is barely equivalent to that of Western European countries. These countries allocate a larger fraction of their physics budgets to AME physics at the present time than does the United States, and there is no indication that they plan to decrease this support. The possible impact of AME physics on problems of pollution, space and planetary exploration, and communications would be seriously impaired or delayed by decreasing support.

If AME physics support at universities were reduced by 50 percent, a fair fraction of the research activity in this field would be assumed by chemistry, electrical engineering, astronomy, and space science departments and probably funded by corresponding sections of federal agencies. There is an example in the recent past of this kind of adjustment. Optics was largely eliminated from physics curricula. It was saved in part by increased activity in electrical engineering and astronomy departments. During the past five years, AME physics also has helped to reintroduce optics into the physics department. This trend should be encouraged rather than discouraged if physics in the United States is to remain healthy.

The decreasing (at an annual rate of 7.5 percent) funding option in AME physics amounts essentially to its elimination as a subfield of physics, with other disciplines taking over those portions relevant to their activities. This development would have grave consequences for U.S. physics,

because it would then become more isolated from other disciplines and technology—a trend directly opposite to that advocated by many physicists.

The long-range consequences for the United States also do not appear to warrant a $6 million annual saving achieved by a 50 percent reduction in the AME physics budget in the next five years. A disproportionate erosion of the national program of education in physics would result, since AME physics is uniquely suited to provide core subject matter for a broad undergraduate science training program and versatile and technologically needed manpower with relevant training at the graduate level. Although other disciplines might undertake a large fraction of the AME research activity, the most innovative ideas and developments traditionally have come from academic physics departments. If drastic cuts in physics support must be made, this Panel urges that they be less severe in relation to this subfield. In comparison with other countries, this subfield has received less emphasis in the United States. For all these reasons, we believe that the relative position of this subfield in the hierarchy of physics should be upgraded.

5   Summary and Recommendations

AME physics is an active and challenging intellectual pursuit that pushes the study of electromagnetic interactions to new orders of magnitude in energy, intensity, frequency, and precision. It maintains close contact and interaction with most of the other subfields of physics and also is relevant to many other sciences as well as to technology. Training in AME physics fosters a flexible, individualistic orientation and, at the same time, provides a knowledge of the fundamental aspects of physics that underlie the work in many subfields and in other sciences as well. Characterized by relatively low costs, the absence of large-scale organization, and high rates of productivity, this subfield should play a somewhat more prominent role in the total physics endeavor in the next decade than it has heretofore. The Panel therefore makes the following recommendations to the Physics Survey Committee:

RECOMMENDATION 1   We recommend that the present level of support of this subfield, amounting to $13 million annually, be increased by 7.5 percent annually over the next five years.

RECOMMENDATION 2 We recommend that this increase be realized largely through an increase in the support of this subfield by the National Science Foundation, and that other government agencies, such as the National Institutes of Health and the Department of Transportation begin to provide support for this subfield, which is basic to certain aspects of their missions. Increased support for basic research should be channeled preferentially into educational and other nonprofit institutions.

RECOMMENDATION 3 We recommend that a larger share relative to other physics subfields than the current 3.4 percent of the funds available for the support of physics in the United States be allotted to AME physics, which is concerned not only with fundamental questions in physics but also is relevant to technological problems and plays a central role in a sound program of general science education.

RECOMMENDATION 4 We recommend that a sum of $1.5 million be made available by the NSF to update obsolescent equipment for AME research at educational institutions.

RECOMMENDATION 5 We recommend that research proposals in this subfield be evaluated and supported solely on the basis of scientific merit or technological relevance. Other factors, such as geographic distribution and creation of new centers of excellence, which have played a role in the past, should be eliminated. Priorities based on subject matter or existing or future large facilities also are unnecessary in this subfield.

Appendix A: Applications in Geophysics

A current major problem of science and technology is to provide a means of reducing damage resulting from earthquakes and other natural disasters. Although an accurate and reliable system for predicting the time and place of large earthquakes probably will not be possible in the foreseeable future, the likelihood of providing sufficient warning to reduce the degree of damage substantially is good. For example, a warning system that permitted gas lines to homes to be shut down if the earthquake probability reached a certain level probably would greatly reduce the amount of fire damage and the number of lives lost.

The most promising earthquake predictor at this time is the probable

acceleration of strain accumulation in the earth before an earthquake. Along some parts of the San Andreas fault and related faults in California, a creep rate of about 2 inches per year has been observed. No motion apparently occurs along other parts of the fault, and a continuous strain buildup of about one part per million per year seems to take place. The "locked" regions of the fault include the sections near San Francisco and Los Angeles, where the great earthquakes of 1906 and 1857 occurred. It appears likely that an increasing strain rate will develop before the next large earthquake and could be accompanied by changes in the magnetic properties of the rock near the fault, which could cause detectable local changes in the earth's field.

Two new and highly accurate methods of measuring earth strain have their origin in recent developments in atomic physics. One method entails transmission of modulated laser beams through the atmosphere. Measurements can be made over distances of 30 km or longer, and the accuracy is limited only by the uncertainty in the air density along the path (therefore, in the index of refraction). A sensitivity of three parts in 10^9 has been achieved with microwave modulation frequency. By using both red and violet lasers, the difference in the measured distances makes it possible to correct for the air density along the path and to give a final distance accuracy of one part in 10^7 or better. This method affords unprecedented accuracy in measurements of geodetic networks near fault zones.

The second method is the use of laser interferometry through a long evacuated pipe. Several instruments of this type with interferometer lengths ranging to 1 km have been operated in different parts of the country. With such instruments, the time necessary to detect a change in strain rate is exceedingly short.

The most suitable magnetometers for investigating magnetic-field changes associated with fault motion are the optically pumped and proton-free precession types. These devices, like lasers and masers, are based on the extremely sharp resonances that have been studied extensively by atomic physicists in recent years. The optically pumped magnetometers are a direct outgrowth of the work by Alfred Kastler, for which he received the Nobel Prize in 1966.

Appendix B: Holographic Optical Memory

Among the numerous applications of the gas laser to technology, the holographic optical memory perhaps most clearly illustrates their fre-

quently novel and unexpected qualities. This technique, currently the focus of exploratory work in several laboratories, shows promise of filling a need not met by other technological capabilities for a semipermanent memory with a capacity of about 10^8 bits and a random access time of about 1 msec. The estimated cost of such an optical memory is approximately 0.1 cent per bit, which is comparable to the cost of a high-capacity, low-speed disk memory but substantially cheaper than high-speed core or semiconductor memories that currently cost about 1 cent per bit.

An example of an experimental holographic memory consists of an array of 10^3 holograms, each composed of a "page" of information containing 4×10^3 bits in the form of bright dots arranged on a two-dimensional binary array. The hologram array may be stored on a photographic plate in an area of 7 cm on a side containing over 4×10^6 bits. The time required for random access is about 1 msec using an acoustooptic light deflector that causes an argon laser beam to illuminate a selected hologram. The 4×10^3 dot page array is displayed in a photodetector readout matrix fabricated by means of integrated-circuit technique.

The key to the practicality of this memory is the ability to ensure that the reconstructions of the various holograms will register on the photodetector matrix. The fundamental nature of the holographic process is such that the hologram can provide information storage, form a real image without a lens, and guarantee the required registration. In contrast to the conventional imaging, each bit of information is stored over the whole hologram, so that the system is remarkably tolerant of dust, scratches, and defects. In principle, the holographic technique allows three-dimensional recording, with a capacity of from 10^9 to 10^{12} bits per cm^3.

Appendix C: Atomic and Molecular Collisions

Much new and detailed information on the dynamics of the interaction among electrons, atoms, ions, and molecules has resulted from crossed-beam scattering studies. The collision energies of such experiments have been extended downward from the kilovolt range to the subthermal (millivolt) energy region. For the most part, research has followed the traditional methodology of nuclear and high-energy physics and produced accurate cross sections and excitation functions. These are important as a check on atomic and molecular theory and also increase our understanding of important processes in plasmas and planetary atmospheres.

An unexpected dividend of the extensive activity in the low-energy

scattering section of AME physics was the spinoff of these techniques to the field of physical chemistry. In particular, chemical dynamics currently is undergoing a renaissance brought about by the theoretical concepts and experimental tools of AME physics. Until 1960, studies of chemical reaction kinetics were confined principally to measurements of the change with time of the concentrations of reactants and products in bulk. The extensive averaging over a very large number of reactant states and collision energies and over products distributed throughout an expensive spectrum of internal states and translational velocities made interpretation difficult. The existence of sequential and parallel steps as well as the concurrence of the inverse reaction process prevented a detailed understanding of the microscopic "mechanism" of the reaction. The single-collision techniques of AME physics were first adapted by chemists in the mid-1950's and have become widely accepted in the past decade.

Experiments involving elastic, inelastic, and reactive scattering of atoms and simple molecules have led to an evaluation of both short-range and long-range interatomic forces, knowledge of the various modes of transfer of translational to internal energy, and the angular and energy distribution of the products of elementary chemical reactions. The existence of relatively long-lived "complexes" or reaction intermediates in certain systems has been demonstrated. Such experiments also have shown the special importance of the reactive orientation of the colliding partners in chemical reaction. In addition, population inversion (chemical laser action) has been observed.

The techniques for crossed-beam scattering studies of neutral and charged particles at thermal and superthermal collision energies now are well developed. After an extensive series of studies on prototype systems, an enormous growth in experimental activity is now likely in reactions of important atmospheric species such as atomic and molecular oxygen, nitrogen, hydrogen, oxides of carbon and nitrogen, hydroxyl radicals, and water vapor. Such studies are relevant to a number of environmental and technological problems.

Graduate students in university physics and chemistry departments conduct much of the current experimental work. As a consequence of this type of experimental research, the graduate student is likely to be involved in on-line computing, extensive data processing and analysis by means of a high-speed computer, or both. Responsibility for developing or applying the necessary programming techniques typically is the student's.

Although the United States probably is a leader in the chemical aspects of molecular scattering, Western Europe and the Soviet Union allocate more effort to the physics of atomic and electronic collisions than does this country. West Germany offers a striking example of the importance that scientific communities in other nations attach to this field. In 1964,

E

the Deutsche Forschungsgemeinschaft initiated a massive effort in the study of atomic and molecular collisions. Together with various university sources, the sum of approximately 6 million DM (over $1.5 million) is invested annually in this one research area. A Max Planck Institute was established in Göttingen in 1969, with two prominent atomic physicists as directors and a staff of more than two dozen scientists to provide a center of excellence in the molecular-beam field. The work in progress has an interdisciplinary flavor; it is concerned not only with atomic collisions but involves laser excitations, inelastic molecular encounters, and chemically reactive systems. In the considered opinion of the West German science policy makers, atomic and molecular physics, with its strong links to solid-state and chemical physics, deserves the highest priority.

Appendix D: Optical Communications

An outstanding example of potential interaction between AME physics and industry is the widespread application of laser technology to electrical communications. Although commercial optical systems have not yet appeared, many features of future optical systems have begun to emerge. Increasing channel capacity requirements for voice, two-way visual communication, and data transmission will necessitate such systems for overland communications. For example, commercial picturephone service requires a bandwidth of 1 MHz—about 250 times greater than that required by conventional telephone service. Some have predicted that before the end of this century picturephone service will displace the communication media currently in use. Clearly the evolution of picturephone service alone, not to mention expansion of other communication use, will require a major enlargement of information transmission capacity.

It has been predicted also that an optical intercity system will become useful in the range above 500,000 two-way channels or, equivalently, over 5000 picturephone channels, or 6×10^{10} bits per second capacity. Smaller needs will be more economically transmitted by some combination of radio relay, satellite, millimeter waveguide, and coaxial cable. The potential capacity of an intercity optical system is at least 100 times the channel number cited above. Intracity optical links now under consideration might consist of multiple-line optical cables, each line containing a single laser carrier modulated with a single picturephone signal or 100 or so voice channels.

The realization of an optical communications system is predicated on

the development of many new techniques and components, most of which will be based on solid-state technology. The most significant contribution of atomic and molecular physics to communications very likely will be laser technology. Present gas lasers, such as the helium–neon, argon, and CO_2 lasers, provide adequate power and are sufficiently monochromatic for transmission system requirements. However, the present large size and limited device life of these lasers are serious drawbacks.

Other applications of nonlinear optical techniques to communications involve modulating signals onto laser carriers and possibly translating frequencies, spatially modulating light beams, and generating trains of short pulses for pulse-code-modulation transmission.

Appendix E: Midcareer Training Opportunities

Large industrial laboratories have recognized the need for midcareer training in scientific and engineering subjects, including AME physics. Bell Telephone Laboratories, whose mission is research and development in communications, provides an example of response to this need. Among the educational activities that Bell Labs offers are internships—that is, temporary assignments of staff members in subjects closely related to their specialties—seminars, journal clubs, guest lecture series, out-of-hours lectures, and various plans for university attendance ranging from short courses to full-time doctoral programs for selected individuals.

Recently, Bell Labs initiated a broad, long-range program of continuing education to update, broaden, and deepen the academic foundation of professional staff members. Bell Labs instructors and outside professors as well teach these courses during working hours. Enrollment in 1969 was over 3300, including 440 PhD's, and represented more than half of the eligible professional staff. Most participants take one course involving two hours per week of lectures and two to four hours of homework. Comparison with the enrollment in graduate engineering courses at Massachusetts Institute of Technology, a total of 2200, provides some perspective on participation in this industrial education program.

Subjects covered in the courses are materials and devices—including optics, quantum mechanics, and band structure of solids—systems engineering, mathematics, physical design, switching, transmission, and computer science.

IV
Physics of Condensed Matter

445

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RALPH O. SIMMONS, University of Illinois
GEORGE D. WATKINS, General Electric Research and Development Center

CONTENTS

IV

PHYSICS OF CONDENSED MATTER

1 Introduction

Condensed matter (that is, solids, liquids, and amorphous substances) makes up the greatest part of the world. The study of the physical properties of such substances and the search for understanding of these properties constitutes, by a substantial margin, the largest subfield of physics. More than 21 percent of the 36,000 participants in the 1970 National Register of Scientific and Technical Personnel indicated the physics of condensed matter as the subfield in which they were employed,* and a recent survey shows that about 40 percent of all articles published in physics deal with subjects within the scope of this subfield.

All physical properties of matter are important, including mechanical, electrical, magnetic, optical, and thermal properties and interactions with all forms of radiation. Many of these properties have been of interest from the earliest days of science, but the modern study of condensed matter has its roots in the discovery of x-ray diffraction by von Laue in 1912, which gave the first quantitative information on the ordered arrangements of atoms in crystalline solids and the quasi-ordered arrangements in other forms of condensed matter. Quantitative understanding of the properties of condensed matter followed from knowledge of the atomic architecture together with the quantum mechanics of atom cores and their attendant electrons, which was developed in the 1920's and 1930's. Today, solids

* For further data see Chapter 9.

are classified according to the predominant binding forces that hold them together (molecular, metallic, ionic, or covalent) and the character of their electrical properties (conductors, insulators, semiconductors, and semimetals). Some substances move from one to another part of the latter classification with changes of temperature or with variations of purity. Many materials of great technological importance are being studied, including the metallic elements, semiconductors such as silicon, and insulators such as ionic crystals, metal oxides, and glasses. Liquids and glassy substances are distinguished from crystalline solids by the less regular arrangements of their atoms and by their generally lower viscosity, which allows them to deform more or less readily under stress. Extremes of temperature and pressure have revealed astonishing variants on these usual states of matter. Thus at very low temperatures some metals abruptly lose all resistance to the passage of electricity. This characteristic is called superconductivity. Although it was first discovered in 1911, at which time it was thought to be characteristic of only a small number of simple metals, physicists have found in the last decade that a remarkable variety of alloys and intermetallic compounds, and apparently some semiconductors, also possess this property. Recently, many practical uses have been discovered for superconductors and more are in the offing.

A somewhat analogous loss of all resistance to flow and other anomalous attributes occur in the liquid form of ordinary helium when this substance is cooled to within about $2°$ of absolute zero. This property is called superfluidity. Superconductivity and superfluidity depend on the quantum-mechanical, as distinct from the Newtonian or classical, behavior of aggregates of electrons and atoms. A satisfactory understanding based on first principles have emerged only recently, although the rudimentary phenomena have been known for many years.

Mechanical properties of solids such as strength, hardness, plasticity, and brittleness depend on cohesive qualities related to the interatomic binding forces, mentioned earlier, and also on certain characteristic imperfections in the ideal lattice structure of the solid. These imperfections or lattice defects have been the subject of intensive study in the last 25 years, and an important general understanding has emerged. However, many parts of the picture, including most of its accurate quantitative features, remain to be supplied.

Likewise, electrical, magnetic, optical, and thermal properties of solids are understood today in general terms, and much quantitative knowledge has been achieved. The richness of the phenomena that matter can present, however, is enormous, and it is possible that today's understanding will someday be considered as crude and naive as that of half a century ago now appears.

Practical applications of the phenomena in this subfield are emerging at an accelerating pace and show no signs of ending. The solid-state diode and the transistor, and the many devices based on them, have revolutionized electronics as well as communication, control, and data processing. Improved materials, with special structural properties, chemical or thermal refractoriness, useful magnetic, optical, or electrical attributes, or combinations of these properties are numerous. Many more detailed accounts of the discovery and development of such substances will be found in Chapter 3. New instruments with revolutionary improvements in sensitivity, radiation detectors with greatly improved resolving power, solid-state microwave oscillators, and lasers are among the recent developments that have made important beneficial inputs to other branches of science as well as to technology. The understanding of the physics of condensed matter has contributed directly or indirectly to all of these developments. The purposes of this report are to examine the state of this understanding and its relation to the usefulness of the physics of condensed matter and to assess the degree to which this understanding is complete or to which further dramatic progress can be expected.

Support for basic research in the physics of condensed matter comes from both government and industry. Table IV.1 shows the distribution of support in this subfield in fiscal year 1970 and also the distribution of support for basic research in physics.

The 1970 figures for total government support of research on condensed matter are about 1 percent less than in 1969; however, this statistic does not tell the whole story. A rise of 5 to 6 percent in the cost of doing research, because of inflation, led to a decline of nearly 7 percent in real funds in this one year. This situation is part of a general trend that has been under way for about four years and shows little sign of abating. Moreover, the decline in the programs of some agencies has been much more severe than the average and has resulted in terminations of research con-

TABLE IV.1   Support for Basic Research in Condensed-Matter Physics and in All Physics during Fiscal Year 1970 ($Millions)

Field	Government				Govt. Total	Indus-try	Govt. and Industry Total
	AEC	DOD	NSF	NASA			
Condensed-matter physics	29.5	15.0	6.2	3.4	56	80	136
All physics	180.0	45.6	31.0	10.8	269	155	424

tracts that have not been renewed elsewhere and in other dislocations. In this respect condensed-matter physics shares a problem that is common to all of physics. Further discussion of the deleterious effects of declining support on training, manpower, productivity, and vitality in the subfield appear elsewhere in this report, particularly in Chapter 9. The contributions, past and future, to technology and economic growth resulting from research in this subfield are the subjects of Chapters 2, 3, and 5. The need for scientific background in the development of new technologies is shown in Chapter 3. The important contributions condensed-matter physics have already made to the economic growth of the United States are described in Chapter 5.

This report complements other studies of solid-state and condensed-matter physics. In 1966, the Kohn Panel* described condensed-matter physics. That report presented manpower requirements for the years 1965–1970 and foresaw a decline in rate of growth in the period from 1970 to 1975. It appears that the decline in rate of growth began earlier than expected and that various factors were responsible, including the current economic situation and the decline in support by the Department of Defense. The 1968 National Academy of Sciences report, *Research in Solid State Sciences*,† discusses a number of areas in which important scientific advances are likely to occur and presents arguments for the usefulness of these developments that complement the case histories given in Chapter 3.

2   Status of the Field

The physics of condensed matter is a vital field and is advancing in many directions. The great diversity of experimental and theoretical work in the field provides intellectual challenge to scientists in university, industrial, and government laboratories. This diversity manifests itself in the great

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*"Solid-State Physics and Condensed Matter," in *Physics: Survey and Outlook. Reports on the Subfields of Physics* (National Academy of Sciences–National Research Council, Washington, D.C., 1966).
†*Research in Solid State Sciences: Opportunities and Relevance to National Needs*, NAS Publ. 1600 (National Academy of Sciences, Washington, D.C., 1968).

number and variety of both fundamental and applied research topics being studied. The separation between basic discoveries and applications in condensed matter is less distinct than in some of the other subfields of physics. As in other branches of physics, basic work is motivated by a quest for knowledge and understanding; however, technological innovations often follow and frequently can be directly traced to fundamental discoveries. This trend is demonstrated by the case histories that appear in Chapter 3 and deal with the development of the transistor, applications of superconductivity, magnetic bubble memories for computers, Gunn effect oscillators, lasers, rare-earth phosphors, junction light sources, and other recent applications. Many scientists and engineers are primarily concerned with applications such as these, and their work is no less exciting, nor less challenging, than effort directed toward improving the conceptual foundations of the subject. In general, the field poses exciting new challenges, both fundamental and applied.

The physics of condensed matter is a mature subject in the sense that advanced understanding and a high level of competence are required for its pursuit. In the last 15 years great progress has occurred in the development of concepts introduced in the first three decades of the century. This progress is especially evident in relation to the theories of cohesion, conduction in metals, and thermal properties of solids. Today solids are in a sense a proving ground for the principles of modern quantum theory as applied to many-particle systems. Large portions of the field have now been brought to the point at which specimens can be made, experiments conducted, and the results interpreted theoretically with unprecedented accuracy. At the same time, the field retains remarkable vitality, with a large number of intriguing new areas emerging in the last five years. Because of the maturity of the field, and in particular because of the existence of a well-established conceptual foundation, these new areas are being attacked with power, precision, and speed.

Although certain developments in condensed matter require extensive materials preparation facilities or relatively large pieces of experimental equipment, other projects can be conducted on a modest scale. Much work can be performed by a single investigator. To some persons it is, therefore, a more satisfying activity than is found in areas of science and technology that require large team efforts. For the same reason, the physics of condensed matter is often very effective for the training of graduate students, giving them research experience together with a necessary sense of independence and self-confidence (see Chapter 8).

The United States has led the world in much of the recent development of condensed-matter physics as well as in related technical innovation and

product development (see Chapter 6). Our past strength has resulted from broad expertise on a variety of fronts, both in science and technology, such as that involved in communications, computers, aerospace, nuclear applications, and specific manufacturing industries. At the present time, this country is in danger of losing scientific preeminence in certain branches of condensed-matter physics, among which are far ultraviolet and x-ray spectroscopy, amorphous semiconductors, high-field superconductors, and certain aspects of optical physics such as exciton–phonon interactions and two-quantum spectroscopy. There are indications that, in those specific fields in which they choose to concentrate, the Germans, Japanese, Dutch, and English, as well as the Soviets, are beginning to move ahead of the United States. Part of this loss in scientific preeminence is inevitable as other countries develop economically. On the other hand, part has resulted from significant reduction in support of research and development in the United States over the last five years and, more recently, from the application of artificial and inappropriate tests of "relevance" to the support for basic research.

In the sections that follow we describe the current status of the physics of condensed matter in more explicit terms and point out specific needs for expanded effort. Rather than directly answering the questions: "What were the major developments during the past five years?" and "What are the implications for growth?" we have compiled two lists of topics attached as appendixes. Appendix A lists current topics of investigation that promise substantial advances in the conceptual foundations of the field. Appendix B presents fundamental discoveries in the physics of condensed matter that have led to technological advances.

The topics of Appendix A concern basic unanswered questions that are under active investigation. Answers to certain of these questions should be forthcoming in the next few years. Appendix B deals with questions that, by and large, have been answered and are now providing the basis for technological innovation. In this list are many problems in superconductors, nonperiodic structures, and thin-film technology.

It should be emphasized that the new ideas that emerge from work included in Appendix A may not lead to significant innovations in technology for 15 to 20 years. It is also difficult or impossible to predict the exact outcome or nature of such long-range applications. Occasionally, however, extremely important ideas and the rapid accumulation of information occur, leading to new technologies within a few years. Although one cannot always predict the applications in advance, one can say that a high level of overall scientific activity is necessary to maintain a high level of technological innovation.

The rapid progress in the last few years is due largely to advancing conceptual understanding in the field, new instrumentation, new computational techniques, and new experimental capabilities. The following developments in instrumentation are especially noteworthy: continuously tunable laser sources of light for spectrometry, advanced data acquisition and digitizing equipment, new tools such as the scanning electron microscope; ion neutralization and Auger spectroscopy for studying surfaces under ultrahigh (10^{-10} Torr) vacuum, and synchrotron radiation sources. Other developments in instrumentation include higher-resolution electron spin response (ESR) and nuclear magnetic resonance (NMR) apparatus and on-line computers that greatly speed data recording and analysis in a large variety of experiments. An exciting possibility for the future is the development of coherent sources for the far ultraviolet and soft x-ray region of the spectrum.

New experimental capabilities exist, among which are high-field superconducting magnets, which make ultrahigh steady fields available to many laboratories; new cryogenic techniques, which make lower temperatures available for experimenters; the techniques of ion channeling, blocking, and anomalous x-ray penetration in crystals, which afford new methods for studying the interaction of charged particles with solids and for studying lattice defects; laser spectroscopy of much higher resolution; picosecond laser pulses for ultrafast time-resolution experiments; high-power lasers for studying nonlinear dielectric and optical properties of matter; and high-flux neutron sources, which have opened new areas of structure determination by neutron diffraction and new possibilities of observing the structural dynamics of solids and liquids by inelastic scattering.

These and other new tools offer prospects for even more rapid development and innovation if basic science receives sufficient emphasis. Physicists are just beginning to acquire the capability of designing a material for a particular purpose or of precisely predicting properties of a new material. Further development of theoretical techniques such as semiempirical pseudo-potential calculations, as well as new approaches such as the dielectric formulation of ionicity, hold the promise of quantitative materials design and engineering rather than rationalization after the fact. The simpler methods of modern band theory have already been applied to complex situations such as phase transitions in alloys. Obviously it will be necessary to understand the behavior of new and more complex solids (ternary and higher compounds and crystals, with a large number of atoms per unit cell) that will certainly be synthesized for specific purposes in the next few years. The beginning of a revolution in materials design is imminent.

3 The Scientific Basis of Solid-State Technology, with Case Histories

3.1 INTRODUCTION

One of the major achievements of physics during the last 25 years has been the success of work in solid-state physics in creating new technologies that have improved the quality of human life. The use of these technologies has enormously increased the ability of people to communicate with each other; it has greatly enhanced their ability to cope with their physical and social environment; and it has facilitated the use of the resources of near-earth space. The transistor and laser are the most commonly appreciated components of this technological revolution, but other technologies based on semiconductors, superconductivity, and magnetism are also of great and growing importance.

The process by which new technologies evolve is not generally well understood, even by the physicists and engineers most involved. Certainly physicists have not succeeded in explaining clearly to their sponsors what has occurred and what can be expected. One measure of this failure is the total misunderstanding of the process that led to the invention of the transistor that is to be found in the Department of Defense report, *Project Hindsight.*[1] Later in this chapter we will offer a series of short accounts of events leading to some of the most important new solid-state technology. These stories can perhaps convey better than any number of words the richness and complexity of these events, as well as the varied motivations of the scientists and engineers involved. Above all, they should convey an appreciation of the effectiveness with which research in solid-state physics has led to important new technology.

The use of case studies to illustrate the impact of fundamental research on technology is not new, and several good collections of such stories have already been made.[2] The examples we will give are intended to provide some fresh material, centered especially around solid-state physics. We believe that they illustrate particularly well the way that basic research is coupled to technology; they also show clearly the time required for some discoveries in research to assume economic value. There has been much misunderstanding about these matters, due in part to physicists. We feel that good judgments about national investments in physics research can be made by the government only if there is a real understanding of the interaction of physics and technology; we would like to contribute to that understanding.

Physics is perhaps the most fundamental of the sciences. As such it is the keystone of broad segments of science and an essential part of a web of knowledge represented by the chemical, electrical, earth and environmental, and space sciences. Advances in physics spread widely through this science-based society and have important influences on national defense, economic growth, the ability to manage the environment, and the quality of life. Therefore, the evidence we present in this chapter is not the only reason that adequate support for research in fundamental solid-state physics is important. However, we would like to try to give a specific answer to those who have raised serious economic questions about the support of physics by showing with several examples how solid-state physics has been the taproot of new industrial technology. Good communications typically have existed between scientists whose main interests are fundamental research and other scientists whose interests are the innovative applications of physics. The interaction between fundamentals and applications is an effective means for creating new technology. The United States would not spend its national research and development dollars nearly so well if it insisted either that all basic research be strongly motivated by and directed to practical goals or that all basic research be isolated from practical considerations.

Another important lesson taught by the case histories is that basic research in this country is a highly relevant activity even when the research is not directly motivated by a specific need or application. This built-in relevance underlies the high return the United States has had on its investment of more than $1 billion to date in research in the physics of condensed matter, and it is a principal justification for a continued investment of about $100 million per year in this field.

3.2   U.S. VIEWS OF BASIC RESEARCH

The U.S. public is confused about the difference between science and engineering and, in fact, usually makes little distinction between the two. They are not helped by the public press, which regularly gives credit to science for technical achievements in space and other fields that are the result of large-scale engineering. The confusion stems from deep-seated attitudes based on our frontier background, which included admiration of practical inventors such as Samuel Morse, Eli Whitney, and Alexander Graham Bell. Early U.S. scientists such as Joshua Willard Gibbs, Joseph Henry, and A. A. Michelson are little remembered by the public, although they ranked with the best scientists in Europe. The confusion about science and technology was, if anything, increased after World War II when the

public was informed about the secrets of radar and the atom bomb and realized that these two events had had a decisive effect in shortening and winning the war. That they were principally engineering achievements based on previously discovered science, some of it new, some of it quite old, made little impression. The work had been done by scientists, and its impact on the public imagination was great. A large reservoir of good will for science was created, but without particular understanding of the normal activities of science and the research work to which the scientist-turned-engineer wished to return after the war.

Against this background, it is natural enough that the U.S. view of basic research should be an extrapolation of the idea of invention as a source of industrial innovation. Research is perceived as an initial link in a chain that leads to new technology, and it has generally received social approval in that context. This is not to say that basic research is not distinguished in government from applied research and development, since there have been many advisers, beginning with Vannevar Bush, who have explained that basic research is a special kind of activity. Although it has been said many times that basic research is a search for new knowledge and understanding and is not directed toward the solution of particular problems, the direct utilitarian view of research still seems to be the prevailing Congressional attitude, and it is not apparent in recent years that the support of basic science has often been urged on any other terms. Consequently, except when basic research is perceived as directly related to an important goal, there seems to be a good deal of skepticism in the government as to whether such work has any useful relationship to real problems.

Our objective is to show that basic research in the physics of condensed matter, performed solely to understand in the deepest possible way the complex behavior of solids and liquids, has been the source of two decades of unprecedented achievement in critical new technologies. We see no way in which these achievements could have been planned in the past and no way in which further progress can be programmed except by continued support of basic research. There are many examples other than those we present showing that engineering innovations develop, in response to engineering needs, from the physical understanding that exists at any given time and are functionally limited by the depth of that understanding.

3.3 ORGANIZATION OF SOLID-STATE RESEARCH AND ENGINEERING

The best justification for a continued national investment in the physics of condensed matter is the existence of effective and proved means by

which new science is converted into needed and useful technology. It is apparent to experienced observers of the scientific and industrial structure of the United States that the scientists and engineers working with solid-state phenomena constitute two separate but interacting communities. This general observation is supported by studies of the patterns of information flow among the communities.[3-5] The communities are strongly linked by common educational backgrounds and by an exchange of manpower that carries with it an interchange of new scientific ideas and technological opportunities.

The scientific community is based in the universities and in industrial and government research laboratories and does research on a broad range of problems in solid-state physics and chemistry, with the primary objective of understanding through theory and experiment the inner nature of condensed matter. The new knowledge that this group has produced has been the indispensable basis for the development, during the last two decades, of such subtle and outstanding advances as the transistor, the silicon-controlled rectifier, coherent laser light, and 100,000-G superconducting solenoids. Like everyone else, these basic research people are subject to judgments on the quality of their work. Since their chief product is knowledge, they ought to be judged not so much by the technical problems that they have helped to solve as by the excellence and significance of their research. The findings of high-quality research can be assembled piece by piece to form the coherent structure of understanding that is an indispensable foundation for significant new engineering developments.

The engineers in industry and government laboratories together with the engineering faculties of the universities form a second technical community whose objective is to solve important problems in technology or to teach others how to solve these problems. It is often necessary for an engineer to invent a basically better and cheaper way to do some job; he may also find a way to do a job that previously could not be done. In such instances, the engineer has to rely on science to provide the knowledge that he needs.

Significant inventions often are based on pre-existing scientific information. We will give several examples. The usual agent is an imaginative engineer who by inclination keeps in close touch with the science related to his work. These individuals have been called "couplers"[5] and have a special ability to take information organized in a scientific pattern and reorganize it into a technological pattern to solve a problem. This reorganization often brings together several lines of scientific development and shows the futility of trying to program research to meet a future need.

Sometimes, however, a field of research like semiconductor physics grows so rapidly that a new technology becomes possible almost overnight.

When that happens, the scientists have special information not easily available to the engineers and have to make inventions and organize themselves to exploit the new capabilities that have developed. Something like this seems to happen about once in ten years. Examples are the discovery of nuclear fission in Germany in 1938, the discovery of transistor action in 1948, and the discovery of laser radiation in 1960. These possibilities strengthen the case for the support of basic science to guard against technological surprise.

3.4   THE TIME SCALE FOR USE OF BASIC RESEARCH

We shall comment briefly on the time scale that we have found to be characteristic of the transfer of information from science into engineering. We believe that it is misleading to imply, as has been done on occasion, that significant science often is converted into a useful device in one or two years. Laboratory models are sometimes built soon after discovery of a new physical effect, but it is not usual for these devices to offer a new economic solution to a critical problem. The process of discovery and invention sometimes can be shortened but seems more often to work on a 10- to 20-year time scale. The laser, for example, was conceived in 1957, but significant industrial applications, as distinct from laboratory tools, are just now appearing, and the massive application of laser technology to communications may not take place until 1980. This longer time scale simply means that it requires a number of years for enough new science to accumulate to make a significant new technology possible and for technology to advance to the point at which it can make use of new science.

   The Department of Defense study, *Project Hindsight*,[1] was able to identify very little impact of recent basic research on new weapons systems. The methodology of this report effectively ignored research contributions that preceded the study by more than 13 years and tended to include science events that were strongly oriented toward particular projects. Therefore, we do not have any particular disagreement with Project Hindsight but feel that it is largely irrelevant to the issue of whether good returns are realized on the national investment in basic science. The study does not seem to recognize either the time scale natural to the science–engineering interaction or the essential nature of that interaction. Investments in basic physics have to be made in full recognition that the science done will have an unpredictable relation to its eventual use and that it could be 20 years or longer before its relevance is apparent. This situation is just as true of military systems as it is of the civilian industrial systems, and national de-

fense is likely to be seriously affected by being isolated from basic science in the name of short-term relevance. Similarly, effective environmental control in the future will depend in part on the basic physics research done today.

3.5 SUMMARY

The following sections present case histories that support the following general picture:

1. Significant inventions almost always result from some practical need that finds a solution in the body of existing science.

2. The necessary science is built up most efficiently by scientists who are concerned with extending accurate knowledge of nature. The emphasis should be on supporting excellence and not on an impossible-to-predict relevance.

3. Occasionally new science accumulates so quickly in some area that it outruns the state of the art and forms the basis of altogether new technologies. This process is also unpredictable but very important when it occurs.

4. Science sometimes is converted to useful ends very quickly, but more often it will take 10 to 20 years for a scientific result to assume economic value.

5. Exploitation of a new invention usually requires the development of ancillary technology—for example, high-purity and controlled-composition single crystals in the case of the transistor. This ancillary technology depends also on the existing state of basic knowledge. Many inventions have lain fallow for years because the state of science did not permit the development of ancillary technologies.

6. Development effort requires many subsidiary decisions as to the proper direction in which to proceed. The development process consists of many branch points, each of which requires the prediction of the most likely outcome of proceeding in alternative directions. The wisdom and discrimination with which these branch-point decisions can be made is highly dependent on the state of basic scientific knowledge in the general area. Thus the economy and efficiency of the development process are strongly dependent on the state of science, even when science does not appear directly as an input to the technology developed. The most likely result of lagging basic science is an increasingly inefficient and costly development process.

3.6   SOLID-STATE PHYSICS AND INVENTION
OF THE TRANSISTOR

There are several excellent accounts[6-9] available that discuss the events
that led to the invention of the transistor in late 1947 by W. Brattain
and J. Bardeen. This discovery is of such importance, however, that any
discussion of the interactions between physics and technology would be
incomplete without at least a short review of the history and circumstances
surrounding it. Moreover, we would like particularly to emphasize the
dependence of the two-year success story during 1946 and 1947 on critical
discoveries in solid-state physics beginning in 1925.

We begin with the period of sustained activity at Bell Telephone Labora-
tories from January 1946 to late December 1947, during which a very
intense study was made of the physics of semiconductors coupled with
persistent efforts to find some method of using semiconductors to achieve
electronic amplification. At that time it was decided to resume and
strengthen research on semiconductors that had lapsed during the war, and
a group was formed for this purpose under the leadership of W. Shockley
that included W. Brattain, G. Pearson, and J. Bardeen. The forces behind
this decision included prewar work at Bell Laboratories that had led to
better crystal rectifiers, thermistors, and other semiconductor devices;
the very great importance of crystal rectifiers for microwave receivers
during the war; and Shockley's conviction that a semiconductor amplifier
could be invented. A firm foundation for the effort was the work on semi-
conductor materials done at Bell Laboratories by R. S. Ohl, J. H. Scaff,
and H. C. Theuerer and elsewhere, notably at Purdue University by K.
Lark-Horovitz. This work had led to methods for producing pure crystals
of silicon and germanium and for doping them with controlled amounts of
donor elements (phosphorus, arsenic, and antimony, which supply elec-
trons to the crystal) and acceptor elements (aluminum and boron, which
create electronic holes in the crystals). The corresponding concepts of N-
and P-type conduction were well established. During the critical two years,
materials research continued at Bell Laboratories and gave important sup-
port to the research on the physics of semiconductors.

Although the notion of semiconductor amplification was always in the
background, the principal strategy of the research group was directed to-
ward increasing fundamental understanding of semiconductors rather than
to the solution of a problem in technology. This emphasis was made pos-
sible by the existence of adequately pure germanium and silicon as an
experimental material. It was also thoroughly understood that the basis of
scientific understanding was not yet sufficient to permit a frontal attack

on the problem of devising a solid-state amplifier. This strategy has been subsequently confirmed by the scientists and administrators involved. The rapid success of the work in leading to a working amplifier resulted from the state of knowledge and materials in the field, both of which were nearly ripe for exploitation.

Early in 1946, Shockley proposed that it would be possible to modulate the resistance of a thin layer of semiconductor by imposing a strong electric field gradient across the layer and changing the number of available carriers of current. It was obvious that electronic amplification could be based on this effect if it existed, and the idea had been in Shockley's mind for some time. Experimental tests of the idea were carried out by J. R. Haynes, H. J. McSkimin, W. A. Yager, and R. S. Ohl, with negative results; the effect seemed to be at least 1000 times smaller than expected from Shockley's calculations.

The failure to find the predicted field effect, together with other experimental results that could not be described by the Mott-Schottky theory of rectification (described subsequently), led Bardeen by April of 1946 to a re-examination of semiconductor band theory and to the idea that electron states exist at the surface of a semiconductor that do not penetrate into the body of the material. Electrons trapped in these surface states could move so as to cancel the electric gradient needed for the field effect. The theory also suggested that at low temperatures electrons would be immobilized in the surface states. Experiments by Pearson and Bardeen showed that the field effect could be observed at low temperatures, but the effect was still much smaller than expected.

Bardeen's theory of surface states provided the conceptual basis that is nearly always essential for the most effective experimental research. Consequently, it was decided to stress research on surface phenomena of semiconductors. This was in accord with the basic strategy of the group, but it was also realized that new information might lead to a way of controlling the effects of the surface states and open a way to field-effect amplification. During the remaining months of 1946 and the early part of 1947, many experiments were suggested by Bardeen, Shockley, and Brattain to test the surface-state theory, and the experiments were performed by Brattain, Pearson, and H. R. Moore. Some experiments in May 1947 showed that the work function of silicon could be systematically controlled in accordance with the theory by changing the impurity concentrations. In April 1947, Brattain conducted an experiment that entailed shining light on N-type silicon, which indicated the presence of a space-charge layer at the surface in agreement with theory. In November, a bigger effect was observed by immersing the front surface of the silicon in an electrolyte and applying a field at the interface through the electrolyte.

When this field was varied in intensity at the suggestion of R. B. Gibney, very large effects were observed that could even be reversed in sign by reversing the voltage applied to the electrolyte.

These experiments indicated that the electrolyte was transmitting a strong electric field gradient to the front surface of the silicon and turned attention back to the field effect mentioned previously. Bardeen suggested a new experimental arrangement for detecting the effect, and the experiment was quickly tried by Brattain with success. Current flowing in a thin surface layer of N-type silicon to a point contact was controlled by a potential applied to an electrolyte surrounding the point in a way consistent with Shockley's theory of the field effect. This was done in late November 1947. The experiment was soon repeated successfully with germanium rather than silicon.

The frequency response of this arrangement was limited by the electrolyte, and it was suggested by Bardeen that the electrolyte be replaced by a metallic contact insulated from the surface. A new experiment was prepared for Brattain and Bardeen by Gibney, who anodized an N-type block of germanium and evaporated onto it a number of gold spots. It was hoped that the voltage applied to a gold spot would affect the current flowing in the surface to an adjacent point contact. At this point it was discovered that the gold spots were not insulated from the germanium as anticipated, but it was decided to try the experiment anyway. A new effect now known as transistor action was observed: when a positive bias was applied, holes flowing into the germanium from the gold spot increased the current flow to the point contact biased in the reverse direction with a large voltage. The field effect would have produced a change in the opposite direction. Voltage amplification was observed on December 15, 1947. About one week later, on December 24, 1947, an improved experiment produced a power gain of 18 and was used to amplify speech.

A study of this short history shows that the key ideas that kept the research moving ahead were Shockley's concept that it should be possible to apply an electric field to a semiconductor and thereby modify its conductivity and Bardeen's introduction of the notion of surface states as an important element in the growing understanding of semiconductors. Under the impact of these two ideas the experimentation grew ever more sophisticated until the discovery of transistor action became increasingly likely. The final achievement, whereby minority carriers injected into N-type germanium were able to control amplified power flowing in the collector circuit, was unexpected. The lesson is that such unanticipated and important results emerge from the rising tide of exact science and usually cannot be reached by the elaboration of known technology.

The key ideas just mentioned depended on pre-existing science and

could not have been formulated without a foundation of work extending back to 1925. This work had led by 1946 to the following picture of a semiconductor like germanium (or silicon). The germanium atoms are arranged in a regular crystalline array similar to the configuration of carbon atoms in a crystal of diamond. Each germanium atom with a nuclear charge of 32 units has associated with it 28 core electrons and four valence electrons that form covalent bonds with the four nearest neighbor germanium atoms. In the language of band theory, a crystal composed of N germanium atoms gives rise to a set of four valence bands separated by an energy gap of about three fourths of an electron volt from a set of four conduction bands. Each band consists of a set of $N/2$ quantum states in which the electron wavefunction extends throughout the crystal. The four valence bands have room for four N electrons, if each quantum state is occupied by only two electrons of opposite spin, according to the Pauli exclusion principle.

The semiconductor model included the following picture of the effect of donor and acceptor impurities. A donor impurity such as arsenic in germanium provides an extra electron in the crystal lattice. This electron resides in a wavefunction localized about the impurity and bound in place below the conduction band by an energy of about 1/100 of an electron volt. At temperatures of about 100° absolute, these electrons are ionized into the conduction band and provide the charge carriers for N-type semiconduction. The concept of holes was subtler but equally precise. An acceptor atom such as boron dissolved in germanium withdrew an electron from the valence band leaving behind an unoccupied state. This state acted in the same way as a positive charge and was bound to the now negatively charged acceptor atom by an ionization energy of about 1/100 eV.

These ideas all stem from the beginning of the modern theories of quantum mechanics in 1926. In that year Schrödinger announced his wave theory of the electron that replaced older and cruder quantum theories discovered by Bohr, Sommerfeld, and others that dated back to 1913. Schrödinger's wave equation, with its quantized energy levels, supplemented by the Pauli exclusion principle introduced by Pauli in 1925, provides a complete conceptual scheme for the description of electronic and atomic systems in the quantum regime. The theory was inspired by and gave an accurate description of the energy levels and optical spectra of the elements.

In 1928, Sommerfeld applied the new quantum mechanics to the theory of metals. In doing this he made use of a new statistical mechanics based on the exclusion principle and independently discovered by E. Fermi and P. Dirac in 1926. Sommerfeld's theory explained many important facts

about simple metals that had not been understood previously, such as their small electronic specific heat and magnetic susceptibility. A remaining major difficulty was the low electrical resistance of pure metals at low temperatures, since it seemed that electrons should be scattered strongly even at low temperatures by collision with the atoms forming the crystal lattice. This difficulty was removed by Bloch in 1928; he showed that a perfect crystal lattice so modified the solutions of the Schrödinger equation that electrons moved unimpeded except by imperfections in the regular lattice structure.

The ideas of Pauli, Schrödinger, Fermi, Sommerfeld, and Bloch were applied by Wilson in 1931 to achieve a comprehensive theory of semiconductors. These materials were intermediate in conductivity between metals and insulators and required a special new concept for their understanding. This new idea was the energy gap in the spectrum of energy levels described above, and it is a natural consequence of Bloch's theory of electrons in a crystal lattice and of the Pauli exclusion principle. Thus, by 1931 a fundamental theory had been developed that was to be the essential basis for the work of Bardeen and Brattain in 1946. The theory seems to have had little impact on experimental work on semiconductors from 1931 to 1941, probably because semiconducting materials of sufficient purity were not yet available to allow good contact between theory and experiment. Wilson's theory was applied by Mott and Schottky in 1938 to describe the process of rectification at a metal–semiconductor interface, but fundamental work soon gave way to the urgency of wartime problems.

This short history shows the great amount of progress in pure physics that was necessary before Shockley, Bardeen, and Brattain could begin their research in 1946 and the critical way in which the progress of their work depended on a physical model of semiconduction. In this light, the transistor emerged as the fruit of fundamental scientific research extending over a period of 20 years. The invention of the junction transistor by Shockley in 1951 and the tremendous progress of semiconductor electronics over the next 20 years was possible only because of the sound and expanding understanding of the basic science of semiconductors.

3.7 SOLID-STATE PHYSICS AND THE COMING POWER CRISIS

The United States is faced with a need for electrical power that will become increasingly difficult to fulfill during the next 20 years unless new technology is developed and put to use. In this section we describe a developing technology, based on discoveries in solid-state physics dating back

to 1911, that might contribute substantially to a possible solution to the problem. The need results from the multiplying population of the United States, its increasing urbanization, and its relentless demand for more and more electric power. New York City alone now consumes a peak load of 7300 million watts of electricity during the summer months,[10] and the demand is expected to double in ten years.

The increasing concern over the environmental effects of electrical power generation is a significant obstacle to meeting this demand in the future. This obstacle probably can be overcome only with new technology that makes possible the generation and distribution of electrical power in ways that are less disruptive of the local environment than present methods. Thus, more power will have to be generated at each site and generated : more efficiently and distributed more effectively than it is now. The deleterious emissions from the plants also must be reduced.

Several new ideas for producing power are being explored in various laboratories throughout the world. Among them are controlled thermonuclear fusion and magnetohydrodynamics (MHD). Both approaches require an intense magnetic field. In a controlled thermonuclear reactor, the fuel is a hot plasma; that is, a gas made up of electrons and ions of deuterium, tritium, and helium. An essential feature of the reactor is the confinement of the hot plasma to a restricted volume where the fusion reaction takes place and the energy is converted to motion of the ions. However, the plasma cannot be confined inside any sort of ordinary container since the walls would cool the plasma below the temperature of 10 million to 100 million degrees needed for the reaction. Instead, the gases must be held inside a "magnetic bottle" that controls the motion of the electrically charged electrons and ions without actual contact with material walls. This effect can be easily visualized by holding a small bar magnet near the face of an ordinary television set and observing the effect of the magnetic field on the charged particles tracing the picture.

In a magnetohydrodynamic power generator, a hot ionized gas must flow through an intense magnetic field at high velocities to produce the electrical power. Again, as in a fusion reactor, one needs an economical means of producing intense magnetic fields.

In recent years, superconductors have been discovered and developed that provide the means of producing intense magnetic fields over relatively large volume in an economical manner.

Superconductors are a class of metal that loses all resistance to the passage of electric current when cooled to temperatures near absolute zero (or $-273°C$). Coils made of superconductors could generate the magnetic fields necessary to contain a hot plasma without using up too much useful

power output as energy loss to resistance of the coils. Some power is needed to cool the coils but is small enough not to be an important factor.

The eventual practicality of either controlled thermonuclear fusion or magnetohydrodynamics is still speculative, and it appears that much experimental work needs to be done before large-scale development is undertaken. But, superconductivity may play an important role in the future technology of power generation and distribution independently of the outcome of fusion and MHD. The reason is that with superconductors one should be able to generate more power in smaller-sized generators than can be achieved by conventional means. The fundamental process of generating electricity is to rotate a conductor in a magnetic field. If one can increase the intensity of the field in a given volume by using superconducting coils, then the amount of power generated in that volume goes up. Because there are large economies of scale in power generation, there is a strong incentive for increasing the power-generating capacity of individual units. This trend is also compatible with conservational and environmental interests, because fewer plant sites would be required for a given power output.

In addition, superconductivity may be used in transmitting large blocks of electrical power in the future. If it becomes necessary—because of limited right-of-way or because of aesthetic considerations—to transmit very large amounts of power underground, then superconducting power lines may be the most economical and satisfactory solution.

There are many superconducting metals, but only a few are of potential use for the foregoing purposes. These superconductors are characterized by relatively high temperatures for transition into the superconducting state (about 20° above absolute zero), the ability to sustain magnetic fields of more than 150,000 Oe, and the ability to carry currents of more than 200,000 A/cm² of cross section. A short history of the research that led to the discovery of these materials[12] follows.

Superconductivity was discovered by Kamerlingh Onnes in Leyden in 1911.[13] Like many other advances in physics, it was an accidental discovery made in the course of fundamental studies of nature. Kamerlingh Onnes had recently succeeded in liquefying helium gas and was able to reach temperatures only a few degrees above absolute zero. He knew that the electrical resistance of many metals decreases as their temperature is lowered, and he wanted to see if this effect continued to the lowest temperatures he could reach. A student working in his laboratory discovered that mercury lost all resistance below a temperature of 4.1 K.* It is reported that Kamerlingh Onnes was so surprised that it was several days be-

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* The kelvin scale of temperature measures upwards in degrees centigrade from absolute zero; 4.1 K means 4.1 degrees above absolute zero.

fore he would believe the student's measurements. However, they soon went on to find that several other elements also showed superconductivity, an example being lead, with a transition temperature of 7.2 K. The possible practical applications of zero resistance so impressed Kamerlingh Onnes that he very soon tried to generate a magnetic field using a lead wire solenoid. His hope was that the superconducting coil would carry large persistent currents without energy loss and produce a strong field. It was a severe disappointment when it turned out that the lead wire lost its superconductivity above a critical current density corresponding to only a few hundred oersteds in the solenoid.* They later also found that a magnetic field greater than some critical value was sufficient by itself to destroy superconductivity even if no current was flowing in the wire. These discoveries seemed to destroy any hope for using superconductors to make an electromagnet. A technological application was clearly visualized by Kamerlingh Onnes, but the scientific knowledge was not then available to understand the failure of the solenoids and to suggest the alternatives now known to exist.

There followed a period of nearly 40 years in which very little experimental or theoretical progress was made in superconductivity. A number of new superconductors were found, but almost always with low values of critical magnetic field. An exception is niobium metal, which, in about 1925, was found to have the relatively high transition temperature of 8.3 K and a critical field of 3000 Oe. In 1930, deHaas and Voogd[14] found that a lead bismuth alloy had a critical field of 20,000 Oe, but the hope sparked by this discovery was lost when the alloy was shown to have a low critical current density. It is interesting that, unlike pure lead, both lead–bismuth alloy and niobium had real technological possibilities, but this potential could not be recognized at the prevailing level of understanding.

In fact, for 46 years following Kamerlingh Onnes' discovery, superconductivity defied explanation and was one of the outstanding puzzles of physics. It was not until 1957 that a successful theory was constructed by Bardeen, Cooper, and Schrieffer based on the interaction of electrons and phonons in metals. This theory gave a firm foundation for a phenomenological theory of high field superconductivity, which had been proposed by Ginsberg and Landau in 1950 and extended by Abrikosov in 1957. These theoretical developments gave important support to the experimental work done after 1957 by providing general guidelines for the research and by establishing theoretical limits to the performance of materials. The Bardeen, Cooper, and Schrieffer theory also showed that superconduc-

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* The earth's magnetic field is about 0.4 Oe, and a small horseshoe magnet produces a field of about 1000 Oe.

tivity is a quantum phenomenon, observable on a macroscopic, as well as atomic, scale. This conceptual breakthrough has led to a number of applications of great potential value that are described in Section 3.11.

The recent history of high-critical-field, critical-current superconductors began about 1950 and had two important branches that converged in 1960. One branch concerned the discovery of important new superconductors, while the other branch involved a growing technological need for a high-field superconducting solenoid to be used in conjunction with a sensitive detector of ultrashort radio waves.

In 1941, Justi and his co-workers[15] discovered that the compound NbN (niobium nitride) had the high transition temperature of 14.7 K, and this result was confirmed by Horn and Ziegler in 1947.[16] Struck by this finding and the fact that so little work had been done on the superconductivity of compounds, Hulm and Matthias,[17] working at the University of Chicago in 1950, began an intensive search for new superconductors. In 1951, they reported the discovery of superconductivity in several new borides and nitrides of niobium and molybdenum, including MoN (molybdenum nitride) at 12.0 K. This latter work was particularly striking since molybdenum was normal to below 0.3 K. In 1953, Hardy and Hulm,[18] working at Chicago, discovered the superconductivity of a vanadium–silicon compound V_3Si* at the record high temperature of 17.0 K. Later in the same year, Matthias,[19] now at Bell Telephone Laboratories, reported that the transition temperature of NbN could be raised to 17.8 K by substituting carbon for three of each ten nitrogen atoms. Another important discovery was that the transition temperature of niobium could be raised to 10.8 K by the addition of 50 percent zirconium. This alloy was destined for important technological applications later on. In this paper Matthias also reported the important observation that all the then known high-transition-temperature materials had an average electron-to-atom ratio slightly below 5. Building on this clue, and the work of Hardy and Hulm, Matthias began to look for compounds crystallizing in the beta-tungsten structure and having a favorable electron-to-atom ratio. In 1954, this work led to the discovery of the niobium–tin compound, Nb_3Sn, with a transition temperature of 18.05 K.[19,20] This was the highest transition temperature then known and remained so for 13 years. Nb_3Sn is still the most important high critical-field superconductor.

The fundamental study of the occurrence of superconductivity continued. Daunt and Cobble[21] found in 1953 that the radioactive element technetium, which has an electron-to-atom ratio of 7, had a high transition temperature at 11 K. This finding suggested to Matthias[22] that a second

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* This crystallizes in the so-called beta-tungsten structure.

optimum electron-to-atom ratio must exist near 7, a hypothesis that was confirmed in 1954 by a study of alloys of molybdenum and tungsten with such elements as ruthenium and rhodium. A compound of equal parts of molybdenum and ruthenium was reported with a transition temperature of 10 K and an electron-to-atom ratio of 7. In this same class of materials, Hulm,[23] now at Westinghouse, reported in 1955 an alloy of molybdenum and rhenium with a transition temperature of 10 K that was also destined to be technologically important because of its ductility.

Some recent developments complete this part of the story. In 1959, E. Corenzwit[24] reported that a compound of niobium and aluminum, Nb_3Al, also crystallizing in the beta-tungsten structure, had a transition temperature of 18 K. Six years later,* Matthias and co-workers[25] discovered that another beta-tungsten compound, Nb_3Ge, this time involving germanium, could also be prepared with a transition temperature near 18 K. The stage was set then for preparation of a solid solution of these two materials. This goal was accomplished in 1967 by Matthias and co-workers,[26] who formed a compound containing about four parts of Nb_3Al and one part Nb_3Ge that showed a transition temperature of 20.05 K. This mixture was selected for testing because, according to Russian reports, it had an anomalously low melting point, and a correlation between melting point and critical temperature had been suspected by the U.S. investigators. This correlation is now rather well established. Later in 1967, the onset temperature was raised to 20.4 K, that is, to the boiling point of hydrogen in an alloy of composition $Nb_3Al_{3/4}Ge_{1/4}$. In 1968, further advances in metallurgy made by Matthias and Zachariasen and their co-workers[27] raised the temperature several tenths of a degree higher, and recently (in 1970) Ruzicka reported the onset of superconductivity to be at 21.5 K in a suitably heat-treated alloy of the same composition. Meanwhile measurements of the superconductivity of some of these materials by Foner[28] and his co-workers indicated that they remain superconducting in magnetic fields of about 200,000 Oe at 14 K and above 400,000 Oe at 4.2 K, far higher than any known materials.

We now return to the second branch of the story. During 1958, S. H. Autler at the Lincoln Laboratories, Massachusetts Institute of Technology, became deeply interested in the application of a solid-state microwave maser as a sensitive detector of radiation for radio-astronomy research. These devices operated at a temperature of 4 K and required a magnetic field of about 4000 Oe. In the configurations enforced by the low-temperature requirements of the maser, a permanent magnet capable of pro-

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* These slow-paced events reflect the difficult metallurgical problems encountered in dealing with these compounds.

ducing this field would weigh more than 500 lb and be very awkward to mount at the focus of the parabolic antenna. Consequently, Autler began to consider the possibility of using a superconducting solenoid to take advantage of the fact that, in any case, the maser has to operate at low temperatures. By July 1959, he had constructed a solenoid using niobium wire that produced a field of 5100 Oe and met his design objectives.[29]

In December 1959, R. Kompfner of Bell Laboratories visited Lincoln Laboratories and learned of Autler's work. Bell Laboratories was also interested in the use of masers as microwave detectors for the Telstar satellite communication system and faced similar problems of magnet design. Kompfner's report on Autler's work led to a meeting to discuss steps that might be taken to construct practical superconducting electromagnets for the maser system. Matthias, who was present at the meeting, was able to suggest several materials that he felt might be superior to niobium. These included niobium alloys, molybdenum–rhenium alloys, and $Nb_3 Sn$, all of which had been discovered in the course of the research programs of Matthias, Hulm, and their colleagues.

Following this meeting, J. E. Kunzler began to study the properties of molybdenum–rhenium alloys and soon found that, as hoped, they were much superior to those of pure niobium for magnet design. Further conversations between Kunzler and Matthias led to an investigation of niobium–zirconium alloys, which turned out to be even more promising.[30] Kunzler also began to study the properties of $Nb_3 Sn$[31] and was astonished to discover that the compound remained superconducting at 88,000 Oe, which was the highest available test field. Extrapolation indicated that the limiting magnetic field might be higher than 100,000 Oe. At 4.2 K and 88,000 Oe, $Nb_3 Sn$ wire could still sustain the extraordinary current density of 150,000 Amp/cm.[2] Practical superconducting solenoids have now been built of $Nb_3 Sn$, which produce fields as high as 150,000 Oe. In the last five years, magnets employing superconductors have found a wide range of applications in the laboratory and elsewhere. A liquid hydrogen bubble chamber 12 ft in diameter at Argonne National Laboratory is supplied with superconducting coils. Superconducting motors have been built in England and the United States. The next generation of high-energy accelerators probably will depend on superconducting magnets for the guide and focusing fields.

We have outlined the story of an extremely important class of solid-state materials that have already had striking applications but whose critical impact on problems of power generation and transport probably will be felt during the next 10 to 30 years. It is important to recognize that the high-transition-temperature superconductors arose from a basic research program extending over many years, with the most important discoveries

coming in 1950 to 1960 but with roots going back to Kamerlingh Onnes' work in 1911. The research program was not a random search for new materials but was systematically guided by well-formulated principles concerning the occurrence of superconductivity; it was thus in the best tradition of basic physical research. It is noteworthy that there was little contact between technology and the research program until 1958, when the problem of light-weight solenoids for microwave receivers came into sharp focus because such receivers were required for radio astronomy. At that time, the science was sufficiently advanced to provide the answers needed because it had been supported and pursued for its own sake for many years.

3.8 SOLID-STATE PHYSICS AND MAGNETIC BUBBLE TECHNOLOGY

Magnetic bubble technology offers a radical new way to store information in thin, transparent magnetic crystals and to perform logical operations by the controlled motion of magnetic bubbles over the crystal. An important incident in the development of these novel memory and logic devices was an encounter between scientists and a group of engineers at Bell Telephone Laboratories one day in March 1966. This story (presented later in this section) illustrates in a striking way how research conducted for many years in search of a basic understanding of magnetism was able to provide the essential key to a new and important technology.

Man lives in a crowded, turbulent world. To survive, his complex, urban civilization must process enormous masses of information for business, government, and industry. Much of this processing is performed by fast digital computers that have electronic memories of great capacity. Their memory is measured in bits, or the ability to store one yes or no answer. A modern computer may have a total high-speed capacity of more than 10^7 bits* and a lower-speed memory much larger than this. The computer not only has to store information but must process the information by performing arithmetical, algebraic, or logical operations. In fast computers, information can be manipulated at rates of more than 10^8 bits per second. Very similar problems occur in the communications industry. The central offices of the telephone system, where telephone calls are now switched electronically, require large memories that, within 10 years, might require the storage of as much as 10^7 or 10^8 bits. In this case, the required retrieval speed is generally not so great as that for the core memory of a

* A large book stores about 10^7 bits of information.

large general-purpose computer, but permanence of memory in case of power failure is very important.

A modern computer has a hierarchy of memories arranged in order of increasing capacity but decreasing speed. The fastest memories are constructed from an array of tiny semiconductor diodes and transistors. Such a memory is backed up by a memory made from very small magnetic rings or cores about 2/100 of an inch in diameter, arranged in a square array and cross threaded by wires. The cores can be magnetized in one sense or the other around the ring, corresponding to one bit of information. The core memory, in turn, is backed up by a magnetic disk file, which is simply a large magnetic disk rotating continuously at high speed. Information is stored on the disk in small magnetized spots as it rotates beneath a recording head. It can be consulted later by a separate readout head. Most of the data processing is carried out by the core memory, which can store about 10^7 bits of information or more, with a cycle time (the time required to put information in the memory and read it out) of a microsecond. A memory of this size would occupy a volume of several hundred cubic inches. The disk file is ten times slower, since it takes about 10 μsec to read out a bit of information stored on the memory disk. A still larger and slower memory stores information on magnetic tapes similar to those used in an ordinary tape recorder. A typical tape memory has an access time of tens of second but a capacity of 10^8 bits on a single reel of tape. With its associated control equipment, it can occupy a volume of several cubic feet.

Magnetic bubble memories may replace both the core and disk file memories in computers and electronic central offices and interface directly with fast semiconductor memories. They may prove to be a very fast, compact, and inexpensive way to store and process data.

The magnetic bubbles are tiny cylindrical regions of reversed magnetization, as small as 2/10,000 of an inch in diameter, that can be formed in thin crystals of a magnetic oxide of iron and a rare-earth metal such as yttrium. In one form, the memory may consist of plates about 1 inch square and 5/10,000 of an inch thick that can store 10^4 bubbles, each representing one bit of information. Other forms of the memory may be able to store as many as 10^6 bubbles per square inch on crystals 4/10,000 of an inch thick. The bubbles can be moved around on the crystals in a precise pattern by evaporated metal control circuits to process the stored information. They can be generated, replicated, and erased to carry out various logical operations and their presence or absence detected electrically. In this technology the storage and logic functions are combined at a great reduction in cost, and the energy required to move a bubble is only a fraction of that needed to switch a transistor. A storage memory has been visualized that will hold 1.5×10^7 bits of information in one or two cubic inches and will be operated by 10 W of power.

3.8.1   *The Technological Background*

In crystals of certain anisotropic magnetic materials, the magnetization of any small region lies along a particular direction or axis in the crystal. When the crystal is completely magnetized, all the magnetization points in the same direction along the axis. In the perfectly demagnetized state, on the other hand, some regions of the crystal are magnetized in one direction along the axis, while other regions of equal volume are magnetized in the opposite direction. The net magnetization is therefore zero. A connected region in which the magnetization has a common direction is called a domain, and the boundaries between different regions are called domain walls.

Beginning about 1958, efforts were made in several laboratories in the United States to perform memory and logic functions by shifting magnetic domains along a magnetic wire or surface. A domain wall shift register was constructed by Broadbent of Hughes Aircraft Company that moved domains in thin magnetic films, and domains were shifted along magnetic wires by Irons at the U.S. Naval Ordnance Laboratory and by Bobeck and Gianola at Bell Telephone Laboratories.[32] These wires were eventually arranged in close-spaced arrays on an insulating substrate, and the concept began to emerge that domains on adjacent wires could interact or be transferred from wire to wire to carry out logical operations. These arrays of wires have not yet found commercial acceptance, but they left an important residue of interest in the motion of domains in two dimensions.

A second important part of the technological background was the work of Bobeck et al.[33] on a magnetic structure known as the waffle iron. It consisted of a grid made of soft (that is, easily magnetized) magnetic material looking like a very-small-scale waffle iron, with only 1/100 of an inch or so between elements of the grid. Wires were laid in the grooves and a plate of magnetizable material placed on top. Information was stored in the plate by magnetizing currents running through the wires. The waffle-iron memory was intended to be an improvement over core memories, but advances in the fabrication of core memories have so far prevented the waffle iron from becoming economically acceptable.

The waffle-iron structure, however, led to experiments of quite a different kind. Early in 1966, P. Michaelis,[34] working with Bobeck, showed that a thin permalloy film laid on the waffle iron could sustain single large reverse domains with the magnetization lying in the plane of the film. Permalloy is a soft magnetic material, and it was found that the domain could be moved intact over the surface in two dimensions by manipulating the magnetizing currents in the wires. However, important difficulties, such as their large size, the slowness with which they moved, and an anisotropy in their motion, stood in the way of the use of these domains in a memory

application. At this time H. E. D. Scovil was very interested in the technical possibilities of reversed domains and brought these problems to the attention of Shockley, who consulted at Bell Laboratories a few days each month. It was realized that a material with properties different from permalloy was needed; in fact, the hypothetical properties that were needed were quite well understood, and a conference to assess the possibilities was called by Shockley for March 22, 1966. A possible material under discussion was a film of manganese bismuth, MnBi. H. J. Williams and R. C. Sherwood of the Bell Laboratories research area were expert on this material and were asked to join the conference.

3.8.2 *The Scientific Background*

When they think about magnetism, many people recall that a magnet will pick up objects made of iron or steel but has no effect on other metals such as copper, aluminum, or brass. Indeed iron, and its neighbors in the periodic table—cobalt and nickel—are the principal magnetic metals. Oxides of these metals, however, are also often magnetic, and a favorite example is magnetite, or lodestone, which is one of the oxides of iron, Fe_3O_4, and was well known in ancient times. Magnetic oxides, in contrast to the metals, are often excellent insulators and in thin sections can be quite transparent to light. Modern research on ferromagnetic insulators begins with the work of J. Snoek in the Netherlands in the early 1930's. After working in secret during the years of German occupation, Snoek announced in 1947[35] that he could make a ferrite ferromagnet with high resistivity. Ferrites are oxides involving two metals such as iron and nickel; they are analogous to Fe_3O_4 and have the type formula $NiFe_2O_4$. Because of their high resistivity, ferrites can be used to make transformers that operate at frequencies up to 10^8 Hz. Such transformers are now vitally important in electronic circuits used for communications and television.

The ferrites accordingly were studied intensively after the war in many laboratories in the United States. At Bell Laboratories a long series of experiments was carried out to increase understanding of magnetic properties in terms of the domain picture. Williams took up work on this topic at the suggestion of Shockley shortly after World War II. Working with Shockley,[36] he performed the classic experiment demonstrating that the domain theory gave a quantitatively accurate description of the magnetization process. Williams perfected many of the techniques used in domain observation and over the years applied them to hundreds of magnetic samples. With co-workers he studied the motion of domain walls in single crystals of silicon iron.[37] Domain wall motion experiments in single crystals of nickel ferrite were performed by Galt beginning in 1952[38] and in manganese ferrite by Dillon in 1956.[39]

The microwave properties of the ferrites also became of great importance in about 1952 because of their potential application to microwave communication systems and advanced radar systems.[40] Fundamental work on gyromagnetic resonance, a phenomenon in which the magnetization of a crystal is made to precess at microwave frequencies about an applied magnetic field, began in many university, industrial, and government research laboratories. Such experiments were first reported by Griffiths[41] in England in 1946. In 1947, Yager and Bozorth[42] reported the observation of gyromagnetic resonance in a nickel-iron ferrite; in 1954, Galt *et al.*[43] reported the temperature dependence of ferromagnetic resonance line width in the same material; and Dillon, Geschwind, and Jaccarino[44] published their work on gyromagnetic resonance of a manganese iron ferrite in 1955.

A major advance occurred in 1956 and 1957 with the independent discovery of ferromagnetic garnets at Bell Laboratories by Geller and Gilleo[45] and in Grenoble, France, by Bertaut and Forrat.[46] The ferromagnetic garnets have the same crystal structure as gem garnets. They are oxides of iron and a rare-earth metal such as yttrium. The best known example is yttrium iron garnet, $Y_3 Fe_5 O_{12}$, usually designated YIG. Nielsen was able to grow good single crystals of these compounds,[47] and they were quickly under intensive study by Dillon[48] in gyromagnetic resonance experiments. The crystal growth process also produced large perfect crystals of another related oxide, yttrium orthoferrite ($YFeO_3$). These were weakly magnetic, with highly anisotropic magnetic properties, and were generally considered then to be of little technological interest compared to the garnets.

The essential experimental parameters of a gyromagnetic resonance experiment are the microwave frequency and bandwidth that will cause the magnetization of a crystal to precess. The fundamental understanding of these parameters began with a paper by Kittel[49] in 1947 on the frequency of resonance. During the next decade a series of significant theoretical advances were reported at the annual National Conference on Magnetism and Magnetic Materials. The work reported at this interdisciplinary conference eventually grew into a sophisticated understanding of the high-frequency behavior of the ferromagnetic oxides. Based on this theoretical background, the suggestion was soon made that gyromagnetic resonance experiments should be conducted with thin disks of YIG instead of the small spheres previously considered necessary. Such experiments, performed by Dillon in 1957,[50, 51] led to the unexpected discovery that thin sections of the garnet crystals were transparent. If the sample was illuminated with linearly polarized light, interaction with the magnetization produced a rotation of the axis of polarization. Since the rotation varied with the direction of magnetization, it was easy to see the way in which the direction of magnetization varied within the crystal; that is, it was pos-

sible to see the domain structure. The visibility of domain structures within crystals of this important material excited wide interest. In particular, Williams, Sherwood, and Remeika[52] found similar transparency in a number of materials, including the rare-earth orthoferrites mentioned above. Since these crystals were so large and perfect, a great deal of research was done on their domain structure and on the basic magnetic properties of the crystals that led to the observed domains.

As mentioned above, work on orthoferrites was only part of the research on domains performed by Williams and Sherwood. In 1957,[53] they made observations on domains in thin films of the intermetallic compound manganese bismuth. In 1958, Sherwood, Remeika, and Williams,[54] reported on the characteristics of domains seen in single crystals of magnetoplumbite, an iron oxide containing lead. In these crystals, magnetic bubbles were seen, but their possible technological significance was not then appreciated.

3.8.3 Bubble Conference

We have now outlined the background of the conference convened in March 1966 by Shockley. Scoville, Gianola, Bobeck, Hagerdorn, and their co-workers were in search of a material better than permalloy to use with the waffle-iron structure. Williams and Sherwood were present because of their research on domains in manganese–bismuth films, which were a possible contender. As the meeting proceeded, the ideal magnetic properties of a material for a memory consisting of reversed domains that could be stored and moved around in two dimensions on a surface were listed. Sherwood pointed out that this list described exactly the properties of the crystals of rare-earth orthoferrite on which he and Williams had been working for several years. Sherwood was able to supply Bobeck with suitable orthoferrite crystals almost immediately. In the next few weeks domains were observed and moved around on the waffle-iron structure, and in a few months stable bubbles were found and were used to make simple devices. Since 1966, the technology has developed rapidly, and bubble-domain devices are nearing practical application. Curiously, crystals of the magnetic garnets are now considered to be the most interesting materials for bubble devices. The promise of magnetic bubble technology to replace slower, bulkier, and more expensive devices such as core memories and disk files seems bright.

This story illustrates an important characteristic of research in solid-state physics and the advancement of new technologies. Very often the essential science and the technology develop completely independently. In this case, the science began with Snoek's work in 1946 and during the next 20 years was intensively pursued in the United States, particularly

in industrial and government research laboratories, but with much essential basic work being done in the universities. Usually this work was not conducted with specific applications in mind but was motivated by an effort to understand the physics of magnetism in a fundamental way. The technological requirement for movable magnetic domains came into focus separately. The engineers and inventors finally knew just what they needed to make a practical device and asked the right questions of the research scientists. As happens encouragingly often when science is flourishing, the answers were available, and an important forward step resulted.

3.9   SOLID-STATE PHYSICS AND THE LASER

3.9.1   *History*

A laser is a source of light that has several unusual and technologically important properties. The light is highly directional; it is monochromatic, that is, of a single pure color; and in many cases it can be generated in intense, short pulses—pulses as short as 1 psec (a millionth of a millionth, or 10^{-12}, of a second) and as intense as 1 TW (a million million, or $10^{1.2}$, watts).

In the process of developing microwave radar for use in World War II, a generation of physicists also developed the principles of generation and transmission of microwave radiation—invisible radiation much higher in frequency than that used for television but much lower than visible or infrared radiation. This microwave radiation was monochromatic; that is, it had a sharp frequency, like low-frequency radio and television sources but unlike them known visible light sources. The frequency could be tuned to coincide or resonate with the natural vibrational or rotational frequencies of molecules or of tiny atomic magnetic tops precessing about an applied magnetic field.

Some of these scientists, moving to universities, used their microwave radar tools to establish a new branch of spectroscopy, appropriately known as microwave spectroscopy, to study these molecular motions. However, they also speculated on whether these same molecular vibrations, rotations, and precessions could be harnessed together to emit coherent, monochromatic microwave radiation. In 1954, one of them, C. H. Townes, invented the maser, which was a *m*olecular, microwave oscillator and *a*mplifier, operating on the principle of *s*timulated *e*mission of *r*adiation set forth by the academic scientist Albert Einstein in 1917.

In 1958, Townes and A. L. Schawlow proposed the principles by which this molecular oscillator could harness the higher frequency electronic oscillations of atoms and generate coherent visible light. This work was undertaken in both industrial and academic laboratories, the latter with

federal financing. The first working laser, which emitted flashes of coherent red light, and which used a ruby crystal as the collection of atomic oscillators, was constructed in an industrial laboratory (Hughes Aircraft) and was substantially financed by defense contracts. (*L* for *l*ight in laser replaces *m* in maser.) This development occurred simultaneously and independently in the Soviet Union.

Lasers can be made from many kinds of systems. In their earliest form they depended on excited atoms in a gas or on excited impurity atoms in a transparent crystal or a glass. In the latter, the energy levels of the impurity atom are altered by its environment in ways that are generally understood from many years of basic investigations in the spectroscopy of solids. This subject, which had become a rather slow-moving field, acquired new interest with the advent of the laser. Semiconductor lasers, which depend on the properties of a P–N junction in a semiconductor, were easily achieved because of the broad background of knowledge of electronic behavior of semiconductors and the sophisticated technology that had been developed for producing semiconductor devices. (See also Section 3.12.) Although not all lasers have a close relationship to the physics of condensed matter, most varieties do, particularly the crystal and glass lasers, the semiconductor injection laser, the solid and liquid organic dye laser, and various other liquid lasers.

This brief history illustrates the intertwined roles of industrial and federally supported military and academic research during an interval of 20 years. However, early basic research directed solely toward understanding the interaction of radiation and matter provided the framework on which to construct practical devices.

3.9.2 *Early Development*

What were the early forecasts and motivations for the research in lasers? The primary motivation in basic research was the widely held belief (since fully justified) that the availability of coherent light sources would open new fields of research in physics and chemistry, even though in 1960 it was not clear what they would be. On the other hand, the only pre-1960 technological motivation appears to have related to communications. Interestingly, although considerable effort has gone into laser communications, this field is still far from the stage of practical application. The experimental achievement of kilowatts of pulsed power from the first ruby lasers enabled the forecasters of 1961 to extend the list of possible practical applications to include "accelerating chemical reactions. . .intense local heating, melting, vaporizing, and welding. . .eye surgery. . .microengraving to produce complex semiconductor structures. . ." (from A. L. Schawlow's

article in the June 1961, *Scientific American*). An experimental military laser range finder had already been built, and laser ranging measurements of the moon have been planned. Thus interactions with military and space technology were already evident. However, the most likely applications were still believed to be in communications: "Message carrying [is] the most obvious use and the one receiving the most technological attention. . ." (quoting again from Schawlow, who also noted the necessity of using light pipes for long-distance terrestrial laser communications).

By the end of 1961, harmonic generation (frequency doubling) had been achieved, at an efficiency of a few millionths of a percent, and the field of nonlinear optics was initiated. A number of other scientific applications were also proposed—for example, to astrophysics, spectroscopy, and various precision measurement, some of which involved tests of relativity theory.

3.9.3   Present Status of Research

Early predictions about the future of lasers had little to say about the improvements in the properties of lasers that were to be achieved within one decade. First, the available power output from lasers increased tremendously, roughly by a factor of 100 every three years. This rate of increase held for both continuously operating and pulsed lasers, as shown in Figure IV.1. Thus the peak output powers increased from 10^7 W in 1962 to over 10^{12} W in 1970, while the available continuous power increased from 0.1 W to 10^4 W. These improvements were achieved partly by the discovery of new lasers and partly from improved materials technology. Major improvements in peak output powers resulted, however, from the discovery of new principles of laser operation that could "time compress" millisecond-long pulses of input energy first to nanosecond (10^{-9} sec) "giant pulse" bursts of energy and then to still shorter picosecond (10^{-12} sec) "mode-locked" pulses. These new techniques were discovered, respectively, in 1962 and 1966.

Other discoveries broadened the range of laser properties in other ways. Laser oscillators have been built at numerous frequencies spanning the entire spectrum from microwaves to the ultraviolet. Some of these, particularly the organic dye laser discovered in 1966, can be tuned by as much as 20 percent around their center frequency. Semiconductor lasers, of which gallium arsenide (discovered in 1962) is the first and best example, are small, efficient, low-powered, and more rapidly modulated than other lasers. Most of these laser advances were made by industrial research teams, some financially supported by industry and some by government.

It is now abundantly clear that the laser has greatly stimulated research
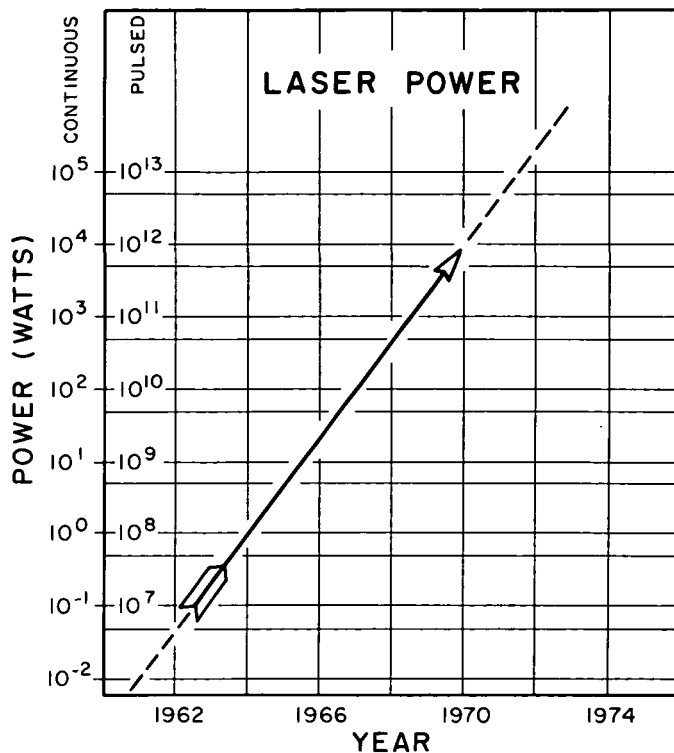
FIGURE IV.1   Laser power versus year.

in many branches of physics and optics. A whole range of phenomena belonging to nonlinear optics has been discovered. These include optical harmonic generation and parametric interaction and stimulated light scattering from phonons, magnons, plasmons, thermal fluctuations, free carriers, and the like, as well as self-focusing of light in liquids and solids. Another nonlinear effect of interest is self-induced transparency. In this effect suitable laser pulses make some materials, which are opaque to weak incoherent radiation, transparent to the coherent laser pulses.

All forms of spectroscopy (including Raman and Brillouin) have received new emphasis by the availability of lasers. It is now possible to study the vibrational spectra of electronically excited states of atoms and molecules, which promise to be of great utility in chemical physics.

The use of laser light scattering as a tool for studying critical fluctuations near phase transitions is another example of the impact of lasers on physics research.

In an entirely different area, lasers are being used in high-energy and atomic-energy research. The intense focused power in picosecond laser pulses has been used to generate tiny nuclear fusion plasmas. Laser interferometers are used to measure plasma parameters. A laser alignment device was the only one precise enough to enable construction of the Stanford superconducting linear accelerator, which is two miles long.

3.9.4   *Present Status of Applications*

Laser range finders, illuminators, and laser-guided bombs are proven military applications, many of which use improved versions of the original ruby laser. In some cases these laser devices displace earlier microwave devices; in some cases they supplement them. The laser devices have higher resolution, with smaller size and less weight, than their microwave competitors, but they are not useful when the visibility is poor. Their role in military applications will continue to grow.

Welding, drilling holes, cutting through refractory material, and microengraving are obvious applications in materials processing of the intense power that a laser can deliver to a focused spot. Either high continuous power or high pulsed power is used for different applications ranging from drilling holes in rubies in the Swiss watch industry to scribing ceramic wafers for integrated circuits. These straightforward laser applications will continue to increase. Further, some more complex operations are emerging.

Surveying and automatic control of machine tools are the current major examples of laser applications in metrology. The laser offers fundamental advantages over previous techniques, both in the high directionality of the radiation and in the ease of measuring distances to a fraction of a wavelength by interferometric techniques. The surveying and alignment uses extend from routine laying of drainage pipes to large-distance mapmaking on earth and precise astronomical measurements of point-to-point earth–moon distances. Laser radar reflections from the corner reflector placed on the lunar Sea of Tranquility by the astronauts of Apollo 11 are used in these measurements. Metrology applications undoubtedly will grow and increase in diversity. More complex measurements employ holographic techniques.

Nonlinear optics is the technique by which light beams of two different frequencies interact in a suitable transparent material to produce sum and difference frequencies, much as lower-frequency radio waves interact in vacuum tubes or crystal detectors to produce sum and difference frequencies. These and similar processes are the basis of radio, television, and telephone communication links that use many different frequency bands

generated and shifted about by various heterodyning and frequency-addition techniques. Not only can two light beams interact but so can a light beam and a sound beam or a light beam and molecular vibrations. (The latter effects are known as Brillouin and Raman effects, named after their academic discoverers in prelaser days when the effects were weak. Now the effects are strong and potentially useful.)

All the above effects can be used to modulate light, that is, to impose information on a laser beam used for communications purposes. Ultimately, they will be so used, since the amount of information that can be put on a red optical beam of, say, 5×10^{14} Hz frequency is 10,000 times that that can be put on a 6-mm-wavelength microwave beam of 5×10^{10} Hz frequency. The potential information content is directly proportional to the carrier frequency.

While predating lasers as a discovery, holography in its practical aspects is also a laser-born technology, a 1962 outgrowth of government-sponsored military research undertaken largely at the University of Michigan. Its emergence was in part a spinoff of the development of side-looking radar, but it has much broader real and potential applications.

A hologram is a photographic recording of interference patterns between two laser beams, one derived from a reference source and one from the object being recorded. One property of such an interferometric recording (which requires the monochromaticity of a laser beam) is that a three-dimensional image of a three-dimensional object can be reconstructed by shining a second laser beam on the hologram. A second property is that high-resolution detail in a two-dimensional object (say a field of recorded dots) can be spread over the entire area of the hologram. Hence, if the hologram is scratched, the reconstructed image only loses a little resolution, rather than losing individual dots. This feature is important for data storage.

Storing three-dimensional images on two-dimensional surfaces has scientific measurement uses. For example, a large field of view such as a water tank full of swimming microorganisms can be recorded in one laser flash and then examined later at varying depths of focus. If images can be followed in real time, the prospect of three-dimensional home television or commercial three-dimensional movies emerges.

3.9.5 *Extrapolations and Summary*

It is difficult to imagine extrapolating Figure IV.1 by many more factors of 100—but actually it is conservative; 10^{13} W, not 10^{12}, have already been achieved. Frequency extrapolations are rather unpredictable, but predictions of x-ray lasers that formerly were almost inconceivable seem now at least possible. If such lasers are achieved, they will surely have applications

qualitatively different from those already in existence and will also quantitatively improve some present applications. For example, the resolution of fine detail in fabrication and measurement techniques increases as the wavelength becomes smaller. Furthermore, the usefulness and range of the laser as a tool of science undoubtedly will continue to expand. New fields of research will emerge, and new applications will be found in the continuing interaction of physics with technology.

3.10 GUNN EFFECT AND AVALANCHE-TRANSIT TIME DEVICES

The generation and amplification of electromagnetic radiation at microwave and millimeter-wave frequencies are of tremendous importance in communication and radar. Although at lower frequencies the transistor had long since replaced the vacuum tube, until quite recently there were no signs of such a revolution at these high frequencies. In the last few years, however, two new discoveries have changed all this by providing new classes of semiconductor devices, which promise an enormous reduction in the complexity of microwave communication and radar equipment. The simplifications resulting from the use of solid-state components will provide the basis for less costly, more reliable microwave systems, which will then find applications in many fields in which the cost of vacuum tube equipment would be prohibitive. Among such increased applications are miniature radar systems for use in automotive collision–avoidance systems, intrusion alarms, and various other sorts of microwave monitoring and sensing and microwave communication equipment.

The new discoveries that provide the basis for these devices are (a) the Gunn effect—bulk negative differential conductivity in gallium arsenide—and (b) avalanche–transit time oscillations in junction diodes. We will trace the history of these two effects, their relation to basic research in the properties of semiconductors, and the way in which basic research, discovery, and applied research have led to the commercial availability of microwave oscillators using these new effects and to the promise of additional classes of practical high-speed devices.

3.10.1 *The Gunn Effect*

The history of the search for bulk, active behavior in a semiconductor in the microwave region of the spectrum is fairly long, and several avenues in this search have been tried without success. However, the discovery of the Gunn effect came as the result of a more fundamental study of the

properties of semiconductors rather than as a result of research directly aimed at producing bulk negative differential conductivity.

The discovery in the 1950's that many binary compounds of elements from groups III and V are semiconductors greatly enlarged the class of materials that could be used in transistors and other semiconductor devices. Among these materials, gallium arsenide was particularly interesting because it had a high electron mobility and a relatively large bandgap, which indicated possible superiority as a transistor material at high temperatures compared with germanium or silicon. Because of this potential, considerable progress had been made in producing relatively pure material in single-crystal form. As with other semiconductors, the physical properties of gallium arsenide, which are essential to understanding device behavior, were being studied in many laboratories. The Gunn effect was discovered in 1963 in the course of studies of the transport properties of "hot" electrons in gallium arsenide (and indium phosphide) in a strong electric field. (The term "hot" electrons is derived from the fact that the average electron energy in the strong field is many times its thermal equilibrium value.) Instead of a steady current flowing with constant voltage applied to the samples, the current oscillated coherently at a very high frequency. In a subsequent series of experiments, Gunn demonstrated that the effect was due to the periodic nucleation, near the cathode end of the sample, of high-field regions that subsequently traverse the sample and collapse at the anode. In addition, Gunn's early work suggested many of the potential device applications. The relation of the current oscillations to the bulk negative differential conductivity (BNDC) and the relation of the BNDC to the electronic band structure and scattering mechanisms in GaAs and InP were soon clarified both theoretically and experimentally. In addition, the various ways in which the BNDC could be used to provide amplification as well as oscillation and other modes of oscillator applications such as the limited-space-charge accumulation (LSA) mode were soon covered.

The potential usefulness of devices employing the Gunn effect in replacing complex microwave vacuum tubes provided the impetus both for the design of practical, continuously operating devices and for intensive efforts to produce extremely pure single-crystal gallium arsenide both in bulk and epitaxial form. The advent of control over the growth and doping of gallium arsenide epitaxial layers resulted in the late 1960's in the commercial availability of Gunn effect microwave oscillators.

Subsequently, intensive efforts aimed at understanding the causes of BNDC led to the discovery of the Gunn effect in about a dozen different materials, some of which became technologically important. In addition, research on some of the more subtle consequences of BNDC has provided potentially a whole new class of devices—logical devices, amplifiers, and

light deflectors, to mention a few that derive their extremely high operating speed from the existence of the B N D C . Although none of these applications has so far been successfully exploited commercially, practical applications of the Gunn effect probably will not be limited to microwave oscillations.

3.10.2   *Avalanche–Transit Time Oscillators*

The possibility of producing a semiconductor microwave oscillator based on the production of electron–hole pairs in the high electric field of a reverse biased P–N junction and the subsequent transport of the excess carrier across a resistive region to the contacts were first proposed by Read in 1958. As with the understanding of the Gunn effect, Read's analysis draws heavily on an understanding of the physics of "hot" electrons and holes. The electrons (or holes) that produce the excess pairs in the junction must have an energy of the order of 1 eV. The subsequent transport processes take place in electric fields strong enough to nearly saturate the carrier drift velocities. Both of these processes, impact ioniza-tion and velocity saturation, were understood in considerable detail in silicon and germanium at the time, but formidable obstacles to the construc-tion of a workable device remained. It was not until 1965 that a prototype silicon device with uniform properties was successfully operated. Again, the limits of operation were rapidly expanded and fabrication problems overcome. Pulsed oscillators of this type have now been operated at wavelengths less than 1 mm, and at lower frequencies high-power, high-efficiency oscillators of several types—I M P A T T (*IMP*act ionization Avalanche-*T*ransit *T*ime), Read diodes, and T R A P A T T (*TRA*pped *P*lasma *A*valanche-*T*riggered *T*ransit)—have been demonstrated using silicon, germa-nium, and gallium arsenide. Theoretical studies of the various modes of operation, in many cases using computer simulation techniques to solve the nonlinear dynamics, have kept pace with the experimental progress, and avalanche-transit time oscillators are now commercially available.

3.10.3   *Conclusion*

The Gunn effect and the avalanche–transit time oscillations are reasonably direct outgrowths of fundamental studies of the behavior of the charge car-riers in a semiconductor in a strong electric field, that is to say, of the trans-port properties of "hot" electrons and holes. The studies of two important aspects of this problem—the nonlinear dependence of carrier drift velocities on electric fields and the dynamics of the pair production in extremely strong electric fields—began in the early 1950's and would be considered relatively basic research. Although its rapid exploitation and development

may tend to obscure the fact, the discovery of the Gunn effect is a direct by-product of such research, which, in addition, provided the environment and understanding essential to the conception and successful demonstration of the avalanche–transit time oscillator.

3.11   SUPERCONDUCTIVE ELECTRON TUNNELING AND COMPUTERS

As the performance of computers is improved by advances in semiconductor technology, a number of problems become increasingly serious. One of the most important of these is the problem of removing the heat generated during the operation of the switching elements. One approach to the solution of this problem has evolved from the study of superconductive tunnel junctions at helium temperatures. These tunnel junctions can outperform semiconductor devices and, perhaps more importantly, totally dissipate negligible amounts of power while doing so.

Although no machines based on this new technology have been constructed, the prospects for their development appear bright at the moment, if a continuation of the present rate of progress toward solutions of certain difficult fabrication and reliability problems is assumed. It should be possible to build computers of ultrahigh performance and very large memory capacity. The origins of this application are very recent. We will briefly outline its development, concentrating on the major elements from which it evolved, that is, the concept of electron tunneling and the science of superconductivity.

The idea that an electron can pass from one allowed region to another through a barrier or unallowed region is as old as quantum mechanics. The theory of electron tunneling was well developed by 1940, and predictions of tunneling through thin insulating films between metals and semiconductors were made. However, almost no experiments were performed because of the technological difficulties of making a tunnel junction. By the 1950's, materials technology was sufficiently advanced to permit tunnelling experiments in solids. A milestone that spurred the entire field was the invention of the tunnel diode by Esaki in 1957.

Superconductive tunneling began with the work of Giaever and Fisher in 1960. They were interested in verifying the basic tunneling idea, that an electron can pass from one metal to another through an insulating layer, provided that this layer is sufficiently thin, 50 Å or so. To do this, they developed a technique for tunnel-junction fabrication that is still in use today. A metal film is evaporated in vacuum onto a suitable substrate. The fresh metal surface is oxidized to form a thin insulator. Another

metal film is evaporated over the oxide to form a sandwich. By applying a voltage across the oxide they found that a current would flow. The current was linear in voltage and, indeed, had all the attributes one would expect of a tunneling current. However, their results were not unambiguous, for other current mechanisms have similar attributes, in particular, a linear dependence of current on applied voltage. To demonstrate tunneling unambiguously, Giaever decided to use superconductors as the metals in his sandwich.

Superconductivity is an old (1911) phenomenon that had defied explanation until 1957, when Bardeen, Cooper, and Schrieffer, of the University of Illinois, proposed their now-famous BCS theory. (See also Section 3.7.) One of the predictions of this theory is that there is a gap in energy, or voltage range, over which there are no electrons as long as the metal is superconducting. Giaever reasoned that there could be a range of voltage over which no current would flow in the sandwich, if the current were due to tunneling electrons. His expectation and the tunneling mechanism were fully confirmed. The energy gap was clearly visible in the dependence of current on voltage; no current would flow through the junction until the gap voltage was reached, but then the current increased rapidly. Because the energy gap is a very important feature of the BCS theory and electron tunneling reveals it clearly, tunneling played a major role in the confirmation of the BCS theory.

Giaever was able to explain his results by treating the two superconductors as being completely independent of one another, except for exchanging electrons. This explanation seemed theoretically puzzling because of the so-called coherence factors, which play a very strong role in calculation of transition probabilities between two superconducting electron states. The issue appeared to be resolved by a calculation of Bardeen in 1961, in which he showed how the coherence factors drop out of the calculation.

The mistaken impression arose that the superconductors comprising the junction should always be treated as independent bodies. That this is not always so was pointed out in 1962 by a young graduate student at Cambridge, Brian Josephson. Indeed, he showed that this occasional lack of independence has spectacular results. He predicted that whenever the insulating oxide is sufficiently thin, say, 20 Å, a zero-voltage current of limited magnitude could flow through the tunnel junction; that the magnitude of this current would be very sensitive to magnetic fields, in fact it should periodically go to zero; and that, if a direct current voltage bias were imposed across the oxide, a high-frequency oscillating current would flow in the circuit, the frequency of which is proportional to the direct-current voltage. These predictions were quickly confirmed by experiments.

Anderson and Rowell demonstrated the existence of the magnetic-field-sensitive, zero-voltage current in 1963, and Shapiro showed the existence of the oscillating current, also in 1963. Subsequently, many experiments were performed to investigate the nonlinearities inherent in the Josephson effect. Many of these have led to practical laboratory devices as well as to measurements of the fundamental constant of physics (see also Section 4.1).

A Josephson tunnel junction has a current–voltage characteristic that shows magnetic-field-sensitive current at zero voltage but almost no current until the gap voltage. That this constitutes a two-state device, usable as a logic and memory element, was already noted by Rowell in 1964, but the idea was not seriously pursued until Matisoo demonstrated in 1965 that the junction switches from one state ($V = 0$) to the other (V = gap voltage) very rapidly. A year later high-speed logic operation was demonstrated.

Josephson had done his work well, in that the equations that he wrote were found to need little modification as experiment after experiment confirmed all the predictions. Today, the Josephson tunnel junction is a well-understood device, and the emphasis has shifted significantly from attempts to understand and confirm to the development of the technology necessary to fabricate reproducibly stable 20-Å oxide layers. Large strides in this direction have been made. The technology has already begun to feed back into physics. The study of the kinetics of oxidation in the previously somewhat uninteresting range of 20–50 Å has received considerable motivation. Also, the newly discovered ability to fabricate desired tunnel junctions repeatedly makes possible for the first time a thorough study of fundamental barrier problems in metal–insulator–metal tunneling, a field that even after 30 years still lacks quantitative confirmation of existing models.

3.12   REVERSIBLE-BEAM-ADDRESSABLE MEMORIES

In contrast to the example of a laser, which is a single fundamental invention offering many applications, a beam-addressable file is a single application drawing on many scientific and technological advances. It is also an application that has not yet been perfected.

The directions of progress in improving large, magnetic, off-line computer memories, also known as files, are always toward increasing the information content, lowering the cost per bit of stored information, and increasing the speed of access. Inevitably these objectives dictate packing the information ever more densely, and this, in turn, pushes the limits of conventional magnetic reading and writing technology in which the

mechanical motion of a recording surface (disk or tape), in close proximity to a tiny current loop (magnetic head), writes by means of reversing magnetized information bits and reads by sensing the flux of these bits.

For some time optical techniques have been considered a potential alternative or supplement to magnetic recording. Certainly, the density with which bits of information can be recorded photographically and sensed optically is much higher than the storage density in magnetic tapes and disks. However, photographically recorded information is permanent rather than reversible—"read only" rather than "read–write." Thus a search has been under way for optical phenomena with reversible recording characteristics. Photochromism is an example of such a reversible optical phenomenon, but it suffers from difficulties not found in magnetic recording.

In the last few years, a combined magnetic and optical technique has developed that promises to combine the good features of both technologies. The approach is to use laser optical techniques both to record and to read magnetically stored information. Hence, ideally, the stability of magnetic storage can be combined with the density of optical storage and freedom from mechanical wear by eliminating magnetic heads.

Development of this laser–magnetooptical-beam-addressable technology has required first the fundamental advance of the laser (see Section 3.9) and then a substantial advance in the understanding of magnetic and magnetooptic phenomena. Neither of these advances has proceeded so far that this beam addressable memory will clearly displace alternative technologies in the near future, but progress in both fields is highly encouraging.

The advent of the injection laser came from basic research in semiconductor physics. Results of sharp-line fluorescence in silicon and other semiconductors stimulated the interest of physicists, and many theoretical calculations concerning the possibility of observing laser action in semiconductors were performed in 1960. The actual marriage of laser and semiconductor technologies was accelerated by two developments in 1962. It was shown theoretically that direct bandgap semiconductors would make suitable materials for electrically pumped lasers, and the high-luminescence efficiency of the direct-gap material, GaAs, was measured. In November 1962, stimulated emission from forward-biased GaAs diodes was observed. By improvements in material, fabrication techniques, and cooling methods, cw (that is, continuous as opposed to pulsed) operation of GaAs was reported in 1963. In 1964, cw operation of the GaAs laser at 77 K, with a useful power output in excess of 1 W, was achieved. It is interesting to note that the first injection laser, GaAs, is still the most efficient one after nearly a decade of research.

In large measure, the useful magnetooptic effects have developed as by-

products of fundamental research on the properties of magnetic compounds. Spectroscopy has long been a most important tool in investigating the electronic structure of solids, and shifts in these spectra with magnetic fields (magnetooptic effects) help to elucidate the magnetic properties of these materials. Particular emphasis was placed initially on investigation of garnet-type ferrimagnetic materials, including gadolinium iron garnet (GdIG), whose easy direction of magnetization changes sign with temperature at a compensation point near room temperature, and on a class of rare-earth chalcogenides, including europium oxide (EuO), which is ferromagnetic below its Curie temperature of 69 K. The studies made on EuO revealed that the magnetism is due to indirect exchange through conduction electrons. By adding conduction electrons, the Curie temperature (and hence operating temperature) can be raised. It has been found that one can dope EuO and raise the Curie temperature to 138 K, which means that if can be used at temperatures above that of liquid nitrogen, which are readily available through the refrigeration technology evolved because of work related to space exploration. The investigation of these and similar materials was spurred by the interest in their fundamental properties.

At some point there arose the concept of initially uniformly magnetizing a thin sheet of one of these materials and holding it at a temperature near the compensation or Curie temperature, under a reversed magnetic field less than that needed to switch the magnetization. By locally heating tiny spots in this sheet with a laser beam, the temperature rises to a point at which the spot will switch in the weak bias field. Thus writing is accomplished and reading is done magnetooptically with a weaker laser beam.

The basic optical and magnetooptical research already performed on these materials provided a starting point from which it was possible to determine that they possessed reasonably favorable properties for the proposed device application. Other materials such as MnBi were also proposed. What was involved was a delicate tradeoff between adequate absorption of light in the thin material, to heat the spot locally to the desired temperature, adequate transmission, to prevent severe attenuation of the reading light, and an adequately strong magnetooptical effect (generally Faraday rotation of the plane of polarization of the reading light)—all of these in a film so thin that small magnetic bits of information would be magnetically stable. That is to say, the material must have a high-resolution capability. Preferably the film should be isotropic and polycrystalline, as in EuO. Also, preferably, the material should operate at room temperature, as does GdIG. The magnetooptic properties must be matched to the properties of the laser source, or vice versa.

At the present time, none of the laser–magnetooptical material com-

binations is ideal, but the combination of EuO with GaAs laser arrays is promising. Lower-than-room-temperature operation has the advantage of higher sensitivity to compensate for its disadvantages. Also, progress is being made in raising the Curie temperature of EuO, as mentioned earlier.

As presently envisaged, a complete high-capacity, laser-beam-address-able, magnetooptic file also encompasses elements to deflect the laser beams and some mechanical transport of the recording system. The latter is a conventional, though difficult, technology, but the former is also a field of current intense research and development in which im-provements will undoubtedly affect the characteristics of ultimate memory systems.

4   Interactions of the Physics of Condensed Matter with Other Branches of Physics and Other Sciences

The discovery and investigation of the microscopic interactions that pro-duce the recognizable macroscopic features of solids and liquids are among the principal responsibilities of physicists in the subfield of condensed matter. When the interactions become better understood, usually by the interplay between theory and experiment, they often lead to new con-cepts as well as to better materials, new devices, and improved methods of measurement and detection. Typical of these new concepts are minority carriers, quasi-particles, elementary excitations such as spin waves, dislo-cations, point defects, *F*-centers, superexchange, chemical shifts, and Fermi surfaces. The end result has been the enhancement of many quan-titative sciences.

Numerous indeed are the interactions and overlaps of condensed-matter physics with other subfields of physics and other sciences, and many ex-amples can be cited. The nuclei in solids are exposed to electric and mag-netic fields from different sources. Some of the techniques of nuclear physics, as well as resonance methods, can be used to probe the electric-field gradients and magnetic fields to which they are exposed. Organic chemists rely heavily on high-resolution proton resonance spectrometers. In addition to nuclear magnetic resonance (NMR), the Mössbauer effect, positron annihilation, and angular correlation studies of nuclear decays are used to give information about the charge distribution in chemical *bonds, about biologically active sites of molecules, and about surfaces*

with catalytic properties, as well as about the properties of nuclei themselves. It is worthwhile to note that the discovery of NMR was motivated by the need for making accurate magnetic-field measurements. In addition to its extensive use in chemistry, NMR is used in geology and surveying in which small changes in the earth's magnetic field are of significance.

What has been learned in the quantum-mechanical treatment of atomic and molecular systems can be used to understand the splitting of the energy levels of rare earth and transition metal ions, and thus in the study of magnetic insulators. The first observation of laser action was made possible by earlier studies of Cr ions in Al_2O_3. Continuous new developments are characteristic of the prolific interface of condensed-matter physics, applied optics, and materials science. Semiconducting lasers that operate continuously at room temperature, electroluminescent diodes, and fiber optics give promise of an optical integrated-circuit technology in the 1970's similar to the microwave integrated-circuit technology of the 1960's. Pulsed laser sources that can be used to make meaningful measurements on a picosecond time scale are giving new information about chemical reaction mechanisms and kinetics. Laser sources are used for Raman scattering to provide new insights into the chemical bond. In applied work, parametric generation of energy at new frequencies in the optical spectrum depends on nonlinearities and phase matching of the velocity of light in transparent crystals. The long-range order and macroscopic quantum effects that are found in superfluid helium and superconductors of electricity are providing insights into the theory of phase transitions and cooperative phenomena. Rather than merely listing the many areas of interaction between the physics of condensed matter and other sciences, we will discuss in some detail a few examples of especially active and productive research areas.

4.1 FUNDAMENTAL MEASUREMENTS

The study of Josephson effects in systems of weakly coupled superconductors has had far reaching implications (see Section 3.11). During the 1940's and 1950's, it became clear that the superconducting state is a collective phase-coherent, many-electron quantum state. In 1962, Josephson discovered theoretically a set of unique effects that can occur in a system composed of two superconducting bodies connected in such a way that there is a weak phase-coherent coupling of the superconducting state in the two bodies. One consequence of this coupling, called the alternating current (ac) Josephson effect, is an oscillating supercurrent that flows between the two superconductors when they are maintained at a potential

difference V. The frequency of this supercurrent is $\nu = 2eV/h$ (e is the magnitude of the charge on the electron; h is Planck's constant). A measurement of this frequency and of the potential difference thus yields the fundamental physical constant e/h. This method of obtaining e/h gives a result several orders of magnitude more accurate than any alternative experimental method. In combination with experimental determinations of other fundamental constants, it has yielded an improved value of the fine-structure constant. This value, in turn, has led to the resolution of a long-standing discrepancy in the quantum electrodynamic theory of the fine and hyperfine structure of the hydrogen atom. It also has helped to clarify significant discrepancies between experiment and the predictions of quantum electrodynamic theory for the Lamb shift in hydrogenic atoms and for the anomalous magnetic moment of the electron. (These discrepancies subsequently have been resolved.) This case is one in which an understanding of the superconducting state in solids has had significant implications for seemingly unrelated subfields of physics, quantum electrodynamics, and atomic physics.

This same development also has had a significant impact on another type of endeavor—the technology of precision electrical measurements. The experimental determination of e/h using the ac Josephson effect has now reached a stage at which its accuracy is limited solely by the accuracy with which the volt itself can be established and maintained. This immediately implies that the ac Josephson effect provides the basis for an atomic standard of voltage, with all the advantages and benefits that have accompanied the establishment of atomic standards of length and frequency. Programs aimed at the exploitation of this possibility are now under way in many of the national standards laboratories of the world. Other laboratories are investigating Josephson infrared generators and detectors, sensitive voltmeters, and gaussmeters.

4.2 MATERIALS SCIENCE

Useful materials are seldom as simple as single crystals. Rather, they are complex multiphase alloys, ceramics, glasses, or polymers, with useful mechanical, magnetic, or electrical properties.

For many years following the first application of quantum mechanics to solid materials, condensed-matter physicists concentrated on the simplest, most regular crystal systems and their theoretical idealization. The consequent need for pure materials stimulated materials scientists to develop a technology of pure crystal preparation. The resulting studies provided the underlying concepts and ideas from which real materials can be

understood in a systematic and fundamental way. The concept of disloca-
tions, their study in single crystals, and the production of dislocation-free
crystals, such as metal whiskers, a joint effort of physicists, metallurgists,
applied mathematicians, and others, have produced a revolution in the
understanding of the strength of materials. The quantitative understanding
of band structures and Fermi surfaces, which can explain some of the
properties of pure metallic elements and simple compounds, is now being
extended to include more complicated compounds. The covalent and
ionic character of the chemical bond is being related to the dielectric func-
tion, which in turn is rooted in the band structure.

Frequently the study of condensed-matter phenomena in relatively pure
single crystals can lead directly to useful new materials. The huge mag-
netic anisotropy found in $SmCo_5$ crystals has led to the construction of
the most powerful permanent magnets known. It must be realized that the
study of magnetism in solids has been active since the advent of quantum
mechanics. However, the concept of domain walls and their absence in
fine powders, the concept of ferrimagnetism and sublattice magnetization,
and the concept of indirect exchange had to emerge before a more power-
ful magnet could be found.

Materials science developed from the combined insights and techniques
of condensed-matter physics, chemistry, and metallurgy. Materials science
has the challenge of selecting from the many millions of possible systems
(including ternary and quaternary systems) those that might have particu-
larly useful characteristics if properly prepared. These include supercon-
ductors, which maintain their superconductivity at higher temperatures,
compounds with nonlinear optical response, magnetic materials, and a
variety of semiconductors in which the electronic band characteristics
determine useful properties such as luminescence or the generation of
high-frequency microwave radiation.

4.3 NUCLEAR PHYSICS

Superconductivity has been of importance to nuclear physics by providing
magnets for bubble chambers, particle analyzers, and other equipment.
The next generation of synchrotrons may well be superconducting.
High-Q niobium cavities that have been developed are now being used to
construct a superconducting linear accelerator of unprecedented beam in-
tensity at high energy.

Semiconductor physics has had an enormous impact on nuclear physics
through the development of semiconductor detectors of nuclear radiation.
Silicon and germanium have been exploited because, when excited by en-

ergetic radiation, they provide a large and well-defined number of hole-electron pairs. The number of pairs is proportional to the energy deposited by the quantum of radiation. Collection of the charge thus released gives an accurate and rapid measure of the energy of the radiation. To have this accurate response, the impurities and defects in the crystals must be very carefully controlled, to a degree that has only become possible with the general development of silicon and germanium technology in recent years. Large crystals provide the capability of detecting radiation of relatively high energy, and the technology of producing such large crystals is also a recent achievement. By slowly infusing part of the crystal with lithium ions under the influence of an electric field over a period of days or weeks, precise compensation of impurities and activation of the crystal over a large volume has been achieved. The conditions for introducing the lithium are much better understood because of fundamental solid-state investigations of the lithium–germanium and lithium–silicon systems made several years ago. These detectors combine high speed, compact size, and extraordinary energy resolution in a way that has opened entirely new realms of investigation in low-energy nuclear physics. Development has been revolutionary.

Lithium-drifted semiconducting particle detectors also have been the heart of many space-physics experiments designed to observe charged particles in the earth's magnetosphere and in interplanetary space. Furthermore, during the last three years, as the energy resolution of detectors, and their associated electronics, has continually improved, these devices have provided a new technique for x-ray detection. This technique is important for x-ray fluorescence used for nondestructive chemical analysis, as well as for further studies of basic phenomena in solid-state and atomic physics. Further advances in this technique are likely, since studies of other semiconductor materials and of new ways of obtaining high-purity germanium and silicon for detector applications continue.

The interplay between nuclear physics and the physics of condensed matter has been extremely important in the study of the hyperfine interaction of a nucleus with its atomic electrons in the restricted environment of a solid. Mössbauer effect and gamma-ray angular correlation studies provide information on either nuclear magnetic and quadrupole moments or on magnetic fields and electric-field gradients in the solid. The coupling of these two subfields has taken a new turn recently with the use of the ordered arrays of atoms in a single crystal for measurement of the lifetime of nuclear states in the 10^{-17} sec region. The measurements involve the blocking of emission of charged particles from nuclei along lattice rows in a solid. If a nucleus that has been struck by an energetic particle recoils from its lattice site before it emits a charged particle, the blocking effect

B

is reduced. The extent of the blocking effect thus serves as a measure of the recoil distance before decay and hence of nuclear lifetime. Blocking is the inverse of the process of channeling, which uses techniques of nuclear physics to provide valuable information on the location of impurity atoms in a crystal lattice. The use of beams of ions for the chemical doping of semiconductors (ion implantation) has great promise of developing into an important method for producing integrated circuits.

4.4 PLANETARY PHYSICS

A few years ago it was discovered that fission fragments that pass through a slab of mica, or other materials, leave tracks that can be revealed by a simple etching procedure. This finding has given rise to a number of interesting applications. Naturally occurring minerals have fission tracks that are stable for billions of years at temperatures below 600°C. In essence these minerals can act as "integrating bubble chambers" with a geological time scale. Measurement of the density of tracks in minerals containing uranium (half-life 10^{16} years) determines the age of the mineral from the time it was cooled below its annealing temperature (600°C). This procedure has obvious applications for studying the upwelling of material from below the earth's crust. Meteorites, because of their long exposure to cosmic rays, contain information about the early history of the solar system. Again, by the track process, microscopic pores, which are extremely uniform in size, can be produced in plastic membranes for biological purposes. For example, such membranes are being used to filter cancer cells from blood and other body fluids by taking advantage of the fact that cancer cells are slightly larger and more rigid than healthy cells.

There are many other ways in which condensed-matter physics is impinging on geophysics. In fact, our knowledge of the earth and its history is based to a large extent on solid-state physics and chemistry. The model for the composition of the mantle is based on the phase equilibria determined for different germanate systems that can then be extrapolated to the silicates, which comprise the earth's mantle. The most convincing evidence for continental drift comes from studies of the reversal of magnetic polarization of the ocean floor. All direct knowledge of the deep interior of the earth comes from acoustical measurements. The knowledge of the response of a solid to sound waves and studies of the equation of state of solids allows one to (a) hypothesize a liquid core, (b) set a lower limit of the temperature and pressure in the mantle, and (c) set some limits to the accuracy of the extrapolations that are made concerning the properties of materials in the earth.

4.5   SURFACE CHEMISTRY

The role of surfaces in determining physical and chemical behavior has
long been recognized, and an enormous effort has gone into studying sur-
faces both from the thermodynamic and atomistic points of view. In spite
of all this work, heterogeneous catalysis remains very much an empirical
field. Why effective catalytic action is often limited to metals of the scarce
and expensive platinum group remains unanswered. Condensed-matter
physicists have developed techniques of cleaving single crystals in very
high vacuum to produce clean and regular surfaces that can be microscopi-
cally characterized. Then, by means of low-energy electron diffraction
(LEED), two-dimensional order in a monatomic layer of adsorbed gas can
be studied. Recently, a new technique, ion-neutralization spectroscopy
(INS), has been developed that is sensitive to processes taking place as
close as 5 Å to the metal–vacuum boundary. The INS method uses infor-
mation extracted from the kinetic energy distributions of electrons ejected
from the surface by slowly moving noble-gas ions. The spectrum of the
ionization energies of electrons in bound states formed from the inter-
action of adsorbate atoms with the substrate atoms is measured. This
spectrum is important because, as for free molecules, it is intimately re-
lated to the local symmetry of the nuclear skeleton. The surface orbitals
form resonances by interaction with electrons of the solid, which broadens
them but not sufficiently to destroy their resemblance to molecular
spectra.

   In addition to the information contained in the form of the spectrum,
the intensity contains information concerning the local electrical charge
in the surface chemisorption site. Thus, INS tells about both the structure
of the bonds in surface molecules and their ionicity, supplementing in-
formation that can be obtained from other methods. It opens a new field
of the study of surface molecules, which can differ in interesting ways
from free molecules. Surface molecules are constrained structures, because
some atomic positions are dictated by the solid. They can be produced in
structures that are unknown among free molecules but that are likely to
be of prime importance in both corrosion and catalytic reactions.

4.6   ASTROPHYSICS

It is often rewarding to study matter under extreme conditions to appre-
ciate its behavior under normal conditions. A most notable example is the
liquefaction of helium by Kamerlingh Onnes 60 years ago to study equa-
tions of state and critical phenomena. His efforts led to the unexpected

discovery of quantum effects in microscopic matter—superfluid helium and superconducting electrons—that have enormously expanded the horizons of the physics of condensed matter (see Chapters 2 and 3). The range of applicability of the concepts of this subfield is illustrated by the fact that knowledge obtained from studies of the superfluids near the absolute zero of temperature is now being used to understand neutron stars, which are believed to contain the densest matter in the universe and to have interior temperatures of the order 10^8 K. To understand the properties of exotic matter in such regimes—so far from those accessible in laboratory experiments—physicists turn to the theory and observation of very cold condensed matter at normal densities. For neutron-star matter, when considered in terms of the phenomena that can occur in it, behaves as do certain kinds of ordinary matter near and below 1 K.

A neutron star has a mass comparable with that of the sun (about 2 × 10^{33} g cm^{-3}) but a radius of only 10 km. Nuclei exist in the outer kilometer or so, where the density is below 5 × 10^{13} g cm^{-3}. They arrange themselves in a body-centered cubic lattice, the melting temperature of which is calculated to exceed 10^{10} K at the highest density. Thus the outer layer of a neutron star forms a solid crust, with a temperature perhaps two orders of magnitude less than that at which it melts. The details of such matter, its strength, dislocations, creep rate, and the like, could play a crucial role in understanding neutron-star geology. Below the crust, the neutrons very probably form a superfluid, the behavior of which is analogous to that of liquid ^{4}He below 2.17 K. Coexisting with the neutrons are degenerate electrons and protons that number a few percent of the neutrons. The protons probably form a superconducting state similar to that of many low-temperature metals. Threading the entire star is a magnetic field of about 10^{12} G, which couples the highly conducting crust rather strongly to the interior electrons and protons. When this remarkable object rotates, it seems to be observable as a pulsar. Small sudden jumps in the angular velocity of the crust have been observed. The coupling of the crust to the dominant interior neutron superfluid is very weak; after a sudden change in angular velocity of the rotating crust it may possibly take years to re-establish a steady state with the core. The spin of the neutron star is not likely to be simpler than that of a container of superfluid helium, which is still an active area of research.

4.7   CURRENT ACTIVITY THAT OVERLAPS SEVERAL OTHER DISCIPLINES

X-ray diffraction, the discovery of which in 1912 is perhaps the most important date in the history of condensed-matter physics, continues to de-

velop and to be useful in a variety of disciplines—biology, chemistry, metallurgy, and ceramics, for example. New detectors that lend themselves to automatic data collecting and processing have enabled biochemists to solve structures of enormous complexity. The supplemental use of neutron diffraction, with its sensitivity to protons, other nuclei that cannot be readily distinguished by x-ray techniques, and magnetism, is also important. The information about elementary excitations in solids that is being gained by a widening capability for studying inelastic scattering of neutrons and of light probably will have implications for the work of materials scientists and engineers.

The study of matter under extreme conditions, which can be generated in the laboratory by a variety of techniques, has general usefulness. High-pressure physics has had an impact on chemistry, geophysics, and materials science. Studies of phase equilibrium in the carbon system were a necessary prerequisite to the synthesis of diamonds in the laboratory. Pressure has recently been found to change the oxidation state and spin state of iron in a variety of compounds, as well as the optical properties of molecular charge-transfer complexes. The electronic switching from metal to insulator (Mott transition) in transition metal oxides (V_2O_3) may be affected by pressure. Studies of the mechanical properties of solids and the increased ductility under pressure have resulted in the ability to perform metal-forming operations of economic importance that were previously impossible. Static pressures are now being produced in the laboratory up to several hundred kilobars, with simultaneous variation of temperature over a wide range. Dynamic methods (shock waves) are producing pressures into the multimegabar range and are being extensively used to study new phenomena and new kinds of matter that occur under these very extreme conditions.

A striking and everyday phenomenon peculiar to condensed matter and not previously mentioned is the existence of phase transitions sometimes called critical points. This research area overlaps a number of other disciplines—metallurgy, via order–disorder; physical chemistry via binary solution and distillations; liquid crystals; and perhaps ultimately biology, because of fibers and membranes. Theoretical and experimental understanding of critical-point phenomena in general have advanced markedly in the last few years.

In the next few years a new decade of the temperature scale, that between 0.1 and 1 m K, should become available for widespread experimentation because of the development of ^{3}He–^{4}He dilution refrigerators, superconducting magnets, and new materials such as rare-earth compounds that can be demagnetized to low temperatures without ordering, because the ground state of the ion is a singlet (for example, $PrTl_3$). Such low temperatures may provide sufficiently high signal-to-noise ratio to extend

astrophysical research. (At present the best detectors operate at "high" temperatures such as 0.3 K.) Gravity-wave detectors, with greatly improved sensitivity, operating at millidegree kelvin temperatures are already being planned.

5 The Economic and Social Impact of the Physics of Condensed Matter

The economic and social impact of the physics of condensed matter on life in the United States has been great. It has led to the creation of a series of major new technologies, based on research conducted 25 to 50 years ago (detailed examples are presented in Chapter 3). The growth and diversification of these technologies depended heavily on a continuing series of scientific and technical innovations. These technologies contribute to the satisfaction of many human needs such as communication, travel, medical care, education, and entertainment. As a result, industries based on solid-state technologies have grown rapidly and have provided new jobs and opportunities for investment and economic growth.

A broad range of research in solid-state physics, from the most basic to the most applied, has provided the technical framework for this rapid growth. Fundamental research suggests applications, while feedback from the more applied scientists and engineers strongly affects the direction of fundamental research. This interaction between basic and applied science has been traditional among solid-state scientists. It is one of the reasons that the subfield has produced so many useful results.

The economic impact of solid-state physics is both direct and indirect. The direct impact is found in those industries that produce solid-state devices and components, largely semiconducting and magnetic products. These industries contributed nearly $2 billion to the gross national product (GNP) in 1969. The indirect economic impact of solid-state physics falls roughly into two categories. First, many products and services today are dependent on solid-state devices to achieve their primary function. For example, large digital computers could not exist were it not for such devices. Similarly, communications systems, radio, hi-fi and television sets, air traffic control systems, and defense systems are totally dependent on solid-state devices to perform their intended functions. Second, an indirect impact of solid-state devices is apparent even in products that do not depend on such devices for their primary function. For example, semi-

conducting devices are beginning to be used in automobiles even though there is obviously no primary need for tl em. In such cases, solid-state devices perform auxiliary functions—such as antiskid protection—that are obviously desirable.

The devices and components directly dependent on solid-state materials research are thus the building blocks of larger subsystems and systems that fill various social needs. For example, semiconducting materials are required for transistors, which are used in the switching circuits that make possible modern telephone communications.

The leverage of the funds invested in solid-state research is particularly impressive. The total annual solid-state research budget represents less than 1 percent of the annual sales of solid-state-based technologies.

5.1 DIRECT ECONOMIC IMPACT

One direct economic impact of solid-state physics on the U.S. economy is the growth of the solid-state components industry, including such items as transistors, diodes, and ferrite devices. This industry developed directly from research in solid-state physics. Its sales reflect the direct value of the solid-state materials and technologies involved. Sales and purchases of large systems that use these components, such as television sets and computers, indicate the indirect economic value, leverage, and social impact of solid-state research.

It is convenient to consider the components arranged in categories according to their constituent materials. Semiconducting, magnetic, and superconducting components are prominent examples. These materials involve markets of very different sizes and illustrate two points: (a) the contribution of research at various positions on the basic-applied continuum and (b) different time lags between basic research and device production.

5.1.1 *Semiconductors*

Semiconductor devices are the most often recognized product of solid-state physics. Figure IV.2 shows the recent sales of various semiconductor components. This industry in 1970 had total sales of approximately $1.5 billion; by almost any standard it is a gigantic industry. Its growth during the last decade has been one of the most rapid of any major industry. This phenomenal growth depended critically on diversification based on continual research and development.

Oft-cited early work at the Bell Telephone Laboratories brought about an increased fundamental understanding of semiconductors (see Section
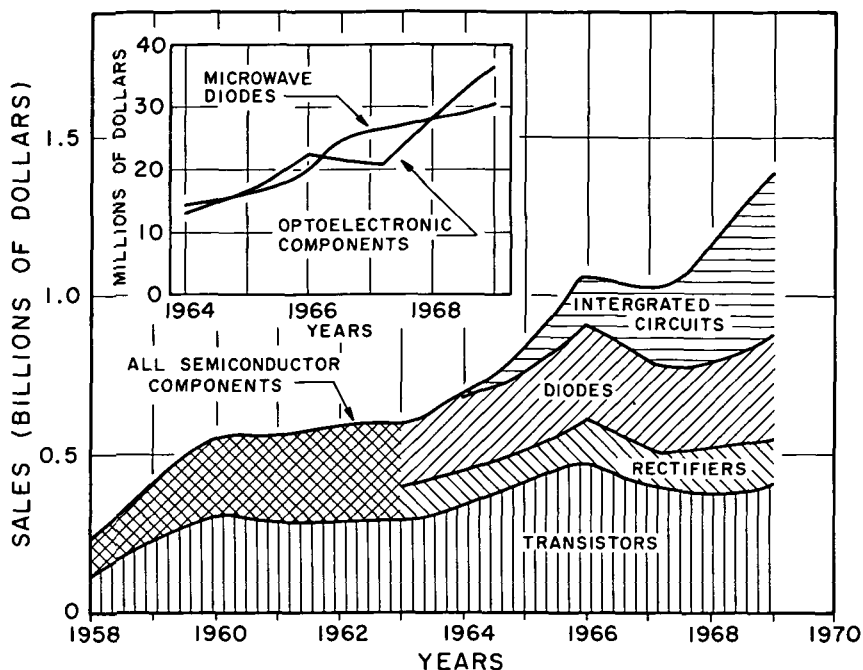
FIGURE IV.2   Sales of semiconductor components, microwave diodes, and opto-electronic components in the United States.

3.6). This new knowledge and understanding made possible the develop-ment of improved amplifying devices, the point contact and junction tran-sistors, during the late 1940's. The application of solid-state diffusion theory to the formation of doped regions in semiconductors during the 1950's and the increased understanding of the oxidized semiconductor surface obtained during the 1960's are examples of scientific investigations important to the growth of the industry. These investigations and the re-sulting innovations were partially motivated by the massive engineering effort required to develop integrated circuits. These small chips of silicon contain hundreds of thousands of electronic components and permit the construction of compact and inexpensive electronic systems. In the past five years (see Figure IV.2) they have accounted for sales of $0.5 billion per year, or about 30 percent of the total semiconductor market.

Recent innovations such as solid-state microwave diodes and electro-optical devices still constitute a relatively small section of the market but have great potential. The Gunn effect is a microwave oscillation phenom-enon that was originally observed in 1963 during a study of the funda-

mental properties of gallium arsenide (see Section 3.10). This effect has been exploited to generate a series of solid-state devices. The sales of all these devices (microwave diodes) are shown in the small insert on Figure IV.2. These devices probably will become widely used in communication and radar equipment in which conventional transistors are inadequate.

Optoelectronic devices such as photoconductors and light-emitting diodes are based on fundamental research conducted continuously during the past 20 years. Their sales, shown in the insert in Figure 4.2, are now only about $37 million per year but are growing rapidly. The current introduction of picturephones and sophisticated character-recognition equipment will stimulate further innovations in this area.

The semiconductor devices discussed thus far have contributed mainly to communications industries. Recent developments are beginning to have a similar major impact on the power industry. Rectifiers, invertors, and cycloconvertors—devices that transform electrical power from one mode to another—allow more flexible power transmission, distribution, and utilization. Solid-state devices are being used increasingly to control electrical motors and drives. The total market for these power devices is estimated to amount to over $1 billion in 1980.

5.1.2 *Magnets*

Components containing magnetic materials now account for about $0.7 billion per year in sales. About one third of these sales are of electrical steels used mainly in transformers. Although their characteristics have been steadily improved, their sales mainly reflect the general growth of the economy and advances in metallurgy.

However, the magnetic materials used by the electronics industry have been based largely on advances in solid-state physics. New systems, in particular television, computer, and radar, require new types of magnetic materials. Ferrites (ferrimagnetic insulators) are now used in television transformers and computer memories (see Section 3.8). Their sales are shown in Figure IV.3. The earliest research on such materials dates from the 1930's, but their widespread current use depends on more recent understanding of such phenomena as domain wall motion and the interaction of crystalline fields with magnetic moments.

Ferromagnetic memory devices, such as tapes, drums, and disks, were originally developed before World War II. Their use in computers has caused the recent dramatic increase in sales shown in Figure IV.3. Solid-state physics has contributed to this growth by the specification of the material parameters required for important improvement in reliability and density of information storage.
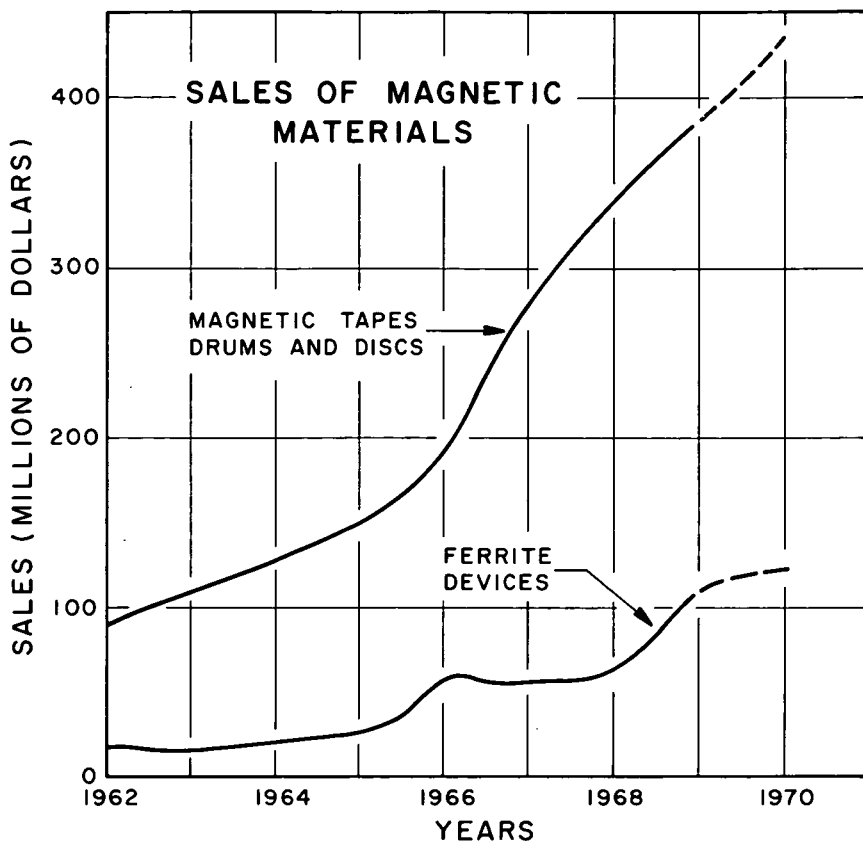
FIGURE IV.3   Sales of magnetic materials used by the electronics industry in the United States.

Recent investigations have emphasized the interrelationship between the magnetic and the optical and electrical properties of solids. For example, laser pulses have been used to change the direction of magnetization of a small spot on a film. Such work may have future impact on the design of display and information storage devices (see Section 3.12).

5.1.3   Superconductors

Superconducting materials have stimulated intense research during the past 15 years. Although some of the basic phenomena were observed much earlier, the recent activity has brought about a series of important fundamental advances. These include a basic theoretical explanation of

superconductivity and its experimental verification (see Sections 3.7 and 3.11).

In contrast to semiconductors and magnets, these materials have not as yet been responsible for major new products. The most important device, the high-field solenoid, accounts for only about $9 million of sales in 1970.

However, a variety of possible major applications of superconducting materials have been suggested and are being investigated, for example, power transmission lines, transformers, motors, and computer memories. Discovery of a way to increase the superconducting transition temperature would greatly increase the possible applications. If even one of these applications becomes important, the growth in the market for superconductors will be dramatic.

5.2 INDIRECT ECONOMIC IMPACT

The sales of electronic components provide an indication of the direct impact of solid-state science on the economy. The examples considered in the preceding section account for over $2 billion or approximately 0.2 percent of the GNP. However, their indirect economic impact is much larger.

The total sales of industries that rely on solid-state components for the construction of their products is one example of this leverage. Table IV.2 gives data from a government tabulation for 1963 of semiconductor sales to various industries and the total sales of these industries. Since 1963, the semiconductor contribution to these industries probably has increased. The sales of manufacturers of communications, computing, power, lighting, and other electrical apparatus accounted for approximately 5 percent of the 1963 GNP. Solid-state components are essential ingredients in such

TABLE IV.2   *Sales of Semiconductor Industry to Other Industries (1963)*

Industry	Semiconductor Sales to Industry ($Millions)	Total Sales of Industry ($Millions)	Semiconductor Sales to Industry as Percentage of Total Sales to Industry (%)
Computers	153.2	3,082.0	4.97
Electric lamps	17.0	618.0	2.75
Communications equipment	269.8	12,343.0	2.18
Transformers	17.4	782.0	2.22
Electronic and electrical equipment	64.9	4,112,0	1.57
Motor vehicles and aircraft	126.6	55,516.0	0.23

products. They sometimes substitute for less desirable elements, but they are often irreplaceable. These systems are also used by service industries, such as banking, telephone, and medical, which are thus indirectly affected by solid-state physics and which represent another sizable fraction of the GNP. Finally, solid-state science makes a smaller and more diffuse contribution to many other industries such as aircraft, appliances, and automobiles that do not depend on solid-state devices for their primary function. These industries typically use a few solid-state components in their products. They are very large industries, and even a small impact by solid-state physics has a pervasive effect on our economy. Moreover, the influence of solid-state physics on improved and novel structural materials, though difficult to document in detail, has been real and, through these industries, has had a positive impact on the economy.

5.3 SOCIAL IMPACT

The effect of solid-state science on U.S. society is only partially described by the economic effects discussed thus far. The nature of the goods produced and the needs that they fulfill are of even greater significance.

As the goals and aspirations of society evolve, the products and systems it uses vary. Solid-state science, however, continues to provide important technology for these new needs.

Figure IV.4 shows the purchases of solid-state-based systems during the past eight years by the federal, industrial-commercial, and consumer sectors of society. In the federal sector, defense is a major traditional activity. It accounts for approximately 85 percent of the federal expenditures. Much of the remainder consists of purchases of space exploration equipment, which have grown to $1 billion per year in the past 10 years. However, the total federal spending has remained relatively constant during this period.

Purchases of solid-state-based systems by the industrial-commercial sector have increased much more rapidly—by a factor of 3 or 4. These purchases consist mainly of measuring, control, communications, and computing equipment. These types of equipment make possible the characteristically high productivity of industry and the consequent low price and high volume of consumer goods—one small recent example is the use of lasers to sense the particle-size distribution in copper ore. A computer then determines further steps in the refining operation. The efficiency and yield of the process are thereby increased. Examples of this sort in industry and commerce are legion.

Consumer purchases of household electronic equipment, such as tele-

PURCHASES OF ELECTRONIC SYSTEM
(BILLIONS OF DOLLARS)

FEDERAL

INDUSTRIAL ➞

CONSUMER ➞

PURCHASE OF ELECTRONIC SYSTEMS
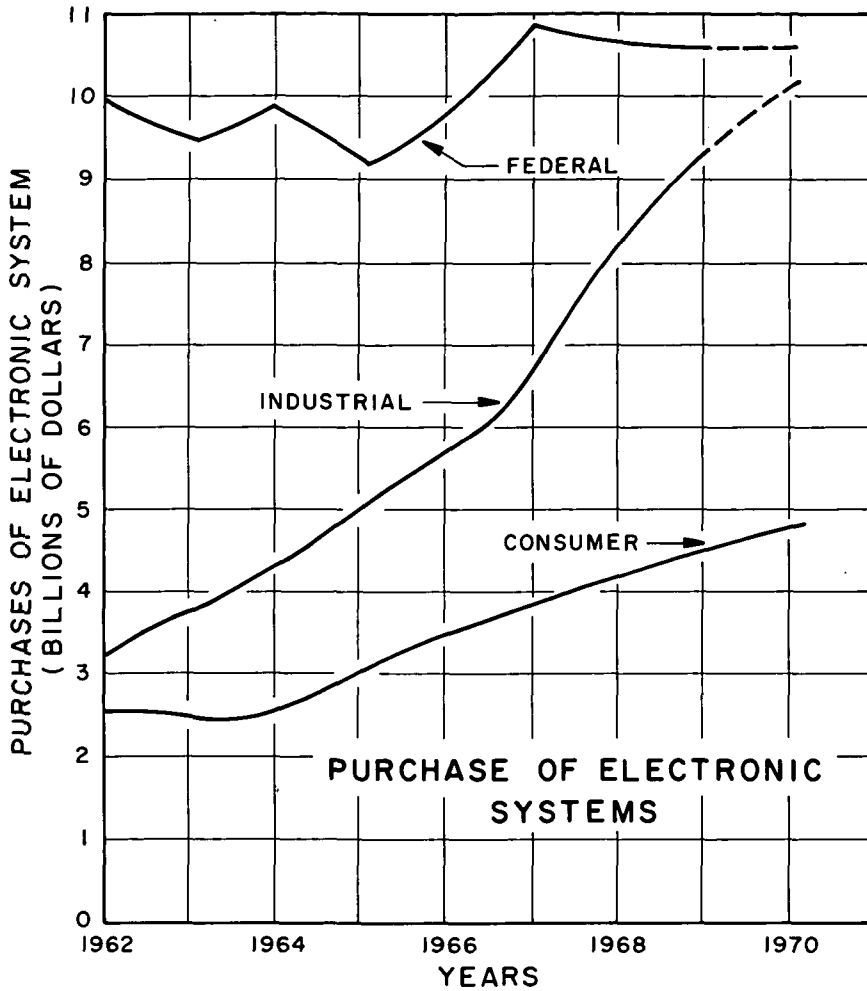
YEARS

FIGURE IV.4   Purchase of electronic systems by the federal, industrial, and consumer sectors in the United States.

vision sets, radios, phonographs, and tape recorders, have doubled during the past 10 years. The rapid communications, education, and entertainment that these goods supply affect the lives of every citizen.

Some examples of future areas of technological and social importance that will clearly involve solid-state materials and devices are the following:

1. Technologies based on past, current, and future solid-state research

will contribute significantly to the maintenance and improvement of the quality of life in this country.

2. Rising expectations and a shortage of medical personnel will require much more efficient medical care based on increasingly sophisticated electronic instrumentation.

3. Computer-aided instruction would greatly enhance the ability to educate successfully. Lower-cost components will be required to make possible this approach.

4. Convenient public transportation in the air will require the availability of increasingly sophisticated navigational equipment. On the ground, new transportation systems may involve superconducting technology.

5. The predicted power crisis during the next 25 years may well be avoided by the use of nuclear power plants. These depend on electronic instrumentation, solid-state detectors, and many radiation- and corrosion-resistant materials for their operation. Cryogenic, or perhaps even superconducting, cables may permit the efficient transmission of this power from generator to user.

6 The Physics of Condensed Matter: The United States and the World

In quantity of research in the physics of condensed matter, the United States has been the world leader during the last two decades, as the data in Table IV.3 show. In 1967, D. J. De Solla Price (*Science and Technology*, Oct. 1967, p. 84) made a more detailed comparison of worldwide scientific publications and showed that for the leading industrial countries (he listed 22) publications correlated very closely with GNP. Thus the U.S. predominance in the physics of condensed matter is not surprising.

However, various questions are suggested by this predominance: What are the consequences of U.S. leadership in the physics of condensed matter? Have foreign countries seen any significant features of this leadership? Have they been motivated to change their approach to the physics of condensed matter, and if so, what have they done? What do foreign countries consider to be significant near-future developments in this subfield?

To obtain some views on these questions, a survey was conducted in the spring of 1970 of observers abroad who were active in physics. Nineteen replies were received (see Appendix C). These replies varied considerably from question to question. Yet the replies, taken in total, presented a sur-

TABLE IV.3   Publication Statistics for Solid-State Physics, Compared
by Country of Publication[a]

Country	1961 Study		1965 Study	
	Number of Abstracts	Percentage of Abstracts	Number of Abstracts	Percentage of Abstracts
United States	1,687	26.7	3,614	32.7
Soviet Union	1,467	23.3	2,434	22.0
Great Britain	911	14.4	1,371	12.4
Japan	712	11.3	561	5.1
West Germany	402	6.4	960	8.7
France	315	5.0	386	3.5
Netherlands	148	2.4	668	6.1
Czechoslovakia	124	2.0	91	0.8
Poland	89	1.4	121	1.1
Denmark	80	1.3	280	2.5
All other	368	5.8	565	5.1
TOTAL	6,303	100.0	11,051	100.0

[a] *Sources:* S. Keenan and P. Atherton, *The Journal of Literature of Physics. A Comprehensive Study Based on Physics Abstracts–1961 Issues* (American Institute of Physics, New York, 1964); S. Keenan and F. G. Brickwedde, *Journal Literature Covered by Physics Abstracts in 1965* (American Institute of Physics, New York, Feb. 1968).

prisingly unified picture. Specific national features correlated closely with those described in Robert Gilpin's recent article in *Science* (169:441, July 31, 1970). A summary of the replies follows, supplemented in some cases with data extracted from the literature on international science and technology.

6.1   WORLD LEADERSHIP

On the question of world leadership, over half of the respondents gave qualified answers. We believe all respondents would agree with these qualifications. Specifically, three types of activities were mentioned. In regard to fundamental concepts and theory, the respondents felt that the United States is on a par with other modern nations. They pointed out that there have been no practical or national barriers to the transfer of fundamental concepts of physics; therefore, there is no such thing as "national leadership" but only the leadership of outstanding individuals. Fundamental ideas have appeared in many different nations and have not been limited to one nation or another.

In experimental work, the United States is ahead, mainly as a result of better financial support, better working conditions, and the like. A major-

C

ity of the science effort and literature is in experimental work, which accounts for the results in Table IV.3.

Third, in the application and exploitation of the physics of condensed matter the United States has had exclusive leadership in the last 20 years. All respondents agreed on this point and all admitted that if this "applied area of research" is included, then the United States has been dominant in the physics of condensed matter in the last two decades. As discussed subsequently and as most respondents implied, the success of the United States has resulted from its ability to "amalgamate" these activities with technology in such a way as to increase the overall output. That is to say, the interaction and stimulation among technology, applied research, and pure research have enhanced all three activities. Until very recently, this productive interaction has not occurred in other countries. Such amalgamation results from the close interaction of universities, government agen-

TABLE IV.4 The Major Inventions and New Technologies in the Semiconductor Industry after 1915[a]

Period	Major Inventions or Major New Technology	Major Contributing Firms (with Country of Origin When Not United States)
1947	1. Transistor effect	Bell
	2. Point-contact transistor	Bell (then Raytheon)
	3. Alloyed-junction transistor	GE, Bell, RCA, Raytheon
Early 1950's	4. Grown junction transistor	Bell
	5. III–V compounds	Siemens (West Germany)
	6. Zone refining	Bell
	7. Diffusion process	Bell (then Texas Instruments,) Mullard (United Kingdom, Netherlands)
	8. Chemical etch process	Philco
	9. Epitaxy	Bell
	10. Integrated circuit	Plessey (United Kingdom), Texas Instruments, Fairchild, Westinghouse
	11. Tunnel diode	Sony (Japan)
	12. Planar process	Fairchild
	13. Gunn effect	IBM
	14. Beam-lead technology	Bell
1960's	15. Metal oxide silicon (MOS) transistor and integrated circuit	General Instrument, General Microelectronics, Fairchild
	16. Large-scale integration	Bell, Texas Instruments, Fairchild

[a] Sources: Discussions with experts; national replies to checklist; visits to firms; "Patterns and Problems of Technical Innovation in American Industry" (report to NSF by A. D. Little Inc., Washington, D.C., 1963); "Integrated Circuits Come of Age," (Air Force Systems Command, Washington, D.C., 1966); Fortune Magazine (company histories).

cies, and industry. This unique system will be referred to as the "U.S. science–technology complex."

As a consequence of this interaction, essentially all the important innovations on which the solid-state electronics industry is based have been made and exploited first in the United States. The Organization for Economic Cooperation and Development (OECD) publication, *Gaps in Technology—Electronic Components* (Paris, 1968), quotes a Japanese source that states that "most of the products shown . . . owe their existence to ideas or inventions made in the United States. This tendency is particularly witnessed in the leading technology promoting the growth of Japan's electronics, such as transistors, semiconductors, integrated circuits and other active elements." Also quoted is an American source: "Virtually none of the new semiconductor products (discrete or monolithic) are based on foreign technology. . . . The most important new products, product improvements (in terms of market size) that have come from abroad during recent years are in the electron tube area." Tables IV.4 and IV.5, taken from the same publication, give further evidence that the U.S. work has had the greatest impact on development of the semiconductor industry.

TABLE IV.5   The Main Integrated Circuit Manufacturers outside the United States in 1967[a,b,c,]

Name of Firm	Main Countries of Operation	Licensor for I.C.
1. AEG	*West Germany*	–
2. COSEM	*France,* Italy	–
3. Elliott-Automation	*United Kingdom*	Fairchild (U.S.A.)
4. Ferranti	*United Kingdom*	–
5. Fujitsu	*Japan*	–
6. Mitsubishi	*Japan*	Westinghouse (U.S.A.)
7. ITT-Europe	United Kingdom, West Germany	Fairchild (U.S.A.)
8. Marconi (EELM)	*United Kingdom*	–
9. Motorola	France	Motorola (U.S.A.)
10. Nippon Electric	*Japan*	–
11. Philips	*Netherlands,* France, United Kingdom, West Germany	Westinghouse (U.S.A.)
12. Plessey	*United Kingdom*	–
13. Siemens	*West Germany*	–
14. SGS-Fairchild	*Italy,* France, West Germany, United Kingdom	Fairchild (U.S.A.)
15. Texas Instruments	United Kingdom, France, West Germany	Texas Instruments (U.S.A.)
16. Toshiba	*Japan*	–

[a]*Sources:* Visits to firms; discussions with experts; *Gaps in Technology—Electronic Components* (OECD, Paris, 1968).
[b] Country of origin in italics.
[c] The table can be regarded as an underestimate of the U.S. lead.

Moreover, it appears that U.S. leadership in other technologies, such as the computer, has been achieved to a considerable extent through its leadership in solid-state technology. Figure IV.5, from the O E C D publication *Gaps in Technology—Electronic Components* (Paris, 1969), demonstrates that U.S. firms began to dominate the computer business with the introduction of the transistor, which created second-generation computers. Table IV.6 is from the same source.

6.2 CONSEQUENCES OF U.S. DOMINATION

U.S. science has had a profound influence on the rest of the world: U.S. science journals are ubiquitous. Doctoral or postdoctoral work in the United States is considered a necessity in some smaller countries. English has become the language of science in many parts of the world. These and other general influences were mentioned by the respondents to the survey. Specific consequences that the respondents felt had a negative effect on their countries were the following:

1. *The brain-drain. This has been commented on extensively in the*

COMPONENT TECHNOLOGY / COMPUTER TECHNOLOGY	→ 1957	1956 →	1961 →	1962 →		after 1967
	GENERATION 0	GENERATION 1	GENERATION 2	GENERATION 3		GENERATION 4
				hybrid circuits	integrated circuits	
	relays, others	tubes	transistors			LSI circuits
GENERATION 0 — 1955	ZUSE Z 3 ASCC		/////	/////	/////	/////
GENERATION 1	FACOM 128 ARRA	EDSAC UNIVAC I IBM 704		/////	/////	/////
GENERATION 2 — 1955-1962	/////		IBM 7090 IBM 1401 RCA 300 ICT 1300	/////	/////	/////
GENERATION 3 — 1961-1968	/////	/////	GE 625 CDC 6000 series	IBM 360 series	RCA Spectra 70 series Burroughs 6500 ICL System 4	/////
GENERATION 4 — 1967	/////	/////	CDC 7600		Burroughs 7500 Univac 1109	Burroughs Illiac IV

FIGURE IV.5 Computer and component generations. [Source: O E C D; discussions with experts. From *Gaps in Technology—Electronic Computers* (OECD, Paris, 1969).]

TABLE IV.6 The Main Inventions and Innovations in the Computer Industry[a]

Description	Type,[b] Country, and Year		Responsible Firm or Individual	Remarks
1. General theory of computers	A. France	1936	L. Couffignal	Unknown outside France
	West Germany	1936	K. Zune	No publications. Totally unknown
	United Kingdom	1937	A. M. Turing	Relatively important influence
2. First electronic computer	B. West Germany	1941	K. Zune	Z3 computer. Little known outside West Germany
	United States	1946	J. P. Eckert and J. W. Mauchley	ENIAC. Important work was also done by G. Stibitz at Bell Telephone (1940), H. Aiken and IBM at Harvard (1944), and V. Bush at MIT (late 1930's and early 1940's)
	C. United States	1951	Remington Rand	UNIVAC I
3. Internally stored program	A. United Kingdom	1937	A. M. Turing	
	United States	1946	J. von Neumann (U. of Pa.)	
	B. United Kingdom	1948	U. of Manchester	MADM ⎫
	United Kingdom	1949	U. of Cambridge	EDSAC ⎬ Close scientific interchange between U.S. and U.K.
	C. United States	1951	Remington Rand	UNIVAC I ⎭
4. Subroutine concept	A. United Kingdom	1937	A. M. Turing	
	United States	1946	J. von Neumann	
5. Read-only memory	A. –		–	The read-only memory has been used in automatic telephone exchanges
	B. United States	1946	J. P. Eckert and J. W. Mauchley	ENIAC computer. Limited storage
	United Kingdom	1949	U. of Cambridge	EDSAC II computer. Storage of the entire control information
	C. Several countries		Most manufacturers	
6. Associative memory concept	A. United States	1946	V. Bush	ATLAS ⎫ The full possibilities of associative
	B. United Kingdom	1952	Ferranti	360-67 ⎬ memories have not yet been exploited
	C. United States	1965	IBM	

517

TABLE IV.6–(Continued)

Description	Type,[b] Country, and Year	Responsible Firm or Individual	Remarks
7. Microprogramming	A. United Kingdom 1948	U. of Manchester, U. of Cambridge	Close interchange
	B. United States 1948	IBM (J. Backus), U.S. Navy (G. Hopper)	
8. First compiler (A2)	B. United States 1951	U.S. Navy (G. Hopper)	In the late 1940's, G. Hopper worked in the U.K.
	C. United States 1951	Remington Rand	UNIVAC I: first computer to have compiler
9. FORTRAN language	B. United States 1953–1954	IBM Users Assn. (SHARE) and IBM	First FORTRAN compiler written by J. Backus of IBM
	C. United States 1954	IBM	
10. High-speed drum printer	C. France 1954	Bull	First application of the "on the fly" principle for printing
11. Ferrite core memory	A. United States 1955	MIT (Lincoln Lab.)	Important work was also done at Harvard
	B.		
	C. United States 1956	Remington Rand, then IBM	UNIVAC 1103A, IBM 704 and 705
12. Transistorized computers	A. United States 1947	Bell Telephone	Discovery of the transistor effect in 1947
	B. United States 1956	Bell Telephone	Leprechaun computer
	C. United States 1958	Philco, IBM, GE	Philco 2000, IBM 7090, ERMR system
	United Kingdom 1959	Elliott	Elliott 803
	West Germany 1959	S.E.L.	ER56 computer (S.E.L. is a subsidiary of the American ITT)
13. ALCOL language	B. Several countries 1958	ACM (U.S.A.) and GAMM (West Germany)	ALCOL was jointly developed by American and European specialists convened in Zurich, Switzerland. The first ALCOL compiler was written by Dijkstra of The Netherlands. ALCOL was subsequently adopted by most manufacturers and is presently more widely used in Europe than in the U.S.
	C. All countries after 1958	Several manufacturers	

	[b]			
14. Multiprogramming	C. United States United Kingdom	1960 1962	Honeywell Ferranti	H800 computer ⎫ Orion 1 computer ⎬ No interchange–independent developments
15. CODOL language	B. United States C. Several countries after	1960 1960	U.S. Department of Defense Most manufacturers	FIELDATA plan
16. Family of compatible computers	B. United States C. United States	1955 1963- 1964	U.S. Army IBM, Honeywell, RCA, GE, CDC	IBM 360 series, CDC 3000 and 6000 series, Honeywell H 200 series, RCA Spectra 70 series
17. Time-sharing	B. United States C. United States	1964 1966	MIT, Dartmouth, GE, GE, then several large U.S. manufacturers (IBM, CDC, etc.)	Civilian application (Project MAC)

[a] *Sources*: U.S. reply to checklist; *Chronology of Computing in Africa, Asia, Europe, and Latin America* (J. Connolly, New York, 1968); discussions and correspondence with experts; visits to OECD; *Gaps in Technology—Electronic Computers* (OECD, Paris, 1969).
[b] A, theoretical advance; B, first application; C, first commercial application.

popular and scientific press and was referred to as having been, at least in the past, a major problem in the physics of condensed matter.

2. Industry has tended to license under U.S. industry (especially Japan and parts of German industry related to computers) rather than to build up their own research and development capabilities. Although Japan has achieved industrial success by buying technology abroad, such a policy is in part self-defeating. As one respondent indicated, "With world science reduced and Japanese industrial size and its consequent large appetite for new science, the scientific reservoir will demand filling and the Japanese will need to do that or stop growing." In other words, Japan is finding itself limited by its past policies in the matter of research and development.

3. A follow-the-leader complex, that is, foreign laboratories will engage in work because the United States is doing it but without considering their own needs. As one example, a respondent indicated that the U.S. influence on his country in the physics of condensed matter was weighted on the academic side, but what they needed was more industrial leadership. Another problem that was indicated was that the slightest wave of something new in the United States often caused a great flurry in foreign laboratories, probably out of proportion to its importance. (It must be said that the same thing often happens in the United States. The difference might be that in the past we could afford this luxury but few foreign laboratories could.)

The consequences that the respondents considered positive were the following:

1. Those who had doctoral or postdoctoral experience in the United States and returned to their native countries greatly benefited themselves and their institutions.

2. In the last 20 years science has become a truly international phase of U.S. life. Scientists from all over the world are at home in U.S. laboratories. This situation has provided access to America for an important segment of world culture. All have benefited as a result.

3. U.S. achievements (largely in the spectacular phases of applied sciences) have interested youth all over the world in science and technology in the last two decades. The very success of U.S. technology and its attendant problems apparently have recently alienated the affluent youth of this country. However, the potential influence of this work on the youth of the emerging nations should not be overlooked.

4. Direct encouragement by U.S. scientists of Japanese efforts has been significant. For example, one respondent stated: "The cooperative and

generous leadership of U.S. scientists in the physics of condensed matter has been, in my opinion, a significant contributing factor to the remarkable rise of Japanese efforts in this field."

5. The success of the U.S. science-technology complex has shown the rest of the world the need for closer cooperation among different groups in their own countries.

6.3 CURRENT ACTIVITIES AND GOALS OF FOREIGN COUNTRIES

The respondents opposed the idea of national leadership in physics or of competition. None of the respondents indicated an attitude of challenging the United States. Their main goal was to use their own talents and resources more effectively. The way of achieving this goal was to develop their own version of the science–technology complex.

(To appreciate their problems, it is necessary to understand the relatively minute interaction among individuals in industry, government, and universities in the physics of condensed matter in foreign countries. Thus, in Japan, it is illegal for a university man to consult with industry; in Sweden, until recently, there has been relatively little government money for research in the physics of condensed matter, except for support of academic work for educational ends. Then, too, the barriers for individuals to change from academic to industrial positions and back to academic ones are only slowly being removed in many countries.)

Specific comments from the survey and the literature are the following:

1. The reversal of the brain-drain has come about because of (a) new academic institutions that have recently been formed, (b) new industrial jobs, (c) actual government subsidies for returning scientists, and (d) the current U.S. curtailment of research funds.

2. An actual redirection of research funds into the physics of condensed matter and, in particular, into selected areas of research that hold particular promise is occurring.

3. Policy committees, councils for allocating funds, nationally sponsored research institutions, and the like have appeared in most of the developed countries in the last decade, many in the last few years. Many of these are either specifically directed toward the physics of condensed matter or have recently emphasized this field in their funding. Examples are (a) "Within four years, the (British) universities and colleges have been equipped with 70 Industrial Liaison Centres, eight Industrial Units, and at

least a dozen variously named liaison offices . . . to speed the transfer of technology from universities to industry" (N. Hawkes, *Science Journal*, March 1970, p. 75); (b) "Concerted Action (Electronics)" Committee formed in France; (c) Deutsche Forschungsgemeinschaften formed in Germany; (d) "Encouragement and incentives (financing, subsidies, tax benefits . . .) have been given to science-based industries (Israel)"; (e) "European governments have recognized the technological importance of solid-state science, and have therefore provided substantially increased support for research. For instance, solid-state physics in Denmark has grown from negligible proportions to its present flourishing state within the last decade . . ."; and (f) "National Group on Matter Structure" formed in 1966 (Italy).

4. Slow changes are being made in the universities from a rigid hierarchical structure to a more democratic structure permitting and encouraging more cooperation, team work, and the like. There is a trend toward developing larger units, such as larger university departments, to promote interactions between individuals, interdisciplinary work, and the like.

Several respondents noted that there is a new attitude toward the physics of condensed matter among governments. Until a few years ago, this subfield was essentially unknown as a distinct research area (as compared, for example, with nuclear physics). Now it is a high-priority subject in most of Western Europe.

6.4 FUTURE TRENDS

As already indicated, many respondents noted that their problem is basically one of the organized use of science. It is as much a social, cultural, and political problem as a scientific one. The respondents indicated a wide variety of opinions concerning the future. A few indicated that they saw no significant changes. The majority felt that the U.S. leadership will shift to other countries, either because the United States is restricting itself irreversibly or other countries will catch up through redirection of effort, or both. Some specific comments are the following:

1. The United States should increase efforts, especially in fundamental work. An era of constructive competition lies ahead.

2. If education is improved ("most important current domestic problem"), if the government research budget is increased, if the political climate is favorable, and if youth remains inspired, then Japan could quickly respond and lead.

3. In the next 20 years, the leadership will fall to the United States or Japan or Soviet Russia.

4. Other large countries will move toward a leadership role; smaller countries must specialize very narrowly "to share in the pie."

5. From *Swedish Physics,* a report in 1967: ". . . physics of condensed matter will soon be the strongest-staffed field of physics [in Sweden] ."

6.5   SOME GENERAL OBSERVATIONS

The brain-drain appeared to be the only common major complaint among the foreign observers in regard to U.S. domination in the physics of condensed matter. Since that trend had stopped and had actually reversed at the time of the survey, no actual sense of danger from the United States was reflected in the replies. Actually, several respondents expressed the fear that curtailment of activity in the United States in the physics of condensed matter could only harm their countries' efforts as well as those of the United States. Two thoughts were apparently behind these fears. One was political. If a particular government sees that the United States is curtailing funds, the foreign government might follow this example without really assessing the problem nationally. The second fear was that the United States was approaching a critical point in its curtailment of research and development work. The United States is the only country that has a truly broad-based, flexible research and development effort in the physics of condensed matter. France tried to build one but failed. Other countries realize that they do not, at the moment, have the resources and, with the exception of Soviet Russia and Japan, are not likely to achieve those resources. The options open to them are to specialize or to buy the technology. Sweden is the best example of specialization and Japan of the purchase of technology. (See Gilpin's article referred to above.)

With the exception of the respondents from France, where research and development work have deteriorated since 1968, all respondents felt that their countries were either holding their own or decreasing the technological and scientific gap in the physics of condensed matter. Agreement that this trend had developed in the last several years was surprising. (Israeli respondents referred to the 1967 war as the turning point. Others usually said "the last two years.") During this time, the physics of condensed matter (defined in the broadest sense) has come to be recognized as "big science" and regarded by governments as an actual or potential asset of commercial, military, or national prestige. Certainly, this marks a significant trend in the world of physics and in the world of science and politics in general.

6.6   CONCLUDING REMARKS

Further comments are necessary in regard to two points, the science-technology complex and academic reform. One observer noted, in discussing the science–technology complex, that: "In fact, if I personally were to choose the aspect in which the U.S. differed from other countries most in the past two decades, it would be the application of solid state physics in industry, the use of physicists as physicists by industry, and of course the federal and private industrial backing of fundamental research." This is the complex. It is the interaction among a variety of people at all levels from industry, government, and universities, working out programs, ascertaining needs, communicating results, and the like. Research work with its funding and reporting schemes through government agencies has provided the nearest thing to a common denominator for this complex. The research base in industry and the response of the academic community to economic needs are integral factors, but it is difficult to point to any single principal feature.

Some further international comparisons indicate the complex character of the problem. Some nations, notably England, have had outstanding records in fundamental solid-state science and have not been able to turn this capability into a technological and industrial advantage. This is evident from the data in Table IV.7, taken from the OECD publication, *Gaps in Technology–Electronic Components* (Paris, 1968), showing import-export balances in electronic components. Therefore, strong fundamental research alone is not enough for industrial success. On the other hand, some nations, especially Japan, have so far been able to use the scientific and technological advances of other nations—notably those of the United States—to build strong, successful industries. This success depends, of course, on the fact that someone else is doing the research and the innovating, that a country knows enough to acquire the technology, that it is willing to pay for the information, and that it is willing to depend on and follow others.

The success of any such science–technology complex in other countries is tied, surprisingly closely, to academic reform. This situation was noted indirectly by a number of observers, who also felt that the academic community would be slow to change. The academic hierarchical structure characteristic of Europe and Japan, which generated the isolated professorial type, is only slowly giving way to more democratic institutions emphasizing team work and activity and recognizing and rewarding young talent. In the United States there is a self-perpetuating system in the physics of condensed matter (and in other fields) that is little appreciated in this country, particularly in the academic community, but that most of the

TABLE IV.7 International Trade in Electronic Components in 1965–1966 ($Million)[a,b]

Export–Import Balance by Country	Total Components	Passive Components	Active Components	
			Tubes	Semiconductors
United States (1966)				
Exports	238.1	85.3	44.1	108.7
Imports	151.2	64.4	44.5	42.3
Balance	+86.8	+20.9	−0.4	+66.4
Japan (1966)				
Exports	176.7	123.3	28.1	20.3
Imports	22.6	10.0	2.6	10.0
Balance	+154.1	+118.3	+25.5	+10.3
United Kingdom (1966)				
Exports	128.2	87.7	30.6	30.6
Imports	103.8	53.2	14.4	36.2
Balance	+24.4	+34.5	+16.2	−26.3
West Germany (1965)				
Exports	165.2	109.3	55.8	
Imports	108.5	54.5	54.0	
Balance	+56.7	+54.8	+1.8	
France (1966)				
Exports	86.7	46.1	24.2	16.4
Imports	107.1	54.6	18.5	34.0
Balance	20.4	−8.5	+5.7	−17.6
Netherlands				
Exports	n.a.	n.a.	n.a.	n.a.
Imports	n.a.	n.a.	n.a.	n.a.
Balance	n.a.	n.a.	n.a.	n.a.
Italy (1965)				
Exports	72.3	49.1	14.2	9.0
Imports	101.4	68.9	32.5	
Balance	−29.1	−19.6	−9.3	
Canada (1966)				
Exports	25.3	n.a.	n.a.	n.a.
Imports	130.4	n.a.	n.a.	n.a.
Balance	−105.1	n.a.	n.a.	n.a.
Belgium (1966)				
Exports	n.a.	n.a.	8.0	
Imports	n.a.	n.a.	22.1	
Balance	n.a.	n.a.	−14.1	
Sweden (1965)				
Exports	2.8	1.6	1.2	
Imports	28.3	10.9	17.4	
Balance	−25.5	−9.3	−16.2	

[a] *Sources:* National replies to checklist question 2.3. From *Caps in Technology—Electronic Components* (OECD, Paris, 1968).
[b] Data for integrated circuits, when available separately, are included in the column "Semiconductors."

foreign observers recognized. Physics graduates can find jobs doing physics in industry and government. Thus the graduate schools are geared (often, it would seem, against their wishes) to supply this competitive demand, with the result that, as one respondent indicated, graduates in the United States are the best trained in the world. Others may disagree with this, but the academic community in the United States is far more aware of the activities and needs of industry and government than are the academic communities of other countries.

Considering all the evidence, this Panel believes that the industrial success of the United States in solid-state electronics has resulted from several factors, each of which constitutes an essential and vital link:

1. Strong research programs widely distributed among universities, industries, and government agencies (these form not only a source of basic discoveries and knowledge but also an infrastructure of competence to disseminate and use the results of research).

2. Strong engineering programs in solid-state electronics, again widely distributed among universities, industries, and government agencies (these provide a means of adapting, modifying, improving, and extending the scientific results for a wide variety of uses).

3. The entrepreneurial and venturesome spirit that has by and large pervaded the solid-state electronics industry, due in part to the transformation of some scientific and technical people into businessmen and into agency heads responsible for the field.

4. Strong government support of research in universities and of some of the more sophisticated and challenging technological developments in industry.

Finally, it is the belief of this Panel that no subset of these factors alone would have been sufficient to produce the position of world leadership that the United States has held in the past.

7 Large Facilities for Research in the Physics of Condensed Matter

For the most part, the physics of condensed matter has been developed in experimental facilities that are not extraordinarily large or expensive. Although the self-supporting effects of large aggregations of scientists in one

laboratory have been beneficial in major centers, it is a strength of this subfield that it can also be pursued successfully at relatively small expense in the smaller universities and the less affluent industrial laboratories. In a few cases, however, major facilities have been found necessary.

The National Magnet Laboratory has provided very high magnetic fields for use by experimenters from other laboratories and has been eminently successful. This facility should be strengthened and maintained in vigorous health for the national benefit. Unfortunately, recent cutbacks are weakening this laboratory.

A second example is the nuclear reactor, which provides neutron beams for diffraction and inelastic scattering experiments and radiation for studies of radiation damage. Two things are necessary: higher-intensity beams ·of neutrons and better instrumentation for their use. Higher-intensity beams will make possible experiments of much greater precision and sensitivity. Instrumentation has been improving steadily over the years and can be further improved. The beam pipe for very slow neutrons, the correlation and Fourier choppers and multiple detector systems for enhanced data rates, and computerized recording systems are fairly recent developments. In the matter of beam intensity, cold and hot moderators are now being employed for improvement of intensity at long or short wavelength, respectively. Newer reactors have demonstrated great increases in flux compared with earlier models, and there is the possibility for an order of magnitude or more of further improvement by design of special pulsed reactors. A pulsed reactor exists in the Soviet Union, and a new pulsed reactor of very much higher flux, which makes possible more intense beams, is said to be under construction. Design studies for an advanced pulsed reactor were under way in this country but have been terminated because of budget problems. The opportunity to provide more intense neutron beams by such a system and the need for such beams in solid- and liquid-state studies constitute a strong argument for reinstating this work at the earliest possible time. The French–German reactor at Grenoble (a very-high-flux, steady-state reactor) is a recent critical factor. Plans for this facility are ambitious and promise to exceed the best U.S. efforts by a substantial amount. The extent of European interest in this subfield is also attested by plans for the design of a very-high-flux reactor in Great Britain.

A third example is the synchrotron radiation source. Two such sources exist in this country and are the sites of stimulating experimental work. The time is ripe for assessing this work to decide whether a new synchrotron designed expressly as an intense source for far-ultraviolet radiation should be built.

At times, proposals have been made to establish new centers organized along other lines. For example, centers have been proposed for work with

high pressures, for preparation of pure materials and the growth of crystals, for applications of computers to the physics of condensed matter, for research at very low temperatures, and for high-voltage electron microscopy (the French and Japanese are far ahead of the United States in this research). In these cases, it is less clear that a large laboratory is necessary, since successful work on all of these subjects is taking place at existing laboratories with multiple interests. In regard to materials preparation and crystal growth, the Panel is of the opinion that every major center of solid-state research should have its own substantial competence in this activity, *and that it would be better to strengthen the local activities before diverting funds for the establishment of a new center.*

On the other hand, there is a discernible shift in solid-state physics toward problem-oriented experimentation and away from technique-oriented experimentation. As sophistication increases, it is more often necessary to attack a problem with many techniques simultaneously in order to advance. This situation confers an advantage on the large laboratories and is an important argument for keeping the larger centers of activity vigorously alive. The trend toward a broader attack on problems also increases the difficulties that universities have in keeping up. A large variety of facilities and services that can be shared on at least a regional basis are very much needed for this reason, even though most of the facilities individually are not large enough or costly enough to require an entire laboratory. The Panels believe that the Advanced Research Projects Agency–National Science Foundation Interdisciplinary Laboratories, especially, should give thought to widening their services in this way, and funding agencies should consider these broader needs when they support costly new facilities at existing laboratories.

8 Training in the Physics of Condensed Matter

This chapter differs in two respects from the others. First, training is primarily the concern of physics in general rather than specifically that of the physics of condensed matter. Second, any discussion of training is almost necessarily addressed chiefly to the physics community rather than to the outside audience of government agencies, Congress, and the public. In addition, we have broadened the scope of this topic slightly to touch briefly on the training of nonphysicists in their cultural understanding of this science, a subject of considerable importance in combating a popular anti-

scientific bias. The discussion begins with graduate education as the key point in the education of the professional, then continues to postdoctoral training, after which it deals with undergraduate and secondary school education. The final section considers midcareer training.

8.1 GRADUATE EDUCATION

The current financial situation raises profound questions for physics as a whole, principally as a result of the changing role of the science. Thirty years ago it was accepted as axiomatic that the physics-educated individual would excel in generating solutions to problems as they were needed. In particular, the wartime successes of physics in radar and nuclear explosives were fresh and compelling in the mind of the general public. Two primary causes have contributed to the change from this situation: First, a popular move away from the physical sciences stems in part from the anti-intellectualism of some of the more extreme elements among the protesting youth (who associate science with war and imperialism) and in part from the realization that the problems of society in the 1970's will require different tools for their solution than did the needs of the nation in the 1940's. Second, the competition from engineering is now much more intense. Educators in this profession have pushed enthusiastically into the new fields and unhesitatingly embraced new techniques. Not only experienced and sophisticated, the new engineers are also numerous. The annual rate of production of engineering PhD's is increasing more rapidly than that of the physical sciences, so that the two rates are now nearly equal.

Although the shift in interest and emphasis from the physical to the biological and social sciences and the vigorous competition from the engineering profession are factors over which physicists have little control, other elements that aggravate the situation are suggested by direct criticisms of training in physics that merit careful consideration by the physics community. The burden of these criticisms, largely from industrial employers, is, first, that the atmosphere in many graduate schools strongly encourages students to continue to conduct basic research, often on the same subject as the thesis study, at the expense of a shift to applied science, and second, that training in physics is too specialized to prepare a student for the variety of problems that he may be called on to attack in an industrial laboratory.

Direct documentation of such criticisms comes from a recent American Institute of Physics report by Susanne Ellis, *Work Study Complex* (1969). The author reports on 161 interviews with supervisory staff in 40 research organizations, the majority of which were smaller industrial laboratories.

There is a clear implication that industry could in time absorb more physicists appropriately sympathetic to its needs, flexible in their areas of interest, and capable of communicating their ideas to others. For such persons, the current PhD program may not be the best training. Again the report indicates that industry is inclined to emphasize training on the job—in the applied areas at least—at the expense of formal education, particularly beyond the Master's Degree level.

Even if the picture conveyed by this report is universally accepted as accurate, the reactions of the various physics graduate departments will differ widely depending on tradition, ties with industry, and relations with other departments. Some subscribe to the philosophy that the proper province of physics is only the frontier of knowledge and that the applied aspects as they develop should be relegated to engineering. The PhD's graduated by such a department will tend to work primarily in the universities and those industrial laboratories that are large enough to support a basic research effort that is partially isolated from application and development. However commendable such a program is, it will almost surely face some retrenchment in size and numbers of students in a time when federal subsidy and popular support hold relevance as the critical factor in the allocation of funds.

On the other hand, the departments that wish to help to meet the general problems of society as they interlock with the physical sciences, and to adapt to the current situation rather than to diminish in size, have a variety of responses open to them. Such a department will try to create an atmosphere that will anticipate and encourage the eventual entry of many of its students into applied and varied work. To bring about closer ties with industry, specialty courses by members of local laboratories are of value to the limited group of students who enroll. For wider coverage, colloquia by distinguished applied scientists from outside the university (or within) would be most desirable. More thoroughgoing measures include the interchange of staff between universities and the applied laboratories for a semester or a year according to individual convenience. Neighboring industry should also be encouraged to offer summer jobs to graduate students who have not yet begun full-time research; such an arrangement has much to offer to both. The graduate student who seeks to broaden his training should be encouraged to move farther afield in his choice of courses, going outside the department, perhaps even at the expense of traditional courses previously considered indispensable in a physics program. He should be led, by example, to visit occasionally the colloquia and seminars of other departments and to consult the members of these departments who may be helpful in his research problems.

The twofold objective, of preparation of students for applied work by

broadening their viewpoints and of interaction with industry, is perhaps most readily attained in the physics of condensed matter. In fact, training in this subfield is especially appropriate preparation for work at the borderline between science and technology. The connection between solid-state physics and the electronics, communications, and instrumentation industries is well known. A student involved in research in such areas as solid-state electronics, lasers, thin films, superconductors, luminescent and phosphorescent materials, or ferroelectrics is very likely to take cognizance of the exciting applications surrounding these subjects. At the same time, this subfield has much to offer in graduate physics education. On the one hand, the principles of modern physics can be illustrated with relatively inexpensive equipment; on the other, in contrast to some other subfields of physics, it still offers a student the opportunity to do a dissertation with considerable independence and ingenuity, including designing his own equipment, making his own measurements, and interpreting his experiments theoretically. Such a complete experience is important in producing a well-rounded individual who is capable of moving into new endeavors, including applied work, after obtaining his degree.

The relatively low operating expense for research in solid-state and related fields also offers the opportunity to establish an effective physics department concentrating on this subject at a smaller school. In fact, a good group consisting of as few as three experimentalists and one theoretician belonging to a physics faculty and working in related areas of solid-state physics can constitute a significant center of research. There are a number of examples of such groups throughout the country.

In view of these benefits to be derived from research in condensed matter, it is unfortunate that in some of the leading universities this subfield has been completely, or nearly completely, eliminated from the physics department and instead has moved into departments of electrical engineering, metallurgy, or ceramics. This trend deprives physics graduate students of contact with what is probably the most technologically significant branch of physics and thereby tends to limit their knowledge of the nonacademic world and the future opportunities open to them in related academic and applied fields.

One area in which improvements can be made in virtually all physics departments relates to the length of the doctoral program and, in particular, the time devoted by the student to the PhD thesis. The Panel on Condensed Matter feels strongly that the now fairly typical time of five and one half to six years for attainment of the doctorate is too long and should be reduced to not more than four to four and one half years. We believe that the additional training does not substantially increase the quality of the PhD product. The problem of the excessive length probably

arises from two sources: (a) The thesis adviser often considers a proposed problem for the student more in terms of its contribution to his own research interest than its suitability for the student and the possibility of his making an adequate contribution in a reasonable time; and (b) the student tends to feel very comfortable when paid a living wage for his graduate research and is willing to allow the arrangement to drag on. The first problem can be attacked by making faculty members more conscious that the training of graduate students is part of their *teaching* responsibility, which is quite different from being a group leader in a research institute. Still another approach is to hold a preliminary examination of the student by his committee at a point when he should be about half way through his research. The second problem calls for a systematic policy of reducing student support after two or two and one half years of research. Several departments are already implementing such a policy with some success.

8.2 POSTDOCTORAL EDUCATION

The support of postdoctoral students generally has proved to be the most vulnerable phase of the science education system in this period of decreasing financial resources. The reasons relate to (a) justifiable criticisms of the way in which the postdoctoral system has been overextended in the past, (b) elimination of postdoctorals as a quick way to release fairly substantial sums of money that can be used for graduate students, and (c) a lack of understanding of what postdoctoral training is supposed to accomplish. The purpose of the postdoctoral appointment is to give an outstanding new PhD the opportunity to broaden himself in research by working in a different university and under the guidance of a different faculty member from the one under whom his thesis was conducted. The field of postdoctoral research should be somewhat different from that of the thesis, so that new techniques or new branches of the student's field of physics should become more familiar to him. Only a limited fraction of emerging PhD's should undertake postdoctoral training, and these should be the people best suited to an academic career or to carrying out fundamental research in a large industrial laboratory.

When sufficient funds were available, there was a tendency to accept almost any new PhD for postdoctoral training. Often the thesis professor kept the man on so that the inertia of the PhD thesis could be maintained in producing further results. Such misuse of the program coupled with the present shortage of funds now threatens to kill the institution of postdoctoral education. Because of the importance of postdoctoral training for

the most able physicists, the physics community must find ways to maintain postdoctoral appointments for such people. If the length of the PhD research is reduced, as suggested above, the extra funds released in this way can be used to maintain at least part of a postdoctoral program.

8.3 UNDERGRADUATE EDUCATION

The impetus for a broadening of scope and a greater zeal for relevance currently is especially characteristic of undergraduate education. It seems reasonable to relax some of the rigid requirements that have developed in strong undergraduate departments that were striving to inject their students into graduate schools at an advanced level. A recognized alternative physics curriculum, which is broader and more interdisciplinary in its scope, also should be offered. When students show interest in biophysics, physical chemistry, geophysics, or nuclear engineering, they should be encouraged to take such technical electives along with, or if need be, in place of, the regular physics sequence. Such a program (titled Applied Physics or Engineering Physics) now exists at many universities but is often given in the engineering school rather than in the physics department where it fits naturally. This type of program can be far better than the usual physics curriculum for the student who will not go on to a PhD in physics. It can also serve as a pre-engineering course for students who will go to graduate school in an engineering department and as undergraduate training for students who will take graduate work in an interdisciplinary area (for example, geophysics, chemical physics, or oceanography). In the long run, a more liberal curriculum attracts more and better students through the challenge it offers the individual than it loses by the shift of students to other fields. The Ellis report emphasizes the predeliction that industrial employers frequently show to individuals who have a background in two or more disciplines.

 This kind of mixing of physics with other disciplines by student eclecticism seems to be more effective for science students than premixed offerings in the form of general science or interdisciplinary courses. The explanation is, in part, that such interdisciplinary courses must rely on somewhat stilted cooperation between instructors from different departments, whose individual flavors are lost in the potpourri, and that students gain in involvement by making their own selection. In regard to science education for the nonscience or nonengineering major, the composite course has an important role. (It is, of course, a very important way to build good feeling for the importance of the contribution of science and technology.)

For example, two such programs are the Physical Science for the Non-scientist (PSNS) and the Science Courses for Baccalaureate Education. The former is designed to offer a joint physics–chemistry course for persons going into secondary school science teaching. It has been professionally planned and executed by a large composite group under the aegis of the National Science Foundation; it is complete with text, laboratory manual, and teaching manual. The second scheme is a highly imaginative effort to teach one year of physical sciences and one year of life and social sciences. The teaching is integrated around the emphasis on a limited number of general principles that act as unifying threads for the whole course. Two texts will evolve. Although this is a less ambitious effort, it has proved to be stimulating in the right environment. In general, there seems to be no dearth in effort in preparing new material for scientific courses. A successful textbook on the undergraduate level could be highly lucrative.

8.4 EDUCATION IN THE SECONDARY SCHOOLS

The nationwide picture as far as secondary education goes is one of static quality and decreasing quantity. Not only are the physical sciences enlisting relatively fewer students than the other disciplines, but, in the case of physics at least, the absolute number is dropping. This trend contrasts with the situation in most of the developing countries and in many European nations where physics is required. Here, it is known as a "hard" subject, and many of the experienced teachers take pride in exerting a tough elitist attitude toward their pupils. On the other hand, undertrained teachers from other fields who are delegated to teach physics frequently flounder helplessly, thereby alienating another group from physics. The Physical Science Study Committee course with its greater demands for intellectual participation, has also made its contribution to the decrease in physics numbers, but here the loss is not so serious, as it is in part offset by the increased attraction afforded to those with some aptitude for following physics as a career. In any case, the narrowing of the base from which the physical sciences build is a serious matter from the standpoint of public relations, if no other, since it will always be more difficult to obtain public backing and understanding for a discipline for which the electorate has little integrated understanding and involvement.

There appears to be a trend away from physics even among the brighter students. In part, physics is losing them to the life sciences; more are going into the social sciences and humanities. Much of this exodus is the inevitable consequence of the search for relevance defined in human terms.

8.5 EDUCATION IN MIDCAREER

The problems of midcareer, or continuing, education differ considerably with the environment of the individual. For the person who accepts employment before completing the formal requirements for the degree to which he aspires, most localities provide opportunities for part-time study in evening or extension work or possibly released time on a more favorable basis. At the other end of the continuum, the mature scientist at a university or large laboratory who wishes to alter his interests has the information, resources, and colleagues at hand to make this change efficiently. In the middle is the professor at the liberal arts college or the scientist in an industrial group of ten or less. Some individuals probably have the greatest need for special training seminars (of two or three weeks) when seeking to expand their fields to enter new ones. Generally, such continuing seminars appear to be available in many fields.

Conferences and symposia in the physics of condensed matter are not so valuable as training mechanisms as they are in helping scientists already working in the subfield to keep up to date. Undoubtedly, too many conferences are held; some serve only the needs of the organizer. Three or four conferences sometimes take place in one specialty in only a few months, with many of the same speakers appearing at each new geographical setting, clearly with no new data. There is need for a clearinghouse to help to regulate the scheduling of such conferences, even if only by making prospective planners aware of the existence of other related conferences.

9 Manpower, Productivity, and Funding in the Physics of Condensed Matter

9.1 MANPOWER CHARACTERISTICS

The number of physics participants in the 1970 National Register of Scientific and Technical Personnel who indicated the physics of condensed matter as the subfield in which they were employed was 7818, or 21.5 percent of the 36,336 physicists included in the Register survey. Thus, the subfield of condensed matter is by a substantial margin the largest of the subfields of physics.

Slightly more than half of the physicists working in this subfield (4240 or 54 percent) held PhD degrees and constituted one fourth (25.5 percent) of the total number of physics PhD participants in the survey. The 3578 non-PhD condensed-matter physicists accounted for 18.2 percent of the total non-PhD population in physics.

The median age of the condensed-matter PhD group in 1970 was 36.4 years, a year less than the median age for PhD's in all subfields taken together (37.4 years). Four fifths (80.5 percent) of the condensed matter PhD's had obtained their highest academic degrees in physics, with 13 percent indicating engineering, and 4.5 percent, chemistry.

Table IV.8 shows the distribution of PhD and non-PhD physicists in this subfield among employing institutions and compares the employment pattern for condensed-matter physicists with that for all physicists taken together. Slightly higher percentages of both PhD's and non-PhD's in this subfield indicated industrial employment than was true of all doctorates and nondoctorates in physics.

Table IV.9 presents data on the primary and secondary work activities of doctorates and nondoctorates in the condensed-matter subfield and of those in all physics subfields taken together. The work activity patterns for the two PhD groups are much alike; however, a slightly greater emphasis on basic research among nondoctorates in the subfield than among all physics nondoctorates is evident. In addition, non-PhD's in condensed matter are slightly more involved in applied research and slightly less involved in teaching than is characteristic of the overall physics nondoctorate population. These data on principal work activities in the physics of condensed matter suggest a healthy mix of basic and applied endeavors, a characteristic of this subfield that has been discussed in greater detail in other chapters.

TABLE IV.8 Employing Institutions Indicated by PhD and Non-PhD Groups in the Physics of Condensed Matter and in the Overall Physics Population

Employing Institutions	Condensed-Matter PhD's N = 4157 (%)	All Physics PhD's N = 16,248 (%)	Condensed-Matter Non-PhD's N = 3126 (%)	All Physics Non-PhD's N = 17,679 (%)	Condensed-Matter Total N = 7283 (%)	Physics Total N = 33,927 (%)
College and university	43.1	50.6	37.0	30.0	40.5	39.9
Industry	35.4	23.4	40.2	30.2	37.5	27.0
Government	9.4	9.0	13.0	14.4	10.9	11.8
Research center	9.0	11.8	4.5	5.0	7.1	8.2
Other	3.1	5.2	5.2	20.4	4.0	13.1

TABLE IV.9   Primary and Secondary Work Activities of PhD's and Non-PhD's
in the Physics of Condensed Matter and in All Physics

Principal Work Activities	Condensed-Matter PhD's $N = 4556$ (%)	Physics PhD's $N = 16,017$ (%)	Condensed-Matter Non-PhD's $N = 3368$ (%)	Physics Non-PhD's $N = 17,001$ (%)
Basic research				
Primary	34.7	34.7	30.9	20.7
Secondary	27.7	24.9	8.3	7.2
Applied research				
Primary	19.9	16.8	24.9	20.8
Secondary	20.8	18.1	24.6	19.3
Design and development				
Primary	2.4	2.3	11.2	9.3
Secondary	7.6	6.6	18.7	14.9
Management				
Primary	15.0	16.0	15.2	19.1
Secondary	10.2	11.4	7.8	10.4
Teaching				
Primary	24.5	25.7	11.5	23.1
Secondary	14.8	18.2	9.1	7.6
Other				
Primary	1.4	2.4	5.0	8.0
Secondary	6.9	8.8	12.9	17.3
No Response				
Primary	2.1	2.1	1.3	2.1
Secondary	12.2	12.1	18.5	23.3

9.2   PRODUCTIVITY

Examination of a sample of 1181 articles produced by U.S. institutions
and listed in 1969 issues of *Physics Abstracts* showed that 41.4 percent of
these publications dealt with the subject matter of the physics of con-
densed matter, the highest percentage for any subfield, and not unexpected
in view of the size of this subfield. Of the 489 articles dealing with con-
densed matter, 48 percent were produced in academic institutions and 31
percent in industrial laboratories. Research centers, government labora-
tories, and other miscellaneous institutions accounted for 11 percent, 8
percent, and 2 percent, respectively, of the published work. Nearly three
fourths of the publications in condensed matter were experimental in na-
ture; 29 percent were theoretical.

A sample of 1102 articles produced by institutions outside the United
States and taken from listings in 1969 issues of *Physics Abstracts* included
43 percent that treated the physics of condensed matter. The major coun-
tries of origin of these articles dealing with condensed matter were the

Soviet Union, with 172; the United Kingdom, with 80; and West Germany, with 56.

An additional worldwide sample that included U.S. institutions numbered 1296. Again, papers dealing with condensed matter accounted for about two fifths (42 percent) of the total sample. The United States was the most productive country, with 35 percent of the 549 publications in condensed matter; the United Kingdom and the Soviet Union were next, with 27 percent and 17 percent, respectively.

The production of PhD theses on the physics of condensed matter was studied through examination of a sample of listings from 1969 issues of *Dissertation Abstracts.* Forty-one percent of the dissertations in the sample from the physics section treated subjects within the scope of the physics of condensed matter, and, in addition, one fourth of the engineering theses that could legitimately have been classified as physics were concerned with condensed matter.

These data from the various samples studied consistently indicated a high level of productivity among scientists working in the physics of condensed matter.

9.3   EFFECTS OF VARIOUS LEVELS OF FUNDING

The Panel was asked to assess the effects on the physics of condensed matter of (a) rising, (b) constant, and (c) declining levels of federal support for this subfield during the next five years. The rising mode is assumed to be such as to exploit the principal opportunities in the field reasonably fully (we assume an increase of 10 percent per year in 1969 dollars); the level mode is assumed to be level in terms of 1969 dollars; the declining mode is arbitrarily assumed to be a reduction of 7.5 percent per year as measured in 1969 dollars.

The physics of condensed matter differs sharply from most of the other subfields because total industrial support of the subfield exceeds governmental support. Much of the industrial effort is applied in character, but there is a continuous gradation between the more applied (or application-oriented) efforts and the basic (or knowledge-oriented) efforts, and the industrial contributions to the basic side are highly significant. According to recent estimates, about 53 percent of the basic research in this subfield takes place in the universities, 20 percent in government laboratories, and 25 percent in industry. About 75 percent of the more applied work is found in industrial laboratories. In assessing the effects of changing governmental support, the possibility of simultaneous changes in industrial

support must be kept in mind, because they would probably exacerbate the effects.*

A second characteristic of the physics of condensed matter is its great diversity. It is difficult to break it down into a small number of separate subsections or categories that can be discussed individually; dozens of subsections would have to be chosen, and then these would be found to overlap and interact in complex ways. A breakdown of present funding in the field by subsections is not readily available and would be difficult to generate. If the current work is described by projects, the typical project is small ($100,000 per year or less), and large devices do not consume a major portion of the funds. The contrast with high-energy physics and nuclear physics in this regard is particularly sharp.

9.3.1 *Making Choices*

Because of the complex and multifaceted nature of the subfield, it is difficult to arrive at simple recipes for distributing various levels of funds. The priority for support of work should be in proportion to the expected benefit of the work. One class of benefits is the advance in understanding of a natural phenomenon or a field of phenomena or the discovery of new phenomena. Another class of benefits relates to the goals of applied research—the discovery of ways to do something or to make something that is new and useful. Benefits of the first type generally lead at some time to benefits of the second type. In the physics of condensed matter this time lag is often short.

One can also distinguish work with high and low probabilities of achieving its goals. The expected benefit with which we are concerned is that which would befall if the work achieves its goals multiplied by the probability of this achievement. More routine work, for example, measurement of some property of a new substance, usually has high probability of success but modest benefits. Other experiments are long shots; the probability of success is low but the benefits, in the event of success, could be enormous. A search for the magnetic monopole is an example. Occasionally the estimate of benefit is wrong, as when a dramatic and unexpected property emerges in a routine search. The discovery of superconductivity

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*The industrial component cannot be assumed to act as ballast, rising when the federal support decreases and decreasing as the federal support rises. In fact, the trend over the past 20 years has been for the two to move in approximate synchronism. Whether this pattern will remain valid over the next five years is uncertain. It seems more probable, however, that a further decline in federal support would be accompanied by a decline in industrial support, particularly of basic research.

was of this character. Thus some balance between work of various degrees of risk is necessary.

The process of estimating the factors in this equation is quite unscientific; it is little more than the exercise of informed judgment. Choices between various lines of research must be made, however, and, contrary to a common opinion, the process of setting priorities in research goes on continually and at many levels simultaneously. In basic research the choice usually starts with the individual scientist, who decides on a problem that interests him and holds the potential of rewarding his time and effort. In the last analysis, the success of his work in the scientific marketplace, from which his scientific reputation is formed, is his greatest motivating factor. Next he must persuade students or colleagues, as necessary, to share his enthusiasm for the proposed work. Then he must persuade a funding agency, its program director, and referees or evaluators of proposals, or his division head, research director, and their advisers, to make sufficient funds available. These persons must balance competing proposals for limited funds, which they have obtained by making sufficiently persuasive arguments about their research programs. The entire process is repeated on a regular basis, usually annually and sometimes more frequently, at which junctures increases or decreases of funds are made, depending on the success of the work. In many respects, the criteria for choice can be summed up in the word excellence. Work that has this quality must have priority. Relevance alone, on the other hand, is an inadequate criterion (see especially the discussions in Chapter 3). These rather obvious facts are mentioned because too often today the plea is heard that scientists must make choices, as if they were not already making choices as an almost daily routine.

It is the Panel's judgment that this process should continue to be the means by which most of the priorities in this subfield are set and that the process should not be augmented by a major new review process except in the occasional instances in which a large new program is proposed, for example, the creation of the Advanced Research Projects Agency Interdisciplinary Laboratories. At such a time, the advice of a knowledgeable and broadly representative group, which can view the entire subfield of condensed matter and judge the probable benefit of the proposed new effort in the light of the criteria described above, should be sought. There are existing panels (such as the Solid State Sciences Panel of the National Research Council) that can perform this function, and *ad hoc* panels can always be formed.

In some fields of physics that develop around large projects, the priorities involve mainly the ordering of the limited number of feasible projects. In a field such as condensed matter, in which there are only a few rela-

tively large projects and the bulk of the expenditures are for many small efforts not classifiable in any simple or foreseeable way, the setting of priorities among subdivisions of the field is a much more difficult problem. This is the basis of the Panel's recommendation to the Physics Survey Committee that, in ordinary circumstances, the setting of priorities in such a field is more suitably done by the complex of processes already described, which starts with the individual scientist, than by the convening of committees of experts.

In support of this recommendation, it is useful to have an independent assessment of how well the existing process for making choices has worked. Within the present level of funding are there parts of the subfield that are *too well supported and other parts that are undersupported? Is there a* significant amount of work being supported that is of poor quality? A study has recently been conducted by Conyers Herring, which throws light on these questions. Herring made a random selection of papers in condensed matter from U.S. institutions from the listings in *Physics Abstracts* in 1969. Each paper was then graded by independent experts in the subfield of the paper according to its impact on progress in that subfield. The rating scale had five grades, as follows:

-1 = a setback to the field
 0 = little or no impact on the field
 1 = noticeable positive impact but of a routine nature or of limited
    duration
 2 = substantial value and longer lasting impact
 3 = truly outstanding impact—a major advance

No paper was rated by any of its authors or by colleagues of any author. The papers were divided into 11 subdivisions representing a major part, though not all, of condensed-matter physics. The subdivisions were defined as follows:

1. *Crystallography.* Crystallography and atomic arrangements including structures of liquids and glasses but excluding work shown under the definitions surfaces and neutron physics.

2. *Luminescence* Luminescence and other optical properties of solid insulators and other work concerned with the electronic level structure of impurities and defects in insulators but excluding work following under the definitions quantum optics, high magnetic fields, and semiconductors.

3. *Defects* Nonelectronic aspects of defects and impurities in solids, including diffusion, ionic conduction, plasticity, acoustics, and dielectric

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relaxation due to defects and impurities, and the like. Excludes work following under the definitions surfaces and crystallography.

4. *Surfaces* Surface physics, including structure, adsorption and migration, crystal growth, contact potentials, electron emission, catalysis, and the like.

5. *Semiconductors* Electronic conduction and all other electronic and optical properties of semiconductors (roughly defined as matter with bandgaps in the visible or infrared). Excludes work following under the definition quantum optics or high magnetic fields.

6. *Metals* Electronic properties of metals (solid or molten), including superconductors and optical properties. Excludes work following under the definition magnetism, quantum optics, high magnetic fields, and neutron physics.

7. *Magnetism.* Magnetic properties of matter, including electron paramagnetic resonance, nuclear magnetic resonance, and ferromagnetic resonance. Excludes work on optical resonances in metals and work following under the definitions high magnetic fields and neutron physics.

8. *Quantum Optics* Masers and lasers using condensed matter, and interaction of maser and laser radiation with condensed matter.

9. *Superfluidity* Superfluid properties of liquid helium.

10. *High Magnetic Fields* Experiments using magnetic fields above 20 kOe.

11. *Neutron Physics* Use of slow neutrons for studies of condensed matter.

It will be noted that the subdivisions are not of equal size, and some are unconventional. The last two were distinguished as separate fields of endeavor in order to have two examples of areas dominated by large and expensive equipment.

The results of the rating process appear in Table IV.10. The table shows that the range of ratings in the subdivisions is relatively small, from metals at 1.03 to neutron physics at 1.33. Only one subdivision shows a significant number of −1 or 0 ratings. Apparently this particular field has experienced an excess of theoretical papers of low importance, which accounts for the result. The rating process used here does not give prominence to potential for technological benefit; in another terminology, it is primarily an "intensive" rather than an "extensive" rating. Thus a full assessment of the relative value of each subdivision cannot be achieved by the use of this approach alone. However, each of these subdivisions has important potential application, and among those at the lower end of the ratings, surfaces, metals, and defects are particularly strong in this regard.

Of the 432 papers that were rated, only 34 had ratings averaging below

TABLE IV.10   Results of Ratings of Papers in the Physics of Condensed Matter

Subdivision	Number of Papers Rated	Average Rating of Papers	Number of Papers Rated 0 or −1 by Any Author
1. Crystallography	52	1.10	1
2. Luminescence	18	1.25	0
3. Defects	70	1.05	6
4. Surfaces	63	1.11	4
5. Semiconductors	24	1.31	0
6. Metals	88	1.03	15
7. Magnetism	32	1.22	1
8. Quantum optics	30	1.13	4
9. Superfluids	20	1.25	3
10. High magnetic fields	23	1.20	0
11. Neutron physics	12	1.33	0

1 and in which at least one evaluator had awarded a 0 or −1. On the other hand, 143 had average ratings greater than 1 and had been given a rating of 2 or 3 by at least one grader.

From this finding we conclude that the work being published by U.S. institutions in late 1968 and early 1969 in the physics of condensed matter was generally of good scientific quality; only a very small fraction was of low quality. Moreover, there are not major differences in quality among a wide selection of its subdivisions. From this study it does not appear that there are grounds for imposing from above, so to speak, a redistribution of funds within the subfield. Thus the complex processes by which priority choices are now being made within this subfield seem to be working relatively well.

A rising level of funding would allow this process to operate in a reasonable way, because the better claimants and a modest number of the larger projects in the field—reactors, special laboratories, and the like—could be supported. With level funding, a number of difficult choices among various desirable projects would be required. With declining funding of the sort hypothesized, many, if not most, of the larger projects and many smaller efforts would have to be discontinued, and only the most extraordinary work would receive adequate funds.

It is the Panel's opinion that in all of these eventualities the existing complex mechanism for deciding on the distribution of funds in this subfield (augmented as needed by special reviews of large new projects) would be adequate over the five-year period contemplated. The Panel believes that the existing distribution of funds over the subfield is reasonable. However, if support is expected to decline at the postulated rate of 7.5 percent

per year for any period longer than five years, it would be desirable to ask
a representative body of experts to monitor the field before the end of the
period and to recommend the best use of the remaining resources. Such a
sharp decline in support would eventually disrupt existing mechanisms for
making rational choices.

9.3.2 Effects of Various Funding Levels on the Use of Highly Qualified Manpower

The hypothesized rising budget would allow most of the well-qualified
scientists who are interested in the subfield and those who will be com-
pleting their graduate training to be usefully occupied during the next five
years. This situation would assure the vigor of the subfield. In the opinion
of the Panel, the only effective limit on the rate at which progress will be
made is the availability of well-qualified manpower. There is no foreseeable
time at which all useful information will have been "wrung out," nor is
there danger that the rate of progress will be limited by the rate at which
new developments can be intellectually assimilated.

Level funding (in real dollars) would allow most of those now employed
in the subfield to remain in it, but most of the students completing their
training in this period would be unable to find employment in the sub-
field. This situaton would have a detrimental effect on the health of the
physics of condensed matter. Moreover, the increasing complexity of re-
search would not be properly accommodated, and the effectiveness of
those conducting research would decline. Opportunities for increasingly
sophisticated research would be missed; some of the larger and more com-
plex facilities would be forced to cease operation and work at the more
eminent universities would be constrained, while work at the less eminent,
presumably, would be seriously curtailed.

Support that declines at the rate proposed would eliminate the oppor-
tunities for all but the most favored few among the new graduates and
would force out or render ineffective a large number of the presently es-
tablished investigators. Many of the larger facilities would have to close,
and the remainder would operate far below the level of full effectiveness.
Morale would decline. In less than five years, the subfield would present
a picture of decay.

9.3.3 Effects on U.S. Leadership

An increasing level of support would permit continued U.S. leadership
(see Chapter 6) in a substantial part of the subfield and U.S. eminence in
most of the remainder. Within five years, we estimate that level funding

would reduce the U.S. lead substantially and create many situations in which the United States would not be regarded as a serious competitor. A decreasing level of funding would undermine U.S. leadership completely, unless the rest of the world emulates or exceeds our funding constrictions, which seems unlikely.

9.3.4 *Effects on Technology and Other Branches of Science*

The arguments presented in earlier chapters suggest that vigorous effort in this subfield will continue to produce important technological benefits and to enrich other branches of science for many years. The stunting of the field by serious contraction of support will strongly limit these anticipated returns. The effects will build gradually, only being fully perceived after several years have passed.

9.3.5 *Effects on Education*

The educational impact of this branch of physics was briefly discussed in Chapter 8. Decreasing levels of support would reduce much of this impact, because students would be driven from the subfield and the general deterioration of the subfield would undercut its place in the physics curriculum. Past accomplishments could still be discussed in courses, which would be poorly attended. Much of the graduate teaching related to condensed matter would presumably shift into departments of chemistry, metallurgy, ceramics, and the like, unless these subfields were similarly constricted. The power and depth that the physics of condensed matter has derived from its place in the mainstream of physics would not be destroyed but would be diluted. Educational benefits to students in other branches of physics would be diminished.

9.3.6 *Conclusion*

From its consideration of these effects, the Panel concludes that the development of a national policy on the support of science is sorely needed. Such a policy should provide stabilization and a long-range purpose. During the early phases of a rapid expansion, the euphoria and general prosperity prevent people from giving serious attention to the long-range developments, and the inevitable slowing of the pace of development finds the system unprepared. In periods of contraction, pessimism, even despair, readily sets in and accelerates the decline. The health and vigor in a field depend not only on the absolute level of activity but also on the rate of change of that level. Fluctuating support also assures a perpetual mismatch

between student output and employment opportunities in a field, with resultant waste and demoralization. Modest growth that persists over a substantial period and can be relied on would produce a healthier situation than cycles of "boom and bust" that lead also to a greater total expenditure.

We hope that this period of retrenchment will result in a national commitment to a more balanced and rational policy for the future support of science.

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Appendix A: Current Topics of Investigation That Promise Substantial Changes or Advances in the Conceptual Foundations of the Field

The following is a list of general research areas and specific topics of current scientific interest in the physics of condensed matter. Some of the topics are in areas that are currently the concern of a large number of physicists and in which major scientific breakthroughs appear likely. Surface physics and the physics of disordered systems are in this category. Other topics in the list are being studied by fewer scientists but have pro-

gressed to a point at which important advances in understanding are regarded as imminent. Some of these advances will be brought about by the power of new methods of computation or new instrumentation and by generally increased sophistication in research. For example, throughout the field there is a tendency toward the synthesis, categorization, and use of more complex materials—crystals with a large number of atoms per unit cell—and disordered as well as ordered systems, including amorphous materials and glasses, higher compounds, and systems with complex microstructure. New methods and a greater background of knowledge make it possible to accomplish things today that were unheard of a few years ago.

It should be stressed that the list is a selective one. Topics currently under investigation by condensed-matter physicists have been omitted, not because they are unimportant or uninteresting but because, in the view of the Panel, they offer less promise of important breakthroughs than the areas listed. If financial limitations should force choices, the Panel believes that the topics contained in this list should receive highest priority. No attempt has been made to order the list or to establish priorities among its items. Obviously it is impossible to be all-inclusive, and undoubtedly there will be important developments in the coming years that are not anticipated. Moreover, the priorities implied by such a list will require re-examination at frequent intervals. (See also the discussion in Chapter 9.)

A.1 SURFACES AND INTERFACES

Rapid advances are taking place in the characterization of the surfaces of solids, largely using new experimental techniques and the improved theoretical understanding of quantum mechanics as applied to interfaces. By characterization is meant a detailed specification of the chemical identity, geometrical position, and, ultimately, the electronic and vibronic structure of atoms at surfaces. The electronic, geometrical, and chemical behavior of defects and impurities adsorbed at the surface is another aspect of this problem. Also included in this category are the phenomena of tunneling in metal–insulator–metal, metal–insulator–semiconductor, and metal–insulator structures. Because of new experimental techniques such as Auger spectroscopy, advanced electron microscopy, and ultrahigh vacuums, rapid progress is being made. Entirely new methods for the control and preparation of interfaces have emerged from the vast amount of engineering and development carried out in recent years to produce integrated circuitry and other devices. The present rate of growth in this area offers the possibility of advanced understanding and control of chemisorption and catalysis.

A.2 OPTICAL PROPERTIES OF SOLIDS

Revolutionary changes in optical technology have taken place in recent years with the discovery of both gas and solid-state lasers. Optical materials have been tailored to meet new demands. Examples include the growth of various crystals for harmonic generation and for modulation and deflection of light beams; the development of new materials for sources, detectors, and optical elements in the infrared as well as in the visible; junction luminescent devices; and developments in dielectric coating and thin-film technology. Here as in other areas, rapid innovation has been aided by an understanding of the concepts involved and by a detailed knowledge of the response of solids to electromagnetic radiation. The development of this conceptual understanding will continue over the coming years and may in fact be accelerated because of new tools and techniques. For example, the picosecond pulse capabilities of a mode-locked laser can be used to investigate the excited states of luminescent systems in a regime of time approaching the period of a lattice vibration. This development opens an entirely new range of experimental possibilities. The use of continuously tunable laser sources will greatly affect spectroscopy by allowing greater resolution and ease of operation. Higher monochromatic intensities will permit strain or field-modulation experiments that are now difficult. Along somewhat different lines, junction and photoluminescence will be investigated in a wider range of III–V, and I–VII compounds. The development of efficient ternary or quaternary junction light sources *throughout* the visible region will be of immense practical value. Fundamental work may hasten such developments. Further advances must be made in the understanding of exciton (coupled electron–hole) phenomena, especially in disordered solids. Experiments on exciton luminescence and exciton absorption will be performed in a wider variety of materials and configurations. Included under new developments here are broad understanding of exciton–phonon, exciton–magnon, and exciton–impurity interactions. Exciton fusion and exciton fission, as well as the contrast between triplet and singlet exciton configurations will be further studied, especially in organic crystals. Finally, knowledge of the optical response of matter will be extended into the soft x-ray region and beyond. Spectroscopy in the range of quantum energies from ten to several hundred electron volts will benefit from the use of synchrotron radiation and possibly from the development of other intense sources. The advent of wide-bandgap lasers employing the condensed rare gases (or mixtures of these gases) might be a significant development here. Recent work with continuum sources has shown that a great deal of structure occurs in the spectra of solids at, and beyond, the thresholds corresponding to the

excitation of core electrons. In absorption, such structure is useful in revealing density of states in the conduction band; in emission, it can tell something about the occupied or valence band densities. Such work will be extended to a wider variety of materials, and the relation between chemical shifts and valence will be clarified. Band structure, collective effects, and atomic processes beyond threshold will be explored. Detailed work on both photoinduced and electron-induced luminescence should be extended farther into the high-energy region. More intense monochromatic radiation sources might be developed, either by dispersing synchrotron or x-ray tube radiation, by development of a periodic bremsstrahlung source, or possibly by inverse Compton scattering of laser photons. Such sources would offer a number of interesting possibilities. In the range of quantum energies of a few hundred electron volts, they would permit electron spectroscopy for chemical analysis (ESCA) to be carried out with greatly increased resolution. At higher energies, up to 100 keV, they might permit the direct and routine determination of the momentum distributions of electrons in solids and molecules by means of Compton scattering, a technique that already shows great promise. The mixing of intense optical radiation and x rays to produce Bragg scattering by induced electric dipoles distributed in the solid is another possibility.

A.3 COMPLEX CRYSTALLINE SUBSTANCES

Condensed matter includes all substances whose atoms are so closely packed together that the interaction among atoms plays an important role in determining physical properties. Certainly the largest class of such substances is that of crystalline solids. The simpler crystals, elements, and compounds with only a few atoms per unit cell, have traditionally been divided into good metals, ionic insulators, and covalent semiconductors; however, not all useful solids fall into these categories. Today, largely in response to the demands of technology, many complex crystalline substances are being synthesized. These include anisotropic crystals, crystals with many atoms per unit cell, ternary and quaternary and higher compounds, rare-earth and transition-metal compounds, and solids containing controlled amounts of impurity. Whether some of these substances are metals, semiconductors, or insulators depends on temperature and pressure. Some are diamagnetic, others undergo phase transitions and change with temperature from paramagnetic to ferromagnetic or to antiferromagnetic. Therefore, possibilities exist for fundamental studies, both experimental and theoretical, on an increasingly rich variety of complex materials. The knowledge gained from such studies should eventually help

in choosing or synthesizing new materials when the need arises. A knowl-
edge of the band structure of a crystal together with trends through similar
compounds is especially important, since in principle the electrical, mag-
netic, optical, and cohesive properties follow from the band structure. The
band theory of solids has now advanced to the point at which the excited
as well as ground electronic states can be correctly evaluated, including
important many-body effects. One expects that some of the newer band
theoretic methods will be extended to more complex solids, including
anisotropic substances and alloys. There is much to be done, since out-
standing problems still exist even in common transition metal compounds
such as NiO. Here the conduction process and the metal–insulator transi-
tion are not yet fully understood. Progress along these lines should apply
to a large number of more complex crystals, among which are important
compounds such as the ferrites and garnets and other magnetic semicon-
ductors such as the chalcogenide spinels. There are also many interesting
layered compounds, especially among the transition metal dichalcogenides,
with electrical properties ranging from superconducting to insulating. A
number of organometallic crystals are derived from this group, and in some
cases the properties of electrons constrained to flow in two dimensions
can be studied. Here new mechanisms of superconductivity are sought,
such as might involve the interaction between electrons and organic mole-
cules. Liquid crystals, which are mainly organic substances, have applica-
tions as heat or radiation sensors and in optical display devices. Critical
phenomena in liquid crystals are under investigation, including short-range
orientational order and details of the transition to the liquid crystal phase.
Structural and collective effects, as well as dynamical and hydrodynamical
phenomena, are of interest. In this list we have mentioned only a few
classes of materials, not including composites, polymers, crystals contain-
ing point defects, and a host of others, but the trend toward a detailed
understanding of more complex systems is clear.

A.4 DISORDERED MATERIALS

Increasing attention is being given to noncrystalline and disordered ma-
terials. These include glasses, amorphous elements and compounds, alloys
(both amorphous and crystalline), and liquid metals and alloys. The basic
conceptual framework of the electron theory of solids is being extended
to include disordered materials, where interest centers on the interplay of
disorder and dynamical interaction. Optical and electrical properties as
they are affected by disorder are under investigation. Examples of interest-
ing and potentially important phenomena are threshold switching and

memory in semiconducting glasses; the metal–semiconductor transition, the effects of concentration fluctuations and clustering (especially in magnetic alloys), and trapping and certain other phenomena exhibited by electrons in insulating liquids and gases. Activity in the area of disordered solids is likely to continue to increase as new materials are discovered. Many new amorphous substances can now be synthesized by vapor deposition, ion bombardment, splat-cooling, and the like. Experiments and recent theoretical developments suggest that the characteristic features and tendencies of the disordered state can be understood in as much detail as can the crystalline state.

   Considerable attention has been devoted over the years to point defects at low concentrations and their effects on properties of crystals. More recently, problems of high-defect concentrations and defect interactions are being attacked theoretically, particularly by statistical-mechanical techniques. Experimentally, such interactions are being studied in nonstoichiometric compounds, in which ordered defect structures, or the formation of extended defects, are observed. Such studies involve many materials of technological interest. In view of the widening range of techniques being applied to these problems, it seems likely that substantial progress in understanding such materials will soon be forthcoming.

A.5 ELECTRONS, PHONONS, AND OTHER ELEMENTARY EXCITATIONS IN SOLIDS

The motion of free carriers in solids and the interaction of such carriers with each other and with other elementary excitations (phonons, photons, and spin waves) offer rich ground for discovery and innovation. As one example, work on bulk negative conductance observed in GaAs has led to useful applications such as Gunn effect and LSA oscillators and amplifiers. More recently, many-body effects in solid-state plasmas are beginning to emerge, and these are certain to lead to a variety of new phenomena in metals as well as in semiconductors. Helicon wave propagation in high magnetic fields and plasma instabilities that arise out of strongly nonequilibrium conditions are examples. These phenomena, as well as their applications, are rich and varied and probably will become increasingly important.

   Turning from electrons to phonons, the modes of vibration of crystal lattices (lattice dynamics) continue to be an active area of research. In part, this activity results from the many interesting new materials (both pure crystals and crystals containing defects) available for study and, in part, from the important role that lattice vibrations play in a wide variety

of solid-state phenomena, for example, superconductivity, ferroelectricity, and antiferroelectricity. There also have been significant improvements in experimental techniques for determining vibrational spectra, for example, by inelastic neutron scattering, Raman scattering of laser light, and electron tunneling spectroscopy. Progress can be expected in the theoretical understanding of dynamical properties and in quantitative calculations of impurities and other defects. Many-body phenomena in phonon propagation are important, especially in relation to the phonon spectroscopy of semiconductors, which is currently under extensive investigation. Ultrasonic effects, including the generation and propagation of surface waves, are another aspect of these problems.

In insulating crystals and semiconductors the polaron or coupled electron–phonon "particle" is of interest. The electrical and optical properties of polarons will be studied in a wider variety of crystals, with potential applications to the behavior of hot electrons and the understanding of dielectric breakdown in insulators. The dynamical interactions of polarons in systems other than solids—for example, electron cavities in liquids—are under investigation.

A.6 CHANNELING, BLOCKING, AND RELATED PHENOMENA

These phenomena are being productively applied to studies of lattice structures and lattice defects. They can provide insight into radiation damage and annealing of radiation damage. Applications to nuclear physics (for example, measurements of very short lifetimes) are also being found. The related technique of ion implantation is useful for the control of semiconductors and the production of specialized semiconductor devices. Further work is needed since new materials may be generated by channeling or ion implantation. The physics of energy loss by fast charged particles in matter, which is an old but important subject, is now being studied in radically greater detail with these phenomena. A better understanding of the steering of ions and electrons by the rows and planes of a crystal is developing. It is conceivable that application to devices will follow.

A.7 LOW-TEMPERATURE PHENOMENA AND SUPERCONDUCTIVITY

Many would argue that the frontiers of solid-state physics lie at the extremes of low temperature. Whether or not one subscribes to this point of view, it is true that the study of the millidegree (less than 0.01 K) range of

temperature is beginning to develop rapidly with use of the He_3–He_4 dilution refrigerator combined with adiabatic demagnetization. The discovery of new phase transitions and novel collective phenomena are certain to follow. Magnetic impurity effects and very low superconducting transition temperatures are two areas for investigation. Already the transition temperatures of certain substances are predicted with assurance in the submillidegree range. In fact, throughout the low-temperature range, the search for new superconducting materials, compounds, and alloys will be broadened and further systematized. Better understanding of the upper limits on transition temperatures is needed. Further study will be made on the space and time variation of the order parameter of superconductivity, including fluctuation phenomena. The knowledge gained from such study will apply toward understanding the motion of magnetic flux in practical superconducting materials and in the control of alternating current losses. Along somewhat different lines, investigations on liquid and solid helium have revealed many fascinating phenomena, for example, vortices, bubbles (around electrons), and snowballs (formed on ions) in liquid helium. The elucidation of the interactions between elementary excitations in liquid helium is of great scientific interest. Bound roton pairs have been shown to exist, and the interaction between such excitations arises, at least in part, because of the exchange of phonons. A close analogy can be drawn here with exchange interactions in high-energy particle physics.

A.8 EXTREME STATES OF MATTER. APPLICATIONS OF CONDENSED-MATTER PHYSICS TO ASTROPHYSICS

Many of the principles of the theory of condensed matter at extreme low temperature can be extrapolated and applied to neutron stars and to other problems in astrophysics. This is especially true of the theories of superconductivity and of quantum fluids, which may apply to the dense states of matter in white dwarfs and neutron stars. These advances could lead to a much better overall view of stellar evolution. In fact, the understanding of unusual states of condensed matter, of superfluids, and of matter at extremes of both temperature and pressure has wide application.

There are certain branches of experimental condensed-matter physics that have found application in astronomy. For example, the development of very sensitive quantum detectors has benefited from studies of electron emission from solid surfaces. Work along these lines will continue. Because of solar ultraviolet and x-ray radiation, as well as the solar wind, the moon is expected to have a sheath of electrons around it. The understanding of such phenomena should benefit from studies of the optical response and

photoemissive properties of lunar samples (also planetary or other samples from space). Along somewhat different lines, laboratory work in the extreme ultraviolet is needed in conection with the discovery of stellar x-ray sources and mapping of the soft x-ray background radiation. Accurate experimental absorption cross sections are required for the most abundant elements in interstellar space, both in gaseous and aggregated form. The condensation of matter under such extreme conditions could have implications for the understanding of the origin of life in the universe.

Appendix B: Fundamental Topics in the Physics of Condensed Matter That Have Led to Technological Advances

A number of recent advances in rapidly developing technologies that depended on fundamental discoveries in the physics of condensed matter are listed below (second column). The fundamental discoveries in the physics of condensed matter that led to these advances appear in the first column. The examples in this Appendix are intended to be illustrative; the list is not complete.

It must be recognized that many engineering accomplishments, abetted by scientific understanding, also have been involved in these advances, and the relative roles of basic science, applied science, and engineering are different in each example. (Case studies of selected innovations appear in Chapter 3.)

Early Discoveries	*Present Technological Innovations*
1. Transistor effect (Schockley, Bardeen, and Brattain, 1947); diffusion, epitaxy, physics of planar configurations	Silicon planar technology and large-scale integrated circuitry (LSI); relation of LSI to semiconductor transport diffusion masking, high-field transport. Computer selection and control in manufacture of LSI
2. Junction electroluminescence; physics of III–V and II–VI compounds (Hopfield and Thomas, 1962–1968)	GaAsP indicator lamps, other junction luminescent devices, panel displays, and numeric indicators
3. Impurity activated photoluminescence and cathodoluminescence; infrared	Improved phosphorluminescence and cathodoluminescence materials.

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Early Discoveries	*Present Technological Innovations*
phosphors; control of materials achieved during World War II and the following years	Rare-earth phosphors for color television. Phosphors that convert infrared to visible
4. The laser (Townes, Basov, and Prokhorov, 1958); Q-switching, mode locking, the organic dye-tunable laser, the junction laser (Hall and Nathan, 1962)	Lasers and application of nonlinear optics. Raman spectroscopy sources; tunable sources for optical instrumentation; new ranging and signaling devices; commercial as well as military applications; optical computer memories; application in materials processing; metrology
5. Secondary emission multiplier (Zworykin, Morton, and Malter, 1936); new high-efficiency photodetectors involving the group III–V semiconductor surfaces	A variety of new quantum detectors for astrophysics and particle physics; Channeltron detectors and arrays; the x-ray intensifier for medical and other applications; other new image-intensifier devices
6. Superconductivity at higher transition temperatures (Ziegler, Hulm, and Matthias, 1947–1950); high-field superconductors (Autler and Kunzler, 1960); Josephson effect	Useful superconductors; sources of high magnetic fields; magnetometers and sensing devices; eventually power distribution, switch gear, and superconducting motors; Clarke galvanometer; logic and memory using Josephson junctions and superconducting tunneling; ultrasensitive electric and magnetic measuring devices
7. Ferromagnetic insulators (Snoek, 1946); development of various magnetic insulators and semiconductors; cylindrical bubble domains in uniaxial single crystals	Bubble memories; other new magnetic core devices; magnetooptical devices; modulation and beam switching devices
8. Physics of ternary compounds such as as mercury–cadmium telluride; manmade adjustable-bandgap semiconductors (1968)	Variable-gap infrared sources (lasers) and detectors; ultrasensitive far infrared devices that bracket the spectrum from the visible to microwaves
9. Transport properties and negative differential conductivity in GaAs (Gunn, 1963); impact ionization and Si avalanche device (1965)	Gunn effect and avalanche-transit time devices as solid-state microwave sources, and higher-speed semiconductor devices, miniature radars, collision avoidance systems, and communication systems
10. Fundamental discoveries in polymers, crystallization, morphology, and the	A host of new materials having a variety of properties, new rubbers and

Early Discoveries	*Present Technological Innovations*
like (1957); ideas relating to defects such as dislocations	shock-resistant materials, low- and high-temperature rubbers, high-strength composites
11. Fundamental studies of radiation damage, beginning with the Manhattan Project and continuing at the AEC National Laboratories and elsewhere over the last 25 years	New reactor materials, corrosion- and radiation-resistant cladding for fuel elements, radiation-resistant materials for space and military purposes, improved solar cells
12. Fundamental studies on strength of materials under both normal and extreme conditions (temperature and pressure) and on alloy phase transitions	Vanadium, zirconium, and niobium metal technology, with applications to reactors, to superconductors, and to high-temperature and high-strength materials; titanium metal technology for airframe construction; single crystalline turbine blades
13. Investigations of structure of membranes; means of making various new membranes	Materials for medical and biological purposes; man-made semipermeable membranes for artificial organs and chemical separation
14. Fundamental studies of diffusion and ionic conductivity in new materials	Solid-state electrolytes, calcium-stabilized zirconia, sodium-doped alumina, rubidium silver iodide, and the like, which provide new means for compact electrical energy storage
15. Fundamental studies on dislocations, point defects, diffusion, and annealing	New steels and alloys, and achievement of a measure of control over defects and dislocations in metals and ceramics. Dislocation-free semiconductors
16. Fundamental studies in silver halide photography, dye sensitization, and the like. Gurney-Mott theory of latent image (1938); electrophotography with sulfur (C. Carlson, 1937), studies of amorphous selenium	Modern photographic emulsions, both black and white and color; the diffusion transfer process; xerography and the office copier

Appendix C: Response to the Survey

The survey described in this panel report was conducted by members of this Panel, with the cooperation of the U.S. Department of State.

The breakdown of the replies to the survey by country is as follows: Britain, 1; Canada, 1; Denmark, 1; France, 1; Germany, 4; Israel, 3; Italy, 2; Japan, 2; Sweden 5. Four of the responses were from individuals in governmental positions, four from industrial laboratories, and the rest from the academic community (professors and department heads). Three of the replies indicated that the survey questions were posed to "a panel or group of physicists." Two others represented "an interview with two professors" and a joint reply of two professors, respectively. Several indicated that their replies were based on "recent interviews" or "discussions with scientists." Thus, we feel that the replies cover a broader consensus than the actual numbers would suggest.

No information concerning the Soviet Bloc countries was obtained. Although only two replies were directly received from Japan, there is currently an explosion of articles on Japan's economic and technological activities. Likewise, much has been written recently concerning England.

Thus, Israel, Japan, and Western Europe are the world referred to in this report. This, of course, is also the world with which we have strong technological ties. The technological and scientific ties with Canada are so strong as to present a different set of conditions.

V
Optics

PANEL MEMBERS

W. LEWIS HYDE, New York University, *Chairman*
J. G. ATWOOD, The Perkin–Elmer Corporation
HERBERT P. BROIDA, University of California, Santa Barbara
PETER FRANKEN, University of Michigan
ARTHUR SCHAWLOW, Stanford University
ELIAS SNITZER, American Optical Corporation

CONTRIBUTOR

JARUS W. QUINN, Optical Society of America

LIAISON REPRESENTATIVES

JOHN D. MANGUS, National Aeronautics and Space Administration
ROLF M. SINCLAIR, National Science Foundation

READERS

JOHN A. ARMSTRONG, International Business Machines Corporation
JOSEPH W. GOODMAN, Stanford University
JOHN NELSON HOWARD, Air Force Cambridge Research Laboratories
CHARLES J. KOESTER, American Optical Corporation
ROBERT V. POLE, International Business Machines Corporation
RODERIC M. SCOTT, The Perkin–Elmer Corporation
F. DOW SMITH, Itek Corporation

560

Contents

V

OPTICS

1   The Nature of the Subfield

Optics is a basic and applied subfield of physics traditionally divided into physical optics, geometrical optics, and physiological optics. Although the major emphasis has always been on visible light, optics usually is generalized to include the techniques and phenomena of electromagnetic radiation extending from the far ultraviolet to the far infrared and occasionally to x rays, microwaves, and even electron optics. Classical nineteenth century optics emphasized optical instruments such as the microscope and telescope, visual phenomena such as the sensation of color, and physical optics of interference, diffraction, spectroscopy, polarized light, and crystals. Modern optics has added a tremendous number of intricate new phenomena produced by the interaction of light with matter. Holography, photon counting, the biochemistry of vision, photoconduction, light-emitting diodes, photoemission, and the laser are examples. Modern optical instruments include luminescent display panels, image intensifiers, electrooptic light modulators, tunable lasers, guided waves, amplifying fibers, pulse compressors, image-enhancement systems, tracking devices, and combinations of these and other new tools.

Optics has also enlarged in another direction—radiation studies. The emission of radiation from hot gases, its transmission through the atmosphere, and its eventual absorption and reradiation have become major fields of study. It is important for microclimatology, for understanding photosynthetic processes, for the heat balance of the oceans and of the

earth itself, for weapons research, for the study of atmospheric pollutants, for chemical kinetics and combustion processes, to name but a few examples.

Throughout the nineteenth century, optics was a central subject of physics and commanded the attention of the greatest physicists of the time, but the skill and insight of these physicists seemingly exhausted the topics to which they addressed themselves. For example, the Abbe theory of the microscope established a clear-cut limit to the resolving power of the instrument and, since commercially available instruments reached that limit, the subject was seemingly closed. The one major exception in optics—the subject that was not exhausted—was, of course, atomic and molecular spectroscopy. This subject, which still challenges many physicists, now constitutes a separate body of knowledge not usually treated as a branch of optics though still very closely related to it.

Under these circumstances, why do we find a flourishing Optical Society and a panel of the Physics Survey Committee devoted to optics in 1970? The example of microscopy gives some insight. As optics matured as a branch of physics, the emphasis gradually shifted to applications of its findings, and optics became an applied science. A few physicists remained interested in the subject, and their work revealed problems and possibilities of great fundamental significance. Abbe and Rayleigh had assumed that the purpose of the microscope was to see small opaque or self-luminous objects, thus they defined the figure of merit according to this purpose. Zernike chose a different figure of merit—the ability to see small transparent living objects. He discovered the phase microscope, a simple method of great subtlety, and won a Nobel Prize.

The pattern continues today. The majority of optical scientists currently are addressing themselves to practical problems in optical engineering or applied optics, but the questions that arise frequently challenge our understanding of the basic physics, demonstrating that there are not only unanswered questions but often that the classical questions were not the correct ones.

Spectrographic instruments provide an important contemporary example. Every physics student is taught something about wavelength resolving power of a spectrometer and shown how it depends on the properties of the dispersing element, but the arguments that he hears do not deal with the time devoted to the observation or the signal-to-noise ratio. The modern Fourier-transform spectrometer, built by Pierre and Janine Connes, has produced planetary spectra with 100 times greater resolving power and 10 times better signal-to-noise ratio than the best prism spectrometers. Much of the Connes' success arises, of course, from their technical skill, but the underlying physics is beautiful and profound. Their new atlas of plane-

tary spectra shows more detail in the absorption spectrum of the atmosphere of Venus than is shown in corresponding spectra of the earth's atmosphere that have been obtained with prism spectrometers (so-called solar atlases).

2   Recent Scientific Advances and Applications

Although optics is now predominantly an applied science, we will look first at some of the recent important advances in fundamental optical physics.

2.1   LASERS

By far the most important recent development is the laser and the new devices and techniques that it has made possible. The invention of the laser can be credited to atomic and molecular physicists, but optical specialists played a basic role from the beginning and have benefited remarkably from its development. In particular, the availability of copious coherent light has made possible a wide variety of new optical techniques such as the hologram and related image-processing methodology, and the high intensity of lasers has led to the development of the exciting new field of nonlinear optics.

Currently, there is no apparent slowing of the pace of development associated with the laser and laser devices. Laser techniques are being extended into new wavelength regions, lasers are being made tunable to different wavelengths, and new techniques are being developed for deflecting and modulating laser beams and for producing and controlling extremely short pulses of light.

A wide variety of special situations has resulted from the use of this new tool. The ability to produce ultrashort pulses of light, for example, has opened the time domain in chemical kinetics for investigation in a way that was impossible with more conventional sources; now the sequence of events can be unraveled and followed. The same very short pulses can produce fantastically high-power densities and field strength—so high that even nuclear forces may be affected. A completely new tool thus becomes available for studies of nuclear fusion as well as for other purposes. Eventually, these high photon densities may allow the direct experimental ob-

servation of the scattering of light by light; however, a vacuum more perfect than any now obtainable will be necessary.

The frequency stability of even a simple laser gives light of quite remarkable purity, but stabilized lasers can be built whose frequency is fixed to one part in 10^{11}. The application of this stability to phenomena now thought to be well understood will certainly result in surprises. Meanwhile, even much simpler lasers in unequal path interferometers allow measurement of strain, tilt, and shear in the earth so that earthquakes can be studied and perhaps predicted with new accuracy.

2.2 HOLOGRAPHY

A particularly significant development arising from the laser is the invention in 1962, by Leith and Upatnieks, of the modern hologram. Gabor had formulated the conceptual base for the hologram in 1949, but it was not until coherent light was available in copious supply that large-scale, intense holographic displays became possible. Our theoretical understanding of the nature of optical images has been challenged by the hologram, and a wide variety of new experimental techniques has become necessary. Throughout this exciting decade of development of the hologram, the optics community has been stimulated constantly by the prospect of valuable useful devices, with the result that very substantial investments of money and manpower have poured into the subfield.

The hologram is capable of preserving and reproducing an image and, within certain limits, a three-dimensional image of an extended object is possible. The image need not arise from a real object; a hologram can be produced from a computer printout. Thus it is possible to generate an image of an object that did not exist or to tabulate the sound field around an object illuminated with coherent ultrasound and to recreate an image of the object for visual examination.

In principle, holography also is useful as a means of storing very large amounts of digital data for use in a computer or other information retrieval system. In the same way that communications engineers have learned to analyze signals in either the frequency domain or the time domain, the hologram makes it possible to transform images between object and aperture spaces. It is not always clear which type of space has practical advantages over the other, but the analysis of the hologram has increased our understanding of information storage systems and offered new possibilities for extending their scope.

The scope of image storage may be increased in another respect as

"photochromic" materials with improved properties become available. With such materials, it is possible to generate and record an image by using one wavelength of light, to read it or extract information from it by using a second wavelength, and, finally, to erase it with a third wavelength or the application of heat.

Holographic interferometry has begun to have substantial application in engineering. If a hologram is made of an object and compared subsequently with the real object, any change in the object can be seen as an interference pattern spread over the surface. Thus the elastic modes of vibration of complex objects can be determined readily or strain patterns can be studied to reveal faults in solid objects.

Another development arising from holography is the holographically produced diffraction grating. During the past two decades, there was a steady development of techniques of interferometry to produce improved diffraction gratings for both terrestrial and astronomical spectroscopy. Interferometer control was applied to engines for ruling very large gratings of high quality and accurate groove spacings capable of producing high-resolution spectra nearly free of ghosts. For the finer groove spacings, it is now possible to produce diffraction gratings by high-resolution photographic techniques of the kind developed for holography. Plane gratings with straight-line rulings have been made with nearly ideal resolution and, unlike the ruled gratings, they can diffract most of the light into a single order, since they contain multiple reflecting layers within the thickness of the emulsion. Moreover, holographic techniques can produce more complex rulings, such as self-focusing gratings corrected for geometrical aberrations, and they can be produced on surfaces that are not flat. The possibilities are not yet fully explored; perhaps some of them are not even recognized. However, it is established that fine-line diffraction gratings of excellent quality can be produced quickly and cheaply by holographic methods.

2.3 THIN FILMS

An important modern branch of optics is thin films. It is now possible to design and construct multilayer thin films that will reflect chosen wavelengths and transmit others. High-pass, low-pass, bandpass, or low-band filters can be built economically and reliably. One interesting class of such filters is designed to transmit only a narrow band of wavelength; when made with one or more layers tapered in thickness, the filter will transmit different wavelengths in different regions. Such wedge filters can be used

to build a particularly simple and compact spectrometer for use in space vehicles or in cheap commercial instruments.

Another important development of thin-film technology is the high-efficiency mirror, reflecting 99.6 percent to 99.8 percent of the incident light in a chosen wavelength region. The availability of such mirrors has been one of the indispensable tools of the technical development of the laser.

Another product of thin-film technology is the well-known low-reflection coating. Without such coating, complex microscope or camera lenses would produce images having only low contrast because of the veiling glare of multiple reflected light inside the barrel of the lens. With such coatings, complex high-performance lenses become possible.

Optical thin films also have played an important role in military infrared devices by making them wavelength-selective so that military targets can be distinguished from background sunlight.

2.4   PHYSIOLOGICAL OPTICS

Physiological optics has always been an important branch of optical science, and physicists still play a part, though it is now secondary to the work of psychologists and electrical engineers. As we search for deeper understanding of both the science and technology of pattern recognition, observations on the eyes of animals and humans will clarify our thinking. Mechanisms have been found in the eye of the frog that respond to certain moving patterns and not to others. Insects have been found that can detect infrared radiation. The human eye loses its response if an image is held stationary on the retina, showing that tremor is a necessary condition and not a defect of vision. New adaptive mechanisms have been found in the eye to assist in the perception of color and contrast. Thus we are gradually learning which parts of the pattern-recognition process are "hard wired" and which are "computed" in the brain. The subject is not only fascinating in its own right but may become a guide to technology.

2.5   OTHER DEVELOPMENTS AND APPLICATIONS

When light from a laser beam is scattered by electrons from an accelerator, the scattered light has a higher frequency because of the Doppler effect. If the electrons have very high energies, the scattered photons become nearly monochromatic gamma rays, with energy in the range of several

GeV, and retain the polarization of the original light. The polarized gamma-ray beam obtained in this way is nearly free of low-energy background and is ideal for many studies in particle physics.

Optical monitoring of the earth's atmosphere already has become an important tool for weather prediction with the now familiar cloud-cover pictures. In addition, and perhaps more important, the properties of the upper atmosphere can be measured and the effects of pollutants on the lower atmosphere can be determined from satellite and rocketborne spectral observations of various atoms and molecules. Satellite observational techniques using ultraviolet, visible, and infrared spectroscopy make it possible to monitor continuously the global distribution and vertical profiles of natural and man-made variations of specific chemicals in the earth's atmosphere and to have rapid knowledge of changes. Temperature profiles can also be determined. These techniques provide an immediate way in which a physicist can apply his specialized knowledge to problems of pollution and ecology.

Recent developments in frequency synthesis from the microwave to the optical region make it feasible to consider the possibility of defining length and time with the same molecular transition. These achievements coupled with the stabilities achieved by coherent laser stabilization and by saturated absorption of molecules probably will lead to the development of new and more precise standards. Frequency synthesis to a CO_2 laser transition near 10.6 μm and stabilization of this transition by saturated absorption in SF_6 have already been demonstrated. The application of similar techniques to the near infrared is anticipated in the next few months—and after that, their extension to visible frequencies.

The newly developed saturation absorption (Lamb dip) spectroscopy is capable of achieving spectral resolution exceeding one part in 10^{10}. Thus an additional powerful tool is available for studying spectral line shapes and level splittings to within natural line widths of atoms and molecules.

In the next few years, laser measurements of variations in the earth-moon distance are expected to give a new and highly accurate check of gravitational theory. Also, these data will substantially improve our knowledge of: (a) lunar size and moment of inertia, (b) the earth's rotation rate and polar rotation, and (c) the present rate of continental drift.

The impact of computers should be mentioned. Many problems in optics demand that very large quantities of data be manipulated. Image processing is one example, and Fourier-transform spectroscopy is another. It has become possible to extract important spectroscopic results out of very weak signals by computer techniques.

3 Manpower and Productivity in Optics

The applied-science character of optics led to a steady decrease in emphasis on optics in the physics departments of U.S. universities until about five years ago when the influence of the discovery of the laser began to be felt. Throughout the same period, however, the Optical Society of America continued to grow as more people found challenging opportunities in optics. In 1962, the Society established a new journal, *Applied Optics,* which now publishes 50 percent more pages annually than the *Journal of the Optical Society of America,* the older (established in 1917) and traditional journal of the subfield. Thus we have a subfield of physics that thrives today primarily through its applications, though it also has strong roots in fundamental physics.

Who are the optical scientists of today? There were some 6000 members of the Optical Society of America in 1970. The 1970 National Register of Scientific and Technical Personnel shows 3280 physicists who indicated optics as the specialty in which they were employed. Of these 3280, one third (1111) held a PhD degree and accounted for 6.7 percent of the total PhD population in physics. The number of PhD's in this subfield shows a substantial increase from the 743 included in the 1968 Register survey, a growth rate of about 25 percent per year at a time when the rate of increase of PhD's for physics as a whole was about 7 percent per year.

Of the physics participants in the 1970 Register survey, 2494 indicated membership in the Optical Society of America. Other disciplines with substantial representation in the Society are psychology, chemistry, and electrical engineering. In addition, many of the members are technicians and manufacturers of optical products.

The median age of the optical science PhD's included in the physics section of the 1970 National Register was 38.7 years—a decrease in median age from 40.2 years in 1968 and 40.3 years in 1964. These data suggest an influx of young PhD's into the subfield in recent years. More than three fourths (78.1 percent) of the optical science PhD's had obtained their degrees in physics; 15 percent held degrees in engineering.

Table V.1 presents data on the institutions in which PhD's and non-PhD's were employed and compares employment patterns for optics with those for physics as a whole. The substantially greater concentration of optical scientists in private industry as compared with the overall physics population is apparent and not unexpected in a highly applied subfield.

The principal work activity of the PhD optical scientists was applied research. More than half (54.4 percent) indicated applied research as their

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TABLE V.1   Employing Institutions of Optical Scientists and Physicists

Employing Institution	Optics PhD's $N = 1078$ (%)	Optics Non-PhD's $N = 2029$ (%)	All Optics $N = 3107$ (%)	All Physics $N = 33,927$ (%)
College or university	27.1	10.9	16.5	40.0
Industry	49.9	60.3	56.7	27.0
Government	10.1	18.7	15.7	11.8
Research center	8.0	3.1	4.8	8.2
Other (hospital, etc.)	4.9	7.0	6.3	13.0

primary or secondary work activity; 39 percent also reported major responsibilities in basic research. The non-PhD's were substantially less involved in basic research and more frequently engaged in applied research
than were the PhD's. In addition, non-PhD's were often engaged in design
and development work, and about one third of the PhD and non-PhD
groups had major managerial responsibilities. Table V.2 presents these
data and compares the work activity patterns for optics with those for all
physicists.

TABLE V.2   Work Activities of Optical Scientists and Physicists

Principal Work Activities	Optics PhD's $N = 1101$ (%)	Physics PhD's $N = 16,017$ (%)	Optics Non-PhD's $N = 2203$ (%)	Physics Non-PhD's $N = 17,001$ (%)
Basic research				
Primary	20.5	34.7	8.9	20.7
Secondary	18.5	24.9	4.8	7.2
Applied research				
Primary	28.5	16.8	33.7	20.8
Secondary	25.9	18.1	27.2	19.3
Design/development				
Primary	4.7	2.3	17.6	9.3
Secondary	11.4	6.6	28.2	14.9
Management				
Primary	22.6	16.0	21.6	16.0
Secondary	16.0	11.4	11.4	10.4
Teaching				
Primary	19.3	25.7	9.0	23.1
Secondary	9.0	18.2	2.7	7.6
Other				
Primary	2.5	2.4	7.4	8.0
Secondary	8.6	8.8	13.3	17.3
No response				
Primary	2.0	2.1	1.7	2.1
Secondary	10.6	12.1	12.4	23.3

The heavy involvement in applied research and in design and development work, especially among the optics non-PhD group, contrasts strongly with the work activity pattern for all physics PhD's and non-PhD's. The optics groups show correspondingly less involvement in basic research and teaching than is true of the overall physics population.

To obtain some indication of the production of published papers in physics from the various subfields, a sample of papers was taken from 1969 issues of *Physics Abstracts*. In a total sample of 1181 papers, 59 (5.0 percent) dealt with the subject matter of optics. Nearly half (44 percent) of these papers were produced in academic institutions, and one third (34 percent) in industrial laboratories.

A second sample from 1969 issues of *Physics Abstracts* included institutions in all parts of the world. In this sample of 1296 papers, 44 (3.4 percent) dealt with optics. Approximately equal numbers of optics papers were produced by the United States (15), Western Europe including the United Kingdom (13), and the Soviet Union (12).

4   The Nature, Loci, and Support of Research in Optics

Much of the stimulus to optics in the last two decades developed as we gained a deeper understanding of the limiting factors in many experiments and devices. Many problems were found to be "optics-limited." That is to say, the speed or accuracy with which a measurement could be made, a device controlled, an object detected, a chemical analysis completed, and the like often was limited by fundamental optical problems of intensity, resolving power, stability, or photon statistics. The search for ways to overcome these limitations led to extensive programs of applied optical physics. In addition, the astonishing attributes of the laser made it important to re-examine all optics-limited situations. For example, it is now possible to determine the Raman spectrum of a few milligrams of a water-soluble biological compound using a laser as a light source. Without the laser, only massive samples can be analyzed.

There appears to be no shortage of frontier areas in optics at this time, partly because so many situations are still optics-limited, so that each new fundamental development has a rapid influence on applied optics. Only within the past year, for instance, has the use of light-emitting diodes in visual displays become practical on a commercial scale.

The discovery that many practical devices were optics-limited led to the

establishment of strong optics groups devoted to basic and applied research in many industrial laboratories. Half of the optics PhD's and nearly two thirds of the non-PhD's work in industry. This pattern is well illustrated by the development of the ruby laser at Hughes Aircraft, the gas laser at the Bell Telephone Laboratories, and the glass laser at the American Optical Company. Exceptional work in ultrashort light pulses currently is taking place at United Aircraft, IBM Thomas J. Watson Research Center, and Bell Telephone Laboratories. Liquid crystals were developed at Westinghouse, and stabilized-frequency lasers at Perkin-Elmer.

About one fifth of the optical scientists (18 percent of the PhD's and 22 percent of the non-PhD's) work for the U.S. Government or in government-funded research laboratories. Since many of these laboratories were only recently established, they probably represent an even larger fraction of the new jobs in optics in the last decade. In addition, the equipment needs and extramural research programs of these laboratories sustain a large fraction of the industrially employed optical scientists.

The *Directory of Federal R&D Installations,* published by the National Science Foundation in June 1970, lists 486 institutions or programs located in 723 separate installations. A few are very large, for example, the Kennedy Space Center, with 22,000 employees and a budget of half a billion dollars.

The agencies and laboratories listed in the *Directory* were asked to describe their work in accordance with COSATI (Committee on Scientific and Technical Information of the Federal Council for Science and Technology) standard codes, and, although the response was inconsistent, it provided some indication of the nature of their work. For example, the code 20-06 is Physics–Optics, under which 23 index entries are found. This and other optics-related index entries are as follows:

20-06	Physics–Optics	23 entries
20-05	Physics–Masers and Lasers	18 entries
17-05	Infrared and Ultraviolet Detection	17 entries
17-08	Optical Detection	13 entires
16-02	Missile Trajectories	20 entries
14-05	Reprography (including Photography)	10 entries

A few other optical programs are found under astronomy, geodesy, atmospheric optics, solar energy, glass technology, and the like.

Scrutiny of the individual listings shows much overlap, but, after sorting, a total of 64 laboratories remain that have optical programs of some kind. These are listed in Appendix A.

There also has been a remarkable development of optical skill at gov-

ernment-sponsored nonprofit laboratories such as the Cornell Aeronautical Laboratory, the University of Michigan Institute of Science and Technology at Willow Run, and the Lincoln Laboratories at Massachusetts Institute of Technology. Most of these groups are overwhelmingly devoted to the development and operation of large special-purpose optical equipment, but many of them conduct significant fundamental studies of the underlying physics and have made important contributions, such as the dramatic development of the hologram by Leith and Upatnieks at Michigan.

Finally, a smaller part of modern optics has developed in university laboratories. For instance, nonlinear optics was discovered at Michigan and has been studied with notable skill by Bloembergen at Harvard. The case is exceptional, though, and optics has received relatively small emphasis in most physics departments. Most scientists entering the subfield have been trained primarily in some other branch of physics. However, this situation is changing rapidly. At the present time, about one fourth (27.1 percent) of the optics PhD's and one tenth (10.9 percent) of the non-PhD's work in colleges or universities.

Much of the nourishment of the roots of optical physics in the last decade resulted from the foresight and generosity of those with responsibility to solve applied problems for industry, the military, or the space program. Although a large part of contemporary optics grew from such sources, the arrangement is somewhat unstable. Congressional insistence on immediately evident applicability, as demonstrated by the Mansfield amendment (Section 203 of the fiscal year 1970 Military Procurement Act), is typical. The same kind of logic pervades nearly every industrial laboratory at times, and its effect has been felt widely in the past two years. From a national point of view, basic research in optics is a luxuriantly growing field but with shallow and unstable roots.

The Air Force recognized this situation some years ago and, influenced by Aden Meinel at the University of Arizona, established an Optical Sciences Center at that institution, with a very generous franchise to develop the science and educate the people to stimulate applied optics. The implementation of the Mansfield amendment seriously threatened the long-range stability of this valuable new national resource; future funding prospects are uncertain.

This problem is not unusual. The director of any mission-oriented laboratory or agency or the director of research in an industry is taking a personal risk of failure when he allocates resources to basic research. Worse yet, the program, even if successful, could yield benefits to others rather than to his own sponsors. A director of research can quickly exhaust his quota of good will and his freedom to take risks.

On the other hand, optics flourishes today in many situations just because its applicability is evident and defensible. The path from the fundamental to the useful generally is short in optics. Even when it is not, a plausible case can be made for potential applications.

5   Trends in Education and the Production of Manpower

The very utilitarian character of optics, which makes *Applied Optics* such a thick publication compared with the traditional *Journal of the Optical Society of America,* tended for many years to separate optics from the university environment. Only at the University of Rochester and the University of Arizona were major separate departments in optics established. Training in optics at other universities was derived largely from experience with the instrumentation aspects of some other fields such as astronomy, spectroscopy, or solid-state physics. Few scientists received extensive formal training in optics. As previously mentioned, most optical physicists were educated in other subfields of physics.

   The Panel does not view this situation with alarm. Rather, we suggest that this career pattern should be recognized as desirable and encouraged wherever possible. Optics is a branch of physics containing many concepts of great power, simplicity, and beauty. Many of these principles are demonstrated and understood most dramatically in their optical form but have rich analogues in other branches of physics, so that a student learning optics will be a better physicist for it. Further, optical concepts, instruments, and demonstrations appeal directly to the strong physical intuition of many young scientists and complement and enrich the more formal analytical training that they receive; in some cases, these ideas and procedures are possibly the only parts well understood and remembered. Many gifted young people specializing in physics are by temperament and ability better able to contribute to an applied field than to basic research. Optics typically obtains most of its applied scientists, and many of its engineers, from the ranks of physics.

   Consequently, we urge our colleagues in the physics community for the above reasons to look carefully at the optics portion of elementary-physics courses. Optics could be that part of the course that captures the imagination of some of the young people who might otherwise drift away from physics, particularly those concerned with the usefulness of their studies

or those having stronger physical intuition than analytical skill. Further, if they enter the job market with a bachelor's degree, they will be more readily employed than those holding this degree and specializing in other subfields of physics.

It has been common to offer just one semester of optics to undergraduate physics students, which is probably sufficient for the majority of the students. However, we believe that institutions with graduate programs should seriously consider offering an intermediate course in advanced physical optics and optical instruments. If such a course is offered at a convenient time of day, a substantial latent demand usually will be found among graduate students, a few undergraduates, a few colleagues and students from other fields, and a substantial number of men from local industry. The content of such a course will depend on the available teaching talent, and it might be necessary to sample the content of a number of contemporary books. The emphasis on lasers in such a course will depend on whether a separate course is available on this important subject.

Recently, the educational pattern that we have described in optics has been changing. Two factors have led to a sudden increase in the production of optical specialists. First, physics departments have begun to reemphasize optics. As recently as 1967, the American Institute of Physics register of graduate programs in physics showed only 16 departments, of 140, that listed optics as a specialty field. By 1969, this number had risen to 71 of 163 departments. In 1971, it was greater than 100. (See Appendix B for a list of physics departments offering a PhD with specialization in optics.) Second, the schools of engineering, particularly electrical engineering, have found optics worth teaching. Appendix C shows 40 universities that graduated at least one PhD in engineering with a thesis in optics in June 1970. There were at least 80 PhD's in this group. Data on a sample of thesis listings in *Dissertation Abstracts* in 1969 showed that optics accounted for 3 percent of the theses appearing in the physics section of the *Abstracts* and for 4 percent of the theses in the engineering section that might legitimately have appeared under physics.

It is important, but not easy, to understand the motivation for this movement of the educational institutions into optics. Apparently, there are two principal causes: first, the laser and quantum electronics generally are regarded as scientifically respectable; second, a new emphasis on applied science is developing in physics departments.

This trend is pleasing but alarming to optical scientists. It is not obvious that so many optical specialists are, or will be, necessary for either science-based or applied purposes. The optics community grew at the rate of 9 percent per year for a decade because new institutions and new national

goals were being established. In particular, the war in Vietnam, the space program, and the ballistic missile program made very large direct demands on optics.

In addition, the general growth of technology and the indirect needs of the military and space programs, the National Institutes of Health, the petrochemical industry, the computer industry, and others led to the establishment of a huge optical industry. The Optical Society of America listed 115 corporate associates in 1969. Wall Street viewed this growth as promising and attractive, and money was readily available through 1968 for anyone who wanted to hire a group of optical specialists and establish a business, but not all such growth was sound. Much of what the government agencies or even the industrial customers wanted was capital goods— missile-range instrumentation, spectrochemical equipment, or stereoscopic microscopes. When many of these sponsoring agencies or industries were "tooled up," they stopped buying. Furthermore, although many of the groups involved wanted an opportunity for experimentation and innovation to deal with new problems they had encountered, they either quickly built the new devices they wanted or discovered how difficult they would be to build and lost interest in the far-out ideas.

Thus we see the convergence of a number of trends simultaneously in optics, particularly in applied optics, and the prognosis is not good. More men are being trained for and entering the subfield just as the demand diminishes. For example, one company that has prospered especially from modern applied optics in relation to both commercial instruments and government research and development has shown an 18 percent decrease in sales during the final quarter of 1970 and a consequent reduction in number of personnel. Other companies and laboratories also are cutting back or curtailing hiring, not only because of decreased government spending and decreased purchases of equipment but for more subtle reasons of changing emphases and growing skepticism about the usefulness of research.

In view of these current trends, we urge our colleagues to give serious attention to the sudden change in the job situation for scientists and to make clear to their students that the pursuit of graduate studies will not be a worthwhile investment for most of them. Only the best should enroll for the PhD, and they should do so because they love science and not to get a better job. The productivity of the graduate schools must be curtailed, probably cut in half. We know of no better way than to urge physics departments to restrain their own ambitions, be honest with their students, and cut back their graduate enrollments. Control or restraint applied by some outside agency or the production of disappointed, bitter, and overtrained young men will be worse than internally applied restraint.

6   The Quality and Funding of Research in Optics

Much of the present activity in optics is caught in an unhealthy trap between basic and applied research. We suspect that this is true in other fields of applied physics. To do basic research in an applied field is just as difficult as to conduct it in more fundamental fields and presents the additional difficulty of the lack of freedom to choose the solvable problems. The hologram of Gabor and Leith and Upatnieks provoked an astonishing flurry of activity but not a corresponding flood of new concepts or useful results (although some of both have appeared). Diffraction theory and optical scattering theory seem capable of yielding an inexhaustible series of papers describing special cases and different approximations, but the production of either basic discoveries or useful results is low.

Whether the fraction of trivial work has changed as the total effort in optics has expanded is uncertain, but the quantity of work clearly has increased. In addition, many men are doing basic research who have no special talent for it. Meanwhile, they are not available for product development, application engineering, high school teaching, and other activities of obvious utility. One result is the penetration of foreign products into the optical market far beyond any position attributable to differential labor costs. Fiber optics was developed almost exclusively in the United States, but the well-engineered gastroscopes are Japanese. Interference microscopes are British, French, and Japanese. Only in spectrographic instruments and lasers do American commercial optical instruments lead the world. The massive government funding of basic military and space research in optics yielded dramatic results, but we have paid serious penalties at the same time by pricing the trained optical specialist above the reach of many of the parts of society in which he is needed, overtraining him for much of the important work that must be done, and establishing a value pattern that draws him away from work of immediate usefulness. If he is truly talented, the rewards are just, the training appropriate, and the values correct, but many men today are tasting the bitterness of having been encouraged to run a race that they can only lose. The solution is not to give more prizes by funding more professorships, institutes, and laboratories but instead to renew the roots of physics and applied physics in the social needs that ultimately must justify the investments.

Underlying those social needs there clearly are needs for basic and applied research to invigorate product and process development and problem solving. Farsighted men in industry and government agencies should be encouraged to identify and support such research everywhere, from the printing industry to the forest and fisheries services.

Basic research in optics as a branch of physics has had very little direct

government support in the past and seems unlikely to obtain it in the future. In December 1970, for example, the Air Force Office of Scientific Research was supporting less than $400,000 per year in optics (excluding lasers). Half of this amount was allocated to a Themis Program in Precise Optical Systems at the University of Arizona, which will be terminated.

In the National Science Foundation in the same year, $267,000 was being spent in the Physics Section for optical programs, including lasers and quantum electronics, while another $350,000 was being spent in the Division of Engineering on the application of optics to basic engineering problems. The majority of these engineering studies deal with pattern recognition.

It was not possible to categorize National Aeronautics and Space Administration funds under optics. Orbiting astronomical telescopes, solar simulation, range instrumentation, re-entry physics studies, simulators for training devices, and the like required many millions of dollars, and the supporting science often produced basic discoveries, but the funding is identified with the primary missions.

The hidden character of optics support makes it difficult to evaluate the impact of various possible trends in government policy, but the Panel resists the idea that this problem should be solved by designating any large sums of money for optics *per se.* In particular, we strongly support the project pattern of government support of science and, from the perspective of optics, view with doubt proposed patterns of institutional support. The scientific community must be adaptable if it is to remain healthy; to be adaptable, it needs techniques for shifting emphasis and funds from one area to another. This adaptability demands that a system of evaluation and criticism be at work to discourage the trivial, obsolete, or redundant and nourish the potent or new.

The project system and the referee process in the journal literature are the tools now used for self-evaluation within the community of science. Both are painful, like any adaptive system, but the pain is greater and more bitter when applied wholesale to entire laboratories and programs than when applied retail to individual projects, judged on their own merits. Thus it is our opinion as a Panel that institutional program grants in general and institutional grants in optics in particular are unwise.

This viewpoint probably results in part from the small critical size for effective research in optics. Large groups are synergistic, of course, but the economies of scale and the interaction effects are unlikely in optics to be worth the loss in flexibility and adaptability. The Themis projects are dying; the NASA Space Science Centers are foundering; the Materials Science Centers are in trouble. It is not a good time to establish new groups in their images.

This argument is not to diminish the importance of the large industrial,

governmental, or academic programs serving a product line, a problem area (for example, reconnaissance), or a group of specializing students. The justification of such groups is from outside rather than from an inner logic.

There are few laboratories in the world organized and equipped to tackle optical problems on a broad base. To do so, it is useful to have lens designers, an optical shop, a thin-film coating facility, a photographic laboratory, a crystal-growing facility, large optical benches and test tunnels, computing facilities, spectrometers for the different wavelength regions, lasers, microscopes, sources, collimators, electronic support, and the like. Such aggregations of skills and equipment are to be found at the University of Rochester, the University of Arizona, Eastman Kodak, the American Optical Company, the Itek Corporation, The Perkin–Elmer Corporation, IBM Thomas J. Watson Research Center, the GOI in Leningrad, l'Institute d'Optique in Paris, and a few other places. However, in general, there is relatively little emphasis on very large, very expensive special-purpose equipment for optics research; many of the difficult problems can be studied with comparatively simple equipment. In addition, the optical industry offers much of the needed support effectively in such fields as thin films, crystal growing, fiber optics, arrays, lasers, and lens design. This situation is undoubtedly one of the reasons that the optics community is less traumatized by the recent dislocations in science than are many other subfields and disciplines. The small groups are more flexible, and, perhaps, if they are miserable, their plight is not so readily perceived.

7   Future Challenges in Optics

In spite of the rapidly growing disparity between optical needs and optical manpower, there is some reason for cautious optimism. Most of those who are engaged in optics research come from other specialties and can return to them if necessary, or, conversely, having concerned themselves with applied research, they can move into product development, application engineering, or sales. Another group has already left or will have to leave science entirely; this group is composed largely of those who were never well equipped to conduct research in optics. Yet another group will move into nonscientific administrative work.

Apparently, some combination of these factors explains why the optics community is experiencing little distress in February 1971. We know of

only a few truly competent optical scientists not fully employed at the present time. In addition, the new research programs of the Department of Justice, the Postal Service, the Department of Housing and Urban Development, the Department of Transportation, the National Oceanic and Atmospheric Administration, and other agencies are already posing interesting and difficult questions in optics.

Consider, for example, the wretched state of technology in the Postal Service compared with color television or the telephone system. Or consider the fragmented leadership and lack of innovation in crime prevention and detection. A great stimulus to research and development, and thus to the effective use of talented manpower, will arise as the relevant agencies establish programs and laboratories of their own commensurate with the complexity of their tasks and the importance to society of their missions.

In addition, development of the picturephone is progressing and will require substantial optical support. Optical communication through glass fibers is beginning to be taken seriously, and useful links probably will be installed experimentally within two years. These two efforts could stimulate one another. Further, the picturephone will call for a wide variety of peripheral devices to introduce signals into it and distribute the output.

Optical memories for computers are still in the development stage, and their utility not yet demonstrated; but the need is so great and there are so many possibilities in optical techniques that substantial additional work will undoubtedly take place. In particular, the costs or the relationship between capacity and read-time may be favorable.

Further in the future lies integrated optics, which combines a laser beam trapped in a surface film that is modulated or deflected by electrical or acoustical signals.

Much emphasis today is placed on optical methods of detecting and measuring atmospheric contaminants. Undoubtedly, there will be new problems and jobs in this field, but it seems unlikely that they will be sufficient to justify more effort than already is being allocated to this branch of optics.

Fiber optics, which for a few years was both challenging and lucrative, is now a mature and highly competitive industry with many groups ready to take advantage of any new applications that are identified.

Pattern recognition seems to hold great promise. It is possible that pattern recognition in the next ten years could change our society in fundamental ways at the same time that it changes our understanding of the way man gains knowledge and interacts with his environment. Much of the drudgery in which man engages in a highly industrialized society consists of optical pattern recognition of some kind, from the checkout counter in the supermarket to the reading of electrocardiograms. Physicists

have an opportunity to make a contribution to this growing field at the most fundamental level.

Finally, it is clear that optics will continue to be of great value in military problems. Aerial reconnaissance and the image-intensifier telescope have proven invaluable to American troops in Vietnam. And without a strong base in optical physics, these contributions to our military security would have been impossible.

Reconnaissance is not only a tool for waging war more effectively but a tool for maintaining peace. It is one of the few techniques available for verifying compliance with international agreements such as arms limitations.

This Panel believes that government agencies should be encouraged to emulate the example of the Department of Defense and the National Aeronautics and Space Administration by identifying their long-range needs with imagination, daring, and vigor. Housing, transportation, hospital operation, education, printing, mining, and farming all have product and process needs. Underlying these needs are basic research problems. The manpower to solve the applied problems and to do the basic research is available. To bring together the needs and the talented manpower is the challenge of this new decade.

Appendix A: Government Laboratories with Research Programs in Optics

Atomic Energy Commission
  Oak Ridge National Laboratory, Oak Ridge, Tennessee
  Pacific Northwest Laboratory, Richland, Washington
  Sandia Laboratory, Albuquerque, New Mexico
Department of Commerce
  Engineering Development Laboratory (Census), Suitland, Maryland
  Geodetic Research & Development Laboratory (NOAA), Rockville, Maryland
  Research Laboratories (NOAA), Boulder, Colorado
  Satellite Experiment Laboratory (NUAA), Suitland, Maryland
  National Bureau of Standards, Gaithersburg, Maryland
  Institute for Applied Technology (NBS), Gaithersburg, Maryland
  Institute for Basic Standards/Boulder (NBS), Boulder, Colorado
  Institute for Basic Standards (NBS), Gaithersburg, Maryland
  Institute for Materials Research (NBS), Gaithersburg, Maryland

Department of Defense
  Avionics Laboratory (Air Force), Dayton, Ohio
  Cambridge Research Laboratories (Air Force), Bedford, Massachusetts
  Eastern Test Range (Air Force), Patrick A F B, Florida
  Army Night Vision Laboratory (Army), Ft. Belvoir, Virginia
  Materials Laboratory (Air Force), Dayton, Ohio
  Missile Development Center (Air Force), Alamogordo, New Mexico
  Rome Air Development Center (Air Force), Rome, New York
  School of Aviation Medicine (Air Force), San Antonio, Texas
  Weapons Laboratory (Air Force), Albuquerque, New Mexico
  Western Test Range (Air Force), Vandenberg A F B, California
  Ballistic Research Laboratory (Army), Aberdeen, Maryland
  Deseret Test Center Laboratories (Army), Dugway, Utah
  Electronics Laboratories (Army), Ft. Monmouth, New Jersey
  Engineer Topographic Laboratory (Army), Washington, D.C., Virginia
  Frankford Arsenal Laboratories (Army), Philadelphia, Pennsylvania
  Limited War Laboratory (Army), Aberdeen, Maryland
  Missile Command Laboratories (Army), Huntsville, Alabama
  Mobility Equipment R&D Center (Army), Alexandria, Virginia
  Natick Laboratories (Army), Natick, Massachusetts
  White Sands Missile Range (Army), White Sands, New Mexico
  Ordnance Laboratory (Navy), Silver Spring, Maryland
  Pacific Missile Range (Navy), Ventura, California
  Naval Research Laboratory (Navy), Washington, D.C.
  Ship R&D Laboratory (Navy), Panama City, Florida
  Training Devices Center (Navy), Orlando, Florida
  Underwater Sound Laboratory (Navy), New London, Connecticut
  Weapons Center (Navy), China Lake, California
Health, Education, and Welfare
  Chemistry and Physics Laboratory (CPEHS), Cincinnati, Ohio
  National Institute of Neurological Diseases and Blindness (N I H), Bethesda, Maryland
National Aeronautics and Space Administration
  Ames Research Center, Mountain View, California
  Churchill Research Range (Wallops Station), Fort Churchill, Canada
  Electronics Research Center, Cambridge, Massachusetts
  Ellington Support Facilities (Manned Spacecraft Center), Houston, Texas
  Goddard Institute for Space Studies (Goddard Space Flight Center), New York, New York
  Goddard Space Flight Center, Greenbelt, Maryland
  Jet Propulsion Laboratory, Pasadena, California
  Kennedy Space Center, Titusville, Florida
  Langley Research Center, Hampton, Virginia
  Manned Spacecraft Center, Houston, Texas
  Manned Space Flight Network (Goddard Space Flight Center), Greenbelt, Maryland
  Marshall Space Flight Center, Huntsville, Alabama
  Space Tracking and Data Acquisition Network (Goddard Space Flight Center), Greenbelt, Maryland

Wallops Station, Chincoteague, Virginia
Western Test Range (Kennedy Space Center), Lompoc, California
National Science Foundation
Kitt Peak National Observatory, Tucson, Arizona
Postal Service
Research and Development Laboratory, Washington, D.C.
Smithsonian Institution
Radiation Biology Laboratory, Rockville, Maryland
Smithsonian Astrophysical Observatory, Cambridge, Massachusetts
Department of Transportation
Field Testing and Development Center (USCG), Baltimore, Maryland

Appendix B: Physics Departments That Indicated in 1969 That They Offered a PhD with Specialization in Optics

Arizona	University of Arizona
California	Berkeley
	Los Angeles
	Riverside
	Santa Barbara
	Naval Postgraduate School, Monterey
Colorado	School of Mines
	State University
Connecticut	Wesleyan
	Yale University
District of Columbia	George Washington University
	Georgetown University
	Howard University
Florida	University of Florida, Gainesville
	University of Miami
Georgia	Georgia Institute of Technology
Hawaii	University of Hawaii, Honolulu
Idaho	University of Idaho
Illinois	University of Illinois
	Southern Illinois University
Indiana	Purdue University
Iowa	Iowa State University
Kentucky	University of Kentucky
Louisiana	Louisiana State University
Maine	University of Maine

Maryland	The Johns Hopkins University
	University of Maryland
Massachusetts	Amherst College
	Boston University
	Brandeis University
	Harvard University
	Massachusetts Institute of Technology
	University of Massachusetts
	Mt. Holyoke College
	Smith College
	Worcester Polytechnic Institute
Michigan	Michigan State University
	University of Michigan
Minnesota	University of Minnesota
Missouri	University of Missouri-Rolla
	St. Louis University
	Washington University
Nevada	University of Nevada
New Mexico	New Mexico State University
	University of New Mexico
New York	City University of New York
	Columbia University
	Cornell University
	Fordham University
	State University of New York, Buffalo
	State University of New York, Stony Brook
	Polytechnic Institute of Brooklyn
	University of Rochester
	Syracuse University
Ohio	Ohio State University
Oklahoma	University of Oklahoma
Pennsylvania	Carnegie-Mellon University
	Pennsylvania State University
	University of Pennsylvania
	Univeristy of Pittsburgh
Rhode Island	Brown University
Tennessee	University of Tennessee
Texas	Texas Christian University
	William Marsh Rice University
Utah	Utah State University
	University of Utah
Vermont	University of Vermont
Washington	Washington State University
	University of Washington
West Virginia	West Virginia University

Appendix C: Optics Doctorates from Schools of Engineering

New developments in applied optics have been found attractive by a number of engineering departments. These particularly include quantum electronics (QE), pattern recognition (PR), lasers (L), optical communications, image processing, holography, and optical propagation through the atmosphere.

   The American Society for Engineering Education publishes an annual list of doctoral degree candidates in engineering. The latest issue is dated 1969 and gives the names and specialty fields of candidates expecting to receive the doctorate in 1970. Below is a list of the colleges having one or more candidates in a specialty that can be described as optical and the fields in which they are working. Where more than one field is given, there is more than one candidate, but the actual number of candidates was not tabulated, because the descriptions are often quite general and the boundaries uncertain.

Arizona	L
Arkansas	L, PR
Cal Tech	QE, vision
Santa Barbara	Laser modulators
Carnegie-Mellon	Data processing, magnetooptics
Case Western Reserve	Magnetooptics, L
Colorado	Holography
Columbia	QE
Cornell	L
Denver	Holography
Florida	Image processing
Georgia Tech	Optical data processing, L
Illinois	L, QE, electrooptics, optical data processing, atmospheric optics
Johns Hopkins	Optics, optical communication
MIT	Optical communication, visual image processing, L, PR., character recognition
Michigan State	PR
U. of Michigan	Optics, optical spatial filtering, holography
Nebraska	Optical thin films, ellipsometry
New Mexico	Optical data processing
Stony Brook	Holography
North Carolina	PR
Northwestern	PR

Ohio State	Nonlinear optics, holography, PR
Penn State	Photon statistics, PR, laser communication, holography
Pennsylvania	PR, holography
Polytechnic Inst. of Brooklyn	Optics, optics communication, L
Purdue	PR
Queens (Canada)	Optical signal processing
Rensselaer	L
Rhode Island	Optical properties of materials
Rice	L
Rochester	L
U. of So. California	Laser modulators
Stanford	L, character recognition, QE, non-linear optics, optical propagation, aperture synthesis, holography, coherent optics, PR
U. of Texas	PR
Utah	Electrooptics, Fourier image processing, nonlinear optics, L
Virginia	Character recognition, PR
Washington U.	QE, optical communication
Waterloo	PR, character recognition
Wisconsin	Intensity interferometry

There are 40 universities on the list, and more than 80 degree candidates were listed.

VI
Acoustics

PANEL MEMBERS

ROBERT T. BEYER, Brown University, *Chairman*
ANDREW V. GRANATO, University of Illinois
THEODORE LITOVITZ, Catholic University of America
HERMAN MEDWIN, Naval Postgraduate School
WAYNE RUDMOSE, TRACOR, Inc.
JOZEF ZWISLOCKI, Syracuse University

CONTRIBUTORS

DAVID T. BLACKSTOCK, University of Texas
FLOYD DUNN, University of Illinois
CYRIL M. HARRIS, Columbia University
HARVEY HUBBARD, National Aeronautics and Space Administration
LEWIS LARMORE, Douglas Advanced Research Laboratories
J. J. G. McCUE, Massachusetts Institute of Technology
KENNETH N. STEVENS, Massachusetts Institute of Technology
J. E. WHITE, Globe Universal Sciences, Inc.
CHARLES R. WILSON, University of Alaska

LIAISON REPRESENTATIVES

EDWARD I. GARRICK, National Aeronautics and Space Administration
ROLF M. SINCLAIR, National Science Foundation

READERS

LEO L. BERANEK, Bolt Beranek & Newman
JAMES H. BOTSFORD, Bethlehem Steel Corporation
JOHN V. BOUYOUCOS, General Dynamics
RICHARD K. COOK, National Bureau of Standards
IRA J. HIRSCH, Washington University
KARL D. KRYTER, Stanford Research Institute
R. BRUCE LINDSAY, Brown University
ISADORE RUDNICK, University of California, Los Angeles
JOHN C. SNOWDEN, Ordnance Research Laboratory
HENNING E. VON GIERKE, Wright-Patterson Air Force Base

Contents

VI

ACOUSTICS

1   The Nature of the Subfield

1.1   GENERAL BACKGROUND

Acoustics can be defined most succinctly as the study and use of mechanical waves in aggregate matter. The waves need not be elastic; they can be inelastic, with attenuation and dispersion, and can have large amplitudes, with consequent nonlinear effects. Acoustics deals in large part with techniques and devices for the generation, transmission, and detection of these waves. The study of the transmission of acoustic waves in matter is one of three main techniques (acoustic techniques, electromagnetic techniques, and the interaction of matter with particles) used to investigate the properties of matter.

Because acoustics is so old a subject, early having been associated with a basic sensory and perceptual mechanism, it sometimes is viewed only as a part of classical physics and as a subject fully explored in the nineteenth century. Lord Rayleigh's famous *Theory of Sound,* first published in 1877, did not, however, represent the end of acoustics. On the contrary, the theory and Rayleigh's subsequent research contained the elements of a new beginning in which new concepts and techniques were generated. These new approaches and methods both caused and resulted from new applications in science and technology. Consequently, acoustics has today both a classical character and a quantum character.

In treating acoustics as a subfield of physics, this Panel subscribes to the

point of view expressed by the late professor of physical acoustics, F. V. Hunt, Division of Engineering and Applied Physics, Harvard University. When advising the Panel, Professor Hunt stated:

> As for the broad front of your acoustics report, I should think that its first burden might be to explain to physicists what acoustics has turned into in these days; that it is not alone the therapy of listening spaces, but has remained a branch of physics. Among the pieces of ammunition to support this "radical" viewpoint are the remaining mysteries of nonlinearity, the stochastics of turbulence, the interactions of light and sound, and the manifold variations of phonon interaction. . . . Among the myths to be opposed [is] : that, by assigning the term, phonons, to the nodes of a standing wave system, they have been removed from the ken of acoustics. The best experimental work in these fields is still done by the people who understand waves in matter.

As with optics, and more generally with electromagnetic radiation, acoustics has a wide spectrum ranging from about 10^{-4} to 10^{14} Hz, as indicated in Table VI.1. Generally, the techniques differ greatly in the different ranges, although the physical principles are similar.

Acoustic waves often penetrate media (for example, the oceans and the earth) in which electromagnetic waves are highly absorbed. Our knowledge about the interior of the earth (liquid core, temperatures, and pressures) comes mainly from studies of seismic (acoustical) waves in the earth, combined with equations of state for rocks, which are determined in the laboratory with the help of acoustical measurements. This example is but one of many in which acoustics provides the only means available for direct study of the properties of matter.

Definition of the scope of acoustics is difficult, for its subject matter cuts across the definitions of the subfields of physics selected by the Physics Survey Committee. In fact, acoustics has extensive ramifications not only in the various subfields of physics but in many other scientific disciplines and in technology. Consequently, people using the concepts and techniques of acoustics can be found in many physics subfields, related sciences, and engineering disciplines. However, in spite of the variety of topics dealt with in acoustics and the strong association with other

TABLE VI.1   Frequency Ranges in Acoustics

Frequency (Hz)	Name of Frequency Range
10^{-4}–20	Infrasonic
20–2×10^4	Audio
2×10^4–5×10^8	Ultrasonic
5×10^8–10^{12}	Hypersonic or praetersonic
10^{12}–10^{14}	Thermal vibrations

sciences, engineering disciplines, and technologies, acoustics remains fundamentally a part of physics. It includes the study of all material media. It requires the mathematics of theoretical physics. Its methods play a primary role in exploring the characteristics of the various states of matter. In addition, it provides many techniques, devices, and inventions for other subfields. And, in education, there is a unity about the subject of acoustics and its manifold applications that tends to be lost if it is taught in a fragmented way in a number of different courses.

The place of acoustics in physics can be illustrated by analogy with optics. Like acoustics, and for similar reasons, optics is an old branch of physics, although the more recent developments such as lasers require full account of the quantum properties of matter. Both optics and acoustics now embrace much wider parts of their respective frequency spectra than they did previously. However, the full electromagnetic spectrum is not always regarded as within the scope of optics. Like acoustics, optics and electromagnetic waves each provide one of the three main techniques for the study of matter. The existence of both transverse and longitudinal types of waves in acoustics is sometimes advantageous in identifying and sorting the effects produced by the two different kinds of waves. The quasi-static (i.e., relatively time-independent) effects in both subfields (electrical and magnetic effects in optics and elastic or, more generally, mechanical effects in acoustics) are regarded as low-frequency parts of the spectra.

The concepts and methods of acoustics are extensively applied in many disciplines, especially in engineering. In fact, acoustics has developed such extensive engineering applications that many subjects of fundamental interest in acoustics have their intellectual origins in problems suggested by applications, even when the work is basic in nature. The various affiliations of personnel in acoustics are one indication of its broad scope. Instruction in this subject and the content of the journals publishing work in acoustics offer additional evidence. Engineering departments of colleges and universities now provide much of the formal instruction in acoustics. However, the shift of training from physics to engineering departments can result in an approach that neglects the unifying aspects of the subject and its implications for widely differing fields.

The constantly changing nature of acoustics as new discoveries occur also contributes to the problem of developing a practical definition of this subfield. Subjects within acoustics grow and gradually separate from it as understanding progresses through the development of the geometry of propagation to the response of materials to acoustic waves. Thus, acoustics stimulates the development of new areas of investigation that later become either independent or subdivisions of other physics subfields that deal with the properties of matter. However, the techniques of generating

and receiving waves, as well as the geometrical aspects (as in geometrical optics) of propagation, remain a fundamental part of acoustics. The fact that many subjects in acoustics have become relatively independent is not sufficient reason to regard them as completely unrelated to acoustics or physics, particularly in the education of physicists. The following case study provides an illustration.

The study of ultrasonic surface waves in solid media is an active branch of acoustics that goes back to Rayleigh. Intense activity has developed within the past five years, and the subject is likely to have a major impact on communications and computer technology. It probably developed within acoustics because the initial phases were concerned with the generation and detection of these surface waves. In addition, findings and experience resulting from the application of acoustics to seismology were relevant to this kind of investigation. The people most involved were physicists in industrial settings, because of the great potential impact of the findings on technology. Progress was rapid, with the development of devices following research in extremely short time intervals (on the order of only a very few years). Present problems concern the study of attenuation mechanisms in a search for low-loss materials in the microwave region. (See also Chapters 2 and 3.) Experience with the development of other subjects in acoustics suggests that the new tools, devices, and techniques produced will be used in the further study of the properties of matter, particularly of surfaces. The work gradually will become a part of the literature on the properties of matter and be regarded as outside the scope of acoustics. What will remain in acoustics will be the geometrical aspects of the transmission of surface waves. This series of events is likely to occur within the next five years. Thus, within a period of about ten years, one can trace the rekindling of interest in the subject, its development within acoustics, the transfer of that part of the content having to do with the properties of matter to other more appropriate subfields of physics, electrical engineering, and the like, and the deposit of the residual geometrical aspects as a permanent subsection of acoustics. This portion remaining in acoustics will include sources, receivers, propagation questions, instruments, and various techniques.

The currently changing emphasis in physics also has an impact on the definition of acoustics. Emphasis on more applied work relevant to critical national problems necessarily means increased attention to acoustical studies. Examples of disciplines that are undergoing major development with acoustical methods are biophysics and geophysics. The large programs planned for oceanography and ocean engineering will be limited by the available understanding of acoustics. These developments suggest, in par-

ticular, a need to re-examine the role of acoustics in the physics curriculum, because of the increasing fraction of college graduates who can be expected to work in these newer and somewhat more applied areas.

In addition, the shift in emphasis of federal programs from defense to environmental and social problems (pollution, public transportation, housing, health and safety, and communications) will have an impact on the relative place of acoustics in the hierarchy of physics subfields. One cause of widespread concern is that the appropriate federal agencies (Housing and Urban Development; Transportation; and Health, Education, and Welfare, for example) have not shown the same degree of interest in supporting basic research that has characterized the defense agencies. However, the defense agencies did not support basic research heavily before World War II; the emergencies of that period required and stimulated a new approach. Examples of the benefits to national security resulting from such support (such as radar, atomic energy, and submarine detection) were sufficient thereafter to make the defense agencies aware of the necessity of maintaining a strong position in basic research.

The problems of the environment are also in large part technical ones. Many students as well as agencies of government seem as yet to be insufficiently aware of this. Sometimes, even a change in the name of a discipline or subfield can foster greater awareness. For example, most sanitary engineering groups have changed their names to environmental engineering. Pollution problems depend critically on questions of catalysis and corrosion, two areas of surface physics currently relatively undeveloped. Catalysts are needed that would eliminate products of incomplete combustion of fossil fuels. It seems certain that acoustics will play a part in the future studies of surfaces, probably through the use of some of the techniques currently being developed.

In public transportation there are critical problems of environmental noise, shock waves, materials, communications, and control. Physics, and acoustics in particular, has much to contribute to their solution.

Recent developments in acoustical holography promise to make striking contributions to problems of health through mapping of the internal structure of the body. Further, safety devices necessarily involve electromagnetic and acoustical signals. Electromagnetic–acoustic devices now exist that will locate astronauts to within 1 ft of a spacecraft at all times. The potential applications of such devices have not yet been fully explored.

The recent developments of ultrasonic surface-wave physics probably will exert a strong impact on the communications and computer industries, which by now pervade all aspects of modern life with examples familiar to everyone.

It is to be hoped that, as examples of the importance of basic research in facilitating the solution of environmental problems emerge, the responsible agencies will recognize the need to support such basic work.

Because of the wide-ranging work in acoustics and its extensive applications, the Panel has adopted for the purposes of this report a broad view of acoustics in relation to education, physics, other sciences, and engineering. We have excluded the study of the properties of matter in research, with the exception of subjects not yet normally included in the appropriate subfields of physics.*

1.2   ORGANIZATION OF THE REPORT

Had John Milton written this report, he might have said that our purpose was to justify the works of sound to twentieth century man. As we have already pointed out, acoustics is an old branch of physics, but the vast range of topics in modern acoustics makes physical acoustics only a branch of the entire discipline. Acoustics itself is an interdisciplinary field par excellence and we do well to emphasize its role as such. A stellar example is that of Georg von Békésy, who, as a physicist in acoustics, won the 1961 Nobel Prize in Medicine or Physiology. However, the present document is also a report to the Physics Survey Committee and our primary viewpoint is that of physics.

The standard subject classification groups of acoustics (for example, those of the *Journal of the Acoustical Society of America*) can be readily assigned to the categories listed in the charge from the Survey Committee to the Panel. Thus, ultrasonics, radiation and scattering, aeroacoustics, and macrosonics will be discussed in Chapters 2 and 3. These topics are regarded collectively as physical acoustics in the acoustical literature, but for the purposes of this report, we also included: (a) techniques of wave propagation (bulk waves, surface waves, low-intensity linear waves, and high-intensity nonlinear waves) in solids and liquids (including quantum liquids) and (b) new sources of acoustic signals. The application of these techniques to the study of the properties of matter is briefly indicated in Chapters 2 and 3.

Acoustics in the ocean, in geology, and in chemistry are covered in

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* For this Panel to review the acoustical study of the properties of matter in detail would be redundant, as this work should be included in the chapters of other panels—atomic, molecular, and electron physics; plasma physics and physics of fluids; condensed matter; earth and planetary physics; and physics in biology.

Chapter 4. The classifications of physiological and psychological acoustics, including speech and hearing, bioacoustics, and medical acoustics, are dealt with in Chapter 5.

Chapter 6 deals with various aspects of the interaction of acoustics and technology, including sound recording and reproduction and shock and vibration acoustics. Chapter 7 examines the role of acoustics in music, architecture, and noise control in relation to society.

Following these content-oriented chapters are four that are more general. The first of these discusses the relationship to national security activities. The next (Chapter 9) briefly describes the academic, governmental, and industrial institutions in which work in acoustics takes place. Chapter 10 deals with manpower characteristics, productivity, and funding in acoustics, and Chapter 11, with training in acoustics.

The Acoustics Panel interpreted the charge of the Physics Survey Committee as an invitation to explore the highly interdisciplinary environmental and societal links that acoustics brings to physics. The Panel strongly endorses, and has tried to incorporate in its approach to the writing of this chapter, the viewpoint expressed by Freeman J. Dyson [in *Physics Today*, *23*(9), 28 (1970)]:

In my opinion, the surest way to save physics from some rather catastrophic stagnation or decline during the next 30 years is to keep young physicists working on the frontiers where physics overlaps other sciences, such as astronomy and biology.

To these disciplines we would add geology. We also believe strongly that the study of acoustics provides an excellent preparation for work in interdisciplinary fields and a good way to become involved in such work.

2 The Status of Physical Acoustics

In the past five years, the trend toward further development of physical acoustics by interdisciplinary groups—most notably by physicists under the auspices of the electrical engineering societies—has accelerated. This section reviews the nature and direction of work in this subfield.

Although there has been great activity in acoustical studies of phase transformation in solids, this subject will not be reviewed, as the emphasis

in this work has been to use standard available ultrasonic tools to study the phenomena of phase transitions and falls within the scope of the Panel on Condensed Matter. However, the rapidly growing field of ultrasonic surface waves has been primarily concerned with questions of signal generation and detection, with little application so far of the techniques developed to the study of the properties of crystals; therefore, this topic will be discussed here.

2.1 SURFACE WAVES

Surface waves on solids have been known since the time of Rayleigh and have been extensively applied in geophysics, but they were virtually forgotten in ultrasonic work. However, in the past three years a spectacular surge of interest in surface waves and their applications in electronic systems has occurred. Through electroacoustic transducers, bulk elastic waves have long been used in electronics to provide delay of signals, because the speed of sound is so much less than the speed of electromagnetic propagation. Moreover, at least at frequencies in the 1–500-MHz range, the product of storage time and signal bandwidth is greater (in the absence of amplifying mechanisms) for acoustic waves in well-chosen media than it is in electromagnetic lines; the acoustic media offer a higher quality factor (Q).

One reason for interest in surface waves was that transducers could be located at will in the path of propagation to tap off signals or to contribute new excitation at selected intervals following the initial acoustic signal. Added impetus to such work resulted from the development of crystals of lithium niobate, on which surface waves propagate with particularly low loss. Another development that favored exploitation of surface waves was the refinement of photolithography for the production of integrated circuits. The use of surface waves probably will lead to the integration of microelectronics and microacoustics. Electron lithography is being explored as a means of constructing microacoustic devices in which the fineness of structure (of transducers, for example) is not restricted to dimensions larger than the wavelength of light. At present, the practical upper limit of frequency for generation of surface waves is about 1 GHz, but fabrication by blasting a resistant layer with an electron beam and then etching it might raise this limit to 10 GHz.

Surface waves have two obvious uses: information storage and signal filtering. Bulk waves also are used for information storage and are competitive in cost with such rival means as magnetic drums. The acoustic devices are said to be more reliable, and they have shorter access time. The use of

surface waves rather than bulk waves offers two special advantages: surface waves require only one highly accurate surface and are better suited than bulk waves to piezoelectric amplification (because the surface-to-volume ratio is lower for bulk waves, and heat dissipation is a major constraint on such an amplification system). At present, a surface-wave device about as large as a 25-cent coin has a storage capacity of several thousand bits.

In the second major use of surface waves—signal filtering—the advantage of these waves is that they allow the designer to prescribe the phase characteristic independently of the amplitude characteristic. This freedom will become increasingly valuable as crowding of the electromagnetic spectrum puts an increasing premium on sophistication in signal processing.

Most of the development of surface-wave devices will result from the efforts of electronic engineers, although some aspects of this applied work probably can best be conducted by physicists. Rapid progress in the development of waveguiding techniques and innovations in the use of transducers are likely in the next five years. The amplification of surface waves through electroacoustic interaction will overcome attenuation and provide isolation against backward propagation; such amplification also is desirable because it increases the dynamic range of the permissable signals.

In currently envisioned applications, surface waves will be propagated on single crystals because these have lower attenuation than other materials. At present, the waves are usually on piezoelectrics, for it is on such materials that they are generated. One area in which effort is needed and being applied is the propagation of surface waves on anisotropic materials. Such studies should take into account the nonlinear elastic properties of crystals, because confinement of surface waves to a very thin region means that the energy density can be high, and nonlinear interaction of two signals (with promise of parametric devices) is an attractive possibility.

Those who study propagation should consider the need to develop waveguide techniques for surface waves. For example, a layer of metal on a piezoelectric material can "short out" the surface electric field and thus modify the elastic constants in such a way that a slow-velocity region is created that can guide the wave. Therefore, propagation studies should include waves on piezoelectric crystals in the presence of controlled or imposed electric fields. They also should deal with the effects of boundaries and should not be limited to plane surfaces. In addition, the effects of temperature change must be considered, because stability against temperature change is vital in some applications.

Some devices entail putting a wave on an overlay to a substrate whose acoustical properties are markedly different. This procedure raises problems not only of propagation but also of epitaxial growth. Growing a thin

crystalline layer (or several layers) epitaxially on a substrate is one of the central problems in the exploitation of surface waves and appears to be essentially a problem of the physics of surfaces.

Partly within the domain of physics is the development of strongly piezoelectric semiconductors, with particular regard for conditions at the surface. Those who use electroacoustic interactions of surface waves are interested in materials that are semiconducting out to the surface.

2.2   ACOUSTIC HOLOGRAPHY

Acoustic holography represents another major development in acoustics in the past five years. This rapidly expanding field is likely to provide an indispensable approach in widely diverse areas of research. Many related sciences and techniques, including the acoustic properties of materials, electronic scanning, laser beams, and electrooptical coupling, contribute to acoustic holography. Geophysics, biophysics, and materials testing are among the many disciplines that should benefit greatly from current holographic studies. In addition, computer reconstruction of acoustical holograms offers challenging possibilities. In geoscience, the time-consuming methods now in use for seismic exploration might be replaced eventually by more sophisticated holographic techniques, thus increasing the efficiency of geologists and petroleum engineers as well as augmenting the techniques available to underwater exploration teams.

The technology of nondestructive testing already includes optical holography, but the development of the new water–air interface method of acoustical imaging adds acoustical holography to the resources of this vital branch of materials science. This imaging technique transfers the spatial modulation of a sound field onto a light field, which can be used to produce optical images. In the case of a water–air interface, the surface of the water is deformed by the incident sound. A light beam is reflected from the water surface from which the beam derives its spatial modulation. The technique has the advantages of simplicity and real-time capability.

For medical purposes, acoustical holography offers hope of eliminating the major dangers inherent in x-ray examination of the human body—that is to say, exposure to potentially harmful radiation—and of obtaining a depth of image not possible with x rays. Many problems still remain to be solved before acoustic holography becomes really effective; therefore, more research is needed in this field.

Computer capability and further refinement of existing equipment for transmission, reception, and focusing of the images point toward increased use of computers for storing and reconstructing acoustical holograms.

2.3   HIGH-ACCURACY MEASUREMENT OF VELOCITY CHANGE

Certain characteristics of solids can be deduced from measurements of the velocity and attenuation of sound. Highly accurate measurements of velocity change have been developed during the past five years, based on measurements of the pulse repetition rate in superposition measurements. Frequency measurements not only are exceedingly accurate but sufficiently sensitive to detect velocity changes of the order of one part in 10^8 Techniques also have been developed to measure attenuation changes as small as one part per million. Now that more accurate measurement of the characteristics is possible, the inferred characteristics of solids also can be determined more accurately.

The increased sensitivity has recently permitted measurements of changes in elastic constants with applied static stresses as small as 1 atm rather than the more usual range of kilobars of stress required previously. Consequently, it is now possible to measure the third-order elastic constants of soft materials; for this purpose, it is necessary to apply static shear stresses which can be relatively small. Hence no plastic flow is produced, and measurements can be made on the nonlinear elastic deformation.

Third-order elastic constants have also been measured by studying the acoustic harmonic signals produced by a finite amplitude wave in nonlinear solids. An investigator can determine the distance such waves will travel before developing shock fronts. One application of such work is in quantitative studies of the effects of N waves on solid media. High-amplitude sound waves (macrosonics) also could have important applications in metal-shaping processes by reducing the external stresses required for the forming processes.

The third-order elastic constants have applications in (a) establishing equations of state of solids (especially important to geophysics), (b) providing more data with which to check theories of the interatomic potentials in solids, (c) providing connections between directly measured mechanical properties and thermal effects through determination of the Grüneisen constant, and (d) calculating the properties of crystals containing defects and defect interactions.

Another example of an application of the newly provided velocity sensitivities is the development of acoustic thermometers. With a typical temperature coefficient of the order of 2×10^{-4} $(°C)^{-1}$ for most crystalline materials, a velocity sensitivity of 10^{-8} provides a temperature sensitivity of the order of one tenth of a millidegree and often less. This measurement is absolute and can be made easily with remote probes and recorded continuously. In fact, acoustical thermometry is developing into a new sub-

field, with research in progress on the effects at high temperature, nuclear reactor ambience, and pressure.

2.4   NONLINEAR ACOUSTICS

The theory of acoustics is usually developed as an infinitesimal theory, by which it is meant that the equations governing the propagation of mechanical waves in a continuous medium are used in the linear approximation, with the assumption that the amplitude of the wave is sufficiently small.

While some nonlinear aspects of acoustics were known, although not correctly identified, in the eighteenth century, and Riemann and others extended the theory in the nineteenth century, systematic study did not begin for the most part until the twentieth century. The work undertaken on the problem of shock waves by Rayleigh and G. I. Taylor, and on general fluid dynamics by von Karman and others, moved rapidly outside the scope of acoustics. The study of the more acoustically oriented nonlinear phenomena, such as wave distortion, streaming, and cavitation, did not develop until well after 1945.

During the past ten years, nonlinear acoustics has had a vigorous growth. Rather thorough studies have been made of the growth of harmonics in an originally sinusoidal wave as a consequence of the finiteness of its amplitude as the wave progresses through the medium. The theoretical methods of Burgers's equation and weak shock theory have been applied with success to various propagation problems with satisfying results.

Such exploitation of the methods of nonlinear acoustics is of practical importance in improving our understanding of the propagation of the N waves associated with the sonic boom as well as the propagation of very-high-intensity continuous sound from such sources as jet and rocket engines.

The study of the propagation of a finite-amplitude wave led to the necessity of determining the coefficients of the higher-order terms in the expression relating the pressure and density in an adiabatic process in a liquid. These thermodynamic coefficients must depend on the nonlinear terms on the force relations between molecules and molecular groups in the liquid, in a way similar to that of the third-order elastic constants in solids. As close an identification as was achieved in solids has not yet been obtained.

The interaction of two high-intensity sound beams with one another to produce combination tones in the inner ear has been known in music from 1754 as Tartini pitch. The study of such nonlinear interactions in air and water did not receive appreciable study in physical acoustics until the 1950's. The parametric end-fire array, predicted by Westervelt in 1960 and

confirmed experimentally almost immediately, is now seen to have considerable application in short-range sonar, especially in shallow-water-bottom studies. The array uses two intense, collinear ultrasonic beams of neighboring frequencies. A difference frequency is generated by the nonlinear effects as the beam travels through the medium, the directional characteristic of which beam is extremely narrow and relatively free from sidelobes.

The nonlinear phenomenon of the self-demodulation of an ultrasonic-frequency pulse of finite length, leaving signals identifying the leading and trailing edges of the pulse, is also of potential sonar application. Another result of nonlinear acoustical research has been the discovery of the limitation put on sonar by saturation caused by finite-amplitude effects.

The particular portions of the field just described have developed rapidly, with interesting commercial applications. Other areas of nonlinear acoustics, such as those involved in reflection, refraction, and diffraction, have been bypassed temporarily. Much further study is necessary before the complete picture of nonlinear acoustics emerges.

Closely related to the topics above are the streaming effects in fluids produced by intense sound waves. The characteristics of this streaming have received considerable attention, but much work remains to be done. For example, a high-intensity sound beam can cause streaming of fluid inside body organs or inside individual cells, with results that are far from completely explained. The correct understanding of the phenomenon would be of importance in medical acoustics.

The cavities produced in liquids by an ultrasonic beam provide an opportunity to study the phenomenon of cavitation under controlled conditions. This subject is of practical importance because of the cavitation-induced damage of moving surfaces, such as propeller blades. Other possible applications involve the radiation ("sonoluminescence") from collapsing cavities, the enhancement of chemical reactions by such cavities, and the role of cavitation in biological fluids.

Along with streaming and cavitation is a third characteristic of a sound beam—its radiation pressure. This usually small dc force accompanies all sound beams. Its properties and effects are intimately related to those of streaming and cavitation and have provided the basis for a number of sound-detecting instruments.

2.5   ACOUSTIC EMISSION

Acoustic emission is the term given to the sound energy released by a solid when it is mechanically deformed. The phenomenon has excited great interest in recent times because of its potential application as a nondestruc-

tive tool. The nondestructive testing industry probably will increase from the current annual level of $50 million to $280 million annually by 1980. An imbalance between theory and experiment exists in this field. The properties of the acoustic emission from a single dislocation wall in a crystalline material have been studied, but much basic research is needed to identify the many different kinds of signatures of the signals found, some of which are believed to be the precursors of failure in the materials tested.

2.6   NEW ACOUSTIC SOURCES

Recently, heat pulses have been used as sources of acoustic waves in the range of 10^{11} to 10^{12} Hz. Although the acoustic radiation is not monochromatic, shear and longitudinal waves can be distinguished by time-of-flight-type measurements.

Pulsing high voltages through a small gallium arsenide Gunn oscillator is a new method employed to obtain monochromatic acoustic waves in the range of 10^8 Hz.

An additional source of 10^{12}-Hz phonons is the optical generation of phonons in ruby. The pumping light generates 30 cm^{-1} phonons that are "bottlenecked" (that is to say, there is no mechanism available for the phonons to get to a thermal equilibrium distribution; thus, the highly localized concentration of energy is an irradiated crystal known as thermal spike exists) and correspond to a temperature of 3 K.

2.7   INFRASONICS

In recent years, the study of pressure fluctuations in the atmosphere, particularly at periods of acceleration greater than 1 sec, has revealed many natural sources of infrasonic waves. These include acoustic waves that are somewhat modified by gravity, interface waves with phase propagation only in the direction parallel to the plane of the interface, and internal gravity waves. The extremely low attenuation of infrasonic waves, due to their low frequency and the waveguide effects of the atmosphere, makes possible the detection of infrasonic signals of only a few microbars amplitude from sources thousands of kilometers away.

Waves with periods in the 10–100-sec range are produced by the following sources: volcanic eruptions, mountain-induced vortex-shedding by the jet stream, severe storm systems that penetrate the tropopause, aerodynamic turbulence, supersonic motions of auroral electrojets, and weather frontal systems. Acoustic infrasound of shorter periods is produced by waves on the surface of the ocean, by large meteors, by the re-entry of

spacecraft, and by the conversion of magnetosonic waves in the magneto-sphere into acoustic waves at the base of the ionosphere. Earthquakes will radiate infrasound into the atmosphere from surface Rayleigh waves, tsunamis, and large-scale surface displacements near the epicenter of the earthquake.

Long-period internal gravity waves are generated in the atmosphere by atmospheric nuclear explosions, large volcanic eruptions, large earth movements in earthquakes, and the auroral electrojet-current systems that appear at times of magnetic substorms in the ionosphere of the auroral zone.

A great deal has been learned about auroral substorms from the study of auroral infrasonic waves. However, the mechanism for generating the acoustic energy in the neutral gas from the auroral electrojet is not yet known. Similarly, we know that much acoustic energy is produced as infrasound from meteorological sources, particularly by severe storms, but there is as yet no clear idea of the manner in which the acoustic energy is produced.

The wind and temperature structure of the mesosphere has been studied by observing the effects on infrasonic wave propagation of acoustic waves of 10–100-sec period of oscillation from volcanic eruptions and from wind turbulence effects in coastal mountain ranges. On the East Coast of the United States, marine storms have been effectively tracked by observing the 8-sec period microbaroms generated by the air–sea interaction. Theoretical work has shown that certain acoustic modes may be excited by tsunami waves as they transmit energy into the atmosphere. Because the acoustic modes travel faster than the seismic sea waves, these infrasonic waves can give an advanced warning of the approach of a destructive tsunami.

Possibly the clear-air turbulence that aircraft sometimes encounter can be detected by its multipole acoustic radiation. There are some traveling pressure disturbances associated with jetstream turbulence that move at subsonic speeds beneath the jet stream. A synoptic study of these pressure fluctuations might allow the location of the jet stream to be established by ground barometric observations.

Internal gravity waves of 15–120-min periods of oscillation have been shown to play a large role in the temperature, wind, and electron density structure of the upper atmosphere. Observations of meteor trails and rocket-released luminous vapor trails coupled with electromagnetic sounding of the ionosphere have provided data for relating internal gravity waves in the upper atmosphere to geomagnetic storms. An unresolved question here is whether joulean heat loss from ionospheric currents or a Lorentz body force or some other mechanism is responsible for the generation of the traveling ionospheric pressure waves.

Much theoretical work has been done on the propagation of infrasonic

waves in the atmosphere, enhancing our ability to detect atmospheric explosive sources, but there has been inconclusive research into the nature of the basic propagation and absorption mechanisms. More infrasonic observatories should be established to determine the characteristics of the very rich atmospheric spectrum of both acoustic and internal gravity waves. A better understanding of the basic mechanisms by which infrasonic waves are produced is essential to our understanding of the energy balance within the atmospheric environment.

We see from the foregoing that the generation and propagation of infrasonic waves in the atmosphere is a geophysical phenomenon, often involving mutual interactions between the atmosphere, the oceans, and the earth. Additional discussion of such interactions is given in Section 4.2.

3 Interaction with Other Physics Subfields

3.1 ACOUSTICS AND SOLID-STATE PHYSICS

In addition to the new areas of investigation developed in the past five years (see Chapter 2), older research areas have continued to produce new developments. Previously conducted studies of crystalline dislocation interactions with acoustic waves have been applied to measurements of the dislocation–phonon interaction. This information has been used in studies of the plastic flow of crystals at high time rates of strain. Also, acoustic measurements have been used in conjunction with other mechanical measurements in plastic-flow experiments to limit the possible models under consideration for the description of plastic-flow processes. Acoustic measurements have wide potential application as nondestructive probes of the properties of materials. In addition, acoustic studies of electron–phonon measurements have revealed dislocation effects in superconductors. This finding stimulated much related work on the plastic flow of superconductors that has implications for the study of ordinary superconductors as well as for current problems related to the plastic flow of the surfaces of neutron stars. Current research suggests that these stars are composed of neutrons in a body-centered cubic array of high density. The questions raised show how much work is needed in relatively "old" fields; for example, scientists are asking questions about creep and starquakes on neutron stars when they still do not fully understand either creep in metals or earthquakes.

Much research during the past five years dealt with the acoustical study

of phase changes, particularly of ammonium halides and ferroelectrics. In addition, the first successful measurements of elastic constants of noble-gas solids were achieved. Other work that made progress in this interval was concerned with the acoustoelastic effect, in which internal stresses act on acoustic waves in much the same way that these stresses act on optical waves to produce the photoelastic effect. The acousto-elastic effect could provide a means similar to the photoelastic effect for measuring strains in optically opaque materials. Work on magneto-elastic, acoustoelectric (amplification of acoustic waves), elastooptic, and impurity-scattering effects also proceeded at an accelerated pace. Studies of the impurity-scattering effects dealt in part with the interaction of acoustic waves with Jahn–Teller ions (ions whose displacement from the lattice site in cubic symmetry creates a new cubic potential of lower energy).

Acoustics has provided a valuable tool for solid-state physicists engaged in the determination of the superconducting energy gap and the measure-ment of the anisotropy of the gap. Acoustical techniques also have yielded a substantial amount of information about the dimensions of Fermi sur-faces.

Illustrating a useful exchange of techniques between acoustics and solid-state physics are spin-lattice interactions, acoustic nuclear magnetic resonance (NMR), and acoustic electron spin resonance (ESR).

Acoustics also has adopted a number of techniques developed in solid-state physics. For example, the concepts of laser action have been applied to the development of a phonon maser. Further, an improved knowledge of semiconducting films has made possible the amplification of ultrasound. And the direct production of ultrasound from the incidence of electromagnetic radiation on a metal has become a practical method of high-frequency sound generation.

3.2 ACOUSTICS IN PLASMA PHYSICS

Charge carriers that can interact with sound are found in liquid metals, electrolytes, and ionized gases (plasmas). In this section we discuss briefly the role of acoustics in plasma physics. Since the Panel on Plasma Physics and Physics of Fluids also will deal with this subject, we will limit our discussion to a brief survey of some problems in plasma physics that are of particular interest to acousticians.

In a fully ionized plasma, longitudinal wave modes exist that might be classed as sound. The high-frequency mode is usually called an electron plasma oscillation, and the low-frequency mode, an ion-acoustic mode. The latter mode corresponds to the ordinary sound wave in a neutral gas;

at low frequencies the wave speed is independent of frequency and is related to particle temperature and mass in much the same way as in a neutral gas.

In a weakly ionized gas, such as that found in a glow discharge, in which the degree of ionization often is only one part in 10^6, the effect of the electrons and the ions on the overall dynamics of the gas is comparatively unimportant. Sound can be generated and transmitted through neutral gas, with practically no influence of the electrons and ions on the wave speed. However, since the electron gas generally is much hotter than the neutral background gas, there is a steady heat flow to the neutral gas, and a modulation of this heat flow can be an important source of sound. Acoustic instabilities may result. Furthermore, as a result of the strong collisional coupling between the neutral gas and the charge of particles, the presence of a sound wave in the neutral gas leads to fluctuations of electron and ion density. These fluctuations offer new possibilities for measuring sound through the use of probe techniques, developed in plasma physics, for electron and ion density measurements.

These two aspects of weakly ionized gas, the heating of the neutral gas by the electrons and the strong coupling between the neutral gas and the charge carriers, are the basis for many applications of interest to acousticians. Thus, the electric modulation of the temperature of an ionized gas is the basis for sound generation in the ionophone, corona discharge (high-voltage lines), and thunder, as well as for instabilities and acoustical oscillations that can occur in laboratory gases and gas lasers. In addition, electrostatic probes that permit measurements of density fluctuations in the neutral gas component in a plasma have been used for measurements of sound speed and gas temperature and in the study of turbulent flows.

In the presence of a magnetic field, the dynamics of a conducting fluid or plasma becomes quite complex. A low-frequency transverse wave can be transmitted as well as ordinary longitudinal sound waves. (This transverse wave mode is named for Hannes Alfvén, who received the 1970 Nobel Prize in Physics for his contributions to the dynamical theory of conducting fluids.)

In these relatively novel aspects of acoustics that overlap other subfields of physics, many challenging research problems related to both basic scientific aspects and applications have yet to be solved.

3.3   ACOUSTICS OF A QUANTUM LIQUID

Superfluid helium is rich in its variety of propagating modes, the study of which involves the fruitful interplay of two normally separate disciplines— acoustics and low-temperature physics.

In the study of liquid helium, the transition from the normal liquid to the superfluid (the lambda transition) has been given extensive attention. It is the earliest known transition of this type and is the most accurately measured one, since temperature measurements at it with resolution to less than a microdegree Kelvin have been possible. Many of the measurements on this transition have involved the measurement of the attenuation of First Sound. This is the mode that is approximately the same as the sound wave found in ordinary liquids. Such measurements yield information about the relaxation times of the fluid-order parameter and the fluctuations that occur in this parameter. The observed results are not completely predicted by current theory; and further development of the theory is needed, as well as dispersion measurements, to supplement the attenuation measurements. In particular, attenuation measurements at the lambda transition in the frequency range 3 MHz to 1 GHz are needed, as well as dispersion measurements and any measurements at pressures other than the vapor pressure. Measurements on this transition are not only of interest in liquid helium research but also should shed light on a wide variety of phase transitions.

In addition, First Sound measurements have provided information on the elementary excitations—phonons and rotons—and their interactions, particularly at low temperatures and, more recently, at elevated pressures.

A noteworthy instrumental achievement has been the development of a spin-phonon spectrometer for measurement at 60 GHz. Acoustic interferometric techniques have also been used successfully in accurate measurements of helium film thicknesses as small as 10 Å. Quarter-wave resonances have been found for films of less than three atomic layers, indicating that the breakdown of the hydrodynamic continuum approximation might become apparent only for even thinner films. The success of the new techniques has increased the upper frequency limit of acoustic measurements in liquid helium by almost two orders of magnitude.

The next mode to be discovered, Second Sound, is a temperature and entropy wave that propagates in bulk helium. It has played a leading part in the development of an understanding of superfluid helium, being predicted by the Tisza two-fluid model and by the Landau theory of the superfluid state. Accurate measurement of its properties has provided both verification of theory and information about superfluid parameters. Recent measurements have been centered on the submillidegree region of the lambda transition and at very low temperatures in mixtures of ^{3}He and ^{4}He. A new instrument here has been a mechanical Second Sound transducer that employs an oscillating superleaker. (Superleak is the name given to superfluid flow in narrow pores.) These transducers produce no heat and may well supplant the traditional heater–thermometer trans-

ducer. Calibration of the superleak transducer, possibly by a reciprocity method, offers a novel and challenging acoustical problem.

Third Sound is a propagating surface wave on a superfluid helium film, involving a longitudinal oscillatory motion of the superfluid component parallel to the substrate, with the normal (nonsuperfluid) component stationary. It is accompanied by a temperature oscillation and both evaporation and condensation of vapor at the free surface of the film. Third Sound provides information about the average superfluid density in a very thin, "unsaturated" film. At a fixed temperature, there is a critical film thickness below which the phenomenon is not observed. For thicknesses just below this value, the superfluid density remains finite, but there is no superflow. There is inadequate understanding of this behavior, as well as of the theory of the depression in the superfluid density and the superfluid onset temperature as a function of film thickness.

Third Sound measurements have been made in films with only four atomic layers, another indication of the widespread validity of continuum hydrodynamics.

No measurements of Third Sound have been made below 1 K. It is important to obtain information about the coherence length as $T \rightarrow 0$.

Superfluid helium is a macroscopic quantum system, and it is to be expected that there is a macroscopic quantum uncertainty principle that governs its behavior. Very recent Third Sound attenuation measurements give the first experimental evidence of this relation. It is possible that the very existence of a macroscopic quantum uncertainty relation will reopen the historic debate of the 1930's on the nature of physical reality. If this comes about, it is interesting that acoustics and low-temperature physics, which on the surface seem remote from such questions, will have played a key role.

Fourth Sound is a compressional wave of the superfluid component through tiny pores of a finely dispersed solid filled with liquid helium. As in Third Sound, the normal fluid component is locked in position. Fourth Sound provides one of the most accurate methods in determining the superfluid fraction of liquid helium. It has proved to be a powerful tool in the study of persistent currents in superfluid helium. The velocity of such currents can be determined by measuring the Doppler shift of Fourth Sound, and such measurements have established the fact that helium in a superleak is completely analogous to an irreversible Type II high-field superconductor.

Except for its use as a refrigerant or as a dense form of helium for storage purposes, there have been no practical applications of superfluid liquid helium. Persistent currents demonstrably have lifetimes that are probably astromomic, so that there should be potential applications in

inertial guidance systems or memory systems. Superfluid films are remarkable for their uniformity of thickness and their wetting ability. Submicroscopic nonuniformities in the substrate on which they are deposited are detected by the scattering they produce in Third Sound waves. Thus, excellent optical surfaces can scatter more than flame-polished, non-optically flat glass surfaces. When such practical uses for superfluid helium are developed, the sound that propagates in the helium will probably play an important part in their development.

Near the lambda transition, the most sensitive thermometer is the velocity of sound in liquid helium. Temperature resolutions, and absolute temperatures relative to the lambda temperature, of considerably better than a microdegree have been achieved.

3.4 APPLICATIONS OF ULTRASONICS TO GASES AND LIQUIDS

Ultrasonic investigation of gases has provided a foundation, principally through measurements of sound velocity and absorption, for both theoretical studies of the structure of gases and practical applications. Such data have proved useful in architectural acoustics and at the same time improved the understanding of molecular structure and the energy-transfer processes in gases. One practical result of these studies has been the development of the acoustic thermometer, based on accurate knowledge of the sound velocity in hydrogen gas, for use as an absolute standard at low temperatures.

Another example can be found at high temperatures. Ultrasonic measurements at temperatures up to 8000 K have been used to measure both temperatures and transport properties (viscosity, diffusivity, thermal conductivity) in monatomic gases. These data are now of considerable practical significance in the solution of atmospheric re-entry problems and in the design of jet engines. Precise ultrasonic measurements in helium over the temperature range up to 2000 K and over pressures up to 200 atm provide useful engineering data and can be used in the monitoring of conditions in gas-cooled, fast-breeder reactors employing helium.

Ultrasound has also been one of the primary tools in the investigation of fast chemical reactions that occur in both gases and liquids. Such studies have yielded information about the individual steps involved in such processes and the corresponding chemical reaction rates. In several recent investigations, the precise measurement of ultrasonic velocity over a range of temperatures and pressures is used to obtain accurate thermodynamic data in liquids that are essential, for example, in the study

of nonlinear acoustics. Such measurements in molten metals and their alloys, together with sound-absorption measurements in the alloys, are useful in developing improved techniques for alloying. Here too there is an important potential application. Liquid metal fast-breeder reactors appear to offer the solution to the problem of generating vast amounts of electrical power without depleting nuclear fuel or polluting the environment. Ultrasonics can be used to obtain the necessary engineering data from the liquid metals and can provide the monitoring devices required to determine the effects of such reactors on the environment.

Ultrasonic studies of glass in the liquid region above the transition temperature permit the study of density variations. These variations, which account for a major portion of the intrinsic optical losses, are frozen in at the glass transition temperature. Furthermore, the process of chemical tempering used for obtaining high-strength glass relies on the diffusion of large ions into the surface layers of the glass. These ionic motions in the glass also depend on the fluctuations present in the liquid. Thus, studies designed to produce improved optical glass for use in fiber optics and glass lasers or structurally strengthened glass must begin at high temperatures in the corresponding liquid; ultrasonic measurements can make a major contribution to these studies.

In the coming decades, the need for ultrasonic techniques in the investigation of liquids and gases at high temperatures is likely to increase. Although no dramatic breakthrough can be predicted at the present time, the role of ultrasonic studies of fluids in efforts to satisfy the increasing demands for electrical power, optical communication systems, structurally strengthened glass, and new alloys is evident. Continued research is warranted in view of the practical results to be gained from ultrasonic research in fluids, especially at high temperatures. Greater emphasis should be placed on studies of extreme temperatures, pressures, or frequencies, from which practical or new findings are most likely to result.

A breakthrough in high-frequency absorption measurements in liquids occurred in the 1960's when the availability of lasers made the Brillouin light scattering a matter of strong acoustical interest. This development forms an excellent study case for the interaction of several branches of physical science and is worth considering briefly.

In 1922, Brillouin developed a theory for the scattering of light by the density (thermal) fluctuations in liquids. The scattered line differed in frequency from the source by an amount equal to the frequency of the scattered thermal phonon. The measurement of the frequency shift was in effect a measurement of the velocity of sound at the frequency of the scattered phonon. If the width of the scattered line could be attributed solely to this process and measured, the sound absorption could also be

measured. Since the acoustic frequencies involved were in the gigahertz range, an upward extension of frequency by a factor of 10 or more was possible.

Development of this method was retarded, however, by the fact that the line widths of the best optical sources were too great to yield any reliable acoustic measurements other than some rough dispersion measurements. The advent of the laser changed the situation completely. As a result, reliable sound absorption and velocity measurements can now be made in liquids up to 6 GHz, and even higher frequencies may be possible.

4   The Role of Acoustics in Other Physical Sciences

4.1   OCEAN SCIENCES

Acoustics has played a significant part in underwater communication and sensing since about 1920. Currently, acoustical scientists are becoming increasingly involved in problems in physical oceanography and geophysics.

Early underwater acousticians gave the name, "transmission anomalies," to the large deviations from spherical spreading of sound rays in the deep sea; later, waveguide solutions were obtained for classes of underwater sound ducts created by velocity gradients or shallow water. More recently, the use of computers in studies of ray and mode propagation has increased understanding of these phenomena. The digital computer has become the standard tool for mapping the intricate fields of convergence zones due to refraction and the gray shadow regions caused by multiple paths and surface and volume scatter. Meanwhile, ocean propagation experiments with improved adaptive beam-forming transducers and adaptive filters have employed statistical decision theory to elucidate and overcome the variability and noisiness of the medium. All these efforts had (and still have) the immediate goal of more effective use of sound for communication, sensing, and navigation. Solutions to these problems call for a better understanding of solid-state transducers, propagation in fluctuating inhomogeneous fluids, statistics, and signal processing. Research on these subjects has been heavily supported by the U.S. Navy because of its importance to antisubmarine warfare.

Other ocean sciences are beginning to depend heavily on acoustical data on absorption, dispersion, nonhomogeneity, anisotropy, nonlinearity,

and temporal fluctuations to identify and measure the physical character-
istics of the ocean medium. Such a role for acoustics is clearly the macro-
scopic equivalent of the traditional use of acoustics or electromagnetics
to deduce the internal structure of solids and fluids. Ocean acoustic physi-
cists now use scattered underwater sound to probe and describe statisti-
cally the ocean surface, bottom, and volume inhomogeneities. They em-
ploy transmitted underwater sound to evaluate ocean turbulence, varia-
tion of refractive index, the presence of internal density waves, and the
molecular structure of the solute–solvent complex in seawater. As a result,
underwater acoustics now interacts closely with a number of diverse
ocean sciences, and the relevance of underwater acoustics to oceanography
is increasingly apparent. Some vital research areas provide illustrations.

At resonance, the acoustical scattering cross section of a common gas
bubble in the ocean is of the order of 10^3 times its geometrical or optical
cross section. Therefore, an acoustical spectrometer (for example, a vari-
able frequency pulse-echo system) or a remote acoustic probe is uniquely
capable of identifying free microbubbles and bubble-carrying fish in the
sea. Vast numbers of near-surface bubbles are produced by photosynthesis
of phytoplankton, breaking ocean waves, falling micrometeorites, up-
welling convergent waters, and biological activity. Chemical and biological
oceanographers have shown that the surfaces of moving bubbles are criti-
cally important in aggregating and scavenging dissolved carbon and
phosphates; meteorologists have postulated breaking bubbles as the princi-
pal source of surface droplets whose airborne salts influence thunderstorm
activity at sea; transport analysis is used to understand the dynamics of
acoustically detected bubbles that act as evanescent tracing particles of
the turbulent, near-surface ocean medium; naval engineers regard the col-
lapse of bubbles as the cause of cavitation damage to ship propellers; and
marine biologists are acquiring a better understanding of ocean ecology by
in situ classification of absorption and scattering cross sections of bubble-
carrying marine animals and subsequently by their acoustic identification
at a distance. In all these examples, the application of acoustical devices
to the study of bubbles is crucial in advancing knowledge and fostering
the solution of major problems. High-intensity sound constitutes a power-
ful experimental tool for studying the dynamics of formation and col-
lapse of cavitation bubbles under controlled conditions. The develop-
ment of practical acoustical instrumentation is essential for the effective
study of the actions of ocean bubbles.

Wave-scattering and transmission theory for randomly rough surfaces
has developed in the past 15 years. Scattering theory is being extended
and applied to electromagnetic remote sensing of the surface of the
planets, as well as to the remote underwater acoustic characterization of

both the ocean surface and bottom. The theory, together with large tank experiments, has shown that the coherent and incoherent components of forward-scattered or back-scattered sound can be used to identify the spectral and statistical characteristics of the surface from afar. The development of side-looking sonars will aid the geophysicist in applying this knowledge of scatter theory to the detailed structure of the topmost layer of the ocean floor. The current theoretical thrusts are toward understanding near-field effects, complex impedance boundaries, nonplane waves, and near-grazing incidence, in which diffracted shadowing and secondary scatter become important. In regard to instrumentation, it will be necessary to devise better sources, signal processors, and information presentation than are available for current seismic research if ocean scientists are to identify and measure the deposition and erosion of viscoelastic sedimentary layers.

4.2   GEOPHYSICS

Geoacoustics is here defined as including any vibration or seismic wave propagation in the solid earth, as well as large-scale wave propagation in the atmosphere and oceans. The breadth of this definition reflects the activity of the acoustical scientist in earthquake seismology, earth tides, seismic prospecting, atmospheric sciences, ocean exploration, and noise abatement. Because lunar exploration also entails the use of techniques of geoacoustics, the definition of geoacoustics probably should include the acoustical wave in satellites and planets as well.

Both theoretical and experimental advances in the observation and understanding of slow oscillations of the earth have occurred during the past five years. Scientists have derived a more accurate model of the earth's interior from observed periods and amplitudes of the fundamental modes of vibration of the earth, viewed as an imperfect solid sphere. Similar studies of the moon have been aided by the recent placement of seismometers on the moon's surface, with seismic vibration up to almost 1 hour being recorded after the durations of impact of meteors and jettisoned space vehicles.

Strain seismometers have recorded the daily tidal movement of the earth's crust, and data are being accumulated on variations in the earth's tides as a function of geographic position. Large areas have now been instrumented (in Japan, for example) to monitor even slower changes in earth displacements, working toward an earthquake-prediction possibility. Strain seismometers are also deployed above a deep well near Denver, Colorado, in which fluid injection apparently triggered a series of earth-

quakes in a formerly quiescent region. One implication of this unexpected occurrence is that accumulated strain can be released at will by a liquid injection, thus avoiding the catastrophic release of strain associated with eventual rupture.

Acoustic detection of atmospheric nuclear explosions (and other distant low-frequency sources of sound) prompted the development of infrasonic microphones and the treatment of wave propagation in the inhomogeneous atmosphere. Acoustic signals from small explosions at controlled heights have provided a direct measure of sound speed in the altitude range of 30 to 100 km, thus improving the model of assumed atmospheric structure. Airborne infrasound can create substantial amplitudes of seismic waves through the process of air coupling. Whenever the apparent phase velocity of the airborne wave matches the speed of propagation of some type of seismic wave, a coincidence effect results in an amplified seismic wave. A fuller discussion of infrasonic waves in the atmosphere is given in Section 2.7.

4.3   CHEMISTRY

The study of ultrasonic absorption processes in fluids, which has already been mentioned in Section 3.4, contributed greatly to the knowledge of fast reaction rates in gases and liquids, an important branch of chemistry. In fact, the 1967 Nobel Prize in Chemistry was awarded to Manfred Eigen for work that began in studies of the ultrasonic absorption in electrolytic solutions.

An obvious example of an electrolytic solution is the ocean. Since about 1955, it has been assumed that the basic mechanism that describes the attenuation of sound in seawater is a relaxation process involving principally $MgSO_4$ in various ionized combinations. The relaxation frequency is in the neighborhood of 100 kHz. Recent research into this physicochemical effect has stressed the need to understand the parameters of temperature and pressure in the reaction. Measurements in the region below 10 kHz suggest still another attenuation mechanism, possibly dissociation of the HCO_3 ion. Since the largest part of the background noise in ocean acoustic propagation occurs at such low frequencies, the need to elucidate this new mystery has stimulated research both in the laboratory and at sea.

5 Acoustics in the Life Sciences

5.1 PHYSIOLOGICAL AND PSYCHOLOGICAL ACOUSTICS

The study of acoustics began in ancient Greece when Pythagoras described the relationship of the length of strings on musical instruments to musical intervals. That type of study would be classified today as psychoacoustics, which is an interaction between acoustics and psychology. Psychoacoustics was not treated separately from physical acoustics until the nineteenth century, after the duality between mind and matter became of interest to a German physicist, G. T. Fechner, who then created psychophysics, a discipline devoted to the study of the interactions between these two branches of science.

Psychoacoustics was and still is primarily concerned with the responses of animals and man to sound, and it has remained an active and innovative discipline. Recently, it has been sharply expanded as a result of two fundamental discoveries. The first was that people can apparently express experienced sensation magnitudes in numbers with reasonable consistency. They also can make direct intersensory comparisons; for example, they can match the loudness of a sound to the brightness of a light. These matches can be predicted from numbers assigned independently to subjective magnitudes of loudness and brightness, which proves that the numbers are assigned meaningfully. This discovery has had a strong impact not only on psychoacoustics but also on general psychophysics and has led to a growing number of social and industrial applications. The second discovery deals with the application of the theory of signal detectability to the detection of small signals, small signal differences, and signals in noise. A large number of experiments have shown that the problem of auditory signal detection reduces to the discrimination between the noise-plus-signal and noise-alone events. The associated theory has increased the rigor of psychoacoustic experimentation and has made it possible to separate sensory characteristics from decision processes.

Traditional psychoacoustics deals with hearing, but more recently the sense of touch has been included. Systematic knowledge of cutaneous sensitivity so far is very limited, and much fundamental work is necessary. One difficulty stems from the problem of stimulus definition. Vibration is propagated in the skin and underlying tissues in the form of shear waves in a nonhomogeneous, viscoelastic, bounded medium. However, a significant simplification was achieved by imposing a rigid boundary at the skin surface. Use of this procedure showed that at least two sensory systems intervene in tactile sensations.

Speech perception is another aspect of psychoacoustics. It strongly interacts with speech production and speech processing and, for this reason, appears in a separate part of this chapter (Section 5.3).

Somewhat parallel to the interaction of acoustics with psychology is its interaction with physiology, often described as physiological acoustics. For the most part, physiological acoustics is concerned with mechanical and nerve processes elicited by sound waves in the ear and in parts of the nervous system. The mechanical processes are reasonably well known, following upon the investigations of Georg von Békésy, who received his Nobel Prize in 1961 "for his discoveries concerning the physical mechanisms of stimulation within the cochlea." The cochlea is the auditory organ in which acoustic vibration is converted to nerve impulses. Mechanically, it consists of a long canal filled with fluid and divided longitudinally by a viscoelastic partition with spatially variable compliance. The transversal waves that are propagated along the partition as a result of sound entering the ear raise some interesting mathematical problems. Currently, the Mössbauer effect is being used in investigations of these problems.

In contrast to the mechanics of the ear, systematic knowledge of the auditory nervous system still is rudimentary. The transduction of mechanical events into nerve impulses provides the first stumbling block. In spite of many experimental attempts, the chain of electromechanical events that leads from the mechanical deformation of the sensory cells to the generation of nerve impulses is still unclear. The problems involved extend deeply into membrane and molecular biophysics.

Much experimentation focuses on the spatial and temporal distribution of neural activity in the auditory nerve and in various centers of the brain. Among the goals of this research are the determination of the way in which the parameters of sound are encoded in the nervous system and, eventually, the explanation of the auditory characteristics. Here, physiological acoustics touches on one of the most intriguing problems of science—the nature of the sensory brain function. No solution is imminent; so far, even reasonable hypotheses about the conversion of nervous impulses into sensations and perceptions are lacking. Nor do scientists yet know how to describe sensations. But psychological and physiological acoustics, as well as other areas of psychophysics and sensory physiology, may lead more directly than other disciplines to the heart of the problem.

Psychological and physiological acoustics were closely related sciences in the nineteenth century. Subsequently, they separated. Determined efforts to reunite them currently are under way. The basis for these efforts is the belief that auditory characteristics can be explained physiologically in reasonably simple terms. A rapidly increasing amount of numerical

psychological and physiological data that make possible the application of mathematical theory support this belief, and recent results are encouraging.

Not only have psychological and physiological acoustics had many applications to medicine and national defense, but they have been involved also in various social problems. Diagnosis of ear disorders relies heavily on psychoacoustic and electrophysiological tests and on correlations that have been found between the results of these tests and anatomical changes. Recently, the instrumentation and methodology developed for acoustic impedance measurements at the eardrum have been accepted for diagnosis of the highly prevalent middle-ear disorders. A modern clinic for ear disorders has a substantial amount of electroacoustic equipment for psychoacoustic tests of hearing, recording of brain-wave changes induced by sound, and impedance measurements at the eardrum. Psychoacoustic investigations also have played an important role in the improvement of military communications, the determination of tolerable levels of noise, and the assessment of the effectiveness of ear protectors. Physiological and anatomical investigations produced evidence of damage to the auditory system resulting from excessive noise. In the past few years, a number of psychoacoustic investigations have dealt with the effects of sonic boom. The results are highly pertinent to the discussion of government policy with regard to supersonic transport planes.

Another aspect of acoustics that deserves separate mention is its interaction with zoology. Although many physiological experiments on animals are performed as a substitute for experiments on man, others are directed toward a better understanding of, for example, various kinds of animal communication systems or disturbances in underwater communication that are produced by marine noise. In view of the increasing interest in the human environment and the recognized need for keeping the biological balance, zoological acoustics propably will increase in scope. This development is especially likely since animal behavior can be usefully influenced by recorded animal sounds.

5.2   MEDICINE

The earliest interaction between acoustics and medicine was in the specialty that treats diseases of the ear, namely, otology. In fact, the early work of the nineteenth century in psychophysics was performed in large part by practicing otologists. Acoustical instruments like tuning forks were used in ingenious ways to diagnose diseases of the ear and were subsequently supplemented by the development of the vacuum-tube and

transistor audiometers, which supplied pure tones at specified frequencies with variable intensity, also masking noise. Since the 1930's, work on speech perception has also influenced audiometry, and even further diagnostic precision was based upon the responses of hard-of-hearing patients to spoken words. Increased knowledge of the mechanism particularly of the middle ear permitted surgical remedies to be introduced to alleviate certain forms of conductive deafness. Miniaturization of acoustical and electronic components has made available to the hard-of-hearing population the great variety of personal hearing aids, and other developments have provided larger classroom units in schools for deaf and hard-of-hearing children. Recent years have seen a closer professional interaction between medical otologists and their fellow professionals, audiologists, with a variety of specialists in acoustics in diagnostic, prophylactic (industrial noise), and rehabilitative procedures.

There remains a pressing need for improved methods of communicating with the deaf. The medical profession generally agrees that complete loss of hearing is a greater handicap than total blindness. If a national effort could lead to a practical solution of bypassing the ear *per se*, for example, by using another sensory system, such as the tactile (cutaneous), to convey information to the brain, the result might be highly beneficial, either as supplementary to or substituting for oral or manual communication.

Acoustics has made many other contributions to medicine, from Auenbrugger's first attempts to study chest diseases by finger-tapping on the thorax and the construction of the first crude stethoscope by Laennec to the rather recent insertion of miniature microphones into the heart through a catheter. The present status of acoustics in medicine and its probable future direction can best be discussed by citing selected examples.

A review of ultrasound in medicine in 1957[1] showed that its use as a diathermy agent was well established in medical therapy but that difficulties were encountered in its application to diagnostic techniques. The information supplied for diagnosis was not at that time sufficiently unambiguous and reliable. A more recent review[2] is revealing in that diathermy does not appear as a separate topic. On the other hand, although many of the problems related to the use of ultrasound as a diagnostic tool 13 years previous had been solved and its applications greatly increased, only recently could it be regarded as having reached the state at which it could influence the treatment of patients appreciably.

Ultrasonic techniques have been employed for some years to diagnose mitral disorders of the heart; more recent studies have opened new possibilities for observation of particular heart functions. Ultrasonic tomo-

graphic cross sections of the heart can now be obtained at a preset cardiac phase, thus making important morphological measurements possible. Doppler methods used simultaneously with pulsed ultrasound can be employed for examining some cardiac disorders. By a Doppler method, aortic blood flow can be observed through the intact chest wall. Recent experiments also suggest that ultrasonic echography of the lung may prove valuable in the diagnosis of pulmonary embolism as well as other lung disorders. Visualization techniques continue to find additional applications, for example, breast and thyroid tumors and kidney and bladder disorders. A recent development, with application in obstetrics and gynecology, makes use of internal scanning to improve penetration and resolution limited by absorption.

In neurology, threshold dosages that will produce lesions in the mammalian brain have been determined in the low megahertz frequency region over a broad acoustic intensity range for single-pulse time durations of exposure extending to 10^{-4} sec. A two-dimensional visualization scheme, using omnidirectional scanning, relief presentation, and computer control of transducer positioning and data handling, exhibits considerable neuroanatomic detail when an opening has been made in the skull bone. However, little or no progress has been made toward transmitting directly through the skull. Advances continue in one- and two-dimensional echoencephalography, and much information is now available on the scattering profile of the skull. Use of the dynamic information obtained from echoencephalography has revealed that some pulsatile echos are influenced by external stimuli and can be correlated with physiological variables. Details of the absorption processes continue to be obtained, but a clear understanding has not yet resulted.

Digital computers have been introduced into ultrasonic visualization systems to control many aspects of the procedures. Use is limited, probably because of the expense, but the potential benefits ensure that computer applications will increase steadily. The development of ultrasonic holography as a soft-tissue visualization technique has received some attention, although progress to date has been disappointing.

The increased use of diagnostic ultrasound has raised the question of safety from prolonged and repeated exposures at these levels. Consequently, an effort to determine the permissible dosages at which patients can be treated before ultrasound becomes hazardous is receiving attention. This problem is one in the study of which the physical scientist can play a significant role. It also is an adjunct to the broader problem of identifying and elucidating the physical mechanisms of interaction between ultrasound and biological systems.

5.3   SPEECH COMMUNICATION

The basic use of speech and the hearing of speech by man is the trans-
mission of thought and memory from the cognition and understanding
of one person to that of another. A major goal of basic research in speech
communication is therefore deeper understanding of the processes of
speech production and perception. Some applications that motivate
this research include improved speech aid and training procedures for
those who have handicaps and the potential development of devices
such as speech synthesizers, speaker recognizers, and speech recognizers.

Speech production involves both acoustical and physiological processes.
The flow of air from the lungs is modulated to generate sound, and the
sound sources are modified by the cavities of the vocal tract. These
sources and cavities are controlled during speech production by contract-
ing a variety of muscles that control the tongue and the vocal cords and
initiate the airflow from the lungs. Current research includes theoretical
and experimental studies of sound generation by sources in acoustic reso-
nators and of the mechanism of vibration of the vocal cords; investiga-
tion of the movements of the speech structure through x-ray and motion-
picture photography and (more recently) ultrasonic techniques; and
electromyographic observations of muscle activity underlying these move-
ments. During the past decade, the digital computer and digital signal-
processing techniques have played an important role here, both in the
acquisition and manipulation of acoustic and physiological data and in
the modeling of various aspects of speech production.

Research in speech perception seeks to understand how a listener is
able accurately and rapidly to decode and understand speech in view of
the fact that aspects of the acoustic signal representing a given phoneme
are strongly dependent on the context in which the phoneme occurs and
overlap with the acoustic cues for other phonemes. Experiments com-
paring the perception of speech and nonspeech sounds have shown that
the processing of speech uses a special mode different from that of other
sounds. The processing of speech involves the categorization of the sound
in terms of specific properties or features that seem to occur universally in
language.

A modern laboratory in the speech sciences must be equipped to con-
duct short-time spectral analysis of speech sound and to display these
results in a convenient form. The capability of generating a variety of
synthetic syllables and speechlike sounds for studies of speech perception
is also desirable. Many speech research groups are now designing their re-
search facilities around a small or medium-sized digital computer, to
which are connected displays, graphical inputs, speech synthesizers, tape

recorders, and other audio equipment. For physiological studies of speech, there is also a need for specialized facilities separate from the computer, such as cineradiographic equipment and electromyographic instrumentation, but the trend is to use the computer to assist in the reduction and analysis of data from these sources.

Research in the past two decades has led to a greatly improved understanding of the nature of the acoustic speech signal and of the acoustic theory of speech production. Less well understood are the physiological processes of speech production and perception. There is need for improved procedures in investigating the activities of the relatively inaccessible speech organs, since present techniques are cumbersome and the analysis of the data is extremely time-consuming. Although investigators who work in the area of auditory physiology are gaining a more complete understanding of the responses of the more peripheral components of the auditory system to simple sound stimuli, essentially no contact has yet been made between this kind of research and research on speech perception. That is, we know almost nothing about the manner in which the relatively complex speech and speechlike sounds are transformed and stored in the various stages of the auditory system.

In the next decade, significant advances in our knowledge of the physiological aspects of speech production should take place, as improved experimental techniques become available for observing those processes and as more investigators become trained in the various disciplines that are involved in research of this kind. The gap between auditory physiology and speech perception should narrow as auditory physiologists devote their attention to higher levels in the auditory system and as the experimental techniques of those interested in speech perception lead to the formulation of more precise theories of speech perception. One practical outcome should be significant improvements in speech aids for the handicapped. Although devices such as automatic speaker recognizers or recognizers of words and sentences will probably never achieve broad use (primarily because of the inherent variability of the speech signal and the role played by semantics in the speech-communication process), it is expected that such devices should achieve some limited application in the years ahead.

6 Acoustics and Technology

Over the years, acoustics has given much to technology and received
much in return, but nowhere has this exchange been greater in its impact
than in the recording and reproduction of sound; a case history is appro-
priate here.

6.1 THE DEVELOPMENT OF SOUND REPRODUCTION

The faithful reproduction of sound by electronic means has now become
so commonplace that little thought is given to how this rather remarkable
achievement came about or how it has affected our lives. There exist few
homes in the more developed countries that are without some means of
recreating the sound of the human voice or the symphony orchestra. The
cultural influence of great works of music is thus brought within the
grasp of almost everyone. A stirring performance of a 400-year-old com-
position can be played identically, not only once but an indefinite num-
ber of times in countless homes. One might say that sound reproduction
does for musical art what photography does for visual art.

Reproduction of sound has only become possible within the last 100
years and exact reproduction (or nearly so) within the last 20–40 years.
Before the invention of musical notation, compositions were passed from
one performer to another by committing them to memory in a way analo-
gous to the way that history, literature, and crafts passed from one genera-
tion to another before writing had been invented.

Musical notation allows another performer at another time and place
to recreate in sound, with a variety of instruments and interpretations,
what has been written on paper. For many centuries it provided the only
means of "reproducing" music, in much the same way that a book or
newspaper "reproduces" human speech.

Sound reproduction as we know it today had its origins with system-
atic studies of how sounds are produced, how they are propagated, and
how they are perceived by the listener. From observations in nature, man
learned very early that mechanical motion was necessary for the produc-
tion of sound. However, it remained for the French philosopher and
mathematician Marin Mersenne (1588–1648) to study the subject scien-
tifically and to produce the earliest work dealing with the theory of music
and musical instruments, *L'Harmonie Universelle* (1636).

The primitive hunter who put his ear to the ground to hear the move-
ments of herds of animals some distance away was demonstrating the

fact that sound travels through a material medium. But the well-known experiment in which a ringing bell is suspended in a sealed jar from which the air is evacuated demonstrated that a material medium is *required* and that, with few exceptions, the human ear receives sound through some motion of the air with which it is in contact.

During the nineteenth century, many advances were made in the detailed understanding of various aspects of acoustics, both theoretical and practical. The still standard mathematical treatise, *Theory of Sound*, was written by Lord Rayleigh, while Helmholtz in his work treated the theoretical, physiological and psychological aspects. In 1807, Thomas Young described a method of recording the vibrations of a tuning fork on the surface of a drum. This discovery suggested that, if the process could be reversed, the original sound could be reproduced. In succeeding years, a number of experiments sought to do this, but it remained for Thomas A. Edison (1876) to develop the first practical method. This consisted of bringing to bear on a tinfoil (later, wax) cylinder a chisel-shaped stylus attached to a diaphragm at the bottom of a funnel-shaped horn that collected the sound. The horn–diaphragm–stylus mechanism together ran on a screw geared to and turning with the cylinder. In operation, this left a fine spiral groove in the bottom of which there were "hills and dales" that, on playback, would cause the diaphragm to vibrate in the same way as it did at the time the recording was made.

With variations and improvements, this system worked reasonably well for 40–50 years. However, it suffered from one fundamental drawback: no more energy was available in the reproduced sound than was present in the recorded sound. In fact, losses always made it much less. At that time there was no way of providing amplification. Based upon some quite different observations by Edison, Fleming in 1904 had produced a two-element vacuum tube or valve. Two years later, De Forest added a third element, the grid, making possible the construction of amplifying circuits. The development of the vacuum-tube amplifier made a great many advances possible. In a short time the sound recording process became completely electrical. The widespread use of electrical sound reproduction techniques followed somewhat more slowly. But by mid-1920 most of the basic principles, upon which the present huge industry is based, were established.

The carbon-granule microphone, which A. G. Bell had fashioned for his telephone, became the means initially of translating the sound waves in air into electrical impulses that could be amplified by the new triode. A device not unlike Bell's telephone receiver, utilizing the principles of electromagnetism, first formulated by Oersted in 1820 and subsequently applied in the telegraph sounder, was then employed to translate the

amplified electrical energy back into mechanical energy, driving a stylus for cutting the groove in the record. Playback involved a similar process, except that to obtain the volume of sound required for general listening it was necessary to set more air in motion than could be accomplished by the usual telephone receiver. This led to a large diaphragm driven by a mechanism similar to that found in the telephone receiver, and the loud-speaker resulted. Over the next 25 years, these basic processes were greatly refined and improved. Other physical phenomena such as piezo-electricity, discovered by Jacques and Pierre Curie in 1880, were employed in various elements (microphone, pickup) of the system at various times. Because of similarities in electrical circuitry and the means of converting the electrical energy back into the mechanical energy of sound, the de-velopment of the phonograph became linked to that of radio, and to-gether they grew into a very large industry, which, in major, though often subtle, ways, affected and continues to affect the lives of everyone in the civilized world.

Over the years, improvements in the electric phonograph have generally been aimed at achieving more faithful reproduction while introducing the minimum of artificialities and complexities. Every element of the record-ing and reproducing system has undergone a tremendous improvement over the first primitive attempt. The almost noiseless plastic disk of today bears little resemblance to the wax cylinders and early shellac records. This improvement has been made possible by major developments in ma-terials and by the development of pickups that follow the grooves while requiring downward forces measured in tenths of grams as opposed to tens of ounces. And with the advent of stereophonic recording, the infor-mation content recorded has doubled.

The loudspeakers were one of the weak links in the earliest systems. The development of the moving-coil loudspeaker (which is the type al-most universally used today) represented a very large step forward. Be-cause even vacuum-tube amplifiers could only supply modest amounts of undistorted output power, considerable effort went into making the loud-speaker more efficient and into devising amplifier circuits that would pro-duce greater power with less distortion. Vacuum tubes of improved de-sign helped, but the basis for solving the problem of realizing high power with the minimum of distortion was largely set forth by Nyquist and Black in the early 1930's. Their work showed that, by feeding back to the input of the amplifier a small out-of-phase portion of the output, distor-tion introduced within the amplifier was dramatically reduced and the frequency response greatly improved. Negative feedback thus became an essential feature of almost all amplifiers—whether or not they were used in sound reproducing systems.

With a smooth extended frequency response and virtually all the power needed available, attention shifted again to the loudspeaker. However, it was some time before those concerned realized that the design criteria for the loudspeaker had been changed in a very significant way. The problem of creating a high-intensity magnetic field that would be uniform over the region in which the voice coil driving the diaphragm had to move to set the air in motion had long been a major difficulty in achieving conversion of electrical energy into sound with the minimum of variation over the full span of audible frequencies. Since efficiency was no longer important, it was possible to sacrifice some of the intensity of the magnetic field to achieve a more uniform field over a larger region. In combination with new approaches in speaker cone suspension and enclosure design, the results have been exceedingly good.

The development of the transistor by Brattain and Shockley in 1947 and the replacement of hot-filament vacuum-tube equipment with solid-state electronics helped to mitigate problems of bulk and heat production while improving frequency response still further and making even more power available at lower levels of distortion.

Thus, to reach its present state of perfection and widespread availability, the high-fidelity phonograph has evolved over a period of 100 years. Its development is marked by dependence upon physical principles and phenomena, in many cases adapted with considerable speed and ingenuity.

The case cited above outlines a major interaction between acoustics and technology. The telecommunications industry, taken as a whole, has had a tremendous effect on electroacoustics and the development of acoustical measurement methods. Reciprocally, the results of research into the acoustics of speech and hearing have influenced the technical requirements for telecommunication systems. We shall treat only a few of these interactions specifically here, but they underlie different aspects in every chapter of this report.

Of particular significance has been the design and production of a wide variety of electronic equipment, particularly pulse systems in the range of 10–1000 MHz. This has made possible many ultrasonic investigations in both liquids and solids. Closely related has been the development of the laser and its use in Brillouin scattering for high-frequency measurements of velocity and absorption in liquids, as is recounted in Chapter 3.

In underwater sound, the need for development of sonar equipment has provided a powerful stimulus to the design and construction of appropriate electronic and mechanical equipment. The invention of ceramic transducers 30 years ago greatly enhanced the flexibility of acoustic sources. At the same time, the efforts to extract information from a noise

background have brought about a steady increase in the sophistication of signal-processing techniques, involving arrays of sources and receivers, filters, fast Fourier transform techniques, and presentation methods.

The search for improvements in military sonar has yielded a variety of products for civilian use, including various depth sounders, underwater telephones, ship velocity meters, and apparatus for fish finding.

Ultrasonics has made an important contribution to the field of flaw detection by techniques of nondestructive testing of solid materials. Another industrial use of acoustics involves the ultrasonic agitation of fluids for such diverse purposes as mixing materials, ultrasonic cleaning, and aluminum soldering.

The interaction of modern psychoacoustics and medical acoustics with technology has been largely in the area of the design of specialized laboratory equipment. The devices used by otologists and audiologists have developed largely from work done in auditory acoustics. The most recent developments are an acoustic bridge for testing the function of the middle ear and an audiometer based on electroencephalic responses rather than on behavioral ones.

In geoacoustics, a coal mine disaster rescue and survival system is being developed for the Bureau of Mines that contains a subsystem to locate trapped miners by means of seismic waves generated by beating against the floor of the chamber. In addition, progress toward a possible means of forecasting earthquake occurrences is under way (see also Chapter 4).

Some applications of communication theory to seismic-wave detection were implemented as a result of the desire to use seismic waves to monitor underground nuclear explosions. This reservoir of knowledge has been applied to seismic exploration studies and its scope greatly extended, largely through the efforts of research teams supported directly by oil companies. Digital techniques have revolutionized data acquisition and analysis; they permit the scanning, merging, and organization of virtually unlimited amounts of data. However, the presentation of data still presents problems; the best graphical means of portraying a parameter that varies with three space coordinates and time leaves much to be desired.

6.2   SHOCK   AND   VIBRATION   ACOUSTICS

Shock and vibration acoustics has expanded rapidly in recent years. Manuscripts published in the *Journal of the Acoustical Society of America* under the heading "Shock and Vibration" comprise the third largest group of papers in this journal. The growth of the field is related to such factors as the following:

1. The industrial and military need for information on shell and plate vibration [with reference to underwater vehicles technology, missile technology, communications technology (vibration of crystal plates), and the like];

2. The rapid theoretical and experimental development of the area of random vibration and the need for greater understanding of structural fatigue under random loading;

3. The increasing use of digital computers to provide numerical solutions to problems not previously amenable to solution;

4. The advances in high-polymer technology that have provided effective damping compounds for use in vibration and noise control (for example, in the manufacture of "deadened" sandwich beams and panels composed of three or more metal–polymer laminations).

Shock and vibration acoustics shares common ground with the extensive physical and physical–chemical investigations of the viscoelastic properties of polymers that have been conducted in recent years. Audiofrequency measurements generally are made through a wide temperature range [and often converted by means of the Williams-Landel-Ferry (WLF) equation to a single temperature and an extremely broad range of reduced frequencies] to yield information on the basic physical properties of the polymers and their internal structure.

Shock and vibration acoustics also interacts with the biological sciences. The physiological effect of shock and vibration on man has been the subject of detailed studies made by the armed services (for example, a study of the effects of vibration on helicopter pilots), the National Aeronautics and Space Administration (a study of the effect of vibration on astronauts during launch), and industry, particularly the automobile industry. Reports of energy-absorbing devices for passenger protection in automobile crashes (inflating cushioned bumpers, collapsible steering columns, crushable dashboards, and viscoelastic seatbelts) are now commonplace. In addition, much effort currently is devoted to measuring the physical properties of biological materials such as bones, blood clots, and human and animal tissue with audiofrequencies.

The benefits of interaction with technology and industry are evident in such useful applications of vibration as soil penetration; rock drilling; rapid excavation*; conveyor and vibratory feeder systems; vibratory mix-

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* The report by the National Research Council Committee on Rapid Excavation, *Rapid Excavation: Significance, Needs, and Opportunities*, NAS Publ. 1690 (National Academy of Sciences, Washington, D.C., 1968), has stimulated consideration of laboratory adjuncts to earth-moving equipment and sonic drilling methods.

ing, separating, and crushing systems; ultrasonic welding of metals and plastics; and ultrasonic soldering, drawing, and machining. Ultrasonic vibration has played a major role in nondestructive testing for many years. The advent of high-speed rail transportation poses new problems of vibration and shock associated with "ride," which relate not only to passenger comfort but also to vehicular stability.

Research on the structural effects of earthquakes is an example of the relationship of shock and vibration to problems of social concern. A formal report prepared in 1969 for the National Science Foundation by the *National Academy of Engineering and the National Research Council* specifically delineates the practical problems posed by earthquakes and discusses the research needed to solve these problems.[3] The following excerpt is from Chapter 6 of that report:

> Our knowledge of the damping factor for higher modes of excitation of a structure is especially inadequate. Better methods of measurement and much more extensive measurements for actual structures are needed.
>
> The importance of damping is indicated by the fact that the dynamic response in an earthquake may be affected to a greater degree by the damping than by almost any other structural parameter. This is especially true in those instances where long-sustained, nearly harmonic motions are involved.

7 Acoustics and Social Man

There are two well-focused areas in which sound interacts with man in his social environment. One is a positive force leading composers and musicians to generate a richer variety of musical art forms through modern acoustical techniques, while the other is negative in that the noise that is a side effect of technological developments in the community, in the home, and at work is pervading greater and greater fractions of a typical man's everyday life.

7.1 ART AND SOUND

An obvious aspect of the interaction of acoustics with society on the cultural level is audio recording and reproduction, which was described in the previous chapter. Tape and disk recorders, pocket radios, and electronic-

ally amplified musical instruments are commonplace and taken for granted by society.

The advent of electronic and computer music has opened completely new avenues. It is now possible to produce practically all possible sounds by electronic means. The initial period of bizarre, experimental musical sounds is passing; music critics speak more frequently of beauty and intellectual challenge. Music produced by the Moog synthesizer, for example, is attracting serious interest. Soon a new version of musical form and sound will evolve, and, as decreasing costs widen the availability of new instruments, recreational composing eventually may occupy the leisure time of many individuals. The on-line computer also has a part to play; it will permit traditional composers to perfect their compositions with an entire orchestra at their fingertips.

Composers in all eras have had some specific hall-reverberation characteristics in mind for each of their works. Some modern composers now can see the exciting possibility of the extended use of artificial reverberation to permit reverberation times that change for different parts of a composition and differ at low, medium, and high frequencies.

Perhaps the greatest progress will be made by those trained from youth in both musical arts and physics. Novel ideas from the two disciplines can be combined to produce results inconceivable to the traditional composer. Early stages of this type of education are under way in universities in the Boston, New York, and San Francisco areas.

Whenever society gathers to enjoy cultural activities, the impact of acoustics is obvious. The control of the acoustical environment of concert halls began in 1900 when Wallace C. Sabine gave room acoustics its classical equation for predicting reverberation time. However, it is now clear that reverberation time is only one of the factors contributing to acoustical quality in concert halls. A hall with either a short or long reverberation time may sound either "dead" or "live." Of greater importance, probably, is the detailed "signature" of the hall reverberation that is impressed on the music during the first 200 msec after the direct sound from the orchestra is heard.

There are many subjective attributes to musical–acoustical quality other than liveness (reverberation time). They include richness of bass, loudness, clarity, brilliance, diffusion, orchestral balance, tonal blend, echo, background noise, distortion, and other related binaural–spatial effects. Computer simulations may lead to the separation of a number of the variables involved, but analog experiments conducted in model and full-scale halls probably will be necesary to improve the understanding of the relative importance of the many factors. The prospect of greater certainty in the design of concert halls makes this an exciting frontier for research.

7.2   NOISE

Perhaps the largest single area of interaction of acoustics and society in the years ahead will be audible noise and its control. One can mark out three major sources of noise: man in his social environment; man in industry; and man in transportation.

The principal interplay between architecture and acoustics has been in the design of concert halls, to which reference has been made above. Of secondary importance has been the use of specific acoustical materials in construction of public buildings, such as schools and hospitals, in order to reduce the level of background noise. Only recently have significant efforts been made to control the acoustic environment of dwellings. City, state, and federal codes are required before major acheivements will be realized. Essentially, the fundamental acoustical knowledge to control the transmission of noise from one apartment to another already exists. New techniques of application will develop as standards for acoustical privacy become an accepted part of the building code specifications.

The impact of noise pollution on society is beginning to be recognized. Recently Congress specifically assigned to the Department of Transportation the responsibility and authority to regulate the noise emitted by aircraft. In May 1969, the Department of Labor issued the first regulation specifying the permissible noise exposure levels for industrial workers based on the knowledge developed by interdisciplinary research involving acoustical scientists. It is now accepted that certain noise exposures carry the risk of producing permanent loss of hearing—a disability suffered by a large number of industrial workers. Although research on noise exposure began in the nineteenth century, only within the past five years has general agreement developed on the acceptability of certain concepts and methods of presenting findings.

An area of potential interaction between acoustics and society is noise pollution as a hazard to health. No definitive research data exist at present that prove that a hazardous relationship between noise and health is possible when the noise exposure is below the levels that are hazardous to hearing. However, the International Health Organization has extended the definition of health beyond the usual concept of medical description to include the capability of enjoying human life. Clearly, noise pollution can interfere with speech, relaxation, sleep, and the like.

The complex relationship between disturbance or annoyance and noise exposure is another example of the interaction of acoustics with society. Recent research has shown that, if the noise exposure is comparable to that which exists within a 10-mile radius of a major metropolitan airport, the correlation between annoyance and noise exposure is very poor. It be-

comes necessary to introduce attitudinal variables with nonlinear coefficients to explain the findings. Acoustical scientists have teamed with psychologists and sociologists to conduct this kind of research. Administrative and legislative decisions must and will be made in the future to control noise pollution; such interdisciplinary teams in which acoustical scientists participate will be important in supplying some of the inputs that will shape these decisions.

Control of noise pollution, except for the special case of noise exposure that is sufficient to produce permanent hearing loss, will not be achieved until society demands that noise be controlled. Once this demand reaches a level of national significance, more acoustical scientists will be required.

The most intense noise sources known to man involves high-speed aerodynamic flows and are associated with the aerospace industry. Examples are the exhaust streams from jet and rocket engines; the rotating blades of propellers, rotors, and fans; aerodynamic boundary layers on flight vehicles; and shock waves (sonic boom). An illustration of the noise-generating capacity of such sources is that the power radiated as noise by the engines of a large commercial airliner would be sufficient to propel the average automobile. As an extreme example, the noise power radiated by the Saturn V moon exploration launch vehicle is comparable with that required to propel a large aircraft carrier. Noise from a space vehicle launch site can be annoying at distances of as much as 20 miles, and sonic booms resulting from a single supersonic aircraft flight are audible over tens of thousands of square miles of the earth's surface. The interactions of such intense noise sources with technology as well as society are numerous and create complex systems problems.

Scientists from many different disciplines have made major contributions toward the understanding and solution of aerospace noise problems. Specialists in mathematics, aerodynamics, physical acoustics, geophysics, architectural acoustics, electroacoustics, and psychological and psysiological acoustics have participated in interdisciplinary research and noise-control activities.

Examples of the interaction of aerodynamic noise with society are the community response problems around airports. Airport noise has a major effect on the quality of life in surrounding communities, and, as a result, substantial adverse reactions have been voiced. Consequently, the federal government has sponsored noise certification rules for new aircraft and is considering extensions of these rules to cover retrofit modifications for current aircraft in the interest of noise reduction. Noise, in fact, is viewed as one of the major roadblocks to the continued orderly development of air transportation systems. It is significant that noise considerations have prevented the establishment of any major airports in the United States

during approximately the past 10 years. Although such factors as land use planning can be effective in airport–community noise control, it is generally recognized that the aircraft is the key factor—hence, the need to develop quieter aircraft. Currently, noise-control considerations are a major factor in the selection of propulsion systems. Noise-reduction technology, in the form of acoustic duct treatments and variable-geometry flow-control mechanisms will be applied in the inlets and exhaust ducts of jet engines.

The sonic boom has stimulated particular concern because of its possible adverse effects on people, animals, structures, and terrain. As a result, military supersonic flight operations over land will be restricted, and commercial supersonic transport operations will be banned completely over many land areas.

Aircraft and space vehicle noise also has an impact on the vehicle itself. For example, the near-field noise from the engines creates high-frequency pulsating loads on the nearby aircraft surfaces; large-amplitude vibrations and associated structural fatigue may occur. Consequently, structures *must be designed to withstand these high-frequency noise loads.*

With regard to high-speed aircraft, the noise impingement resulting from the aerodynamic boundary layers is intense. Special fuselage insulating structures are needed to protect the occupants. The engine exhaust noise during launch of large rocket vehicles and the aerodynamic flow noise during the exit flight phase are sufficiently intense to damage the vehicle structure and cause on-board systems to malfunction unless they are designed properly.

In the future, additional local, national, and international rules probably will be formulated to control aircraft noise. Compliance with such rules will be an important factor in the design of transportation systems. Only when noise considerations have been properly dealt with will a widespread and generally accepted steep takeoff and landing transportation system develop in this country. Some aircraft noise control technology also will be useful in high-speed ground transportation vehicles that employ aircraft-type propulsion systems.

The inclusion of noise reduction as a constraint in design and operation will require knowledgeable people at all organizational levels and will result in generally increased transportation costs.

8   Relationship to National Security Activities

Each of the three branches of the military service has had its interests in the field of acoustics. Both the Army and the Air Force have been especially concerned with problems of speech communication against noise backgrounds, while the essential role played by acoustics in underwater communication has caused the Navy to make large-scale investments in acoustical research, exploratory development, and engineering.

Examples of Navy-supported projects in acoustics are sonar atlases; sound velocity studies; acoustically determined bottom contour charts; studies of sound propagation, reverberation, ambient noise, and sound channels; reflection properties of the sea floor; acoustic signaling; Doppler navigation; acoustic surveillance; active and passive detection and identification of submarines; and the location of mines.

The increasing emphasis on using underwater acoustics to aid in unraveling the characteristics of the convoluted medium (see Chapter 4) comes at a time when government funding has begun to shift from military to civilian research projects. To sense the direction, magnitude, and national security consequences of that shift, a survey was made of 95 university and university-affiliated nonprofit laboratories that are active in underwater acoustics, oceanography, and geoacoustics. These schools were asked the question: "What has been your organization's annual expenditure in underwater acoustics and geoacoustics research for F Y 1971 and later?" The annual budgets were also broken down into sources of funding. The results of the survey appear in Table VI.2 for institutions with annual budgets of $10,000 or more in underwater and geoacoustic research.

The gross figures of Table VI.2 show that total dollar funding has been dropping by about 2 to 3 percent per year over the three fiscal years, 1969, 1970, and 1971. About 90 percent of all funding in underwater acoustics came from the Navy.

An important and ominous trend appears when we examine the data on the so-called nongiants—the schools in which underwater acoustics research is an adjunct of the graduate education program rather than a government-supported branch of the university. These data show annual cuts of Navy support by 25 percent to 50 percent or more. These universities have been among the leaders in the training of physicist–acousticians and have contributed substantially to the fundamental research on which the Navy has relied. For example, Brown and Harvard were the sources of much of the basic work on nonlinear sound propagation, transducer radiation, scattering by targets, cavitation, and the Deltic correlator—

TABLE VI.2   University and University-Affiliated Nonprofit Laboratories Underwater Acoustic and Geoacoustic Budgets ($ Thousands)

Institution	Budgeted Expenditures ($ Thousands)[a]								
	1969			1970			1972		
	USN	NSF	Other	USN	NSF	Other	USN	NSF	Other
Brown U.	35	0	15	27	0	17	12	0	12
California, U. of Scripps, M.P.L.	2,600	100	73	3,100	120	75	2,600	150	119
Columbia-Lamont	1,800	0	840	(1,800)	0	(840)	(1,800)	0	(840)
Catholic U.	250	0	0	250	0	0	(75)	b	(75)
Harvard U.	122	0	0	91	0	0	b	b	b
Hawaii, U. of	930	300	0	865	300	0	675	380	0
Houston, U. of									
Electrical Eng. Dept.	90	20	80	60	20	90	50	0	100
Mechanical Eng. Dept.	5	0	2	3	0	2	(3)	(0)	(2)
Johns Hopkins U.	c	c	c	c	c	c	c	c	c
Massachusetts, U. of	11	0	0	11	0	0	10	0	0
Massachusetts Institute of Technology									
Naval Architecture & Marine Eng. Dept.	66	0	4	49	0	8	49	0	4
Geophysics & Earth Sciences Dept.	c	c	c	c	c	c	c	c	c
Miami, U. of									
Div. of Ocean Eng.	600	0	0	600	0	0	600	0	0
Naval Postgraduate School	105	0	0	103	0	0	98	0	0
New Hampshire, U. of	25	0	0	20	0	0	18	0	0
North Carolina State U.	25	0	10	25	0	0	0	0	0
Northwestern U.									
Civil Eng. Dept.	0	15	70	0	20	60	0	40	60

Institution									
Pennsylvania State U.									
O.R.L.	500	0	0	500	0	0	(500)	(0)	(0)
Physics Dept.	12	0	0	12	0	0	(10)	(0)	(0)
Rhode Island, U. of	20	5	10	10	10	10	10	15	12
South Florida, U. of	0	0	0	0	0	0	0	(50)	(15)
Texas, U. of									
Applied Research Lab.	2,025	0	0	1,935	0	0	1,933	0	0
Mechanical Eng. Dept.	0	10	8	0	0	7	(0)	(0)	(7)
Electrical Eng. Dept.	8	3	6	0	10	27	(0)	(6)	(21)
Texas A&M U.	95	40	46	103	62	50	145	74	56
Vermont, U. of	0	3	20	0	0	1	0	0	(20)
Washington, U. of									
Oceanography Dept.	29	76	10	30	47	0	(0)	155	60
Div. of Marine Research and Applied Physics Lab.	380	77	0	410	53	15	430	117	135
Wisconsin, U. of	70	0	0	55	0	0	22	0	0
Woods Hole Oceanographic Institution	1,300	0	0	800	0	0	(800)	(0)	(0)
TOTAL	$11,103	$649	$1,194	$10,859	$642	$1,202	$9,840	$987	$1,538
TOTAL PER YEAR	$12,946			$12,703			$12,365		

[a] Figures in parentheses are extrapolations.
[b] Laboratory closed.
[c] Information not available.

developments that are currently in common use in the U.S. Navy. Although Harvard's termination of interest in underwater acoustics resulted from the retirement of its very effective leader in this work (Professor F. V. Hunt), no such reason exists at a number of other institutions.

Universities that are not included in the Navy's program must turn to other sources of support if they are to continue to play a part in underwater acoustics research. It would appear that across-the-board funding decisions that result in the loss of inexpensive, dedicated, productive facilities are not based on cost-effective concepts.

Military applications of geoacoustics are numerous, for example, the development of seismic intrusion detectors and their deployment in combat areas. The emphasis on size, weight, and power consumption requires full use of solid-state advances in circuit components and integrated circuitry.

A number of military applications of acoustical holography have been suggested, such as antisubmarine warfare, jungle warfare, and aircraft blind-landing systems. However, the security agencies have shown little interest thus far in these possibilities.

Yet another aspect of defense interest is noise control; for example, the noise from military jet aircraft (with applications to civilian aircraft) and the noise to personnel on helicopter gunships are key problems. The hearing problems of military personnel, especially noise-induced deafness, also are of major concern.

9 Institutions

9.1 UNIVERSITIES

Because of the interdisciplinary character of acoustics, research and educational activity are found not only in physics departments but increasingly in engineering, geophysics, oceanography, and life science, as well as music, departments. The Acoustics Panel welcomes this diversity of interest in acoustics. It is, however, of real concern to us that acoustics be taught somewhere in a university; for example, the teaching of acoustics should not be abandoned by the physics department as the responsibility of engineering and, at the same time, neglected by engineering as the responsibility of physics. Our particular concern for the teaching of acoustical topics to undergraduate physics students is stated in Chapter 11.

The diversity of the subject of acoustics makes it difficult to identify all departments and laboratories active in the field. However, Appendix A, which is based principally on an article published by the Education Committee of the Acoustical Society of America,[4] presents the institutions that have been identified as active in graduate research in the various subject areas of acoustics.

Comparison of this Appendix with information from previous years shows a shift in the locations in which physical acoustics research is taking place. In addition, significant work in acoustics has appeared in mechanical, electrical, and civil engineering departments. Underwater acoustics is beginning to appear in departments of geophysics and oceanography in which sound is being used not only as a tool but for fundamental studies of the nature of the medium.

In regard to biology, most acoustics-related research is performed at universities and medical schools. Some of the most fundamental research was conducted in departments of psychology and physiology in the past, but more recently a gradual shift has occurred to specialized interdisciplinary laboratories affiliated with departments of electrical engineering and otolaryngology.

Because of the extensive ramifications of acoustics in many disciplines, interaction among different groups within a given institution, as well as among different institutions, is essential. The physicist, the engineer, the psychologist, and the biologist who work in different aspects of acoustics at the same institution must be brought together. Regional chapters of the Acoustical Society of America have been of some help in improving communication among these groups, but their success thus far has been rather limited. Possibly the new interdisciplinary programs of the National Science Foundation will have a favorable impact in this respect.

9.2   GOVERNMENT

In addition to university activities, government laboratories are very active in acoustics research and development. The most easily identified facilities are those operated by the U.S. Navy that work in underwater acoustics, acoustic signal processing, speech communication, mechanical vibration and noise control, macrosonics, and acoustic instrumentation, but there are many others.

A list of government agencies carrying on work in acoustics is given in Appendix B. The list is undoubtedly far from complete. Particularly mention should be made here of the recently created Office of Noise Abatement and Control [ONAC of the Environmental Protection Agency

E

(EPA)]. The ONAC is currently completing a survey of government lab-
oratories conducting research in the field of noise and noise abatement
that should provide a more complete enumeration in those areas.

9.3   INDUSTRY AND PRIVATE RESEARCH INSTITUTIONS

The number of industrial laboratories conducting work in acoustics is a
large one, and the size of the organizations involved is quite varied. Any
listing that this Committee might supply would be partial and no doubt
leave some major groups unmentioned. The lists of sustaining members of
the Acoustical Society of America and of the Audio Engineering Society,
the names of companies who exhibit at their meetings or advertise in their
journals supply clues, as do the affiliations of those who contribute papers
at meetings. The devices developed in every field of acoustics, from hearing
aids to sonar, from acoustical wall board to electronic music, from trans-
ducers to jet aircraft noise control, all have their soucres of industrial sup-
port. In physical acoustics, for example, the wide-ranging research in solid-
state acoustics is stimulating many other industrial laboratories to initiate
projects in this field. As new applications develop, the number of such in-
stitutions can be expected to increase. The same can be said for the field
of noise control and for biological and medical applications of acoustics
and acoustical devices.

Somewhat the same comments can be made in regard to private founda-
tions and institutes conducting research in acoustics. While these are few in
number, they have pioneered in a number of fields and contributed sig-
nificantly to the development of special areas of the subject.

10   Manpower Characteristics, Productivity, and Funding of Physicists in Acoustics

In Chapter 1, it was pointed out that the Panel had adopted a broad view
of the subject of acoustics for the general purposes of the report. At the
same time, however, it was decided that the approach toward manpower
and funding problems should be a narrower one. In this chapter, therefore,
we shall concern ourselves with physicists in acoustics (in all its aspects)
and with their funding.

10.1 CHARACTERISTICS OF PHYSICISTS IN ACOUSTICS

Physicists who designated acoustics as their subfield of employment on the 1970 National Register of Scientific and Technical Personnel represented 3 percent of the 33,336 physicists surveyed. These data indicate that acoustics is among the least populous physics subfields and that a gradual decrease in the number of physicists working in acoustics has occurred in recent years. For example, in 1964, acoustics accounted for 6 percent of the total physics registrants as compared with 3.5 percent in 1968 and 3 percent in 1970.

The PhD physics population in acoustics in 1970 represented only 2 percent of the total number of PhD physicists; nearly 4 percent (3.9 percent) of the non-PhD physics population was identified with acoustics. Figure VI.1 presents these data and shows also the proportion of the total physics population identified with three other subfields to which acoustics is closely allied.

As earlier sections of this report have shown, a knowledge of acoustics and the use of acoustical instruments and procedures are required in a
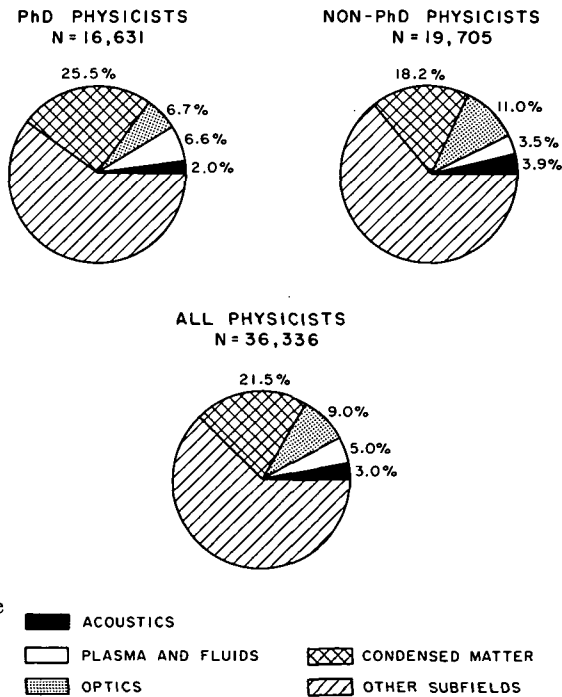
FIGURE VI.1 Percentages of physicists indicating acoustics as the physics subfield in which they were employed compared with the percentages of those working in three other closely related subfields.

number of physics subfields as well as in other scientific and engineering disciplines. Consequently, training and work in acoustics are far more widespread than the data on physics manpower identified with this subfield would suggest.

The median age of doctorates in acoustics in 1970 was 40.1 years, the oldest median age of any of the physics subfields. The median age of the non-PhD's in acoustics was 37.5 years. Though the median age of doctorates still exceeds that of other physics subfields, this age is some two years less (40.1 years as compared with 42.3 years) than that found in the 1968 survey.

Only 70.1 percent of the PhD physicists who indicated acoustics as their subfield had obtained their highest academic degrees in this subject. Nearly one fourth (22.8 percent) earned their highest degrees in engineering. Small percentages reported degrees in chemistry, earth sciences, mathematics, and biology. The relatively low concentration of physics PhD's in acoustics contrasts sharply with such physics subfields as elementary-particle physics and nuclear physics in which virtually all of those working in these fields had earned their highest degrees in physics. Other subfields also having low concentrations of physics PhD's are physics of plasmas and fluids and physics in biology.

Physicists identified with acoustics are found principally in industry; 44.1 percent reported employment in industry in 1970. More than one fourth (28.8 percent) worked in government laboratories, and 17.1 percent in colleges or universities. The employment pattern for PhD and non-PhD physicists in acoustics differed slightly, as Figure VI.2 shows. Academic employment was nearly four times greater for the PhD's than for the non-PhD's, with roughly equivalent percentages of doctorates indicating academic and industrial employment. Nearly half of the non-doctorates worked in industry, and one third of them in government.

The employment pattern for acoustics differs markedly from that for the overall physics population, as depicted in Figure VI.2. Substantially higher percentages of doctorates and nondoctorates in acoustics indicated industrial and government employment than was true of all physicists. Academic employment was reported by half of the physics PhD's and one third of the non-PhD's; only one third of the acoustics PhD's and one tenth of the acoustics non-PhD's worked in academic settings.

The principal work activities of physicists in acoustics were applied research and the management of research and development, as Table VI.3 shows. Design and development work also was a major activity, especially among the nondoctorate group. Basic research and teaching received more emphasis among the PhD's in acoustics than among the non-PhD's, but involvement in these activities was far less in acoustics than in all physics subfields taken together (see Table VI.3). Figure VI.3 compares
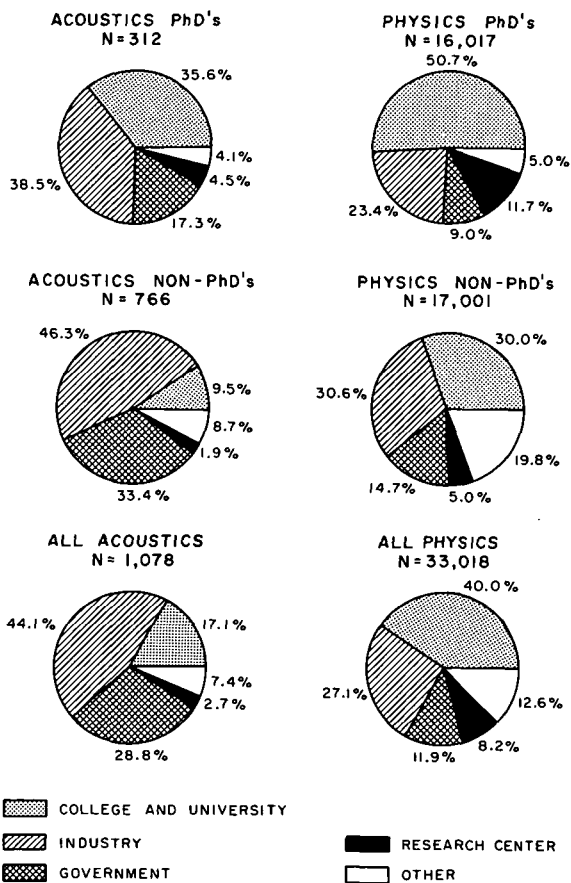
FIGURE VI.2   Comparison of employment patterns for PhD's and non-PhD's in acoustics with those for all physics subfields taken together.

COLLEGE AND UNIVERSITY

INDUSTRY

GOVERNMENT

RESEARCH CENTER

OTHER

the principal work activities in acoustics and physics and shows the substantially heavier applied involvement of the acousticians.

The median annual income for those employed in acoustics was $19,300 in 1970. This median salary was among the highest reported by the various physics subfields, being exceeded only by the median salary in optics ($19,600 per year). The high median salary in acoustics is consistent with the higher median age of this group and with the concentration in industrial rather than academic work settings.

10.2   PRODUCTIVITY AND FUNDING

Doctoral dissertations in acoustics represented only 2 percent of all PhD dissertations in a sample from the physics section of *Dissertation Ab-*

TABLE VI.3   Primary and Secondary Work Activities of Physicists Identified
with Acoustics and Those of the Overall Physics Population

Work Activity	Acoustics PhD's $N = 312$ (%)	Acoustics Non-PhD's $N = 766$ (%)	Total Acoustics $N = 1078$ (%)	All Physics Subfields $N = 33,018$ (%)
Basic research				
Ranked first	11.9	7.6	8.8	27.5
Ranked second	19.2	4.6	8.8	15.8
Applied research				
Ranked first	31.4	35.4	34.2	18.9
Ranked second	29.2	27.8	28.2	18.7
Design or development				
Ranked first	3.8	11.1	9.0	5.9
Ranked second	9.0	19.2	16.2	10.8
Management of R&D				
Ranked first	21.5	20.2	20.6	10.8
Ranked second	7.4	9.4	8.8	6.0
Other management				
Ranked first	3.5	8.1	6.8	5.2
Ranked second	1.6	4.3	3.5	4.8
Teaching				
Ranked first	19.9	3.5	8.3	24.4
Ranked second	11.9	2.6	5.3	12.8
Other				
Ranked first	4.8	12.3	10.1	5.3
Ranked second	12.5	21.9	19.2	13.1
Not indicated				
Primary activity	3.2	1.8	2.2	2.1
Secondary activity	9.3	10.2	9.9	17.9

stracts that included theses appearing in 1969. Small though this percent-
age is, it represents an increase from 1965, in which none of the PhD
theses in a like sample from *Dissertation Abstracts* dealt with the subject
matter of acoustics. An examination of engineering theses in 1969 that
were concerned with physics-related subject matter and could legitimately
have appeared in the physics section of *Dissertation Abstracts* showed
that only about 1 percent were concerned with acoustics.

A sample of articles drawn from journals published by the American
Institute of Physics (during the year April 1969–March 1970) showed
that 23 (3 percent of the 834 articles included in the sample) represented
the subject matter of acoustics. All these articles appeared in but one
AIP journal, *Journal of the Acoustical Society of America*. The principal
source of support indicated for the work reported in these 23 articles was
the Department of Defense, which funded 12 of the 23 studies. Five were
supported by industry, two by universities, and four by other government
agencies.
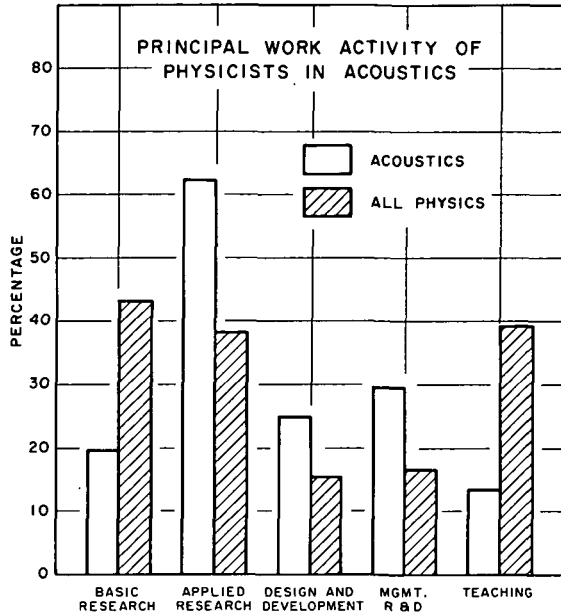
FIGURE VI.3 Comparison of principal work activities for acoustics (percentages include those who ranked each activity either first or second in the allocation of time) for acoustics (*N*=1078) and for all physics subfields taken together (*N*=33,018).

A second sample drawn from 1969 issues of *Physics Abstracts* and containing publications on research conducted in U.S. institutions yielded a total of 1181 articles. Of these, 1.52 percent (18) dealt with the subject matter of acoustics. Eight of these 18 papers were produced by physicists working in academic institutions; four came from industry, and five from government laboratories.

The overlaps of acoustics with other subfields of physics as well as with engineering and scientific disciplines make it difficult to develop a clear-cut picture of productivity. Much work in acoustics is published in journals not usually regarded as within the scope of physics. In addition, acoustics research tends to be applied rather than basic and does not always lead to publishable papers.

Data on the support of work in acoustics also are difficult to obtain. However, patterns of support for basic research in physical acoustics leading to publishable (i.e., unclassified) results for the years 1965–1970 in three federal agencies appear in Figure VI.4. The Department of Defense is the major source of support for basic research in acoustics. In addition, some relatively small-scale support came from the National Aeronautics and Space Administration (NASA) and the National Science Foundation (NSF). Figure VI.4 shows a small but steady increase in Department of Defense funding from 1965 to 1967 and a slight decrease in 1968. In

FEDERAL FUNDING
FOR ACOUSTICS

MILLIONS OF DOLLARS

O   DOD
●   NASA
△   NSF

YEAR
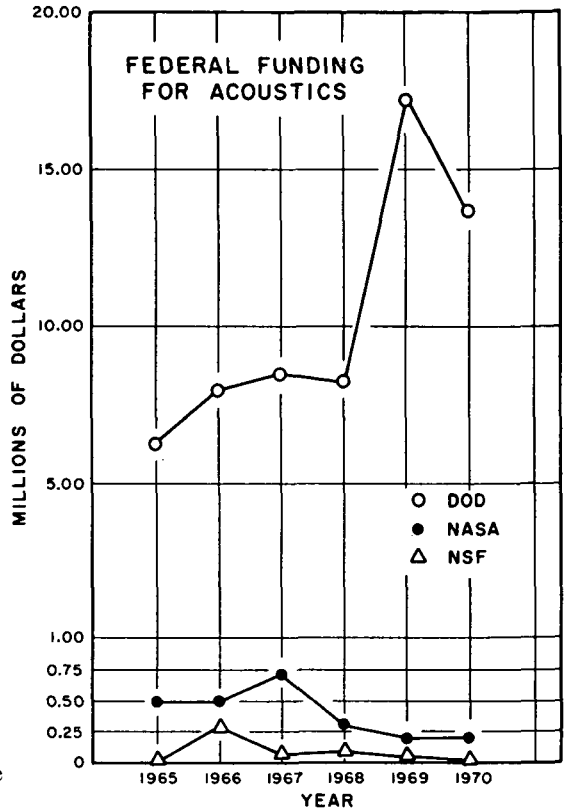
FIGURE VI.4   Funds allocated
in three federal agencies to the
support of basic physics research
in acoustics leading to publishable
papers.

1969, support from this source climbed steeply, only to decline again in
1970. The funding patterns for NASA and the NSF do not reflect the
1969 increase; instead, funds from these sources continued to diminish.

Funding of basic research in physical acoustics by federal agencies was
lower than that characterizing other subfields of physics. However, the
heavy concentration of acoustics personnel in industry suggests that this
subfield derives much of its support from the private sector. Further,
applied work in acoustics is not included in these funding data. Conse-
quently, the data depicted in Figure VI.4 present only a partial picture of
funding sources and trends in acoustics.

10.3   MANPOWER AND FUNDING PROJECTIONS

Any attempt to develop projections of either manpower or funding in
acoustics requires first an estimate of the present distribution of support

among the subdivisions of acoustics. This estimate is made complicated by the wide range of definitions of what constitutes acoustics. There are those who would accredit to acoustics only those portions of the subfield that cannot by any stretch of the imagination be assigned to other subfields of physics such as plasmas and fluids; condensed matter; and atomic, molecular, and electron physics. The structure of the program divisions of the NSF, in which all of acoustics is subsumed under other programs, illustrates this point of view. At the other extreme is the Acoustical Society of America, which encompasses the entire range of mechanical wave and vibration phenomena and their applications.

The breakdown of manpower and funding in acoustics depicted in Table VI.4 follows the program elements for acoustics that were developed by the Physics Survey Committee; therefore, it is closer to the first of the two previously mentioned approaches than to the second. A survey based on page counts of the *Journal of the Acoustical Society of America* would suggest that a much larger percentage should be assigned to ultrasonics and infrasonics. The results of this survey are indicated in Table VI.5, which gives the percentage of papers appearing in the *Journal of the Acoustical Society of America* from 1966 to 1970, assigned to subject subdivisions used by the Society. To apply these percentages in this Panel report, it would be necessary to redistribute them over the program elements and then to determine the fraction of each that is contributed by physicists. Such an evaluation would be highly subjective and this Panel did not attempt it.

Table VI.6 gives the percentage of manpower activity in the various program elements, taken from Table VI.4, together with the percentages of papers in the same categories from the *Journal of the Acoustical Society of America.* Aeroacoustics and macrosonics, radiation and scattering, and acoustic signal processing are assigned in equal portions to the second, third, and fourth program elements. The principal differences between this distribution and that of Table VI.4 are that much work on underwater sound is classified and nonpublishable and that most of the work in hearing, speech, and bioacoustics that appears in the *Journal* is done by nonphysicists—for example, by psychologists and physiologists.

It is probable that the general funding of underwater sound will decrease in the next five years. On the other hand, the need for increased support in noise and biological applications should result in increasing levels of support in both these areas. Financial support of the other program elements should remain nearly constant, with some changes of emphasis occurring within elements. Consequently, the variation in levels of support should be somewhat as shown in Figure VI.5, in which the 1970 values correspond to those of Table VI.4.

It is the recommendation of this Panel to the Physics Survey Committee

TABLE VI.4   Manpower and Funding in Acoustics

	Total Manpower– All Activities 1	Total Industrial– All Activities (44% of 1) 2	Total Univ. etc.– All Activities (56% of 1) 3	Basic Research– Industry (6% of 2) 4	Basic Research– Univ. etc. (27% of 3) 5
Manpower[a]					
Noise and vibration (5774, 5731)	236	125	111	7	30
Underwater sound (5778)	577	250	327	10	88
Music and architecture (5771, 5773)	61	10	51	0	14
Ultrasonics and infrasonics 1/3 (5777 + 5901)	69	40	29	6	8
Electroacoustics and instrumentation (5789)	92	40	52	5	14
Hearing, speech, and bioacoustics 3/5 (5772 + 5776 + 5961)	43	10	33	1	9
TOTALS (Man-Years)	1078	475	603	29	163
Funding					
Noise and vibration	13.0	6.9	6.1	0.4	1.7
Underwater sound	31.7	13.8	18.0	0.6	4.8
Music and architecture	3.4	0.6	2.8	0.0	0.8
Ultrasonics and infrasonics	3.8	2.2	1.6	0.3	0.4
Electroacoustics and instrumentation	5.0	2.2	2.9	0.3	0.8
Hearing, speech, and bioacoustics	2.4	0.6	1.8	0.0	0.5
TOTALS ($Millions)	59.3	36.3	33.2	1.6	9.0

[a] Four digit numbers refer to specialties in the 1970 National Register of Scientific and Technical Personnel.

All Activities—Industry Supported (Arbitrary Percentage of 2) 6	Basic Research—Industry Supported (Same Percentages of 4) 7	All Activities—Federally Supported (1–6) 8	Basic Research—Federally Supported $(4 + 5 - 7)$ 9	All Activities Other Than Basic Research—Industry Supported $(6 - 7)$ 10	All Activities Other Than Basic Research—Federally Supported $(8 - 9)$ 11
$\frac{125}{2}$ = 63	$\frac{7}{2}$ = 4	173	37−4 = 33	59	140
$\frac{250}{5}$ = 50	$\frac{10}{5}$ = 2	527	98−2 = 96	48	479
$\frac{10}{1}$ = 10	0 = 0	51	14−0 = 14	10	41
$\frac{40}{5}$ = 8	$\frac{6}{5}$ = 1	61	14−1 = 13	7	54
$\frac{40}{10/8}$ = 32	$\frac{5}{10/8}$ = 4	60	19−4 = 15	28	32
$\frac{10}{10}$ = 1	$\frac{1}{10}$ = 0	42	10−0 = 10	1	41
164	11	914	181	153	787
3.5	0.2	9.5	1.8	3.2	7.7
2.8	0.1	29.0	5.3	2.6	26.3
0.6	0.0	2.8	0.8	0.6	2.3
0.4	0.0	3.4	0.7	0.4	3.0
1.8	0.2	3.3	0.8	1.5	1.8
0.0	0.0	2.3	0.5	0.0	2.3
9.4	0.5	50.3	9.9	8.3	43.4

TABLE VI.5   Distribution of Papers in the *Journal of the Acoustical Society of America* by Subject Classification of the Society, 1966–1970

Subject	Year					Aver- age (%)	Grouped by Pro- gram Element (%)	
	1966 (%)	1967 (%)	1968 (%)	1969 (%)	1970 (%)			
Noise	4.9	1.3	2.7	1.9	3.4	2.8 ⎱	15.6	(1)
Mechanical vibra- tion and shock	15.3	10.9	12.0	11.2	14.8	12.8 ⎰		
Underwater sound	15.6	11.5	18.0	16.2	17.2	15.7	15.7	(2)
Acoustic instru- mentation	3.3	8.2	3.4	4.2	2.9	4.4	4.4	(3)
Ultrasonics	10.1	14.6	8.0	10.9	4.6	10.6	10.6	(4)
Physiological and psychological acoustics	22.7	22.4	25.4	24.6	23.2	23.7 ⎫		
Speech	8.7	8.9	7.3	7.1	5.5	7.5 ⎬	33.4	(5)
Bioacoustics	0.6	0.8	5.2	2.1	2.4	2.2 ⎭		
Music	1.8	4.7	1.1	0.7	0.9	1.9 ⎱	4.7	(6)
Architecture	1.3	1.9	2.8	2.9	5.1	2.8 ⎰		
Radiation and scattering	9.2	9.7	8.3	8.4	10.4	9.2 ⎫	9.2	Shared among (2), (3), and (4)
Aeroacoustics and macrosonics	3.7	1.6	2.2	5.6	2.4	3.1 ⎬	3.1	
Acoustic signal processing	2.6	3.5	3.8	4.2	2.4	3.3 ⎭	3.3	

that the various agencies supporting acoustics, and especially the National Science Foundation, take particular cognizance of the interdisciplinary character of acoustics and provide funds for high-quality research in the various facets of this subfield rather than allowing it to remain in an intermediate position without specific allocations of support for any of its components. We also recommend to the Physics Survey Committee that

TABLE VI.6   Distribution of Acoustic Activity over the Program Elements

Program Element	From Table VI.4 (%)	From Table VI.5 (%)
Noise and vibration	21.9	15.6
Underwater sound	53.5	21.2
Electroacoustics and instrumentation	8.5	9.9
Ultrasonics and infrasonics	6.4	15.1
Hearing, speech, and bioacoustics	4.0	33.4
Music and architecture	5.7	4.7
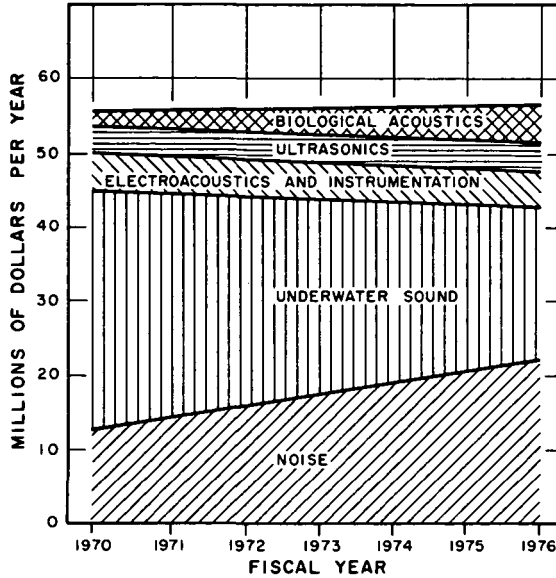
FIGURE VI.5 Support projections in acoustics, 1970–1976.

the National Science Foundation take the responsibility for monitoring the total support of the smaller areas in science, including acoustics, and maintain in such areas a reasonable level of funding.

11   Training in Acoustics

A major theme of this report is that a thorough grounding in acoustics will prepare the physics student for work in a variety of subject areas and types of basic and applied problems. Therefore, this Panel strongly recommends to the Physics Survey Committee that acoustics be taught at the undergraduate level in physics departments. We believe that the broad, basic training an acoustical scientist will receive in other parts of his physics course will better prepare him for interdisciplinary work than will undergraduate training in a specialized engineering department. Further, acoustics courses offer the only opportunity for a physicist to receive training in wave motions in deformable bulk matter. If electromagnetic wave theory is retained as part of the physics curriculum, then it seems reasonable that wave motion in deformable bulk matter also should be part of the same

curriculum. Such an undergraduate course could be included as one semester of a full-year course in mechanics of deformable bulk matter. (A suggested course outline appears in Appendix C.) In this section, we summarize our reasons for recommending the incorporation of this course in physics curricula.

First, the current changes in emphasis in physics toward more applied work can be reflected in such a course. A larger fraction of graduates must expect to seek work in the future that is more of an applied nature. In addition, many students wish to study topics that they regard as more relevant to critical environmental and social problems. The mechanical properties of deformable bulk matter and its dynamical aspects (acoustics) is a subject well suited to these objectives. Evidence of the interdisciplinary nature of the subject is that at the present time it is taught principally in engineering departments.

The topics of such a course are natural to physics, although much of the material is as legitimately a part of mechanics as of physics. However, these topics have gradually disappeared from physics curricula in the past 20 years. The mechanics of continua and acoustics have become basic tools in many science and engineering disciplines, yet they are fundamentally a part of physics, for they involve all material media, require the mathematics of theoretical physics, and provide a major tool for the study of matter.

Like optics and the study of electricity, magnetism, and the electromagnetic spectrum, the mechanics of bulk media is one of the three major means of investigating the properties of matter. But, although electricity and magnetism—the electromagnetic spectrum—and optics of bulk matter receive adequate attention in the undergraduate physics curriculum, the mechanics of bulk media does not.

The elimination of formal acoustics from physics programs has been compensated in part by its incorporation in the curricula of engineering departments. However, this development has some disadvantages. Often, the resulting treatments of the subject neglect its unifying aspects and its relevance to widely differing disciplines and subfields. A broader approach is more likely to characterize training in a physics setting. Such training should emphasize the ease with which concepts can be transferred from one subject to another, for example, the analogies between acoustic and electromagnetic fields and the formal similarity of acoustic waves in geophysics to the much higher-frequency acoustic waves used in the study of the properties of crystals.

In the future, physicists will compete increasingly with engineers for jobs. Past projections of the need for physicists now appear to be overestimates. This situation probably results in part from the rapid growth of

the number of PhD's in engineering. Harvey Brooks of Harvard University reported the following at a recent meeting of the American Physical Society:

There were more than 3000 PhD engineers turned out in 1970, more than twice the number of physicists, and their training is often more immediately relevant to industry. On the other hand, advanced engineering training has a tendency toward overspecialization. In the past, physicists have been able to sell themselves as generalists, with an unusual ability to attack problems holistically, and greater flexibility in changing research interests than other groups. Unfortunately, with engineering taking over much of classical physics, the claim of physics training to breadth is becoming somewhat tarnished.

In recent years, many physicists have gone into engineering and now occupy many positions in electrical, metallurgical, and mechanical engineering groups that required more modern training than was then traditional in engineering departments. Such opportunities also appear likely in geophysics and biophysics. However, if the desired mobility to other disciplines is to occur, this possibility should receive attention in the development of curricula in physics departments.

The course we recommend could assist in meeting the needs of students who will go into teaching. Undergraduate curricula are based heavily on mechanics and electricity and magnetism, rather than on the properties of matter. In addition, large numbers of undergraduates eventually enter other disciplines in which such a course might be useful.

Physics students who specialize in physics that is of a less applied nature also would benefit from such a course. A good acoustics course offers excellent background in wave propagation, which is useful in understanding quantum mechanics.

Finally, the recommended course would provide ready access to advanced engineering courses in the mechanics-based engineering disciplines (such as mechanical, aeronautical, civil, or metallurgical engineering) for students interested in arranging interdisciplinary programs. At present, physics students make such arrangements most often with electrical engineering departments, since physics provides intensive background training for electrical studies. However, engineering students seeking a more general approach might be attracted to such a course.

Obsolescence in training in acoustics is not a major problem at this time, although it probably will become increasingly important in the next decade. Most of the summer school programs and special conferences and symposia in acoustics are oriented toward training workers in other disciplines who find that they need a knowledge of acoustics. These workshops are increasing, especially in noise control, because of present and impend-

ing regulation. Eventually most mechanical and electrical machines, vehicles, and appliances will require noise-control features at the design stage. Thus the need for these symposia will continue until newly graduated students with training in acoustics begin to fill this gap. The comprehensiveness and adequacy of these conferences and symposia are limited by the time allowed—typically about one to three weeks. It is doubtful that industry can afford a longer training period. However, the initiative in offering such sessions was that of acoustical scientists, and, if longer courses are required, they probably will respond to this need.

12   Conclusion

In concluding this report, the Panel is happy to be able to say that acoustics is flourishing today in all its aspects. We have tried to report its needs and its concerns, but we have no real worry about its future. Acoustics has much to contribute to science and technology, to man and his society. It will endure and prosper.

In regard to this Panel report, we are all too well aware of its weaknesses and omissions and can only recall in our defense the answer given by the pianist de Pachmann to an admirer, who, after a concert, had complimented him for not missing a single note. "Madam," said de Pachmann, "with the notes I missed, I could have given another concert."

References

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3. NAE-NRC Committee on Earthquake Inspection, *Earthquake Engineering Research* (NAS-NAE-NRC, Washington, D.C., 1970).
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Appendix A: Institutions with Graduate Work in Acoustics

[Based on information from the *Journal of the Acoustical Society of America*, 48(2), Part 1, August 1970]

Field of Acoustics

Institution and Department(s)	Architectural	Physiological	Psychological	Instrumentation Apparatus	Musical	Noise and Noise Control	Speech Communication	Ultrasonics	Radiation and Scattering	Mechanical Vibration and Shock	Underwater Sound	Macrosonics; Aeroacoustics	Acoustic Signal Processing	Bioacoustics
*Air Force Inst. of Technol.; Wright-Patterson AFB														
Mech. Eng.										x				
Alaska, U. of														
Marine Science											x			
*American U.														
Physics									x					
*Boston Col.														
Physics	x													
*Bowling Green State U.														
Speech			x					x				x		
*Brigham Young U.														
Physics					x									
British Columbia, U. of														
Otolaryngology		x					x							
*Brown U.														
Physics		x	x				x							
Appl. Math.								x		x	x	x		
Biomed. Sci.											x			x

657

Appendix A (Continued)

Institution and Department(s)	Architectural	Physiological	Psychological	Instrumentation Apparatus	Musical	Noise and Noise Control	Speech Communication	Ultrasonics	Radiation and Scattering	Mechanical Vibration and Shock	Underwater Sound	Macrosonics; Aeroacoustics	Acoustic Signal Processing	Bioacoustics
*Bryn Mawr Coll.														
Physics														
*California, U. of, Berkeley														
Appl. Mech.	x			x		x			x	x	x	x		
Psychology		x	x											
Linguistics							x							
Elec. Eng.								x						
*California, U. of, Los Angeles														
Psychology		x	x		x		x							
Physics								x	x					
Mech. and Structures								x	x	x	x	x		x
*California, U. of, San Diego														
Appl. Physics				x				x	x		x		x	
California State Col., Long Beach														
Speech		x	x	x		x	x							
Mech. Eng.										x				
Elec. Eng.											x			
Biology											x			x
California State Col., Los Angeles														
Psychology			x											

658

*Case Western Reserve U.
  Physics
  Speech Commun.
  Chemistry
  Med. School—Surgery
*Catholic U. of Am.
  Mech. Eng.
  Elec. Eng.
  Physics
  Space Sci.
*Colorado State U.
  Physics
*Columbia U.
  Elec. Eng.
  Col. of Physicians & Surgeons
  Psychology
  Music
  Mech. Eng.
  Civil Eng. & Appl. Mech.
Columbia U., Palisades, N.Y.
  Lamont-Doherty Geol. Obs.
Connecticut, U. of
  Marine Science Inst.
*Denver, U. of
  Physics
  Mech. Sci. & Environ. Eng.
*Duke U.
  Mech. Eng.
*Florida, U. of
  Architecture
  Speech/Psychology
  Mech. Eng.
Florida Atlantic U.
  Ocean Eng.

659

Appendix A (*Continued*)

Institution and Department(s)	Architectural	Physiological	Psychological	Instrumentation Apparatus	Musical	Noise and Noise Control	Speech Communication	Ultrasonics	Radiation and Scattering	Mechanical Vibration and Shock	Underwater Sound	Macrosonics; Aeroacoustics	Acoustic Signal Processing	Bioacoustics
George Washington U.														
Eng. Mech.		X	X	X		X		X		X		X		
Georgtown U.														
Physics								X	X	X				
*Georgia Inst. of Technol.														
Eng. Sci. & Mech.										X				
*Harvard U.														
Psychology			X											
Physics								X						
Eng. & Appl. Physics									X	X	X	X		
*Hawaii, U. of														
Mech. Eng.						X								
Ocean Eng.											X			
Elec. Eng.													X	X
*Houston, U. of														
Speech Path. & Audiol.							X							
Mech. Eng.						X		X	X	X	X		X	
Elec. Eng.									X					
*Illinois, U. of														
Biophysics		X												
Psychology			X											
Elec. Eng.				X	X	X	X	X	X	X			X	X

660

Speech
Civil Eng.
*Iowa, U. of
  Speech Path. & Audiol.
  Physics
John Carroll U.
  Physics
*Johns Hopkins U.
  Elec. Eng.
  Laryngol. & Otol.
  Mech.
*Kansas U. Medical Cent.
  ENT
  Surgery
  ENT–Audiol.
  Speech & Hearing
Long Island U., Mitchell Campus
  Marine Science
Louisiana Polytechnic Inst.
  Mech. Eng.
*Massachusetts, U. of
  Elec. Eng.
  Mech. Eng.
*Massachusetts Inst. of Technol.
  Architecture
  Elec. Eng.
  Physics
  Mech. Eng.
  Aeronaut. & Astronaut.
  Naval Architecture
Miami, U. of
  Ocean Eng.
*Michigan, U. of, Ann Arbor
  Program in Physiol. Acoustics

Appendix A (Continued)

Institution and Department(s)	Architectural	Physiological	Psychological	Instrumentation Apparatus	Musical	Noise and Noise Control	Speech Communication	Ultrasonics	Radiation and Scattering	Mechanical Vibration and Shock	Underwater Sound	Macrosonics; Aeroacoustics	Acoustic Signal Processing	Bioacoustics
*Minnesota, U. of														
Otolaryngology		x	x											
Elec. Eng.				x					x				x	
Speech & Hearing Sci.							x							
Aeron. Eng.										x				
*Mississippi, U. of														
Physics								x						
Missouri at Rolla, U. of														
Mech. & Aerospace Eng.	x			x		x								
Murray State U.														
Physics								x		x				
*Naval Postgraduate School, Monterey, Calif.														
Elec. Eng.													x	
Physics				x				x	x		x			
Mech. Eng.										x		x		
*Nevada, U. of														
Elec. Eng.						x	x	x					x	
*New Hampshire, U. of														
Elec. Eng.				x							x		x	
*New Mexico State U.														
Psychology							x							x

Field of Acoustics

662

Speech
Mech. Eng.
New York, The City U. of
  Physics
*New York at Buffalo, State U. of
  Speech
  Elec. Eng.
  Mech. Eng.
  Interdiscip. Studies
*North Carolina State U.
Surgery, U. of N.C., Chapel Hill
  Indus. Eng.
  Mech. & Aerospace Eng.
North Dakota, U. of
  Mech. Eng.
*Northeastern U.
  Elec. Eng.
  Psychology
*Northwestern U.
  Audiology
  Elec. Eng.
  Civil Eng.
  Mech. Eng.
*Notre Dame, U. of
  Aerospace & Mech. Eng.
Nova Scotia Tech. Col.
  Appl. Math.
  Mech. Eng.
  Elec. Eng.
*Ohio U.
  Hearing & Speech Sci.
  Physics
*Ohio State U.
  Mech. Eng.
  Eng. Mech.

663

Appendix A (Continued)

Institution and Department(s)	Architectural	Physiological	Psychological	Instrumentation Apparatus	Musical	Noise and Noise Control	Speech Communication	Ultrasonics	Radiation and Scattering	Mechanical Vibration and Shock	Underwater Sound	Macrosonics; Aeroacoustics	Acoustic Signal Processing	Bioacoustics
*Oklahoma Med. Cen., U. of Commun. Disorders			x											
*Oklahoma State U.														
Architecture	x													
Mech. Eng.						x			x					
Physics								x		x				
*Oregon Med. School, U. of														
Anatomy		x												
*Pennsylvania State U.														
Archit. Eng.	x													
Speech Pathol.		x												
Psychology			x											
Eng. Acoustics				x										
Mech. Eng.													x	
Speech							x							
Physics								x	x					
Eng. Mech.						x								
Ordnance Res. Lab.										x	x			
Aerospace Eng.												x		
*Pittsburgh Med. School, U. of														
Biophys.														x
Audiology		x	x											x
*Princeton U.														
Psychology		x	x											x

664

*Purdue U.
  Psychology
  Mech. Eng.
  Elec. Eng.
*Queens College, CUNY
  Commun. Arts & Sci.
  Physics
*Rhode Island, U. of
  Elec. Eng.
  Ocean Eng.
  Physics
  Mech. Eng.
  Grad. School of Oceanography
*Rochester, U. of
  Elec. Eng.
  Physics
San Jose State Col.
  Mech. Eng.
South Florida, U. of
  Marine Science Inst.
*Southern California, U. of
  Physics
*Southern Mississippi, U. of
  Speech & Hearing Sci.
*Stanford U.
  Speech & Hearing Sci.
  Appl. Physics
*Stevens Inst. of Tech.
  Ocean Eng.
  Mech. Eng.
  Physics
*Syracuse U.
  Lab. of Sensory Commun.
*Tennessee, U. of
  Audiol. & Speech Pathol.

665

Appendix A *(Continued)*

Field of Acoustics

Institution and Department(s)	Architectural	Physiological	Psychological	Instrumentation Apparatus	Musical	Noise and Noise Control	Speech Communication	Ultrasonics	Radiation and Scattering	Mechanical Vibration and Shock	Underwater Sound	Macrosonics; Aeroacoustics	Acoustic Signal Processing	Bioacoustics
Tennessee, U. of														
Physics								X						
Eng. Mech.										X				
Tennessee Tech. U.														
Mech. Eng.										X	X	X		
*Texas A&M U.														
Oceanog.											X			
*Texas at Austin, U. of														
Speech		X					X							
Architecture	X													
Psychology			X											
Elec. Eng.				X		X		X		X	X		X	
Mech. Eng.				X		X		X	X	X	X		X	X
Physics									X				X	
*Toronto, U. of														
Inst. for Aerospace Studies						X			X			X	X	
Psychology			X											
*Vanderbilt U.														
Physics & Astronomy								X						
*Vermont, U. of														
Physiology		X												
Psychology			X	X										
Elec. Eng.												X		
Physics								X			X	X		X

Victoria, U. of, British Columbia
  Physics
*Virginia, Med. Col. of
  Biophysics
*Washington U., Central Inst. for the
  Deaf, St. Louis
  Speech & Hearing
  Psychology
  Elec. Eng.
*Washington, U. of
  Audio Visual Services
  Otolaryngology
  Psychology
  Speech
  Phys. Med. & Rehabil.
  Elec. Eng.
  Div. Marine Resources
  Physiol. & Biophysics
  Mech. Eng. & Multidisc. Accoust.
    Program
*Waterloo, U. of, Ontario
  Elec. Eng.
*Wayne State U.
  Psychology
  Physics
West Virginia U.
  Speech Pathol. & Audiol.
*Wisconsin, U. of
  Elec. Eng.
  Linguistics
  Geol. & Geophysics
  Eng. Mech.
*Woods Hole Oceanographic Inst.
  Geol. & Geophysics

*Indicates that PhD is awarded.

667

Appendix B: Partial List of Governmental Laboratories Active in Acoustics Research

Aerospace Medical Research Laboratory
    Wright-Patterson AFB, Ohio
Acoustical Technology Section
    National Gypsum Research Center
    Buffalo, N.Y.
Air Force Cambridge Research Laboratory
    Bedford, Mass.
Air Force Weapons Laboratory
    Kirtland AFB, Albuquerque, N.M.
Argonne National Laboratory
    Argonne, Ill.
Army Aberdeen Research & Development Center
    Aberdeen, Md.
Army Ballastic Research Laboratories
    Aberdeen, Md.
Army Electronics Command
    Fort Monmouth, N.J.
Army Materials & Mechanics Research Center
    Watertown, Mass.
Army Medical Research Laboratory
    Fort Knox, Ky.
Department of the Army
    Harry Diamond Laboratories
    Washington, D.C.
Environmental Research Laboratories
    National Oceanic and Atmospheric Administration
    Boulder, Colo.
National Oceanic and Atmospheric Administration
    Geoacoustics Section
    Rockville, Md.
Fleet Numerical Weather Central
    Monterey, Calif.
Human Engineering Laboratories
    Aberdeen Proving Ground, Md.
Institute for Basic Standards
    National Bureau of Standards
    Washington, D.C.
National Bureau of Standards
    Sound Section and the Vibration Measurements Section, Mechanics Division
    Washington, D.C.
Naval Aerospace Medical Research Laboratories
    Pensacola, Fla.
Naval Air Development Center
    Warminster, Pa.

Naval Civil Engineering Laboratory
   Port Hueneme, Calif.
Navy Electronics Laboratory Center
   San Diego, Calif.
Naval Oceanographic Office
   Washington, D.C.
Naval Ordnance Laboratory
   White Oaks, Silver Spring, Md.
Naval Research Laboratory
   Washington, D.C.
Naval Research Laboratory
   Underwater Sound Reference Division
   Orlando, Fla.
Naval Ship Research & Development Center
   Carderock, Md.
Naval Ship Research & Development Laboratory
   Annapolis, Md.
Naval Ship Research & Development Laboratory
   Panama City, Fla.
Naval Ships Systems Command
   Washington, D.C.
Naval Undersea Research & Development Center
   San Diego, Calif.
Naval Undersea Research & Development Center
   Pasadena, Calif.
Naval Undersea Warfare Center
   San Diego, Calif.
Naval Underwater Systems Center
   New London, Conn.
Naval Underwater Systems Center
   Newport, R.I.
Naval Weapons Center
   Earth and Planetary Sciences Dept.
   China Lake, Calif.
Naval Weapons Laboratory
   Dahlgren, Va.
Submarine Medical Research Laboratory
   Naval Submarine Base
   New London, Conn.
Veterans Administration Hospital
   Cleveland, Ohio
Veterans Administration Hospital
   Miami, Fla.
Veterans Administration Hospital
   Columbia, S.C.
Veterans Administration Hospital
   Atlanta, Ga.
Watervliet Arsenal
   Watervliet, N.Y.

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Appendix C: A Possible Course Outline for a Physics Course on Mechanics of Deformable Bulk Matter*

1. Stress and strain in continuous media
2. Compatability equations for elastic media
3. Stress–strain relationships in elastic, anelastic, piezoelectric, and viscoelastic media
4. Equations of motion of fluids; real and ideal fluids
5. The energy principle in continuum mechanics
6. The Stokes-Navier equation; Reynolds number
7. Phenomenological discussion of laminar and turbulent flow; the boundary layer and its effect
8. Characteristic dimensions and constants for fluid flow
9. Waves in fluids
10. Attenuation and dispersion mechanisms
11. Waves in solids
12. Elastic constants of crystals
13. Applications to the study of the properties of fluids and crystals
14. Applications in geophysics and biophysics

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* Such a course could be incorporated most easily into a second semester of a full-year course in mechanics. The first semester would presumably cover principles of mechanics applied to particles and systems of particles.

VII
Plasma Physics and the Physics of Fluids

PANEL MEMBERS

STIRLING A. COLGATE, New Mexico Institute of Mining & Technology, *Chairman*
STANLEY CORRSIN, The Johns Hopkins University
T. K. FOWLER, Lawrence Berkeley Laboratory
HAROLD P. FURTH, Princeton University
UNO INGARD, Massachusetts Institute of Technology
RICHARD J. ROSA, AVCO-Everett Research Laboratory
ASCHER H. SHAPIRO, Massachusetts Institute of Technology

CONSULTANTS

ROBERT H. KRAICHNAN
CECIL LEITH, National Center for Atmospheric Research
DOUGLAS K. LILLY, National Center for Atmospheric Research

LIAISON REPRESENTATIVES*

HARRY HARRISON, National Aeronautics and Space Administration
ROBERT L. HIRSCH, Atomic Energy Commission
ERWIN R. SCHMERLING, National Aeronautics and Space Administration
ROLF M. SINCLAIR, National Science Foundation

READERS

S. J. BUCHSBAUM, Bell Telephone Laboratories, Inc.
GEORGE F. CARRIER, Harvard University
JOHN M. DAWSON, Princeton University
MELVIN B. GOTTLIEB, Princeton University
HAROLD GRAD, New York University
ROBERT A. GROSS, Columbia University
GARRETT GUEST, Oak Ridge National Laboratory
FRANCIS H. HARLOW, JR., Los Alamos Scientific Laboratory
WALLACE D. HAYES, Princeton University
ROBERT L. HIRSCH, Atomic Energy Commission
RUSSELL G. MEYERAND, JR., United Aircraft Corporation
RICHARD MORSE, Los Alamos Scientific Laboratory
HARRY E. PETSCHEK, AVCO-Everett Research Laboratory
RICHARD F. POST, Lawrence Berkeley Laboratory
E. L. RESLER, JR., Cornell University
NORMAN ROSTOKER, Cornell University
WILLIAM R. SEARS, Cornell University
R. A. SHANNY, Naval Research Laboratory
A. W. TRIVELPIECE, University of Maryland
JAMES L. TUCK, Los Alamos Scientific Laboratory
PETER P. WEGENER, Yale University

*In general, no agency was represented on the Panel by more than one person at a given time.

Contents

VII

PLASMA PHYSICS AND THE PHYSICS OF FLUIDS

1 Introduction

The physics of fluids is the study of liquids and gases. Plasma physics is the study of ionized gases. Human beings spend their lives in intimate contact with air and water, both inside and outside their bodies. The science that studies the forces and motions of liquids and gases is called fluid dynamics (the physics of fluids or fluid mechanics). When a liquid is heated it becomes a gas; when a gas is further heated the negative electrons are thermally stripped from the positive ions giving rise to an "ionized" gas or plasma. The study of the behavior of such ionized matter is called plasma physics. Almost all matter in the universe is in the ionized state except for the relatively small—but important—fraction in planets and gas clouds. We spend our lives immersed in fluids but are surrounded on the cosmic scale by plasma.

2 Scientific Status

2.1 PLASMAS

The theory of plasmas rests on a set of simply formulated and well-known equations: the Boltzmann equation, the Lorentz force, and Maxwell's

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equations. Real plasmas in the laboratory and in nature, however, are so remarkably rich in phenomena that some of the most basic physics of the plasma state is only now in process of being understood. For example, this is particularly true for the high-temperature, virtually collisionless plasmas that have been of great interest in connection with controlled fusion power and in space-physics applications.

During the last five years, the sophistication of plasma theory has been growing. Even certain classical areas that had apparently been closed for over a decade—notably the transport properties of plasmas with long Coulomb mean free paths in inhomogeneous magnetic fields—have turned out to yield complex and surprising new effects. Experimentally attainable laboratory regimes have meanwhile expanded, especially in the directions of higher temperature and better confinement. Plasma diagnostics, both in the laboratory and in space research, have continued to improve. As a result, the trend toward detailed agreement between plasma theory and experiment, which was in the process of becoming discernible five years ago, has now produced some notable successes.

The next five years are likely to see the detailed resolution of many of the most celebrated mysteries of the past two decades of plasma research: Bohm diffusion, enhanced resistivity, anomalous velocity, space diffusion, and collisionless shocks.

A valuable link between the richness of experimental plasma phenomena and the finite resources of theoretical analysis has been provided by the evolution of two-dimensional and even three-dimensional computer plasma experiments. This approach (which has been paced by, and has in turn stimulated, the recent progress of computer science) is naturally suited to the structure of the plasma problem. The computer plasma can use the known basic equations with fewer approximations than can analysis and lends itself to much finer diagnostics than a laboratory plasma. Even the modern computer, however, is still far from being an adequate match for the full complexity of plasma phenomena.

2.1.1 *Collisional Plasmas*

The plasma subject naturally breaks down into collisional and collisionless regimes, both in regard to physics and major applications. At low temperatures and high densities, two-body collisions are so frequent that a plasma has well-defined fluid properties, and local thermodynamic equilibrium applies. Under these conditions, the so-called magnetohydrodynamic (MHD) equations are correct. Many important technological applications, such as MHD power converters, plasma lasers, arcs, and various other light sources fall into this range.

2.1.2 *Collisional Magnetohydrodynamics*

Classical MHD theory is based on Maxwell's equations, the equations of hydrodynamics, and the coupling of these two through Ohm's law, the Lorentz force equation, and Faraday's laws of induction. This being a formidable array of equations to solve with any degree of generality, it has been necessary to make one or another simplifying assumption in order to obtain either physical insight or mathematical solution. The problem has been to determine and stay within the range of physical validity of any given simplifying assumption.

The field of MHD and plasma physics has recently been dignified by the award of a Nobel Prize to Hannes Alfvén, a pioneer in this area. In 1965, he pointed out a basic dichotomy between classical MHD theory and the world of real phenomena; namely, that while theory dealt with more or less uniformly conducting continua, observation revealed nothing but filaments, for example, the arc, the lightning bolt, and the solar prominence. He urged, therefore, that the theoretician should consider the possibility that volume currents are inevitably unstable, while the experimentalist should look diligently for examples of a uniform situation. In the last decade, whether by accident or design, both have happened. For example, experimental MHD generators have provided a large volume uniform plasma and demonstrated the validity of the continuum equations, but only under rather carefully prescribed conditions. Meanwhile, a body of theoretical work has grown up to explain what happens when those conditions are not satisfied. The essential ingredients of the new theory have been, first, the recognition that even small departures from uniformity can have a large effect and, second, the introduction of yet another set of equations—those relating gas chemistry and ionization to the magnitude of the current. (In part, this has been a resurrection of classical work on discharges dating back to the turn of the century.) It has been shown that if the Hall effect is not unduly large and if current density (or electron energy) is too low to influence ionization appreciably, then uniform continuum theory and experiment agree. Present theory seems also to predict rather well the point at which uniformity will—literally—break down, but the theory for what happens thereafter is still open to question. There is the interesting possibility that, in a strong magnetic field, breakdown will not necessarily occur along filaments but may instead give rise to something akin to isotropic turbulence.

Another outstanding problem in MHD is the generation of the magnetic fields of the earth or the stars or of both. In the case of the earth, it is abundantly clear that an actual generator of magnetic field must be present. A star, on the other hand, could possibly trap some of the magnetic

flux of the galaxy during formation, and possibly the decay time would be adequately long.

Current theories of field generation require the occurrence of rotating convective eddies or cyclonic storms in a rotating conducting fluid. The combined rotation and translation of flux lines is the present best guess for the generation of the earth's field within the radioactively heated convecting earth's core. There is as yet no proof, and the understanding of the coherence required in the cyclonic storms is at best embryonic.

2.1.3 Collisionless Plasmas

At high plasma temperatures and low densities, Coulomb collisions become infrequent. In this case, an adequate description of plasma behavior cannot be found in the macroscopic equations. Local thermodynamic equilibrium does not prevail, and one is forced to consider the particle distribution function in six-dimensional phase space, i.e., by means of the Boltzmann equation. A whole new theoretical structure has grown up in this complex regime, which is the typical regime of controlled fusion and space plasma physics.

Collisionless laboratory plasmas of 5–10 years ago characteristically involved Coulomb mean free paths very long compared with the gyroradius but not yet very long compared with the physical scale of the magnetic confinement geometry. In this transitional stage between collisional and truly collisionless plasmas, a variety of non-MHD instabilities, notably the collisional drift waves, were discovered. Thanks to experiments on both toruses and small linear research devices (e.g., Q-machines, using contact-ionized alkali metal plasmas), these instabilities are now largely, though not entirely, understood. The closely related problem of understanding anomalous plasma transport on the so-called Bohm time scale is also beginning to yield. As predicted by the theory, the recent experimental advances toward longer mean-free-path plasmas in toruses have produced much better and more controllable plasma confinement.

In toroidal plasma devices attention is now focusing on residual enhancement of the classical Coulomb transport coefficient in the limit of extremely long mean free path. A neoclassical theory has been developed, which takes into account the realistic drift-orbits of particles in inhomogeneous magnetic fields and predicts transport coefficients considerably larger and more complex than those of simple classical theory. These predictions have already gained at least partial experimental verification. A new class of slow-growing instabilities related to the realistic orbits (trapped-particle modes) has also been predicted and partly verified.

In open-ended (or mirror) confinement geometry, where the problem of

grossly anomalous plasma losses was solved some years ago by the Joffe "minimum-B" principle, the suppression of velocity–space instabilities has been the next most urgent task. In the mirror configuration, the plasma is confined between two regions of strong magnetic field that reflect the motion of plasma particles. (Natural examples are the Van Allen radiation belts trapped between the strong magnetic poles of the earth.) Mirror-confined plasmas are automatically highly collisionless, since the whole plasma only lives for about one collision time: particles scattered into the "loss cone" (high velocity along magnetic field) are not reflected by the mirrors and escape. Mirror-confined plasmas also inherently depart from an iso- tropic Maxwellian distribution in that particles with velocities too nearly parallel to the magnetic lines are missing. Such nonthermal distributions can amplify waves, mainly in the frequency range near the gyration frequency of plasma ions in the magnetic field, and these strong waves can eject plasma through the magnetic mirrors. Encouraging criteria for minimizing this difficulty have now been derived theoretically and partially tested. The most successful experiments have actually been those operating at the highest plasma density—as well as at multi-keV temperature—and the plasma confinement time was found to approach the classical lifetime calculated for mirror leakage by collisions among the ions. New experiments at even higher ion energies, where collision times are still larger, should resolve the question of whether any residual weak instability (i.e., wave amplification) is also contributing to plasma leakage.

A cornerstone of the theory of velocity–space instabilities is the mathematical concept of Landau damping and inverse Landau damping by resonant particles. During recent years, the physical validity of this concept has been verified in detail by controlled experiments on long-mean-free-path plasmas in linear low-density research devices. The related phenomenon of "plasma echoes" has been predicted and observed and promises to furnish a particularly sensitive tool for the study of weakly dissipative processes in confined plasmas.

Strong amplification of plasma waves may be caused by directed beams of charged particles penetrating a plasma; or, if desired, this amplification can be suppressed to permit stable propagation of the beam. Much interest in this subject has been generated by the development of accelerators producing intense electron beams of thousands of amperes at relativistic energies (millions of volts). While such charged beams would blow apart if unneutralized, beams propagating in plasmas are neutralized by the plasma. Beam pulses can also be trapped and stored in a plasma medium to give large circulating currents that alter the magnetic-field configuration. Other potential applications of such beams include their use as intense x-ray sources, as a means of heating fusion plasmas, and as the basis for

some new types of high energy in accelerators. Different applications require the ability to control plasma wave amplification at will, suppressing it to enhance beam transport, or using it to dump beam into the plasma at the appropriate moment. As with lasers in the last decade, there will be many practical uses of these beams as concentrated energy sources in the years ahead.

2.1.4 High-Pressure Plasmas

The diverse regimes of plasma physics are characterized by many dimensionless parameters aside from those relating to the collision frequency. For example, if the Debye length is large compared with the dimension of a system, then collective behavior disappears; if the gyroradius is large for a particle species, then magnetic confinement effects become unimportant for that species. The parameter β, which measures the ratio of plasma pressure to magnetic pressure, also determines the ability of the plasma to deform the confining magnetic field. For controlled fusion purposes, β should be typically in the range 10^{-2} to unity, with the economics favoring the higher β-values.

Most of the illustrations discussed in the preceding sections relate to plasmas with β-values well below unity. Extremely interesting results on plasma heating and confinement have been obtained, however, for β close to 1, in experiments using various pinches and shock tubes. In particular, it has been found that advance to higher β-value does not obviously impair plasma confinement (and may even improve it) as long as the gross MHD stability conditions are observed. An initial intuitive fear by many that high β would lead to instability was validly denied by others. Even at the present technological stage, high-β plasmas have become copious neutron sources, reaching yields of 10^{12}–10^{13} per discharge, in the case of the so-called "plasma focus," a small high-axial-current pinch.

During recent years, the rapid progress of laser technology has provided the means of reaching in the laboratory a new range òf extremely dense high-temperature plasmas, by laser heating of small levitated solid pellets or liquid droplets. Such plasmas have been used to study transient magnetic interactions with $\beta \gg 1$—a range that is also of considerable interest for studying solar-wind phenomena.

2.1.5 Computation in Plasma Physics

The computer has become an essential tool in the theoretical study of plasmas. In the past few years, the numerical simulation of plasmas has

become a well-established field of study with several active groups engaged in this research.

The models used to study the dynamics of plasmas can be divided into two categories—collisional and collisionless. Among the collisional models are the magnetohydrodynamic or fluid descriptions. Another important collisional model is the Fokker-Planck kinetic equation. The principal model used for a collisionless plasma is the Vlasov kinetic equation.

The Vlasov equation is defined on a six-dimensional phase space. The direct solution by finite-difference methods on a fixed (Eulerian) grid has been accomplished in both two- and four-dimensional phase space. The usual method of simulating a collisionless plasma is by the particle method. The motion of a large number of charged particles is computed, and, from their positions and velocities, charge and current densities on a fixed grid are determined at each time step. The electric and magnetic fields are then computed self-consistently by solving the differential equations by difference methods. This method is easily generalized to two and three space dimensions, but then the limitation of computer size and speed becomes important. The earliest work on this approach started almost 20 years ago, but only limited results were obtained until about 1966 when a new generation of suitably fast computers became available. The applications are primarily to the study of plasma instabilities and turbulence.

Computational work on the collisional models is more developed. One-dimensional MHD codes have been in use for at least 10 years in the study of high-density plasma experiments. Recently, two-dimensional codes have been developed and applied to the plasma focus experiments and also to the expansion of a laser-produced plasma in a magnetic field. The solution of the Fokker-Planck equation has been pursued principally in connection with the magnetic mirror confinement program. These codes solve the equations by finite-difference methods on a fixed (Eulerian) grid.

2.2   FLUIDS

The generalized description of a fluid and a plasma may in many circumstances be identical, as, for example, the hot gases of a star. The more restrictive distinctions of "plasma" are usually made when the electrical and magnetic forces associated with its ionized state become important. In the example of ionized stellar gases, the "fluid" description is applicable to the behavior of the medium when the forces derived from pressure, viscosity, gravity, and rotation are considered. When the further constraints of magnetic and electrical fields are added, "plasma" becomes the appro-

priate description. Ideally, a fluid may be indefinitely distorted or sub-divided with negligible change in state, while a plasma, on the other hand, is subject to indefinitely large electrical restoring forces depending on the details of distortion or subdivision. Because of the electrical neutrality of cold condensed matter, we are surrounded by fluids on the earth.

Another part of the physics of fluids is the study of the microscopic bases of the macroscopic properties of gases and liquids. This depends on the interactions and collective behavior of molecules and atoms, often under the rubric of "kinetic theory of . . ." or, more generally, under "statistical mechanics." Those parts of "material science" are, with a few exceptions, *not* included in the following lists and comments.

2.2.1 Scientific Research Areas of Present and Future Importance in Fluids

The areas of research in fluids are indeed diverse. Before discussing a few of these topics in detail, we list the major areas of current and future concern.

1. Separated (and separating) flows
2. Unsteady flows, especially with separation or turbulence or both
3. Turbulent flow—basic dynamics
4. Turbulent flow—effects on mixing, diffusion, chemical reactions, scattering
5. Flow through porous materials
6. Two- (or more) phase flows, including effects of phase changes (freezing, melting, cavitation, etc.)
7. Nonlinear fluid dynamic instabilities
8. Flows with large nonuniformities in physical properties of a single phase
9. Chemically reacting flows, especially gas-phase combustion, ignition, quenching
10. Sound generation by fluid motion, including cavitation in liquids
11. Flows in accelerating systems, especially in rotating systems
12. Analytical foundations of continuum mechanics, including non-equilibrium thermodynamics in fluid flows
13. Interaction of interphase interfaces with flows, e.g., dynamic surface tension and its effects on wetting, surface waves, liquid advance through porous media
14. Flow phenomena of non-Newtonian materials such as viscoelastic materials and those with nonlinear connection between stress and strain rate

15. Finite-amplitude surface waves and internal waves, waves, including runup on slopes, "breaking," etc.

16. Electromagnetic fluid dynamics

17. Structure of strong shock waves and interactions with shear flows and with solids

18. Detonation waves, both chemical and nuclear

19. Flows past bodies at speeds very near that of sound (Mach number near 1.0), i.e., transonic gas dynamics

20. Relativistic fluid dynamics

21. "Superfluid" hydrodynamics, including the implications of quantized vorticity

22. Heat and mass transfer in many of the above flows, including radiative heat transfer

23. Flows with molecular relaxation effects

Any attempt to describe the present scientific status in all the foregoing areas would be much too ambitious for this report. Therefore, more detailed statements are presented on only a few of them.

2.2.2 *Turbulence*

A qualitative understanding of many general physical features of turbulent flows has been achieved—there are probably no major qualitative "paradoxes." For example, it is known qualitatively how the turbulence extracts its kinetic energy and its vorticity from the general fluid motion; how these two properties are exchanged among eddies of different sizes and among (vector) components oriented in different directions; and how turbulence in a shear flow magnifies mean momentum transfer, thus increasing the "friction" by as much as several thousand times the laminar flow friction. Correspondingly, a good physical notion has emerged of how turbulence greatly increases the transfer rates of heat and mass and the homogeneous mixing rates of such scalar properties.

For some kinds of turbulence, it is possible to make rough theoretical estimates of most of the foregoing phenomena. But we are still far from having scientifically satisfactory theories for accurate prediction.

2.2.2.1 *The Structure of the Turbulence Inertial Range*   Recent experiments give increasingly detailed information about the spatial intermittency and probability distribution of the very small scales of high Reynolds number turbulence. The results heighten suspicions, raised by Kolmogorov himself and others, that the Kolmogorov similarity hypotheses leading to the famous $-5/3$ inertial-range law may need revision. On the other hand,

good experimental agreement with the $-5/3$ law has been observed for a dynamic range of $\simeq 10^4$ in the energy dissipation. These measured flows were in the atmosphere and in tidal channels and hence were inhomogeneous in the large and nonstationary in time. Clearer determinations need to be made of controlled turbulent flows at approximately the same (high) Reynolds numbers. This research should be pursued vigorously, both because the inertial transfer process is so basic to turbulence dynamics in general and because increased understanding of the small-scale dynamics may lead to better representations of their effects in computer simulations of larger-scale motions. Solid theoretical progress in the dynamics of the inertial energy transfer and the development of higher statistical properties is badly needed. It is especially important, for meteorological applications, to understand better the inertial transfer, and associated diffusion processes, in two-dimensional turbulence and the intermediate case where large scales constrained to two-dimensional flow interact with three-dimensional smaller scales.

2.2.2.2   *Finite-Amplitude Instabilities and the Transition to Turbulence*
The last 10 years have seen substantial progress in hydrodynamic stability theory, including especially finite-amplitude instability. There also has been progress in identifying some of the typical flow structures that play prominent roles in the breakdown into turbulence. Much of the recent theoretical work is unchecked by experiment. This area of research is an essential complement to statistical theory of turbulence. Further experimental work in finite-amplitude instability and transition to turbulence is indicated.

2.2.2.3   *Intermittency of Turbulence, Turbulent–Nonturbulent Interface, Entrainment of Fluid into Turbulent Boundary Layers and Wakes*   Extensive experimental work in recent years has yielded a wealth of information on statistical characteristics of these phenomena, together with visualizations of typical flow structures. Theoretical analysis and understanding lag behind. The turbulent–nonturbulent interface and the associated intermittency and entrainment phenomena are a basic aspect of turbulence and an important illustration that there is well-defined structure to turbulence, in addition to randomness.

2.2.2.4   *Highly Compressible Turbulence*   Although there has been extensive study of turbulence in hypersonic flows, the Mach number of the velocity fluctuations themselves typically is fairly small. There is comparatively little understanding of genuinely compressible turbulence in which the Mach number of the turbulent velocity itself is large.

2.2.2.5   *Numerical Solution of Analytical Turbulence Theories*   Several of the analytical approximations, which in recent years have led from Navier-Stokes equation to predictions for isotropic turbulence, give promise of excellent numerical agreement with experiment (spectra, mean velocity profiles, temperature profile, turbulent intensities) if applied to turbulent shear flow and Boussinesq turbulent convection at moderate Reynolds and Rayleigh numbers. There are, however, severe computational difficulties associated with the optimum representation of the statistical functions by as few numbers as possible, so as to make tractable the complicated equations that arise. Effort spent on these problems promises both advances in theoretical prediction of turbulence and numerical techniques of broad applicability.

2.2.2.6   *Noise Produced by Turbulence; Interaction of Turbulence with Sound*   Turbulent airflow over the skin of the aircraft is responsible for much of the cabin noise of a jet plane, while turbulence in the heated jet exhaust produces the roar heard from the ground during takeoff and flight. Despite 20 years of intense effort, there remain basic gaps in the theory of noise produced by turbulence, and little progress has been made in reducing jet noise sufficiently to alleviate substantially its environmental impact. The present empirical engineering efforts at reducing jet noise should be augmented by a continuing theoretical and experimental study of basic mechanisms of turbulence—acoustic-wave interaction. This interaction is also important in a variety of astrophysical contexts, including the internal dynamics of stars and the evolution of galaxies.

2.2.2.7   *Predictability of Turbulent Flow*   One factor in the question of weather predictability is the possibility that the larger spatial scales, which one hopes to predict, may be contaminated by interaction with smaller scales, which one cannot hope to measure (for want of enough measuring stations) in enough detail to give an input to the forecast. During the past few years, preliminary studies of the propagation of uncertainties through different scale sizes have been made by computer simulation and theoretical modeling. The importance of this question for weather forecasting makes a continued effort desirable, particularly in the context of two-dimensional turbulence. More generally, the predictability and stability of an initially specified turbulent flow is an extremely basic property of turbulence, which, so far, has received much less attention than it should.

2.2.2.8   *Turbulence in Non-Newtonian Fluids*   Extensive experimental research during the past decade has demonstrated that minute quantities of certain polymers added to a fluid can markedly inhibit the onset of

turbulence and alter the character of the turbulence, once formed. The nature of the phenomenon is not fully clear, but one important mechanism appears to be the dissipation of turbulent kinetic energy by the radiation of viscoelastic waves. Viscoelastic turbulence is an especially promising area for attack by analytical turbulence theory, because the presence of the wave mechanism adds an essentially linear element to the highly non-linear dynamics of ordinary turbulence, thereby increasing the accuracy of perturbation-theory-related approaches. Non-Newtonian turbulence is of great practical importance because of applications to drag reduction on hulls and in pipes.

2.2.3   Non-Newtonian Flows

Water and air, the two "fluids" most common on the earth's surface are both Newtonian under ordinary flow conditions (that is, they show a simple proportionality between shear stress and rate-of-shearing deforma-tion). Yet many important fluids depart from this simple behavior. Bio-medical examples are blood, mucous, and the contents of our intestines. Familiar industrial examples are molten plastics and slurries of particulate solids. A still more familiar example is found in kitchen and bakery: dough. So-called "slime molds" growing on pipe walls can increase the general flow resistance; in contrast, the addition of a tiny amount of high polymer to water can reduce the resistance by drastically altering turbu-lence dynamics.

   The departures from Newtonian behavior take a variety of forms, often in combination. Some fluids are elastic (as well as being linearly viscous, like Newtonian fluids), some are nonlinearly viscous, some behave like solids under small forces and like fluids under large ones, some even develop stresses in directions different from those in which they are de-formed.

   Most of the well-understood phenomena of fluid dynamics are restricted to Newtonian fluid flows, the objects of quantitative research for over a hundred years. Serious experimental and theoretical research on non-Newtonian fluids has been pursued only during the past 25 years. Well-defined and precise measurements of the anomalous properties of these fluids have been made only in the past few years; the experimental task has just begun. Theoretical analyses, based on formal mathematical dis-covery of just what classes of stress/strain-rate laws could possibly exist without violating the laws of mechanics and thermodynamics are now yielding an improved understanding of the mathematical structure of these two basic branches of physics. Both experimental and theoretical research on actual non-Newtonian flow phenomena are in their early

phases. The same is true for attempts to understand quantitatively the molecular bases of the properties of these strange fluids.

2.2.4 *Sound Generation in Flows and Surface Interaction*

The current interest in the scientific aspects of sound generation by fluid flow, a subject largely overlooked and neglected in the past, has to a great extent sprung out of public reaction against noise. Noise produced by fluid flow in various fluid-mechanical devices in industry and transportation (jet engines) plays an important role in this context.

Basically the scientific problems in this field involve (a) the identification of the sources of sound in a fluid flow and the study of the dependence of these sources on various flow parameters and boundary conditions and (b) the transmission of the sound to the observer through the fluid flow.

Although an important contribution to the theory of fluid-generated sound was made some 15 years ago, much remains to be done before a thorough understanding of the noise problems connected with the complex flows encountered in practice can be claimed.

In the basic theory, the sources of sound in a flow are expressed in terms of the fluctuations and spatial variation of the fluid-flow momentum flux. However, the relationship between this momentum flux fluctuation and available, or easily measured, flow parameters is poorly known (see Section 2.2.2), and the direct measurement of the momentum flux fluctuation, and appropriate required correlation functions, is quite complex. Therefore, the progress in gaining a useful understanding of sound emission from flows, such as turbulent jets and boundary layers, has been slow.

In recent reformulations and extensions of the theory, the sources of sound fluid flow have been expressed in terms of pressure fluctuations within the flow, and the role of their convection by the flow has been clarified. This has simplified, conceptually, the theoretical description of the problem. However, experimental difficulties still remain, although some progress has been made recently. Additional complications arise when the flow is supersonic, in which case the interaction of turbulence with shock waves gives rise to sound. Although this particular phenomenon is understood in simple cases, its role in supersonic jets is unclear.

The effect of boundaries on sound generation in a turbulent fluid, particularly if the boundaries are moving (as in fluid-mechanical machinery such as compressors), is not well understood at present. At low Reynolds numbers the sound reflected from the boundaries can react back on the flow and give rise to a variety of acoustic flow instabilities, often sources

of intense sound. A better understanding of this, and many other aspects
of the interaction of sound with flow, is urgently needed to aid in efforts
to control and use such effects.

2.2.5   Electrostatic Fluid Dynamics

When a fluid is a good insulator such that electrical charge remains local to
a fluid element for times comparable to or longer than hydrodynamic
motions, then it is possible for the stress created by an electric field acting
on charge immobilized within the fluid to alter the dynamical behavior of
the fluid. A classical example of an electrostatic hydrodynamic fluid is
the thunderstorm. Electrostatic charge is immobilized on cloud droplets,
and these, in turn, because of viscosity are convected by the fluid. The tur-
bulent fluid is subjected to two principal stresses: the pressure forces
associated with thermal convection and the electrostatic force existing be-
tween the cloud drops and the ambient electric field. The electrostatic
forces are independent of scale, whereas the dynamic forces of turbulence
become smaller as the scale becomes smaller. As a consequence, the
Kolmogorov turbulence spectrum is expected to be truncated at some
critical scale and a modification of turbulent viscosity ensue. The dis-
tribution and gradients of electrostatic forces within the cloud are believed
at present to have a major effect on the coalescence rate and, hence rain
rate, in clouds.

Electrostatic fluid dynamics is different from magnetohydrodynamics
(MHD), as a magnetic field is divergence-free; that is, there are no mono-
poles of magnetic field such that neutralization of the field can occur. A
consequence of this is that in MHD turbulent motions an equipartition of
energy between kinetic and magnetic is expected. However, no such equiv-
alent concept as equipartition exists in turbulent insulating fluids because
an electric field is *not* divergence-free, i.e., lines of electrostatic force
terminate on charges. The formulation of the conditions limiting equi-
partition are not fully understood and are of critical importance to an
understanding of the mechanisms of charge transport in thunderstorm
clouds. Electrostatic sound created by a lightning-stepped leader, the low-
frequency late time rumble following a lightning discharge, and new hi-fi
speakers are also examples of electrohydrodynamics.

2.2.6   Relativistic Fluid Dynamics

In the violent explosions of stars called supernovae, or in the still more
violent birth of our universe—called the "big bang"—and probably in the
collapse of neutron stars to "worm-holes" in the space–time metric, mat-
ter acquires a velocity approaching that of light, and, as a consequence,

the physical laws describing its motion must include not only special relativity but, in the latter two examples, general relativity. Not only are the laws of fluid dynamics different, but the fluid has vastly different properties. In the case of the outer envelope of a supernova star, the high temperature of the matter caused by the strong shock wave causes the electrodynamic degrees of freedom to dominate the energy density, and, as a consequence, photons and positron–electron pairs become vastly more numerous than the original ions. The fluid-dynamic behavior of such a fluid is only now becoming understood. The basic equations of special relativistic fluid dynamics are well understood, but their complexity has demanded computer solutions. The behavior of such a stellar envelope is important to the energetics of our galaxy, cosmic rays, and gas clouds. The envelope is most probably the matter from which we are created. Its "collective" interaction with interstellar magnetic fields becomes a complex and little understood problem of plasma physics, and the accumulation of this matter into planets, e.g., the earth, is also unknown and is of more than modest curiosity to man.

Numerical calculations of the general relativistic dynamic collapse of a neutron star have been made, but many uncertainties remain. Relativistic fluid dynamics has the added difficulty of a time coordinate that is a function of velocity and space.

General relativity has the further difficulty of the curvature of space due to strong gravitational fields. Just as man's curiosity searches for the way our universe arrived—the big bang—so, too, the problem of how it may leave—by the expansion of the universe or "down" a gravitational "wormhole"—is equally exciting.

3   Social Relevance

The social relevance of a science is currently of critical concern to decisions on funding. Relevance to immediate practical concerns should certainly be an important ingredient of such decisions; but if we are to create a rewarding way of life for the future, we must give consistent support to the quest for knowledge.

Scientific knowledge, unlike most other forms of human creation, is cumulative from generation to generation and relatively insensitive to taste or aesthetic judgment, though not entirely so. This cumulative character, with each generation building onto what was learned by the last, is

analogous to what happens with deoxyribonucleic acid (DNA) in biological evolution, but on a much shorter time scale. Science bears the same relation to cultural evolution that DNA does to biological evolution. Technology is analogous to environmental factors, while science is analogous to genetic factors in the process of adaptation. Technology is less universal, more specific to the social situation. Just as genetic information determines or delimits the ways in which the organism responds to the environment, so science delimits the technological response that each generation can make to its social and environmental situation.

Plasma physics and the physics of fluids are outstandingly rich in immediate useful applications, some of which are listed and discussed below.

3.1   PLASMAS

3.1.1   *Controlled Fusion*

The power requirements of the world are now rising steeply and can be extrapolated to show a sharp increase in the cost of fossil fuels and high-grade fissionable materials, well within a century from now. The present concern about the preservation of the world environment is more likely to speed this process than to slow it, since the effective treatment of pollution problems calls for vast additional expenditures of power. The world must therefore look with increasing urgency toward the two major "unlimited" sources of energy: fusion and breeder reactors.

Too little is known at present about the direct and indirect costs of these two approaches to make reliable economic comparisons. Both now appear economically attractive and potentially unlimited, and the very uncertainty about the detailed comparison underscores the great importance of widening the technological base by exploring both approaches intensively.

Aside from the direct economic question, fusion power has two inherent features that make it attractive as the long-term supplier of the world's power needs: (1) radioactive waste products resulting from normal operation are greatly reduced, though not altogether eliminated; (2) the margin of safety against abnormal release of radioactivity—through reactor failure, natural disaster, or clandestine diversion of weapons-grade material—is greatly increased.

The worldwide controlled fusion research effort began in earnest about 1950 and exponentiated rapidly during its first decade. In this era, the impetus was provided by the enormous prospects of fusion power, rather than by the experimental accomplishments, which indeed fell far short of what had been expected. Though many technical approaches were being pursued in parallel, grave difficulties of a plasma-physical nature seemed to frustrate them all. As a result, worldwide research support tended to

level off or—as in the United States—decline somewhat during the period 1960–1970.

During this same decade, a spectacular evolution of high-temperature plasma physics set in, which has been partly described in Chapter 2. The apparent obstacles of 1960 gave way on a broad front. It is one of the ironies of controlled fusion research that good plasma confinement properties turn out to be far more difficult to obtain for the modest plasma parameters typical of the initial efforts than for the denser, hotter, and larger plasmas that lie in the direction of the desired reactor regimes.

A second remarkable feature of the advance in controlled fusion research is its breadth. The multiplicity of initial approaches had been considered as a prelude to probable concentration on a single survivor. At present, there has been meaningful success along a number of very different lines of approach. This ability to proceed toward the reactor goal without narrowing the breadth of the technological thrust appears particularly promising with regard to the ability of controlled fusion research to meet engineering and economics requirements once the plasma-physics problems themselves have been solved. A variety of fusion reactions appear economically attractive and lend themselves to quite different types of reactor: for example, deuterium–tritium has the virtue of functioning at relatively low temperatures, with large energy multiplication; but several of the higher-temperature reactors deposit their energy in charged particles and thus lend themselves to extremely low heat pollution.

In regard to the probable time scale of success in controlled fusion, the following estimates are typical of present opinion in the field. It is now expected that the nature of the plasma-physics limitations will become well understood during the 1970's, and that the scientific feasibility of fusion power will be demonstrated experimentally around the end of this decade. If this goal is met, another decade may well be required to solve the engineering problems of a fusion reactor. Fusion power could see its first economic application in the last years of the present millennium. These estimates are based on the assumptions that the general trend of present scientific results will continue, and that the level of support for controlled fusion research will rise to keep pace with the requirement for increasingly realistic research facilities.

3.1.2 *Magnetohydrodynamic Power Generation*

Nearer at hand than the fusion reactor but using related technology is the direct conversion of fossil, and perhaps fission, fuels to electricity by the use of MHD generators. Technically, the MHD generator is a step along the path to fusion, and socially it offers some of fusion's benefits. It reduces thermal pollution because MHD plants require less cooling. This is because

the MHD generator, like the gas turbine, may be used either as an open-ended Brayton cycle, i.e., exhausted to the atmosphere, or the exhaust heat may be used in a heat exchanger, and then the combined MHD "topping" and conventional "bottoming" cycle results in a major increase in fuel economy. MHD facilitates the use of existing fossil fuel reserves, much of which are in water-poor areas of the country, and expands the capacity of those reserves by obtaining approximately 50 percent more power per unit of fuel consumed. MHD plants also promise more economical cleanup of particulate and chemical exhaust pollutants than is possible with conventional plants. Relatively unsophisticated and inexpensive MHD plants promise to be ideal for emergency and peaking service to prevent brown-outs and blackouts. Here the extremely rapid startup and response capability of this gas-phase device as compared with conventional rotating machinery are unique advantages.

In recent years, financial support in this area has been much less than optimum; however, there appears to be a growing awareness within both government and industry of the need to support MHD generator research and development, spurred primarily by a recognition of MHD's environmental advantages. There is considerable foreign activity in MHD power generation, chiefly in Japan, West Germany, and Russia. In Russia, a 75-MW pilot plant containing a 25-MW MHD generator was scheduled for completion in 1970. While the development of MHD generators had its start in this country, foreign efforts now substantially exceed our own.

MHD generators face some of the tensor effects and instabilities faced in fusion research, but to a far more benign degree, and these effects are coupled to phenomena familiar in classical fluid dynamics. The insight gained in dealing with these effects in the MHD generator, where they can more easily be dealt with and manipulated one at a time, is sure to be of value to those working toward fusion.

While the emphasis at present is on fossil-fueled MHD plants, it may eventually be feasible through advances in the MHD art and the development of high-temperature reactors to use MHD for the conversion of nuclear energy. The most obvious advantage of doing so would be a reduction of one of the biggest drawbacks of present nuclear plants, namely, thermal pollution. In the long run, of course, fusion holds out the promise of supplying mankind with inexhaustible power together with the minimum of undesirable side effects. Here again MHD will almost surely be used to convert the heat released into electric power.

3.1.3 *Space Environment*

The theoretical techniques developed in the controlled fusion program have been successfully applied and extended to explain many of the

plasma phenomena that occur in nearby space. For example, the physics that governs the maximum radiation flux in the Van Allen belts and the qualitative properties of the magnetosphere are well understood. However, our knowledge of these regions is still far from complete. Although many individual processes have been identified, a quantitative description of nearby space has not yet been achieved because of (1) the difficulty in devising theoretical descriptions in which the various individual processes interact with each other and (2) the lack of an understanding of the dissipation processes that must occur at the boundary between magnetosphere and solar wind.

The plasma phenomena that occur in nearby space provide—in large measure—the driving mechanisms for the formation of plasma density fluctuations, which are found in the ionosphere and can influence communications and radar tracking of satellites, particularly in the auroral zone. In midlatitude and equatorial regions, the instabilities responsible for the occurrence of plasma density fluctuations such as sporadic-E and sporadic-F have been identified with general (but not unanimous) acceptance. But there remains a large gap between identification of an instability in simplified geometry and the detailed understanding of real geometry and nonlinear phenomena. This gap must be closed before the effect of these density fluctuations on communications can be assessed and, possibly, can be used to improve communications.

3.2   THE RELEVANCE OF FLUIDS

3.2.1   *Examples of the Relevance of Fluids*

3.2.1.1   *Geophysical Problems*   (1) Meteorology, including weather forecasting and control, clear-air turbulence, storm dynamics (including hurricanes and tornadoes), atmospheric diffusion, dust storms; (2) oceanography, including general circulation, wave generation (including tidal waves) and control, interaction with the atmosphere, oceanic mixing and turnover, tides, beach erosion; (3) motion of other surface waters, including lake, reservoir, and river dynamics, silt transport, rainfall runoff, groundwater movement, motion of underground oil and gas, freezing and melting of moving water, glacier flow; (4) external environmental engineering, especially the dispersion of contaminants in atmosphere, oceans and other waters, including the containment and removal of oil spills, particle fallout, sound pollution; and (5) motion in the earth's core and generation of the earth's magnetic field, studies of other planetary interiors and atmospheres.

3.2.1.2   *Astrophysical Problems*   (1) Stellar structure, including stellar

interiors and atmospheres; (2) galactic structure and stability, interstellar gas clouds; (3) cosmological problems, especially the condensation of galaxies, stars, and planets out of more diffuse matter.

3.2.1.3 *Agriculture* (1) Irrigation, crop spraying and dusting, water seepage in soils, evaporation, hydraulic problems of dams, micrometeorology, soil erosion by wind and by runoff water, field and forest fires; (2) plant physiology, especially the motion of water and other fluids inside of plants, evaporation from leaves, and seed dispersal.

3.2.1.4 *Transportation Engineering* (1) Land: external aerodynamics of automobiles, surface trains, tube trains, etc., ground-effect machines on land, bearing pad trains, unwanted skidding of tires on wet roads; (2) Sea: hydrodynamics of surface vessels, ground-effect machines over water, submarines, including hulls and propellers or jets; (3) Air: aerodynamics of aircraft and missiles, including sonic boom, flight efficiency, short-takeoff-and-landing devices, persistence of wakes at airports and in flight; (4) Pneumatic (and hydraulic) pipeline transportation of gases, liquids, and granular solids such as coal and jet-bearing support of sheet metal in processing.

3.2.1.5 *Power Generation and Transmission* Flow of fuels, oxidizers, combustion products, etc. in various parts of internal combustion engines, steam engines, turbines and compressors, rocket engines; the myriad problems of machinery lubrication; fluid logic and control devices; magnetohydrodynamic generation of power; ordinary hydroelectric plant turbines, tidal turbines, windmills; heat transfer to and from flowing fluids in fission nuclear power plants.

3.2.1.6 *Manufacturing and Processing Industries* Chemical manufacturing in flowing gases, liquids, fluidized beds, fixed beds; industrial furnaces; processing and forming of plastics, glass, metals, and paper; printing; fire-fighting equipment; desalination of seawater; coating of solid bodies by dipping, painting, and spraying.

3.2.1.7 *Indoor Environmental Engineering* Flow and combustion in furnaces, gas and liquid flow in heating and refrigeration systems, room ventilation, maintaining sterile air in hospital rooms, hydrodynamics of plumbing systems (can they be made quiet?), laundering technology, wind and blast loads on buildings and other man-made structures.

3.2.1.8 *Biomedical Fluid Dynamics* (1) Human and other mammalian

physiology. Blood flow, airflow in lungs, flow of urine, flow of mucus
in trachea and elsewhere, flow of lymph and spinal fluid, lubrication of
joints, flow in intestines and in male and female reproductive systems,
including motility of sperm, eyeball hydrodynamics (including contact-
lens mechanics), interaction of fluid and solid motions in the ear, heart
pumps and other prostethic devices. (2) Motility of flying and swimming
animals, bird and insect flight, fish and mammal swimming, flagellar pro-
pulsion of various microorganisms.

3.2.2 *Relevance of Turbulence*

Most naturally occurring fluid flows are turbulent; there is an irregular
smaller-scale motion, in addition to the overall motion of the fluid. A
familiar example is the gustiness of wind. Turbulence on a variety of scale
sizes, from millimeters to kilometers, pervades atmospheric motion. The
chaotic billowy structure of a thunderhead provides a visualization of one
kind of atmospheric turbulence. The flow of water in rivers, oceans, and
pipes is also typically turbulent. The dynamics of fire and flames is funda-
mentally connected with turbulent motion, while turbulent jet exhaust
produces the noise heard from the ground.

Turbulence plays a central role in major environmental problems. Tur-
bulent transport, by the irregular motion, dominates the diffusion of pol-
lutants (gases, particles, heat) in the atmosphere and oceans and domi-
nates, as well, the diffusion of momentum, heat, and moisture in natural
meteorological and oceanic processes. All flows on scales important to
the environment are turbulent. They can be treated as laminar (nonturbu-
lent) in computer modeling of, say, pollution problems, only if the effect
of the turbulence is represented by some sort of equivalent transport
mechanism. It is of prime importance for such computer modeling that
the most faithful and computationally economical representation of the
turbulent transport mechanics be found.

Improved knowledge of turbulence transport dynamics, and the related
question of the effect of atmospheric turbulence on predictability of
weather, is one of the major objectives of the Global Atmospheric Re-
search Program, an important international cooperative venture.

Another area in which research on turbulence is socially and economi-
cally important is clear-air turbulence, the cause of frequent, dangerously
violent buffeting of jet aircraft. More basic knowledge is needed to im-
prove predictability of this phenomenon.

Some recent turbulence research offers hope of reducing the frictional
drag on aircraft and ships as they move through air and waters by inhibit-
ing and altering the turbulent boundary layers that form on their surfaces.

3.2.3 *Numerical Weather Forecasting*

Weather dynamics results from a complicated interplay of many factors including atmospheric fluid dynamics, the earth's rotation, thermal and frictional interactions with oceans and land masses, solar radiation effects, water evaporation, and condensation and precipitation. The weather experienced at any particular location is the combined effect of weather systems of such diverse sizes as cumulus cloud elements, thunderstorms, fronts, migratory anticyclones and cyclones (highs and lows), and circumpolar jet streams. Day-to-day changes in the weather result from the motion, deformation, and strengthening or weakening of existing systems, the birth of new systems, and the death of old ones.

Before 1950, nearly all weather forecasting was "subjective." This term means that the forecaster would identify the existing weather systems and then displace and deform these systems on the weather map and introduce new ones and remove old ones according to rules derived mainly from long experience with the weather. Subjective forecasts reflect the skill and art of the forecaster. They have never attained consistently the quality hoped for.

Numerical weather forecasting has been made possible by the high-speed computer. In fact, weather forecasting was one of the principal motivations in the original development of high-speed computers. A numerical forecast is produced by solving a set of equations governing the meteorological variables—pressure, temperature, moisture, wind components—using appropriate initial conditions. Operational numerical forecasts are now routinely prepared by the weather services of several nations, while experimental forecasts are performed by research groups. At the present time, numerical forecasts have surpassed subjective forecasts in predicting certain features, such as upper-level winds, while they are competitive in other respects.

The quality of a numerical forecast depends on the number of meteorological variables included, the correctness of the dynamical equations assumed, and the accuracy of the initial-condition data. A present-day large computer may represent an instantaneous state of the atmosphere by perhaps 100,000 numbers; effectively it solves 100,000 simultaneous ordinary differential equations. The numbers might be the values of five meteorological variables at each of five elevations at each point of a grid of 4000 points distributed over the earth's surface. Each grid point, or weather station, must therefore account for conditions over the order of 125,000 km^2. Systems of thunderstorm size generally will be lost between stations, while somewhat larger systems will be recognized but poorly de-

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scribed. It is essential that the subgrid scale features appear in the dynamical equations in some kind of averaged or estimated form because they contribute importantly to transport of momentum, heat, and water. Effectively, the subgrid scale systems must be treated as some kind of turbulence.

Current efforts at improving numerical forecasts are of three general kinds, all of which are essential to progress. Efforts are first of all directed toward increasing the density of observing stations and obtaining new kinds of observations over regions where conventional observations are prohibitively expensive, such as oceanic regions other than the main shipping lanes. Recent advances are measurements of worldwide temperature fields by infrared sensors on satellites and photographs of global cloud patterns from satellites. Some recent work indicates that it may be possible to make a tradeoff in data acquisition, using dense and frequent temperature measurements from satellites, together with some of the dynamical equations, to infer missing wind data.

The second way in which improvements must be sought is in the dynamical equations of the weather models. The subgrid motions, and other phenomena that cannot be included explicitly, should be treated as faithfully as possible, consistent with economy of computation. Given a computer capacity and speed, there are many ways to pick the meteorological variables that appear explicitly in the equations, and continuing research is needed to find the optimum choices. This problem involves many aspects of the physics of the atmosphere and earth, as noted in the opening paragraph of this section.

Finally, continued effort is needed to increase the memory capacity and speed of the computers and also to improve the techniques of getting data from the measuring stations to the computer center. The needed improvements in speed are a matter not only of reducing the time for individual arithmetic operations but also of designing the machine to take optimum advantage of the sequencing of operations called for in the solution.

Current procedures afford useful predictions of migratory systems for several days in advance. Indications from the present state of the research are that this time can be improved by a factor of 5 or more to the order of two weeks. Such an increase would have enormous economic implications. Beyond the order of two weeks, the return in increased forecast time for an added investment in measuring stations drops with prohibitive rapidity because the forecast time becomes, probably, a logarithmic function of the smallest scale size resolved. However, certain statistical properties, such as monthly averages, appear to be predictable at longer range.

The achievement of reliable numerical forecasts extending over the

order of two weeks is an attainable goal. An important by-product will be increased knowledge of what are the sensitive parameters to work with in weather-modification programs.

3.2.4   Re-entry Wakes

The ionized, chemically reacting, high-Mach number wake of a missile re-entering the atmosphere is of key importance in the recognition of missiles by defense systems. Basic experimental and theoretical understanding of such turbulent wakes, and the many complex phenomena that affect them, is growing but incomplete.

3.2.5   New Techniques of Measurement and Analysis

Recently developed new methods of probing turbulent flows, including laser and interferometric techniques, offer the possibility of measuring the velocity vector anywhere within a flow without disturbing the flow. Such methods, together with modern data-reduction methods, make possible the generation of sophisticated statistical data not before accessible.

3.2.6   Biomedical Fluid Mechanics

There are several organ systems in the human body that involve in a central fashion the circulation and processing of fluids: the cardiovascular system, the pulmonary system, and the urinary system. Associated with these are external devices either to support or replace the natural organs: extracorporeal blood oxygenators, heart-assist devices, kidney dialysis machines, and the like. Fluids and their motions are important also in the eyes, the ears, the mucous systems, the gastrointestinal tract, and the reproductive system.

   While the training of physicians and medical researchers is strong in biology and chemistry, it is notably weak in physics and in such branches of applied physics as fluid mechanics. The physiology underlying the various topics mentioned above has in a sense been crippled in the past because important physical aspects of each subject, including fluid-mechanical considerations, have not been sufficiently brought into the picture. While with the onrush of biomedical engineering in the past few years this has begun to change, rich opportunities lie ahead as fluid-mechanical understanding—and other aspects of physical understanding—is brought to bear on our knowledge of normal and pathological states. A far better knowledge of physiological fundamentals can be expected, and with it, improved methods for early warnings of disease, for clinical diagnosis,

for treatment, and, of course, a range of new medical devices, instruments, and prostheses.

The cardiovascular system provides dramatic examples of serious problems involving large numbers of the population, problems to whose solution fluid mechanics will inevitably contribute in significant ways. Some of these are described below.

Apart from being a complicated plumbing system for conveying blood, the arterial tree is a many-branched, nonuniform transmission line for the propagation of pressure pulses arising from the rhythmic, pulsatile action of the heart. Clinical observation of these pressure pulses (e.g., by means of volume-displacing cuffs) offers possibilities of noninvasive diagnosis of potential disease, or of actual pathologic states, associated for example with aneurysm, with blockages, and with atherosclerotic narrowing. Similarly, the understanding of the arterial tree as a transmission network offers the possibility of simpler and more accurate means to measure the flow from the heart and through various organ systems, as against the present cumbersome and tedious methods for measuring cardiac output. Again, the pulse-transmission properties of the arterial tree influence significantly the design and operation of the great variety of heart-assist and heart-replacement devices that are the subject of current research efforts.

Blood is a plasma (Newtonian in its behavior) that is highly concentrated in formed particles, of which red blood cells are the largest in size. The red cells are also the principal constituent as regards volume occupied, their normal composition being about 45 percent by volume. This concentrated, rich mixture, almost a slurry, has peculiar properties. In the large vessels it is like an ordinary liquid; in smaller vessels it begins to appear non-Newtonian; and in small capillaries the red cells are squeezed through single file. In the larger and medium-sized vessels the relative translations and the rotations of the red cells generate mutual velocity fields that produce perturbations akin to turbulence; these perturbations augment the diffusion of dissolved molecules and probably account almost totally for the self-diffusion of the red cells themselves. A host of fluid-mechanical problems are present here. Their elucidation will be important to our understanding and control of atherosclerosis, thrombus formation leading to strokes and coronary failure, and hemolysis, as well as to the proper design and functioning of prosthetic grafts, blood oxygenators, and artificial kidneys.

Similar examples of significant opportunities for fluid mechanics to contribute to medicine can be cited for the pulmonary system, the urinary system, the reproductive system, the gastrointestinal system, the mucous system, the ears, and the eyes.

3.2.7 *Noise*

Turbulent airflow over the skin of the aircraft is responsible for much of the cabin noise of a jet aircraft, while turbulence in the heated jet exhaust produces the roar heard from the ground during takeoff and flight. Despite 20 years of intense effort, there remain basic gaps in the theory of noise produced by turbulence, and little progress has been made in reducing jet noise sufficiently to alleviate its environmental impact substantially.

At sufficiently low Reynolds numbers, flow instabilities can arise, particularly when flow interacts with a boundary. The sound emitted from this interaction often produces a feedback to further enhance the instability, and this process can build up to produce violent, often destructive, flow oscillations. A specific example is the "stimulated Karman vortices" that have led to mechanical failure in industrial plants such as heat exchangers and in jet engine test cells. The "water hammer" is another example.

3.2.8 *Pollution*

The pollution of the earth's atmosphere and waters, mostly by mankind, has been too well publicized lately to need justification as an ominous threat to human health today, probably to human survival tomorrow.

The diffusion of fluid and particulate contaminants discharged into the air and into natural waters is a lively branch of fluid-dynamics application. Diffusion and particle fallout rates must be estimated well, because they affect the concentrations of contaminants, which in turn affect the health—or the very survival—of all living things.

In the 1970's practical meteorologists and engineers are still trying to estimate pollutant diffusion by indiscriminate formulas that are only slightly improved versions of those used in the 1930's and 1940's. Considerably more research in turbulent flow convection will be needed before the basic knowledge can be provided on which to build accurate and diversified calculation procedures.

A major strategy for minimizing pollution will be to extract more of the noxious matter from gas or liquid effluent before it enters the environment. The technological development of improved chemical-conversion and centrifugal or electrostatic precipitation devices will require a more detailed understanding of the associated flow phenomena. It is notable that the improvement of electrostatic precipitators may rely on electrohydrodynamics as well.

3.2.9   *Erosion*

One form of particulate diffusion is more important for its geological effects than for contamination: the wind and water erosion of soil and of river beds and beaches. Our ignorance of the fluid-dynamic foundations of dust and silt transport and fallout is exceeded only by our ignorance of how, in quantitative detail, the dust and silt are picked up from the surfaces by (turbulent) flows in the first place.

3.2.10   *Oil Pollution*

The problem of pollution due to oil released at sea has developed because of the ever-increasing scale of offshore drilling and oil transport. At present, a single ship or oil well has an awesome capacity to pollute vast areas of seashore—a capacity documented by such incidents as the *Torrey Canyon* or the oil well in the Santa Barbara Channel.

   When these accidents first became newsworthy, and hence drew public attention, the only method of dealing with the problem was to clean the beach by removing the oil as it came ashore. Several methods were tried, such as absorbing the oil on straw or washing the beach with detergent. However, experience soon showed that these methods, conceived for cleaning up small amounts of oil, did little to protect the environment from massive oil spills.

   Because additives, such as detergents, are generally toxic in the marine environment, attention was directed to simple mechanical means of containing and removing the oil. However, it was soon apparent that the various booms and barriers proposed to contain the oil, as well as devices to collect it, simply did not work in practice.

   There are a number of reasons for this failure of existing technology, but the most basic was the lack of physical understanding of how one fluid—oil—spreads and is collected while floating over the other fluid—the ocean.

   It seems worth pointing out that until two years ago, the total scientific literature on oil at sea consisted of three papers, one of which was in error. At present, knowledge of these fluid-mechanical problems is expanding fairly rapidly. It is now known how much oil a hydrodynamically stable barrier will hold in wind, waves, and current. From a series of oil-spreading experiments, it can now be predicted how long it will take a given spill to reach beaches, and hence the hazard of oil transport close to beaches can be evaluated.

   Such studies lead to fruitful cross application from other fields: for

example, devices to remove oil from the water surfaces have a flow that is analogous to the flow of a compressible gas in a sonic orifice. This analogy gives an explanation of why the oil removal rate for present devices is so low.

Basic fluid-mechanical studies also suggest what new difficulties will be encountered in the future. For example, if oil is spilled in an ice pack in the Arctic, it will flow under the ice and collect in pockets on the rough undersurface of the ice rather than flow over the ice. In this situation, the oil will remain until enough of the lighter hydrocarbons are leached out by the seawater (another fluid-mechanical calculation!) until the oil becomes more dense than the seawater and sinks.

It can be said that these fluid-mechanical studies are required in order to develop a workable technology for present oil-pollution problems and will be required in the future in order to assess properly the potential damage to the environment as the scope of offshore oil activity expands.

3.2.11   *The Earth's Atmosphere*

The study and understanding of the earth's atmosphere is perhaps the most relevant application of fluid dynamics. This is because the vagaries in the behavior of the relatively thin layer of gas on our planet affect us all—the seasons, the storms, the rain and snow, hurricanes, tornadoes, and, now of most social concern, the pollution of our atmosphere.

Not enough is known of fluid dynamics to predict, understand, modify, control, and finally abate each of the above natural—and unnatural— phenomena. The fact that so common a phenomenon as a thunderstorm is not yet understood is an indictment of both science and the support of science. Thunderstorms are probably responsible for cleaning the pollution from our atmosphere more than any other mechanism, yet how we may wittingly or unwittingly augment or hinder this phenomenon is unknown. There is some evidence that silver iodide seeding hinders thunderstorms, and heat and smoke pollution fortunately increase the frequency of thunderstorms, but far too little—tragically little—is known in view of the current need for knowledge.

Tornadoes kill several hundred people a year and do several hundred million dollars of damage. A major argument currently exists among atmospheric scientists whether electrical energy is an important part of the tornado mechanism. If tornadoes are primarily electrical it may be possible to modify them; if something else is critical to a tornado, that too might be man-controlled, but without knowledge nothing constructive can be done.

Hurricanes are now far better understood than in the past—thanks to

a major scientific effort; but despite prior attempts, the quantitative analysis of the effectiveness of modification is only now being performed.

The phenomena of mountain lee waves is presently fairly well understood, so that the initial conditions can, in principle, predict the final outcome. The spatial gradients of wind motion over mountain ranges allow a prediction of the intensity of lee waves. Lee waves impinge upon the operation of both man-made aircraft and the larger scales of atmospheric motion principally when they reach an amplitude large enough to break and form turbulence, often severe. The conditions for breaking are also moderately well understood. What is still missing is the prediction of the conditions.

An example that may be calculated soon is the high-altitude jet stream. The jet stream in the upper atmosphere is now accepted as the unwrapping of vorticity transported from the equator. However, the details of its behavior on the smaller scale, 50 km and less, are not understood. The notion that a state of the fluid that we might have called turbulent, i.e., the vorticity transported from the equator, might be unwrapped to result in more coherent motion is indeed strange in the face of our general notions of entropy. However, the state of two-dimensional turbulence may be a state of relatively low entropy, such that the production of a wandering jet stream from nearly random eddies represents an increase in entropy. The general phenomenon is now recognized, but the method by which the jet breaks up in an environment of the eddies that drive it is only now being considered.

3.2.12   *Clear-Air Turbulence*

Turbulence has always been a major problem in the operation of aircraft. In thunderstorms it is particularly severe and can cause failure of the airframe or other vital components. This risk has been minimized, however, by fairly effective procedures for avoiding thunderclouds. They are based on visual observation and on airborne radar to detect the large raindrops. Turbulence in clear air unfortunately is not detectable in this way and is especially troublesome because it is encountered unexpectedly, causing passenger discomfort and injuries and occasional loss of control of the aircraft.

Although it is known that clear-air turbulence is more likely to occur over mountains and near jet streams, the phenomenon is ill-understood, forecasting techniques are primitive, and workable airborne remote detection devices have yet to be developed. It is recognized in the Federal Plan for Clear Air Turbulence (FCM-69-2, p. 5) that, among other things, a basic research program is needed "to develop a better understanding of

the physical processes in the atmosphere which produce and maintain turbulence." Such a program is essentially concerned with the fluid dynamics of the turbulent patches, their structure and relation to the surrounding wind and temperature fields. Much of the information and insight about such mechanisms must come from laboratory investigations and theoretical studies. Such an understanding is an integral part of, and probably a prerequisite for, the successful forecasting of clear-air turbulence and is vitally needed for the correct interpretation of signals from remote detection devices (radar, lidar, infrared radiometers, etc.) currently under development.

Clear-air turbulence is important in other ways. It has been increasingly recognized that it plays a significant role in the general circulation of the atmosphere, and that further progress in weather prediction requires a systematic approach to all the detailed processes (including clear-air turbulence) that influence the development of the weather. From this point of view also, better understanding of the basic properties of turbulent flows is of the highest importance.

3.2.13   Fire

At the current state of knowledge of fluid dynamics, chemical reactions, radiation transport, fluid film flow, and multicomponent fluids, it is impossible to calculate the burning or extinguishing of a match. The basic problem in understanding fire is that a fire combines all the complexities of turbulent diffusion with radiation transport in a time-dependent medium, whose opacity and temperature are in turn dependent on chemical reactions and mixing rates. The detailed quantitative scientific understanding of the very first of man's inventions–fire–still eludes us.

If this problem could be calculated and understood in detail, there is a reasonable probability that more effective fire-prevention and fire-fighting techniques could be devised. High-polymer additives already have reduced fluid turbulent transport of momentum. How an ablating surface layer may act to similarly reduce thermal transport is not understood.

3.2.14   Fluid Logic

Bistable fluid jet devices that can assume either of two states, and that can be switched by a relatively small signal, may be used either in logic circuits or as power relays. As compared with electronic or electrical devices performing the same functions, they are not so fast, but they have advantages in cost and in freedom from damage caused by heat (as in proximity to jet engines) or by radiation fluxes (as in the interior of nuclear reactors).

Such devices, fluid mechanically, are examples of the *Coanda effect,* in which a jet will stick to a curved straight wall once it has been attached thereto. In order to improve fluidic devices, further research is needed on turbulent wall jets, on means for switching the jet from one wall to the other with the least expenditure of energy, and on efficient ways of slowing the jet in a diffuser and recovering the maximum amount of pressure head.

3.2.15   *High-Polymer Fluid-Resistance Effects*

The majority of fluid flows are turbulent, greatly increasing drag, resulting in increased power requirements or range–payload limitations or both on vehicles that move in the air or oceans and in increased pumping costs in the pumping of fluids. In pumping applications, and for undersea vehicles and planing hulls, the turbulent drag is virtually all of the drag; for surface-displacement hulls, the percentage of total drag attributable to turbulence depends on the speed. A substantial portion of the drag of large, slow vessels, such as super tankers, is due to turbulence.

Several substances, added to turbulent liquid flows in small amounts, can reduce this drag. For the most part, these substances are either soaps or polymers. For example, 18 ppm by weight of poly(ethylene oxide) will produce a 33 percent reduction in turbulent drag. Reductions as high as 80 percent have been observed due to polymer additives, at concentrations generally less than 100 ppm. The polymers involved are generally inexpensive and nontoxic.

The military and commercial applications of this drag-reduction phenomenon are numerous. Military applications include the possibility of decreasing the power and/or increasing the speed, range, or payload of torpedoes, submarines, and super tankers. Applications in the nonmilitary field include increasing the capacity of pipeline systems, decreasing hose sizes or increasing water volume delivered by fire-fighting equipment, increasing capacity of storm water sewers at peak loads, and increasing capacity of irrigation systems. In the aerospace field, the power absorbed by the fuel pumps in a large booster is substantial; the use of additives could reduce this and, consequently, the size and weight and fuel requirements of the pumps. Generally speaking, reductions of drag by 50 percent are achievable in most practical applications.

Considerable development work is going on in connection with the practical applications of this phenomenon, despite limited understanding of how it works; relatively little research is being done to improve this understanding. As a result, design must proceed on a semiempirical basis, and the effect of design changes cannot be reliably predicted. An understanding of drag reduction should make possible design to make more ef-

fective use of the phenomenon. For example, little is known about the molecular characteristics of a beneficial additive. Without an understanding of drag reduction, it is difficult to infer which characteristics of known effective drag reducers are desirable or essential. If such information were available, more suitable molecules could probably be fabricated.

3.2.16   Gas Centrifuge

One of the most complex applications of gas dynamics in recent years has been the development of the gas centrifuge for isotope separation, particularly of ^{235}U from ^{238}U. In recent years, the development of the ultra-high-speed centrifuge has progressed to the point where a gas centrifuge plant for the preparation of reactor grade fuel is being built in Belgium with cooperation of the Dutch and West Germans. The gas centrifuge develops a gaseous density gradient much like the earth's atmosphere but this is due to centrifugal force rather than to gravity. Phenomena analogous to jet streams and general circulation of the atmosphere develop, and only the most detailed analysis and application of the foremost knowledge of gas dynamics has proved successful in understanding the behavior of the machine.

3.3   THE PHYSICS OF PLASMAS AND FLUIDS IN NATIONAL DEFENSE

An important aspect of national defense is to prevent an unexpected confrontation of our nation by a totally dominant new technology. The classic case of such technological dominance is that which confronted Japan and terminated World War II. Although the national conscience may lead to a rejection of the use of such weapons of human destruction, nevertheless, the decision to refrain might better be made from a position of knowledge rather than ignorance.

Contrary to a popular conception, the development of nuclear weapons is as much a problem of the physics of fluids as of nuclear physics. The vast computational effort brought to bear is a direct reflection of the intricacies of the numerical simulation of fluid dynamics. The physical state of these fluids is that of an ionized gas—hence, in one sense "plasma"—but the principal dynamic behavior during the major course of the explosion phenomena is that of fluids. Only at very high altitude beyond the earth's atmosphere does plasma physics become important when the explosion products are affected by the earth's magnetic field. Important data on high-altitude nuclear explosions were obtained from the

Starfish, a 1.4-megaton detonation at an altitude of 400 km near Johnston Island in 1962. Many of these data have been presented in unclassified professional journals, and their analysis has attracted considerable interest in the plasma-physics community. Plasmas are a key to the understanding of secondary nuclear weapons effects—radar blackout, radio communication disruption, and problems of space environment.

One particular topic of current concern is the detection of the wakes caused by the passage of missiles, submarines, and ships. The first concerns plasma physics, aeronomy, and compressional waves; the second, turbulence decays; and the last, waves and turbulence. The obvious military need and tactical advantage of detecting wakes makes both plasmas and fluids particularly relevant.

While nuclear weapons technology represents one of the most sophisticated applications of the physics of plasmas and fluids, in fact almost all problems of military technology depend on the physics of fluids—as examples, the gas dynamics of guns, missiles, airplanes, and bombs and the fluid problems of ships and submarines. By and large, military technology depends on an application of classical physics. (The quantum-mechanical aspect of lasers is an exception.) The physics of fluids and plasmas is a major fraction of the basic classical-physics knowledge that supports these applications.

4 Recommendations to the Physics Survey Committee Concerning Experimental Research and Education in Turbulent Flow

4.1 RESEARCH

There are many facets of turbulence that demand more reliable and detailed measurements. Some seem worth special mention either because the cost would be relatively large or because some new technique may be needed.

(a) Development of rapid, high-capacity systems for data acquisition and analysis. Within the past few years, some turbulence research laboratories have developed and/or acquired high-speed digital recording and computing systems for measuring statistical properties of hot-wire anemom-

eter signals. Wisely used, such a system is an order-of-magnitude improvement in the possibilities for an experimenter to learn about turbulent flow. Unfortunately, not all of the (few) frontier laboratories have been able to afford such systems, so some have been rendered in one sense obsolescent. The modernizing of these laboratories is important if a broad attack on the turbulence problem is to be maintained.

(b) Internal intermittency. Significant improvement in data-collecting rates will permit the measurement of the more complex, multivariate statistical properties needed to characterize the peculiar splotchy nature of the turbulent fine-structure. This is one of the current frontier areas.

(c) High-Reynolds number turbulence under controlled conditions. The degree of universality of turbulent flows increases with increasing Reynolds number. Up to now the really large Reynolds number cases have all been in natural flows (tidal channel, atmosphere, etc.), which are inevitably subject to general "unsteadiness" and complicated inhomogeneities in the large. It would be desirable to have large Reynolds number "laboratory" flows developed and studied in great detail.

(d) Examples of needs in new instrumentation. The laser Doppler techniques now under development in several laboratories promise some kinds of measurement not possible with the hot-wire anemometer, which has been the mainstay of turbulence research for over 30 years.

There still exists no unequivocal device for measurement of static pressure fluctuations within turbulent flows. Developments in this direction would be important.

Physical insight into "spectrally local" properties has always been blurred by the inherently one-dimensional (hence spectrally nonlocal) character of the information. As long as theoretical analyses utilize Fourier modes, it would be most useful to devise experimental techniques for extracting spectrally local information, preferably "instantaneous."

The foregoing studies should, of course, be started (or have been started) in the simplest cases of constant density turbulence away from boundaries. The host of added phenomena brought in by mean shear, fixed and free boundaries, compressibility, density stratification, body forces, system rotation, chemical reactions, etc., will nearly all require laboratory facilities of better quality than are now available.

4.2   EDUCATION IN FLUID DYNAMICS

It was pointed out recently by an American Institute of Physics committee appointed to survey the matter that most fluid-dynamics teaching (and

academic research) in the United States goes on in engineering departments. The tremendous growth of modern physics over the past 50 years has simply squeezed classical physics into an ever smaller corner of physics curricula. Even there it is represented chiefly by electromagnetic theory and by rigid-body dynamics.

It seems desirable that some fluid dynamics be restored to physics curricula, not only because of its intrinsic importance but also because it provides nonlinear field problems that are both tangible and to some extent understood.

With some understanding of both fluid motion and electromagnetic theory, students will be well prepared to learn plasma dynamics.

Turbulence research in particular is handicapped by an "education gap." There are few universities in the world where regular courses are offered. There are even fewer where graduate students are brought to one or more of the current research frontiers. As a complex and evolving field, turbulence does not lend itself easily to capture in a textbook. Furthermore, the physically and mathematically straightforward approaches quickly become terribly complex, while the analytically simple approaches require for their *a priori* justification an immense amount of empirical information mixed in with heuristic arguments.

Because of the foregoing difficulties, a large fraction of the people doing turbulence research at any one time are cripplingly unfamiliar with the subject—and often are unaware of their handicaps.

This shortcoming is even more endemic to the vast engineering and applied science community, which is trying to estimate the behavior of complicated technological and natural turbulent flows. They are not familiar enough with the basic mechanisms to exploit them for practical estimates or to use them as go–no-go gauges for *ad hoc* assumptions.

A long-term solution to the education gap is to increase the good expository writing on turbulence (at least two good books are now in press) and to make the subject more generally available to students (who already know some fluid dynamics), at least on an elective basis. A short-term solution may be a postdoctoral fellowship program especially aimed at encouraging able young physicists, engineers, applied mathematicians, meteorologists, and oceanographers to spend a year at a university or research institute where basic research in turbulence is pursued.

5   International Cooperation

In the period 1950–1958, controlled fusion research was carried on
secretly in the United States, the United Kingdom, and the Soviet Union,
and to a much lesser extent in other countries. The increasingly evident
long-range nature of the research task encouraged declassification in 1958.
It was then found that very similar theoretical approaches and minor
experimental achievements characterized the various national programs.
Relations with the Soviet effort at this time were personally amiable but
distant and highly competitive.

The Geneva II Fusion Exhibits, particularly that of the United States,
stimulated worldwide participation in controlled fusion research. In 1961,
the first International Atomic Energy Agency sponsored conference on
Plasma Physics and Controlled Nuclear Fusion Research was held in Salz-
burg and attended by 29 national delegations. The conference was marked
by highly productive scientific exchanges, as well as by vigorous contro-
versies along national lines.

Since 1961, the worldwide fusion research effort has moved dramati-
cally toward real integration. There is a large-scale exchange of visitors and
visitors-in-residence with participant nations, including the Soviet Union.
Personal relations have become extremely cordial, and controversies,
though still lively, form entirely along the lines of scientific rather than
national territoriality.

Some major inputs that the West has received from the Soviet Union
since declassification are the Joffe geometry, which confirmed early
American predictions of the stabilizing effect of negative curvature and
was the key to success in recent open-ended experiments; the theory of drift
waves; and the Tokamak device, which has provided the principal break-
through to high temperatures in toroidal experiments. Some major inputs
by the United States are stellarator theory, the theta pinch and adiabatic
compression techniques, and neutral-beam injection. The scientific value
of this interaction has been so important for the U.S. fusion program
that it is difficult to imagine what alternate course it might have followed
in the absence of declassification. Possibly the program would have stag-
nated entirely and dropped to a small-scale holding action.

The effect on international relations has been altogether positive,
though its magnitude is perhaps difficult to assess. The controlled fusion
project has served as an illustration of how easily and cordially intelligent
people of all political persuasions can work together on a task of common
usefulness to the world. If the present relationship in worldwide con-
trolled fusion research can be preserved as fusion power enters the era of

practical application, this common success may well have a measurable impact on further East–West cooperation.

A similar impact on East–West relations can be claimed for meteorological research. During the original development of computer analysis of global circulation, the weather reporting of Soviet stations was crucial. The initial cooperation in exchanging northern hemisphere weather observations has now been extended to the worldwide effort known as GARP, in which almost all the nations of the world are cooperating in a Global Atmospheric Research Program to understand weather on a worldwide basis. The impact of this and other international programs of cooperative meteorological research are of major importance to East–West relations.

In addition, regular cooperation exists between the U.S. and NATO allies through the organization called Advisory Group for Aerospace Research and Development (AGARD). AGARD sponsors several international symposia each year, some of them dwelling with research problems basic to aerodynamics. The vehicle for even greater international cooperation has been in areas that might be termed "geophysical fluid dynamics." This has most commonly taken the form of international scientific conferences sponsored by various international scholarly bodies such as the International Union of Geodesy and Geophysics and the International Union of Theoretical and Applied Mechanics.

6   Interaction with Other Sciences

One of the major reinforcements of the creative processes is the application of knowledge in one field to the understanding and growth of another. Plasma physics and the physics of fluids interact in a major fashion with each other and with most other sciences. A few of these interactions are given as examples below.

6.1   FLUIDS

Examples abound of the basic role of the physics of fluids in other disciplines such as civil, chemical, and aeronautical engineering; meteorology; and the medical and biological sciences. Further progress in understanding fluid turbulence can have a significant impact on all these fields. Hy-

drodynamics continues to provide a strong stimulus to mathematical research on partial differential equations and boundary value problems. Beyond classical fluid physics lies the only partly explored superfluid state, already important in astrophysics in connection with neutron stars and in low-temperature physics concerning the remarkable properties of liquid helium.

6.2   PLASMAS

The growth of plasma physics as an independent discipline continues to benefit astrophysics, geophysics, and gaseous electronics from which it emerged. This is especially true in the complex area of collective phenomena about which much has been learned in response to the needs of controlled fusion and space research. Examples are solar and supernova phenomena, geomagnetic phenomena, and the earth's radiation belts. In solid-state physics, many properties of metals and superconductors are plasma phenomena. The propagation of helicon waves in solids, corresponding to whistler waves in plasma, and the Gunn effect, analogous to the plasma two-stream instability, were known in plasma physics well before they were recognized in solids. In other areas, hot plasmas have provided a new research environment for high-temperature chemistry, and plasma physics has created a demand for new atomic cross-section data. Scientific spinoff from plasma research includes such diverse areas as high vacuum technology, improved laser holography, and new approaches to computer simulation of complicated physical phenomena.

6.3   INSTRUMENTATION

The importance of both fluid and plasma physics to instrumentation for other disciplines should be emphasized. A case in point, and a good example of cross-fertilization of very different areas of physics, is elementary-particle physics. The measuring instruments of particle physics have frequently employed properties of fluids, first in the cloud chamber and later in bubble chambers and spark chambers. It now seems likely that the next generation of "instruments" to produce elementary particles may be electron ring accelerators, which rely upon collective plasma phenomena. A striking instance of interdisciplinary effort, research of these accelerators, was initiated in this country by a study group in Berkeley in 1968 at which plasma physicists and accelerator experts around the country gathered to evaluate this new approach to high-

energy accelerators shortly after the idea was announced by the Russians. In part thanks to the state of development of plasma physics at that time, it was possible to appraise the problem of ring stability with reassuring thoroughness, a fact that contributed materially to the decision to embark on a research program. Subsequently, some of the more detailed analysis has used computer simulation codes developed at Los Alamos for controlled fusion plasma research, and the first experiments utilized the Astron accelerator also borrowed from the fusion program. Another cross-fertilization from plasma physics presently under investigation is a medical application of a high-current ion accelerator first developed as a means of creating fusion plasmas in the laboratory. This study utilizes the accelerator to produce 14-MeV deuterium–tritium neutrons, which are much more effective than x rays or gamma rays in treating cancer.

6.4 INTERACTION WITH ASTROPHYSICS

Plasma physics and astrophysics have been intimately interdependent since the emergence of each as a science. The gravitational force field between stars has the same mathematical form—inverse radius squared—as the electrostatic force between ions and electrons. The concept of "dynamic friction" (the manner in which a test particle interacts with all its neighbors) was first developed for stars to understand the relaxation of star clusters and hence to establish an age for our galaxy. The identical concept and mathematical formalism were later used to find the thermal relaxation of the particles of a plasma.

The bulk of the gas of the galaxy is neutral, and hence we might expect the dominant behavior to be that of an unrestrained fluid or gas. The neutral gas, however, is closely coupled to an ionized fraction (roughly 2 percent), which is strongly influenced and restrained by magnetic fields. In addition, the cosmic rays represent a suprathermal gas that exerts a stress comparable to that of the magnetic field. The whole complex system is held together by the gravitational field of the galaxy and modulated by the rotation of the galaxy. The theories of the plasma coupling to the field, of the magnetohydrodynamic motions, and of the instabilities that take place are all a part of plasma physics.

The cosmic-ray gas diffuses through the medium in a manner dependent upon velocity–space instabilities. Its escape or confinement from the galaxy is entirely analogous to the confinement problems of controlled fusion. The galactic "halo" is an extension of the concept of galactic confinement to explain the radio brightness surrounding the galaxy.

The new phenomena associated with pulsars have been explained by

recourse to some of the most exotic magnetohydrodynamics conceivable. A rotating neutron star presumably has an off-axis dipole field that rotates with the star. At, or near, the radius where co-rotation would imply magnetic flux lines moving at the velocity of light, plasma is whirled to relativistic energies, and high-energy particles, x rays, light, and radio waves are emitted.

Sunspots are a familiar phenomenon to many of us, as an astrological sign (if one tends toward the occult), as a vexing problem to those in radio communications, and as a hazard to those contemplating a journey to the moon or other close-by heavenly bodies. To the plasma physicist, sunspots still represent a challenging scientific problem. It appears that magnetic tubes of force are confined within the sun by plasma—rather than the inverse, as attempted for fusion in the laboratory. The stability conditions simply change sign: what before was magnetohydrodynamically unstable is now stable, and vice versa. How the tubes of magnetic force are released deep within the sun is a mystery, but of such mysteries the cross-fertilization of sciences is made.

The largest known organizations of matter are galaxies (we do not know about the structure of the universe). How matter is assembled in the galactic forms is still a puzzle. The explanation may lie entirely with fluid dynamics—initiated by turbulence in the big bang—or may lie with the gravitational or hydromagnetic instabilities of a plasma or fluid. The answer awaits the better understanding of plasma physics and the physics of fluids.

The study of fluid turbulence is significant for other parts of science in two ways. First, the phenomena of turbulence play a role directly, notably in geophysics and astrophysics, where turbulent transport processes are important.

Second, classical fluid turbulence is valuable as a prototype, analogy, and testing ground for formal techniques and approximation methods. The theory of fluid turbulence has suggested methods of attack for plasma turbulence, including turbulence in solid-state plasmas. More generally, Navier-Stokes turbulence represents a stochastic nonlinear field problem in which the fundamental difficulties associated with the fact of nonlinearity are displayed in a particularly simple way and are directly confrontable with experiments. The turbulence problem is a prime example of strongly nonequilibrium statistical mechanics. It provides, also, a close formal analogy to some problems of spin statistics in ferromagnets and, somewhat less directly, to a variety of quantum field theory and quantum many-body problems. Similar techniques to obtain approximate equations for correlation functions can be used in all these problems. The limit of infinite Reynolds number in turbulence, which is physically

meaningful, corresponds to infinite coupling constant in nonlinear quantum field theory.

Superfluidity is a field in which techniques of turbulence theory can be applied semiphenomenologically, to the statistics of vortex lines and, more fundamentally, to the equations for the second-quantized field directly.

Many advances in turbulence theory have been made by physicists originally trained in other areas. Turbulence remains one of the most difficult and challenging problems in nonequilibrium statistical mechanics and nonlinear field theory; further attacks on it by physicists from other disciplines should continue to produce valuable cross-fertilization.

The statistical-dynamical techniques of turbulence theory also hold great potential, just beginning to be explored, for a number of topics in biophysics and biology, including the statistics of polymer chains, the dynamics of genetic material, and the organization of neural nets.

6.5   APPLIED MATHEMATICS

The interaction of fluid dynamics with mathematics has been long and fruitful. Primarily, of course, mathematics has provided powerful tools that can be used in the analysis of flow phenomena. Yet there has been a significant contribution in the other direction as well. Analytical peculiarities in the theoretical formulation of fluid-dynamic problems have raised mathematical questions that might not otherwise have come up. These questions have stimulated some developments in pure mathematics.

One example is the influence of fluid-dynamic stability research on work generalizing the mathematical arguments on topological degree in differential equation theory. The primary question in stability research is the following: What is the value R_c of the "Reynolds number" such that a given flow is "asymptotically" stable when $R < R_c$ and unstable when $R > R_c$? The point $R = R_c$ is a "branching point" of the governing nonlinear partial differential equation. Furthermore, questions of how many new solutions (i.e., flow configurations) can exist as the Reynolds number is increased beyond R_c have led to generalization of the previously bivalued concept of uniqueness–nonuniqueness of solutions of equations.

A second and more famous impact of fluid dynamics on mathematics has been a result of the questions raised by the attempts to carry out formal theoretical analysis of the thin frictional "boundary layer" adjacent to an airplane wing or like surface. In the differential equation whose solu-

tion describes this flow, the coefficient of the (dominant) highest-order term is extremely small. Yet the total term remains important. The interesting behavior of this class of problems has stimulated development of an area of mathematical analysis usually called "singular perturbation theory" and has led to the study of "matched asymptotic expansions" in terms of families of limit processes. Thus theoretical study of these fluid-dynamic problems has encouraged mathematicians to address themselves to broader questions concerning the dependence of solutions of partial differential equations on parameters in the equations.

6.6 PHYSIOLOGY

Animal bodies, including the human body, incorporate some remarkable fluid-mechanical systems involving the circulation and processing of gases and liquids. The best known ones are the circulatory system (heart, arteries, capillary beds, veins), the pulmonary system (nose, trachea, lungs, alveoli), and the urinary system (kidneys, ureter, bladder, urethra). But there are other important organs involving fluids and their motions: the eyes, the ears, the mucous systems, the gastrointestinal tract, and the reproductive system. There is good evidence that fluid flow phenomena may play a significant role at the cellular level as well.

Thus, fluid mechanics as a discipline is not only intimately related to physiology, even more it is an essential science underlying physiology. Strangely, the large body of knowledge in fluid mechanics has only been recently turned toward physiological problems. It is clear that many unresolved questions in physiology will be in part answered by fluid mechanics, and that the study of physiology as well as the textbooks of the subject will profit from the application of fluid-mechanics methods.

6.7 COMPUTER DEVELOPMENT

During the last two decades, there have been two major classes of problem in fluid dynamics that have been considered of sufficient importance to stimulate the development of computers of ever greater speed. Initially the most important class involved problems arising in the design of nuclear and thermonuclear weapons. The cost of weapons tests was so great that even partial replacement of such tests by numerical simulation was a bargain.

One of the earliest computers designed to handle such problems was built in the late 1940's under the direction of John von Neumann at the

Institute for Advanced Study. This computer was used not only for weapons-design calculations but also for the beginnings of what has become the other major fluid-dynamics stimulant to computer design, namely, numerical weather prediction.

During the 1950's, many computers based on the von Neumann design became commercially available, and their increasing speed permitted calculations with finer space meshes and thus with higher resolution. Yet for the most part these calculations were limited to problems with only one space dimension, e.g., with spherically symmetric configurations.

In the mid-1950's, it became clear that even more powerful computers were needed to begin to handle fluid-dynamics problems of two space dimensions. The people working on such problems at the Atomic Energy Commission laboratories discussed specifications for such computers with manufacturers. These discussions led in the early 1960's to the UNIVAC LARC at Livermore and the IBM STRETCH at Los Alamos. Although these computers were large and costly and only a few were ever built, they did bring about a shift to transistor technology in computer design.

During the 1960's, the use of computers for numerical weather prediction became routine, but perhaps the first significant influence of meteorological problems on possible computer design came from the recognition that these as well as other fluid-dynamics problems involve the same arithmetic operations being performed over an array of space mesh points. Such arithmetic is performed sequentially on present computers, but if it could be performed simultaneously, a large speed increase would be realized. Out of such considerations has come the design of the ILLIAC IV the first of which will have 64 parallel processors and is expected to be operating in 1972. A lesser amount of parallelism is to be available at about the same time in the CDC STAR which uses string array processing or "pipelining" of its arithmetic operations. The first STAR is to be used for solving weapons-design problems; the first ILLIAC IV is to be used, in part, for solving meteorological fluid-dynamics problems. Each will require changes in programming techniques in order to realize its potential speed.

6.8 FLUIDS IN ASTROPHYSICS (CONVECTION IN STARS)

Virtually every star from white dwarfs to the postulated supermassive objects has convection in it somewhere. The efficiency of convective heat transfer must be known to calculate their structures. But a number of other effects of convection must be understood.

Acoustic generation by convective motions must be calculated to provide theories of coronas and chromospheres with their inputs. The effects

of penetrative convection must be assessed to understand observed chemical composition. Convective dynamics are crucial for the understanding of the solar cycle. The coupling of rotation and convection needs to be studied especially in connection with solar spin-down; this problem is important for distinguishing various gravitational theories and unraveling the failure to detect solar neutrinos. Time-dependent convection has to be understood to explain certain pulsational instabilities in stars. Solutes in convection act like chemical inhomogeneities in stars and are important in advanced stages of stellar evolution.

6.9   RADIATIVE FLUID DYNAMICS

The chief problems are radiative shock waves, how sound waves steepen into shocks, and how the shocks heat the medium in order to understand the chromosphere and corona of the sun. Similar problems arise in pulsating stars. Shock waves are also important in the interstellar and possibly in the intergalactic medium.

In the early phases of the universe, hydrodynamics is radiation-dominated. In this phase, pair formation is important and the effects (especially on turbulence and gravitational instability) need further clarification.

In planetary atmospheres, radiative fluid dynamics is also essential, for example, in the circulation of the Venus atmosphere. The differential radiative heating of the earth is central to the atmosphere circulation problem.

7   Statistical Description of Plasma Physics and the Physics of Fluids

The combined physics research in plasmas and fluids produces roughly 7 percent of the world's physics publications and 8 percent of the U.S. publications. The fields involve 8 percent of the registered U.S. physicists, roughly equally divided. A partially misleading impression from these figures arises because a very large part of fluid-dynamics research is performed by people who call themselves engineers or applied mathematicians. Of the physics theses produced in 1965 and 1969, roughly 4 to 5 percent were produced in plasma physics and only 1 percent in the phys-

ics of fluids. On the other hand, 30 percent of the engineering theses (theses that "might equally have appeared in physics") were produced in fluid dynamics, so that the combined plasma and fluids theses are 17 percent of the total, with three times the contribution by fluids.

The median age of plasma and fluid physicists is 37.3 years, which is essentially the same as the median for all physicists of 37.7 years. The age for plasma physicists in fusion research is 38 years. The corresponding number for one major plasma-physics laboratory in the Soviet Union is 30 years. The median dollars per research scientist in industrial or university research is roughly $60,000. In fusion—because of the high cost of experimental equipment—the corresponding figure is $160,000. In nuclear weapons or wind-tunnel research the figure would be still higher if facility monies are included.

8 Funding

There are two classes of funding in science each of which is essential to the other. The one is the funding directed toward an applied goal, which may, and wisely often does, entail basic research but is directed overall with the applied goal in mind. The second is funding of basic research that seeks to increase the total store of knowledge and is only secondarily related to a specific goal. The mechanism for each type of funding must of necessity be quite different.

It is modestly straightforward to decide administratively on a goal, fund it, and choose scientifically competent administrators. This is the situation in the large plasma efforts on fusion and magnetohydrodynamics, and in the fluid-dynamics problems relating to airplanes, missiles, etc. However, in the case of basic research, this method of determining what to do next has been found lacking time and time again. A method of funding basic research that has worked well is competition among scientists with grant proposals to the National Science Foundation. (This method originated at the Office of Naval Research and is used by other agencies as well.)

The proposal—written by the scientist himself—is his personal offer of creativity. He knows that it will be read, criticized, and evaluated primarily by his peers. If the value of his ideas and competence prevails, then in general he expects and will be granted financial support for his research work. Almost always this support will be to a university for the support

of his research. This does not mean—contrary to popular belief and occasional exceptions—that the scientist works less hard but rather that his teaching load is reduced from full time to somewhat less and that for the remainder of his time the pressure of his work increases enormously. He is now in keen competition with his peer group; his career and reputation are continually at stake.

Why should basic research be performed primarily in universities? The objective is evidently to let the generation of new knowledge and the communication of that knowledge mutually reinforce one another. It might be equally "efficient" to generate new knowledge in national centers (although universities typically support one half of the research environment), but such a separation would gravely weaken the processes of teaching and learning.

The effective "currency" of a PhD education in the sciences is roughly 6 to 7 years. To remain up to date in his field, a professor must therefore re-educate himself continually. Almost without exception, the only way to achieve this is to be involved with research at the scientific frontiers. The currency of a professor is just as relevant at the undergraduate level as at the graduate level. An undergraduate science student knows the difference between a teacher who has been repeating the same thing for the last 10 years and one who intersperses his teaching with comments on the relation between a principle and the newest discovery.

An overall research funding policy must meet both the need to attain practical goals and the longer-range need to produce basic science and creative scientific personnel. With these two separate objectives in mind, we discuss the present funding of plasma physics and the physics of fluids.

8.1   PLASMA PHYSICS

Plasma physics is dominated by the objective of nuclear fusion. Because of the extraordinary subtleties involved, it has been necessary to fund a major fraction of the project as basic research. The current budget is about $30 million per year. Figure VII.1 shows the trend of scientific manpower in the Atomic Energy Commission (AEC) supported fusion laboratories, which have received about 85 percent of AEC controlled thermonuclear research funds for the last 5 years and illustrates that it has been near constant. In view of the major potential importance of controlled fusion to the world economy, and of the steady progress being made toward the goal, the lack of reinforcement by increased support is surprising. By way of comparison, Figure VII.2 shows the ratio of U.S.
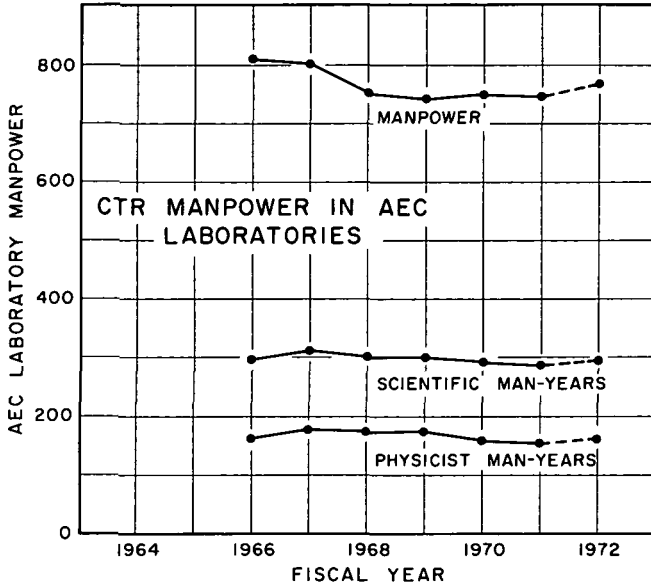
FIGURE VII.1   Manpower engaged in controlled thermo-
nuclear activity at five A EC laboratories during the period 1964–
1972. Over this period the trend of scientific manpower has been
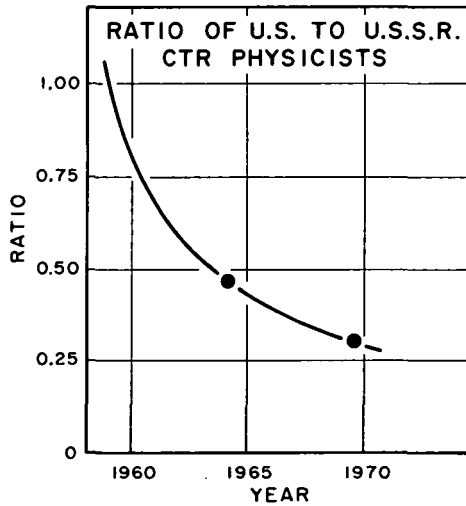nearly constant.

FIGURE VII.2   The ratio of U.S.
to Soviet physicists engaged in
controlled thermonuclear re-
search during the period 1959–
1970.

to Soviet manpower over the same period. The increasing level of support in the Soviet Union is an exciting reinforcement of their project.

The support of controlled fusion research in the United States and elsewhere is concentrated on large confinement experiments in a number of major laboratories. The universities have pursued smaller—but often very important—experiments, have trained young scientists for the major laboratories, and in turn have tended to recruit their professors from major laboratory personnel. Due to the stagnant support situation,* this symbiotic relationship is now running into difficulties. The major laboratories are virtually unable to hire new scientists at this point. Many trained young plasma physicists of good qualifications are therefore unemployed and looking for other work—though their help could be effectively utilized if there were to be a real drive for fusion power in this century. The small university research projects are now in keen competition with the major laboratory experiments for dwindling research funds. The ironic result is that the drive for major experimental success in controlled fusion is hampered by the need to support university groups, the main purpose of which is to turn out the students who are supposed to find jobs as controlled fusion research swings into high gear. The entire situation could be restored to its former healthy state by providing for a moderately rising manpower level at the major laboratories.   ·

It can be argued that all projects should have a "modestly increasing manpower" to remain "healthy." This is clearly not the case for a technology that has been solved. Because of the continual shift from production to service in our economy, a constant manpower ceiling in a service function means a time-absorbed emphasis, i.e., the expectation that the relative importance of a project is a decreasing function of time. In these terms the Panel strongly believes that controlled thermonuclear research as well as the overall average of research should be of continually increasing relative importance in our society. Therefore, the plea for moderate manpower increase is a reflection of at least constant relative importance in an ever-changing society.

The present funding of magnetohydrodynamics (MHD) is even more dismal. By 1965, in a joint $10 million development program with a utility group led by American Electric Power, AVCO had solved most of the basic problems and with Department of Defense (DOD) support had built a successful pilot generator that was designed for short bursts of power. The necessary superconducting magnetic technology also had been solved by AVCO. In 1966, therefore, AVCO proposed to build a 30-MW MHD prototype base-load plant. The utility group agreed to raise some $13 million

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* While actual dollar levels have increased, inflation has caused a static, if not slightly decreasing, manpower effort.

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for it, providing the federal government put up a matching sum; but federal funds were denied, and the utilities withdrew. By way of comparison, nuclear fission power research was supported at roughly $300 million last year.

The current funding of plasma physics as a basic science is such as to reduce drastically the dollars available to support new proposals. As is well known by now, the retraction of the DOD from supporting basic research has meant that the support of this fraction of basic research has fallen to the National Science Foundation (NSF). Plasma physics is funded through several programs at NSF, for its value to several disciplines. The program in the Physics Section, in which experimental plasma research is judged as physics, places all proposals on extranuclear research in competition. By judging new and renewal proposals together in this program, new grants can be made, although the total funds have stayed constant; in the past two years 15 percent of the grants made were for new activities, by terminating the less competitive on-going research. But these new grants were drawn from a much larger total pool of new proposals: only 5 percent of all new proposals considered in fiscal years 1970 and 1971 could be granted.

The dollar investment by the NSF (Physics and Engineering) and the Atomic Energy Commission (AEC) in support of university proposals in basic plasma physics actually declined in 1971; yet the motivation for federal support of education, research, and new knowledge in this field is vastly greater. The continuing decrease in support is shown in Figure VII.3. The funding pattern in 1971 shows that NSF is picking up a significant fraction of previously supported work in plasmas. The breakdown indicates that the major fraction is supported as relevant research.

Approximate figures for federal support of basic plasma physics in universities in 1971 are as follows:

	Annual Rate (total)	No. of Grants
NSF		
Physics	$1,012 K	13
Astronomy	137 K	6
Atmospheric Sciences	1,811 K	39
Polar Programs	625 K (est.)	?
Engineering	1,660 K	51
	$5,245 K	109+
DOD		
ONR	$ 513 K	13
AFOSR	1,597 K	34
ARO-(D)	15 K	1

	Annual Rate (total)	No. of Grants
DNA (ex-DASA)	260 K	4
ARPA	0 K	0
	$2,385 K	52
NASA	$2,208 K	32
AEC*	4,856 K	.43
	$14,694 K	236+

*Includes basic component at the Lawrence Berkeley Laboratory and the Princeton University Plasma Physics Laboratory; does not include Wisconsin Multipole.

The *applied* plasma-related research supported by federal agencies is significantly larger (in weapons effects, missile wakes, space environment, etc.). Considering the large application and relevance of plasma physics, it would seem that basic work should be supported at least at a level that preserves the creative energy inherent in the small fraction of scientists performing it. The basic research fraction is almost always the leadership fraction. With current fiscal policies it is being deeply harmed.

8.2 FUNDING OF THE PHYSICS OF FLUIDS

As opposed to the intense pressure experienced in plasma physics due to a rapid decline in total support, the physics of fluids funding has changed less precipitously. This is most clearly reflected in the proposals received in the Engineering Division of the NSF, where the clearly identifiable "drop-out" (by other agencies) proposals in fluid dynamics submitted have been only a few percent of the total in number and less than 10 percent in total dollars.

The approximate funding in NSF of fluid mechanics and the physics of fluids corresponds to a modest increase of 5 to 10 percent per year over the last several years. The breakdown for fiscal 1970 for the various NSF sources is as follows:

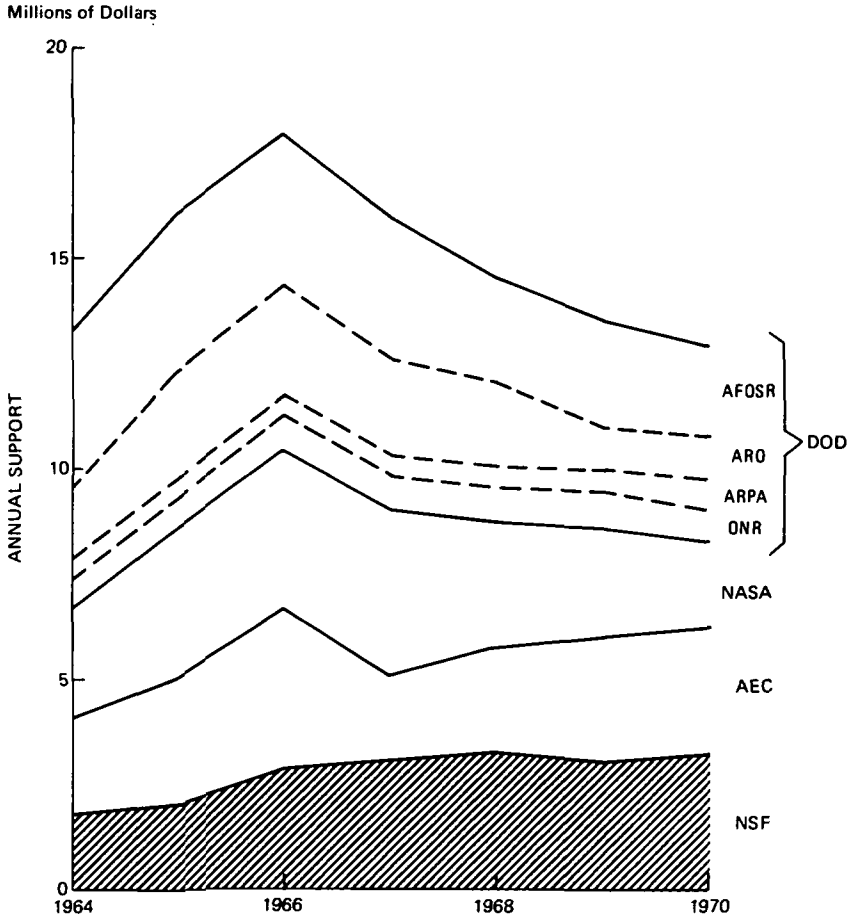|	
$2.86 million	Engineering Division and Mathematics Division
0.54 million	Engineering Division initiation grants
0.68 million	Oceanography
0.75 million	Meteorology
0.50 million	NCAR
0.05 million	Physics Section
$5.38 million	Total NSF

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FIGURE VII.3 Federal support of university research in atomic, molecular, and plasma physics.

DOD funding of the physics of fluids in millions of dollars is as follows:

Year	Army	Navy	Air Force	Total
1967	6.9	7.3	7.8	22.0
1968	6.6	5.9	6.6	18.1
1969	6.9	5.8	6.7	19.4
1970	7.7	5.4	7.2	20.3

NASA funding of the physics of fluids in millions of dollars:

1965	7.8
1966	7.6
1967	8.3
1968	4.0
1969	5.0
1970	about the same

NOAA funding of the physics of fluids in millions of dollars:

1969	1.8 Geophysical Lab
	0.2 Hurricane modeling
1970	1.88 Geophysical Lab
	0.22 Hurricane modeling

AEC funding of the physics of fluids in millions of dollars:

3.5 to 4, depending on the fraction included of reactor coolant work. The total funding was essentially constant for the years 1968–1970.

The total funding of the physics of fluids by the federal government then amounts to approximately $37 million a year. The amount spent by industry is estimated to be a major fraction of the total of $10 million spent on plasma physics and the physics of fluids combined.

It would appear, therefore, that the funding in physics of fluids has remained nearly constant during the last 3 to 4 years and that necessary contraction of 7 percent each year due to inflation has been made with some serious dislocation of the field. Considering the relevance of the field to so many aspects of our life and the continuing unknown and even surprising behavior of turbulent fluids, this continuing reduction in the effort seems questionable indeed.

8.3   EFFECTS OF ALTERNATE FUNDING

The consideration of the effects of alternate funding policies on plasma physics and the physics of fluids is dominated primarily by one criterion, namely, is the total manpower increasing or decreasing? An increasing number of scientists means that

1. Young scientists with new ideas are entering the field.
2. Young scientists are being educated. The art of education stimulates creativity in both the educator as well as the educated.

The pertinent time constants are

1. An inflation of 5 percent per year means the net effort decays to one half in 14 years.
2. The useful life or currency of a PhD education *without re-education* is roughly 7 to 10 years.
3. Perhaps 20 to 50 percent of scientists do not re-educate themselves, a phenomenon that reduces the effectiveness of any research organization that does not have the strictest personnel policy—and none but private corporations do.
4. The result of 2 and 3 is that the decay time of creativity with a static manpower is roughly 15 years. Any government laboratory with a static manpower ceiling is a case in point. To remain abreast of inflation and maintain a constant creativity requires a budgetary increase of roughly 10 percent per year.

A decay of creative effort should not be allowed to occur under any circumstances in these two specific areas of fluids and plasmas:

1. Turbulence research in the physics of fluids and
2. Collisionless plasma research.

These two fields of classical physics are not yet well understood. The first has a wide range of application to practical problems, and the second a specific major application, i.e., controlled fusion. Each is of fundamental importance to understanding presently unknown laws of nature. Each has wide applicability to the understanding of stars, space, galaxies, and our universe.

If the funding in these two subjects falls below a rate of increase of 10 percent per year, then we are significantly less likely to master that understanding and benefit from the results. In both cases, the integral cost over the pertinent decay time of 15 years is exceedingly small compared to the potential gain. Fusion budget = 30×10^6/year $\times$ 10 years $\times$ 2 = 6×10^8.

Investment in fusion power installations providing 50 percent of installed power 30 years from now = 10^{11}.

The Panel, therefore, strongly urges that an adequate rate of growth be programmed for the next 10 years in these two fields. We do not recommend such a large growth rate in any other branch of plasmas or fluids and as a result recognize a gradual reduction of capability. Naturally the observation of new phenomena not explainable with current laws of physics could easily alter this view, but without such a confrontation of theory and observation, we would expect the two major problems cited to dominate the physics of fluids and plasmas for the immediate future.

In the event that funding is decreased, either by 0 percent or by 7.5 percent, the effect is the relatively rapid decline of both fields. The young scientist sees a declining field of endeavor and knows that the expected intellectual decay will take place. The creative scientist avoids the field. The rate of decay is of course determined by the dollars. At 0 percent, the intellectual aging is about equal to the inflationary decrease, and some significant effort can still be made to save the best. At 7.5 percent annual decrease, the most able staff will usually try to leave first, because they are mobile; consequently, the intellectual decay is accelerated. A 7.5 percent annual decrease, therefore, means a rather rapid termination of the project—usually 5 to 8 years.

With these thoughts in mind, we consider the effects and alternatives of a decreasing budget—first to fusion research and then to plasmas and to fluids.

1. Fusion research in universities (by which we have in mind primarily AEC funding of the applied goal of fusion power) could no longer be justified on the basis of training new fusion plasma physicists, because virtually no jobs would be available.

2. Because fusion research absolutely must have a few large facilities to measure and understand progress toward fusion power, the several major experimental facilities that test the principal confinement geometries must be preserved as the absolute minimum condition for a national program.

3. A further reduction (to less than approximately 50 percent of the current program) would mean that the United States should consider entering into a cooperative agreement with a foreign government, with each government concentrating on one or several different approaches.

4. In a time of decreasing funds the preservation and possibly limited extension of fundamental knowledge should be of primary concern. These objectives are met only in the dual functions of basic research and teaching.

NSF funds in physics should be reserved entirely for basic research, with small application of a criterion of relevance. If the fraction of fundamental plasma physics supported should change drastically when in competition with other branches of physics, then a review mechanism must be formulated that allows for reinforcement within each of the subfields of physics.

The effect of a decreasing budget in fluids is significantly different from the effect in plasma research, because, in general, there are no large facilities. Wind tunnels for aerodynamic research on flight vehicles are supported almost entirely by private industry or by defense-oriented work. No

other large facilities dominate the field, so that no alternatives in respect to a major shift in experimental funding are open. As a consequence, the administration of a decreasing budget should be directed primarily toward maintaining a creative effort in the forefront of the field. The forefront should be redetermined continuously, as in all of science, by the mechanism of proposal competition among peers. This means that the funding level of NSF (engineering, mathematics, and physics) should be maintained above or advanced ahead of the more applied projects of other agencies. In other words, the funding of applied projects might decrease half again as fast as would be the case with a uniform reduction, so that the NSF funding could increase at least as fast as required to offset inflation.

The stimulation of the creative understanding of new laws of nature by combined research and education is the first priority of science funding.

9 Recommendations to the Physics Survey Committee

9.1 PHYSICS OF FLUIDS

The overwhelming relevance of fluids to our environment has so far maintained the level of funding in the physics of fluids without the drastic decline of the other fields. As a consequence, the number of dropout proposals submitted to the National Science Foundation is small. On the other hand, the near constant funding for the last 4 years implies a fractional decrease of 20 to 25 percent in the net effort expended.

In view of the fact that the foremost phenomenological aspect of fluids, namely turbulence, is not yet subject to a quantitative analytical description, such a reduction of effort seems indeed shortsighted. Compounding this gradual inflationary reduction of effort is the current political expediency of "relevance." Application of this criterion to proposals dealing with a basic understanding of turbulence does a great disservice to all concerned. First and foremost, it discourages work on the most important problem of fluids. Second, it encourages the people who could be solving the problem to waste critical analytical effort on problems of current expediency—thus creating a legacy of ignorance for the future.

We, therefore, recommend the following to the Physics Survey Committee:

1. Funding of the physics of fluids and fluid dynamics of basic problems at a level consistent with a gradual growth of the whole field. Start-

ing with the last satisfactory year, 1967, the growth should be at about 6–7 percent per year.

2. That basic research proposals in physics of fluids be supported with the *a priori* recognition that the criterion of relevance is inherent to the subject. Although we may object to the excessive application of the criterion of relevance to research, the relevance of a major fraction of fluid research can be taken for granted.

9.2   PLASMAS

1. That plasma physics be supported at a level that:

(a) Ensures the funding of new research proposals and at the same time a reasonable continuity of the past programs. This means that NSF funding would have to roughly double to partially replace the loss from other agencies.

(b) Fusion research be funded such that the total scientific staff grows between 7 to 10 percent per year, at the least.

(c) Magnetohydrodynamics power generation be supported substantially (roughly $4 million per year) and look to a similar growth rate as fusion.

2. That the federal government provide for centralized evaluation of all power proposals, nuclear and nonnuclear, so that an open comparison of goals and possible achievements can be made.

3. That all federal agencies be encouraged to use a previously agreed upon small fraction of their budget for basic research. The method of awarding grants should be uniformly and federally specified to be through open competition—and above all, through review by one's peers. The pattern of the NSF should be extended to other agencies in order to encourage agency awareness of the newest ideas and thinking within rather broad outlines of that agency's mission. For this reason the judgment of the grant acceptance should be only partially determined within that agency and should include the judgment of appropriate peers. The operation of such a program should be directly responsible to the top management of that agency so that the very act of consideration of new grant funding encourages an intimate acquaintance with the newest applicable ideas.

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APPENDIX:

PHYSICS SURVEY—

A CHARGE TO THE

SURVEY·PANELS

The following are topics on which the Survey Committee requests input information from the Panels:

THE NATURE OF THE FIELD

It is vitally important that we communicate to our audiences some coherent presentation of what we believe physics is all about. Please help us in this by considering how best to present your field to (a) other physicists, (b) other scientists, (c) nonscientists. Particularly in the latter case it will be helpful to provide the Committee with what the Panel may well consider an oversimplified and overpopularized view—previous panels have erred in the opposite sense. Examples, illustrations, case history—and indeed some historical perspective generally—will be most helpful.

THE STATUS OF THE FIELD

(a) What have been the major developments (both in theory and experiment) during the past five years? If possible, put these into context with reference to the status statements in the Pake Survey and Panel reports.

(b) What are the implications of these developments for the growth of the field during the next five years?

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(c) What are specific examples of major changes or advances that these new developments afford? Can we do things now that were simply impossible before? Are there examples that could provide striking graphic treatment in our report?

(d) What are the present frontier areas of the field? How are these defined?

(e) Is the balance between experimental and theoretical activity in the field at a desirable level? If not, what are the Panel recommendations concerning an optimum balance and how might it be achieved?

INSTITUTIONS OF THE FIELD

(a) How is activity in this field now divided among the various types of research institutions, i.e., academic, national laboratory (e.g., Brookhaven), government laboratory (e.g., NRL), industrial laboratory, etc.?

(b) What recommendations does the Panel have concerning this balance and its possible modification in the next five years? The next decade?

(c) In this field, what are the characteristic features of activity in the different institutions?

(d) What are the interactions between these institutions? Are there areas where this interaction could or should be improved? What are the effective barriers, if any, that may prevent ready communication between, or direct exchange of, personnel for example?

INTERACTION WITH OTHER AREAS OF PHYSICS

(a) Illustrating with specific examples wherever possible, what have been the outstanding examples of interaction between this and other fields of physics recognizing that this is almost always a two-way interaction?

(b) What are specific examples of techniques—either experimental or theoretical—that cut across field boundaries? Detailed studies of selected examples would be particularly useful.

INTERACTION WITH OTHER AREAS OF SCIENCE

Questions identical to those above seem appropriate again with stress on the desirability of specific examples and possible illustrative material. The most important interactions will, of course, vary with the field; areas such as chemistry, medicine, biological sciences, ecology are obvious candidates for consideration.

INTERACTION WITH TECHNOLOGY

Research and technology have long advanced through mutual stimulation. In this field, what are the outstanding examples of such interaction in recent years? Case studies are particularly useful here. Purely as an example that has been suggested, it might be useful to consider an essay covering a tour through a modern hospital, a chemical processing plant, a paper mill, or the like, noting in passing those techniques and instruments that have arisen from work in the field. Cooperative efforts with other Panels would seem profitable. The inverse should not be neglected; some emphasis on the great dependence of research progress on technological progress is clearly indicated.

The Data Panel will attempt to arrive at methods of quantifying some of the available information in the area—both within and outside of this country. Close collaboration with the Data Panel in identifying areas of particular importance and interest would be most helpful.

INTERACTION WITH INDUSTRY

(a) Illustrating, wherever possible, with *specific* examples, what have been the outstanding interactions between this field of physics and the industrial sector in the past five years?

(b) Can any of the recent developments in the field be extrapolated, at this time, as having such interaction in the near, or distant, future?

(c) What is the inverse situation? What impact have techniques, products, or people in the industrial sector had on this field?

(d) How can the interaction between this field and the industrial sector be made more effective?

(e) It has been suggested that the development of biotechnology represents the conversion of the last of the guilds into an industry. What contributions has this field made to this conversion?

(f) Succinct case studies would be very valuable here.

INTERACTION WITH SOCIETY

(a) In what areas is the field already having major impact on questions of direct social importance?

(b) What other areas are candidates for such interaction?

(c) What aspects of training in this field are of particular importance for utilization in problems of broader social implication—which of these latter in particular?

(d) What would be the Panel recommendations concerning broader

utilization of present personnel and facilities on such problems? Examples of possible situations would be most helpful.

(e) A few groups have already decided to devote some selected fraction of their effort to such activities. A discussion of such approaches would be helpful.

(f) One of the major questions facing physics (and science generally) is that of educating the nonscientific public to its very real relevance— however defined—in a technological civilization. The Survey Committee would welcome suggestions, case histories, examples, and any material that would assist in its consideration of this question for physics generally, as well as more specifically within the context of the Panel's subfield.

RELATIONSHIP TO OTHER AREAS OF SOCIAL CONCERN

Traditionally, physics has been recognized as being relevant to national defense, atomic energy, space, etc., and has enjoyed support from the corresponding federal agencies. Today our society is moving its center of concern to areas for which, at first sight, physics is less relevant: health, pollution, racial tension, etc. The new federal agencies organized to deal with such questions, such as NIH, HUD, DOT, accept much less, or no, responsibility for physics. How strong a case can be made for the relevance of your subfield to the achievement of the missions of these other agencies? In general, this will come through the help your field can give to technologies that will further these social ends: for example, the role of computers (and therefore solid-state physics) in automating hospital care. However, there may be other more direct inputs to your field that do not go through technology.

CULTURAL ASPECTS OF PHYSICS

Knowledge of the physical universe has more than utilitarian value. Each advance in fundamental understanding becomes an indestructible asset of all educated men. It is not suggested that each Panel should provide an essay on the contributions of its field to human culture, but it would be helpful in developing a broad exposition of this aspect of physics to have suggestions or compelling examples related to your field. A rather obvious concrete example: we know how old the earth is; that knowledge came through physics. Examples less obvious, and especially examples of important questions that may be answered in the foreseeable future, would be welcome.

We would welcome assistance from the Panel in answering such questions as (a) How best do we bring out the cultural relevance of physics?

(b) To what extent should our report develop the cultural arguments as a basic justification for continuing support of physics? (c) How can we best address ourselves to the resurgence of mysticism and of anti-intellectual and antiscience attitudes among students? Among the citizenry generally? (d) What is the role of physics in countering these developments?

Relationship to National Security Activities

(a) What role has the field played in national defense activities?

(b) What future role is envisaged? How important is the field to these activities? Disarmament activities should be carefully considered in this context.

(c) What have been the respective roles of the different institutions of physics in this area?

(d) Again the Committee would welcome the assistance of the Panel in addressing the general questions relating to the overall interaction of physics in national security activities.

Training in the Field

(a) It is often implied that contemporary graduate and postdoctoral training is becoming so narrow that students have lost the traditional breadth of outlook and flexibility expected of a physicist. Is this situation true in this field? What can be done to improve the situation? What recommendations does the Panel have for modification of contemporary training programs?

(b) In what ways is this field of particular importance for physics education?

(c) Although clearly the question relates to all of physics, can the Panel provide relevant input to the Committee concerning (i) the adequacy of current secondary school training in physics and mathematics; (ii) the effectiveness of some of the more modern secondary school curricula, e.g., pssc; (iii) the relative intellectual standing, at the secondary school level, of those students who choose to major in undergraduate physics? (There is a widespread element of folklore that suggests that physics no longer attracts the most intellectually gifted secondary students. Can this be supported or refuted? What is the significance of this statistic in whichever case emerges?)

(d) Again, although relating to all of physics rather than to this Panel specifically, the Committee would welcome input concerning such topics as (i) what has been accomplished in bridging the gap between physics and other disciplines at the undergraduate level? How successful have general

science or interdisciplinary courses been for entering—for advanced—students? How can we better illustrate the fundamental impact of physics as an underlying discipline in many areas of undergraduate education? (ii) Are teaching materials adequate? Do presently used textbooks adequately reflect the contemporary structure of physics? (iii) How important a demand for trained physicists will teaching requirements represent at established university centers—at newer campuses—at the colleges?

(e) To what extent has obsolescence of training overtaken members of the field? What can be done about it?

(f) What effective mid-career training opportunities now exist in the field? What are the Panel recommendations in this area?

(g) How effective are existing summer school programs in meeting the need for continuing training and education in the field?

(h) How effective are conferences and symposia in the field as training mechanisms?

(i) What are the Panel recommendations concerning the number and character of such conferences and symposia now available in the field?

POSTDOCTORAL TRAINING

(a) What is the role of the postdoctoral appointment in the field? This will, of course, be different in the different institutions.

(b) What is the average duration of the postdoctoral appointment? How has this changed with time?

(c) What has been the distribution, by nationality, of postdoctoral people in the field, and what fraction of these have remained in the United States following their postdoctoral training? How has this changed with time?

(d) How has the leveling of funding affected the availability of post-doctoral appointments in the different institutions (e.g., industrial laboratories, national laboratories, government laboratories, universities)?

TRAINING IN APPLIED AREAS OF THE FIELD

(a) What are the applied areas that draw most heavily on this field?

(b) Does the supply of physicists in this field suffice to meet the demand in these areas?

(c) Is the current training adequate? Would modification of current training patterns be expected to open up significant new employment opportunities?

(d) It might be argued that there has been a significant failure in communication between prospective applied physics employers and the aca-

demic groups involved in the applied training. Is this true in this field? If so, how can it be improved?

(e) How is the applied work distributed with regard to the type of institutions involved?

MANPOWER PROJECTIONS

(a) What is the current population in the field, and how has this population developed since 1965 (as covered in the Pake reports) in (i) academic research, (ii) industrial research, (iii) government laboratory research, (iv) postdoctoral training, (v) graduate student training, (vi) other?

(b) During the same period what migration has occurred into—and out of—the field? What have been the major sources and recipients of this migration?

(c) In the light of current challenges in the field and/or new or anticipated facilities, what projected manpower needs can be expected in each of the above area in the next five years—the next ten (recognizing that this latter is an extreme extrapolation at best and closely related to available funding)?

(d) The argument is often advanced that the shortage of jobs requires additional funding in the field. This is more frequently reversed in Washington to imply simply that there are too many physicists being trained. What is the situation in this field?

(e) To what extent is the claim of inadequate employment opportunities legitimate (i.e., to what extent does this simply reflect the fact that for perhaps the first time physicists are not able to obtain the job that they would find most attractive)? What fraction of current PhD graduates were unsuccessful in finding employment where they were in a position to utilize their broad physics training if not their immediate specialty training?

(f) Will adequate manpower be available to staff emerging institutions in the field? How can qualified staff be attracted to and retained by such institutions?

(g) Does this field have unique or special characteristics that recommend it for consideration by an emerging institution?

(h) With leveling funding it may well be impossible for new (and indeed old) institutions to span as broad a spectrum of fields of physics as has been traditional, and while regrettable from a training viewpoint further specialization may be required in any given institution. How feasible are joint activities in this field as compared to others in physics? What recommendations would the Panel have in this difficult area?

FACILITIES

(a) Existing Facilities

(i) What are the major facilities in the field, and how are they distributed as to type?

(ii) Are the existing facilities now being utilized to full capacity? If not, explain.

(iii) How are present facilities being utilized, i.e., are they shared by more than a single group, how are decisions made regarding the research scheduling?

(iv) What are the outstanding problems now faced in the use of existing facilities?

(v) Is the distribution of existing facilities adequate?

(vi) Is modernization of the existing facilities feasible? What is the estimated effective lifetime of typical existing facilities in the field?

(vii) What criteria should be applied in reaching decisions to close down existing facilities?

(viii) To what extent do such criteria differ in different institutions (e.g., a facility might have training potential in an academic environment when it has reached a stage of unacceptable obsolescence elsewhere)? Is relocation of facilities a viable suggestion under such conditions? There are clearly pitfalls of which the receiving institution should be aware. What are they in this field?

(b) New Facilities

(i) What new facilities will be required to exploit the potential of the field? What is the priority ordering of these facilities? Please support with detailed discussion.

(ii) To what extent could existing facilities now used by other areas of physics be adapted for frontier use in this field?

(iii) What are the Panel recommendations regarding siting and operation of new facilities?

(iv) Within this field, what is an optimum balance between large centralized facilities and smaller more widely distributed ones? Please discuss.

(v) What new developments now on the horizon show promise of evolution as major facilities in the field? Is an estimate of the probable gestation period and possible cost now possible for each?

THE IMPACT OF COMPUTER TECHNIQUES ON THE FIELD

(a) What have been the outstanding impacts of computer technology in this field?

(b) Would larger and/or faster computers be of significant value? What would be the relative priority assigned to the higher costs that would be involved here as compared to other major capital needs of the field?

(c) Has any particular scheme of utilization, i.e., small local computers, institutional computer centers, regional computer centers emerged as preferable in this field?

(d) Do existing software and languages pose significant limitations in the field?

(e) What estimate does the Panel have for the present and projected utilization of computers in the field? Can a dollar level be attached to this?

(f) What impact has the field had on computer technology?

(g) Are there outstanding examples of studies that would simply have been impossible without sophisticated computer utilization? Specific examples would be most useful.

COST INCREASES

(a) Selecting, say, ten instruments much used in the field spanning the cost range involved—how have the individual costs varied with time in the last decade?

(b) How has the average (very crudely defined) overall cost of an experiment, typical of those at the frontier of the field at the time, varied with time in the last decade?

(c) How have average postdoctoral and student training costs varied over the same interval? It would be advantageous to consider experimental and theoretical situations separately in this instance.

(d) Illustrating with specific examples, what would be a reasonable annual estimate of the cost escalation in the field reflecting increasing sophistication of the studies themselves? Reflecting aging of the institutional staff?

(e) To what extent is progress in the field *really* dependent upon the availability of the most modern instrumentation? It has been suggested that in some fields the instrumentation has become over-sophisticated, over-flossy and that in at least some instances the Ferrari could be replaced by a Ford without undue restriction of the research quality and productivity. To what extent is this suggestion true in this field? To what extent can (and should) it be countered? Specific illustrations and examples would be extremely helpful here.

FUNDING LEVELS

(a) What have been the actual funding levels *and* expenditure levels annually in the field since 1965? Compare these with the Pake Report

projections. Insofar as possible, separate academic, industrial, and governmental laboratory operations for consideration. In some instances the leveling off of federal funding has been counteracted, for a time at least, by infusions of institutional funds, so that actual expenditure levels have not tracked funding limitations. What information is available on such phenomena in this field?

FUNDING MECHANISMS

(a) How has the available funding been distributed among these sources: federal (AEC, DOD, NSF, NASA, others), state, industrial, local (university contributions, etc.), foundations, and other sources?

(b) How does the funding process actually work for each of the above sources? What are the relative distributions, advantages, disadvantages, etc., of grants and of contracts? What are the effective differences between these two approaches? What improvements might be suggested?

(c) What is the relative importance of project and of institutional grants in this field?

(d) How are decisions reached concerning grant and contract applications? Please comment on the decision-making processes at the national level—for example, by administrators in the various federal agencies and by advisory committees to these agencies. Is the present practice satisfactory or would change be desirable? What are the Panel recommendations?

THE IMPACT OF LEVELING FUNDING

(a) Discuss in some detail, with specific illustrations, the overall impact of leveling funding on the field. The following subtopics might prove useful:

(i) Utilization of current facilities

(ii) Exploitation of new discoveries

(iii) Employment of physicists

(iv) Support of the young researcher

(v) Alienation of young physicists

(vi) Possible new approaches to training in the field

(vii) The support of offbeat proposals. There is always a tendency, under limited funding conditions, to eschew risk or adventure, to bet on the sure thing.

(viii) Long-range implications for the field generally.

(b) It is clear that level funding is not synonymous with level productivity. The Committee will welcome case histories, etc., to illustrate this general point.

(c) What are the relative advantages of expanding (or contracting) activities in this field by expanding (or contracting) the size of existing groups

active in the field as opposed to proliferating (or reducing) the number of such groups?

FUNDING PROJECTIONS

In the past, survey reports have generally made specific projections and recommendations which have very often been negated by large departures of the total budgets available from those on which the recommendations were based. To be responsive, our Report must provide for a spectrum of possible situations; in doing so it must carefully spell out, in as detailed fashion as possible, both the short- and long-range consequences of funding at levels below those necessary for both orderly growth and exploitation of new developments in each of the fields of physics. Specific examples and case histories will be particularly effective in illustrating these consequences. With these points in mind,

(a) What level of funding, quite apart from any current estimate of future funding, would be required to enable this field to realize its full potential during the next five years? The next ten years? How would it be distributed broadly over the subareas of the field—recognizing that detailed projections are, in many cases, impossible?

(b) Consider a spectrum of possibilities ranging downward in 10% increments from that developed above to a level some 10% below that currently in effect. At each step indicate as clearly as possible,

(i) What opportunities would be missed—what developments would not be exploited?

(ii) What new facilities would necessarily be postponed or eliminated entirely from consideration?

(iii) What programs or facilities would necessarily be phased out or closed down?

(iv) What would the impact be on the manpower and employment situation?

(c) A detailed discussion of the basic issues that underlie the Panel's assignment of priorities within the field would be an essential component of the Panel report. It is essential that long-range implications be developed realistically; it is essential that we not predict greater catastrophic impact than can be clearly justified.

(d) Separate discussion of major new facilities—in order of priority— with careful discussion of the bases for the priority ordering and of the relative justifications will be particularly important.

(e) The question of laboratories, as distinct from facilities, will be appropriate in some fields. The need for and justification of such laboratories will require careful consideration. What are the recommended

criteria for closing down an existing laboratory in this field? To what extent are the laboratories in the field adaptable to broader use and to alternate modes of support during periods of fiscal stringency?

(f) To what extent can the Panel assist in developing a balanced presentation of the overall impact on the continuity of physics (i.e., the faucet effect—it is not generally appreciated that the re-emergence of funding after an indeterminate drought will not guarantee re-emergence of a healthy physics—or science—community)? Can this be quantified in this field? Are there relevant examples or case histories?

(g) A clear statement of the basic fiscal assumptions underlying the Panel projections is essential. The Data Panel will provide basic information concerning inflation rates, etc., which should be used systematically by all Panels to permit later direct comparisons by the Committee.

PHYSICS DATA IN THE FIELD

(a) How effective is communication of scientific information in the field generally? Are there adequate review articles—conferences and conference proceedings? Are there too many of the latter?

(b) What is the role of the preprint in this field? Is the present system effective?

(c) How adequate are the present data compilation and dissemination mechanisms in this field?

(d) What are the Panel recommendations in this area? Are new approaches or mechanisms required? How can manpower, adequate both in quantity and quality, be integrated into the data compilation activities?

(e) What is the estimated cost involved?

(f) Quite apart from data communication and compilation within the field, (i) how effective is communication with related fields that may have need of your data, and (ii) how effective are your data formats and presentations for their use?

INTERNATIONAL ASPECTS

(a) Where does this field in the United States at the present time stand with respect to the same field abroad?

(b) How does U.S. activity in the field compare on a manpower or funding basis with that in the most active foreign countries? What are the relative growth rates? What are the major points of similarity or dissimilarity in the overall programs? What has been the significance of the different funding techniques and levels?

(c) What international cooperation now exists? What would be the

direct and indirect benefits to the United States in expanding such coopera-
tion in this field? Are there particular facilities that should be considered
in this light?

(d) What problems now exist with regard to the implementation of
foreign cooperation and exchanges? Have problems been encountered in
the obtaining of requisite visas—of permission to travel freely across inter-
national boundaries—of access to national or governmental laboratories
in this country or abroad?

(e) What is the situation vis-à-vis international cooperation in physics
in the industrial sector? Are there outstanding difficulties in this area?
How important is fostering of such cooperation in this field?

(f) To what extent does this field encompass well-defined national
schools of thought (e.g., the Copenhagen School in quantum mechanics
and nuclear physics)?

(g) What has been the impact of foreign work and foreign research
centers on activity in this field in this country?

(h) How do developing countries attain critical mass in this field? Are
there specific mechanisms in this area? Should there be?

(i) What international laboratories *should* be developed in this field?
Upon what criteria should the establishment of such laboratories be based?

ILLUSTRATIVE MATERIAL FOR THE SURVEY REPORT

It will be particularly important that the Committee receive from each Panel
a selection of illustrations and photographs carefully selected to highlight
progress or particularly interesting vignettes in each field. It would be help-
ful if the Panels would address themselves to this request at an early stage
of their deliberations. The members of the Data Panel will devote con-
siderable effort to the development of new techniques for the presentation of
statistical data and will cooperate closely with each of the subfield Panels.