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High Energy Physics Advisory Panel

Report of the 1980 Subpanel on Review and Planning for the U.S. High Energy Physics Program

July 1980

U.S. Department of Energy
Office of Energy Research
Division of High Energy Physics

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July 15, 1980

Dr. Edward A. Frieman
Director of Energy Research
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Washington, D. C. 20585

Dear Ed:

I am forwarding to you the report of the 1980 HEPAP Subpanel which was formed in response to the Charge to develop a general strategy and long range plan for the U.S. High Energy Physics program over the next decade. In particular, the Subpanel was asked to assess the program balance between research, equipment, and construction over the period from FY 1982 to FY 1987 and to make specific recommendations for FY 1982. Two funding constraints were given as guidance to the Subpanel: the DOE/OMB Long Range Plan of 1978 - i.e., $325M in FY 1979 dollars for DOE plus NSF; and a funding level that is 10 to 15 percent higher. In addition, the Subpanel was asked to identify the physics opportunities that would have to be deferred, or that would be lost, under those constraints.

In addressing this broad and difficult Charge, the Subpanel worked both hard and effectively in the time available to it, and I greatly appreciate their efforts. HEPAP discussed the Subpanel report extensively at its June 30-July 1, 1980 meeting at DOE Headquarters in Germantown, Maryland. We endorse its general conclusions and recommendations as described below. In this transmittal letter I will convey HEPAP's own set of priorities on the main recommendations of the report, where they have not been expressed by the Subpanel. I also wish to express HEPAP's serious concerns about the future of the national High Energy Physics program in view of the gap between the current funding levels and the guidance given to the Subpanel.

High Energy Physics is in the midst of very exciting developments, both experimental and theoretical, that radically altered our views of the basic forces and elementary constituents of nature during the 1970's. Many of the key discoveries during the past decade were made in the United States. We are now entering the 1980's with a powerful and beautiful new conceptual framework being constructed which unifies three seemingly very different forces with one another: the electromagnetic forces, the weak forces of radioactivity, and the strong nuclear forces. At the same time many basic questions about the nature of these forces and the elementary constituents which they affect still remain unanswered. We see the possibility of confirming or denying our emerging picture of nature on the sub-nuclear frontier with decisive experiments using the new detectors and accelerators presently starting initial operation as well as the facilities now under construction. Other facilities currently being designed and analyzed would permit further crucial tests of the validity of this emerging picture. HEPAP recognizes that it is essential to formulate long range as well as short range plans in a field of such fundamental importance as High Energy Physics that requires large and expensive facilities as well as talented scientists in order to achieve its progress.

The national High Energy Physics program of the United States is heavily committed at this time to two major construction projects based on superconducting magnet technology at Fermilab and at Brookhaven. These are bold and exciting ventures that HEPAP endorsed in its earlier facilities reports. We again emphasize our strong support for these projects although they are turning out to be both technically more arduous and more expensive than previously anticipated. Though great strides have been made with the technology of superconducting magnets, some hurdles in this respect remain before both construction projects can be completed. These hurdles are described in the Subpanel report which notes that Fermilab, with its earlier commitment to substantial R&D, is further along toward its goal. When they are operating as proposed, the Saver/Tevatron I/Tevatron II at Fermilab and ISABELLE at Brookhaven will permit major advances in the research frontiers of elementary particle physics. We believe that this complex of new construction projects should proceed at this time with highest priority. The necessary R&D funds to assure their success must be provided during the coming year. At the same time we recognize that there is a great need to provide for high utilization of the forefront accelerator facilities in the ongoing research program--i.e., the Fermilab 400 GeV accelerator, the newly-commissioned PEP storage ring, the CESR facility at Cornell. As part of this recommendation, increased support is required for user groups to exploit these facilities for physics. The third component of a balanced research program--the detector and accelerator R&D that are essential for further progress--also requires significant support as emphasized by the Subpanel report. The technologies of very high charge-density bunches with low emittance in linear accelerators, very high-field superconducting magnets, and superconducting rf cavities were singled out by the Subpanel as being of great importance.

For the present we can meet the above goals of a healthy and balanced research program at the funding level of the 1978 DOE/OMB guidance. However, such a funding constraint still means the loss of very good science with existing facilities supported significantly below their potentials. In particular, at this level it is necessary to operate the currently interesting, important, and productive lower-energy fixed target programs at the Brookhaven AGS and SLAC linac with lower priority, thereby providing fewer opportunities for physics for considerable numbers of University and Laboratory-based user groups. This level of funding will also require deferral of other new construction initiatives. Specifically, the SLC electron-positron linear collider project has been proposed by SLAC, and other e^+e^- and ep colliders have been discussed. We would anticipate from projects such as these a very rich output of physics as well as advances in accelerator technology.

It is important to recognize that with only a modest (15 percent) increase in funding above the DOE/OMB guidance we could achieve our major goals of: timely progress toward completing current construction projects; major improvements in the level and efficiency of facilities operations, and correspondingly of valuable physics output from the ongoing programs; a strong program of advanced accelerator R&D. Simultaneously, an early new construction initiative such as described above, could be accommodated. This program would indeed ensure a broad-based, preeminent U.S. national program, both exciting in its own right and complementary in scope with Western Europe's very strong and advancing program. A U.S. program of such

vigor remains our immediate and long range goal. As your Advisory Panel, charged with the responsibility of helping define a strategy and long range plan for a preeminent U.S. national High Energy Physics program, we can recommend no less.

The painful reality we must recognize and confront, however, is this: the High Energy Physics operating budgets for FY 1980 and, as currently envisaged for FY 1981, are well below the long range DOE/OMB plan of 1978. The best current estimate is that by the end of FY 1981 the accumulated shortfall since the initial implementation of this plan in FY 1979 will be about $45M. Budget stringency during this period has severely limited the R&D program underpinning the ongoing construction, and has also reduced the level of utilization of existing facilities to the point of causing a serious loss of physics as well as great inefficiencies and difficulties for the research groups. This loss will continue unless the funding is restored at least to the level of the DOE/OMB 1978 plan. Continuation of the current funding restrictions may force substantial reduction in the scope of the U.S. High Energy Physics program. If this occurs, the U.S. program will inevitably lose its eminence, due both to inadequate opportunities for U.S. scientists as well as to our inability to compete with Western Europe. We then will face the prospect of the U.S. High Energy Physics program being less and less competitive to share in making the major advances and discoveries on the sub-nuclear frontiers.

We hope that this will not be the fate of a U.S. program that has so distinguished a record of major accomplishments in the study of nature's most basic processes and constituents; a field that has flourished here for close to 50 years since its birth at the Lawrence Berkeley Laboratory under the initial leadership of E. O. Lawrence; and a field that has contributed so importantly to advancing the technology base of the U.S. In particular, pioneering advances in superconducting magnet and cryogenic technology, spurred by the Fermilab Energy Saver/Tevatron project and the ISABELLE project at Brookhaven, have important practical applications. This technology, which is so vital for further advances in the high energy frontiers, is also required for progress in the development of fusion energy and magneto-hydrodynamics, and will contribute to new techniques for energy conservation.

We expect that another review of the general strategy and plans for the U.S. High Energy Physics program may very well be desired a year from now. This summer proved to be an especially difficult time to formulate long range plans; hence, it was almost inevitable for the Subpanel report to focus heavily on problems, moods, and opportunities of the ongoing program. A year from now, however, there may be a considerable clarification of uncertainties that weighed so heavily on the deliberations of this year's Subpanel. For one thing, the current funding projections were very uncertain. Further clarification of the financial plans for High Energy Physics can be hoped for following completion of Congressional action on the FY 1981 budget and after discussion of the long range program plans between the Congress and DOE/OMB subsequent to study of the impending report of the General Accounting Office. In addition and most importantly, a year from now we may expect to have higher confidence in clearing the technical hurdles in our

current construction programs based on superconducting magnet technology, as well as more definite information on Western Europe's plans for their High Energy Physics program. It may then be timely to accomplish an updated review of our plans and strategy for ensuring continued U.S. leadership in the quest for an understanding of nature's fundamental laws.

Sincerely yours,

Sid

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Chairman, HEPAP

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June 24, 1980

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Dear Sid,

Enclosed is the Report of the 1980 Subpanel on Review and Planning for the U.S. High Energy Physics program, which met at the National Academy of Sciences Study Center, Woods Hole, Massachusetts, from June 1-7, 1980.

Sincerely,

Sam Treiman

Sam Treiman

SUMMARY

High energy physics addresses the fundamental structure of matter, a central theme of science. The field is in a state of high accomplishment and promise. Striking experimental, technological, and theoretical developments over the past decade have combined to transform our understanding of the basic constituents of matter and of the forces that govern their interactions. The picture of protons and neutrons as composites built up out of more basic entities, the quarks, has been strengthened and elaborated. The strong forces, responsible among other things for binding of nuclei, and the weak forces, responsible for radioactivity, have come to be understood at a deeper level corresponding to the understanding of electromagnetism, pioneered by Maxwell in the 19th century, and of gravitation, pioneered by Einstein in the early years of this century. The weak and electromagnetic forces, and perhaps also the strong ones, now all appear to be unified in a common theoretical framework of great breadth and elegance. These theoretical advances have enriched the traditional connections between particle physics and other disciplines--nuclear physics, condensed matter physics, pure mathematics, the physics of the big bang universe, etc. The technological advances have similarly spread to other areas of science and technology. The cultural and technological attraction of high energy physics is reflected in the growing level of research activity and investment abroad, particularly in Western Europe and the USSR but also increasingly in Japan and the Peoples Republic of China.

The accomplishments in high energy physics spring from a variety of research approaches and experimental tools. At the heart of the effort are the high energy accelerators--the descendants of Ernest Lawrence's cyclotron. In the U.S. these are now centered at Brookhaven National Laboratory (BNL), the Stanford Linear Accelerator Center (SLAC), Fermi National Accelerator Laboratory (Fermilab), and the Newman Laboratory of Nuclear Studies at Cornell University. They serve the research needs not only of the high energy physicists at these centers but also of the wider community at the Universities. The current array of proton and electron accelerators in the U.S. constitutes a powerful and diversified set of research tools; and major new proton and antiproton accelerator projects underway at Fermilab and Brookhaven will extend our reach to higher domains of energy and surely to new discoveries.

The U.S. has long occupied the position of world leader in high energy physics, thanks to our national tradition of technological ingenuity, diversity, and generous public support. However, that leadership is now coming to be shared with Western Europe; and amidst all its high scientific achievements and promise, the U.S. program finds itself confronted with a number of present difficulties and future challenges and choices.

(i) The level of financial support in Western Europe now substantially exceeds that in the U.S. and it is necessary for the U.S. to rely increasingly on special effort and ingenuity to keep our program at least well represented at the major forefronts. Recent scientific developments point to the great promise for the coming decade of very high energy electron-positron and electron-proton colliders. Ambitious and costly projects along these lines are presently under

serious discussion in Western Europe. While it no longer seems financially possible for the U.S. to proceed in parallel by the scaling up of conventional technology, there is scope for more financially modest projects based on inventive new technology. Several attractive possibilities are under discussion in the community and were reviewed by the Subpanel.

(ii) In the current program, financial stringencies are imposing increasingly severe limitations on the exploitation of existing facilities, hence limitations on the opportunties available to the talented community of high energy physicists at the national laboratories and universities to mount experiments and follow out important scientific leads.

(iii) The new Fermilab and Brookhaven projects, which are based on pioneering superconducting technology, are turning out to be more arduous than originally anticipated. This is a serious challenge in its own right. It also impacts on the resources available to other elements of the national program.

Conclusions

The Subpanel was charged, among other things, to address the future directions of the national program: (a) on the basis of a constant dollar support level corresponding to the current financial plan; (b) on the basis of a 10-15% increase in the level of support. Since the situation is complex our evaluations and recommendations have to be read with care, in the context of all the supporting commentary. The following is a rough and compact summary of the main points.

1. General

(a) Utilization of the forefront accelerator facilities in the ongoing research program should be intensified, to exploit for physics the investments already made, and university groups should be given increased support.

(b) The new superconducting projects, the Energy Saver/Tevatron at Fermilab and ISABELLE at Brookhaven, must proceed with all deliberate speed. It is the responsibility of the entire high energy physics community to help in surmounting the difficulties now being experienced.

(c) Increasing support should be devoted to detector and accelerator R&D.

2A. Constant Level of Support

(d) The financial stringencies imposed by the present level of support will have to be accommodated by carefully planned reductions in the programs at the lower energy facilities.

(e) New construction initiatives will have to be foregone, at least for the nearer future until the time scales associated with the Fermilab and Brookhaven superconducting projects can be better assessed. Nevertheless, intensive R&D looking to the farther future should begin now.

2B. Increased Support Level

A modest (15%) increase in support, beginning in FY 1982, would make possible major improvements in the level of utilization and physics output for the ongoing programs. Moreover, it would provide the possibility of an early new construction initiative which addresses promising new areas of research. Specifically, the future U.S. program would be greatly strengthened by an electron-positron collider operating in the energy region between about 30 GeV and 100 GeV, where a rich output of physics is anticipated. Similarly, a facility designed to study high energy electron-proton collisions promises exciting physics opportunities. It may be that both the above goals can be met with a combined facility.

The Subpanel reviewed a number of attractive possibilities now under discussion in the community, in particular, the electron-positron linear collider SLC proposed for construction by SLAC. The Subpanel recommends that no action on this proposal be taken now but it recommends that it be considered, along with other emerging alternatives, by a similar panel convened within a year or at most two. At an increased level of support, we anticipate that the time would then be right for positive action on a new facility.

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Appendix A Charge to Subpanel

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SECTION I

INTRODUCTION

A High Energy Physics Advisory Panel (HEPAP) Subpanel was established in the spring of 1980 to address the Charge reproduced here in Appendix A. The Subpanel met at Woods Hole, Massachusetts, June 1-7, 1980. A list of the Subpanel members, consultants, and other participants at Woods Hole is provided in Appendix B. In preparation for its final deliberations the Subpanel carried out site visits to Fermi National Accelerator Laboratory (Fermilab), May 1-2; Stanford Linear Accelerator Center (SLAC), May 9-10; and Brookhaven National Laboratory (BNL), May 16-17. The Subpanel also heard presentations from Lawrence Berkeley Laboratory (LBL) at the SLAC site visit, and from Argonne National Laboratory (ANL) and the Newman Laboratory of Nuclear Studies (Cornell) at the BNL site visit. The input at the site visits was augmented by written responses to questions put in advance to the Laboratory Directorates concerning their current programs, future plans, and general opinions about the direction of the national high energy program. In addition, the Subpanel solicited the views of the entire high energy community, through a call for written statements from individuals and users groups and through wide-ranging discussions during the site visits. The responses were numerous and well thought-out and have figured into the deliberations of the Subpanel in an important way.

The 1980 HEPAP Subpanel is the fourth in a series of Subpanels formed from time to time to review the status of the national program in High Energy Physics and to address long-range plans. The earlier Subpanels (1974, 1975, 1977) were concerned chiefly with the issue of new research facilities required to advance the vitality of the program after a substantial hiatus in major construction projects. Several projects addressed by these Subpanels have reached completion or are now underway. In the meantime, numerous scientific and technological developments have opened up promising opportunities for further progress in the coming decade. It is timely, therefore, to review our present status with respect to facilities and to consider what new research tools are needed over the coming years for orderly exploitation of these opportunities. Beyond the matter of facilities, however, it seems especially appropriate just now to also address more general issues concerning the structure of the national program in High Energy Physics. This is so because an unusual number of problems, promising scientific prospects, and critical choices presently confront the U.S. high energy physics community.

The situation is surveyed briefly in the remaining paragraphs of this Section. More detailed discussion and specific recommendations appear in later Sections.

The accelerating pace of experimental discovery and theoretical development over the past decade have brought particle physics to a state of high promise and have influenced the pattern of facility construction addressed by the earlier Subpanels. The e^+e^- collider (CESR), which came into operation at Cornell in the fall of 1979, has turned out to be remarkably well-placed in energy to pursue the physics of the new "bottom" quark discovered somewhat

earlier at Fermilab. The e^+e^- collider PEP is just now coming into operation at SLAC in a higher energy region appropriate for the physics of quark and gluon jets and other fundamental issues. The Energy Saver/Tevatron facilities under construction or planned at Fermilab are designed to provide a window on ultra high energies (the $p\bar{p}$ collider at 2 Tev in the center of mass) and to pursue varied physical phenomena induced by the high energy secondary beams of the fixed target Tevatron program. The pp collider ISABELLE under construction at BNL is designed to combine high luminosity and high energy (800 GeV in the center of mass) for study of hadron collisions.

The fixed target physics of the Tevatron project, when it comes into operation, will upgrade the 400 GeV program that presently constitutes the major high energy element of the national effort. The ongoing programs at the BNL Alternating Gradient Synchrotron (AGS), the SLAC electron-positron storage ring (SPEAR), and the SLAC linac carry much of the remaining current effort at lower energies. These latter facilities represent a valuable national investment already in place, and they cover regions of particle physics in which much important work remains to be done. Moreover, the SLAC linac serves as injector to the Positron-Electron Storage Ring (PEP), and the AGS is slated to inject protons into ISABELLE. The national program in High Energy Physics also involves experimental and research and development (R&D) activities not associated with high energy accelerators: e.g., cosmic ray experiments, searches for proton decay, neutrino experiments based on nuclear reactors, weak interaction studies at Los Alamos Scientific Laboratory (LASL) based on the Los Alamos Meson Physics Facility (LAMPF), quark searches, etc.

At all major levels -- experimental, technological, and theoretical -- the U.S. community has played a leadership role over the past several decades in the expanding science of particle physics. The facility construction projects undertaken in the 1970's were designed to sustain a vigorous U.S. position for the coming years, following a period of decline in real dollar funding and the phasing out of a number of lower energy accelerator facilities. The current level of funding for the national program is nominally governed by the DOE/OMB Long Range Plan of 1978. This is supposed to correspond to a steady annual funding level (DOE plus NSF) of $325M in FY 1979 dollars. It represents the lowest of several alternative levels of support which the 1977 Subpanel was charged to investigate for future planning. That Subpanel regarded the ongoing program and the ISABELLE and Energy Saver/Tevatron projects as workable on a "best effort" basis at this level, but it foresaw the possibility of strains developing.

Several problems have indeed arisen. For one thing, inflation in power and certain other technological costs has not been fully allowed for in the conversion to real dollar budgets, so that a deficit has begun to develop with respect to the DOE/OMB Plan. This has entailed a decrease in the utilization of existing facilities and has led to delay and in some cases abandonment of promising experiments. Moreover, the new superconducting technology involved in the ISABELLE and Tevatron projects is

turning out to be more arduous than was anticipated in the reports of the earlier Subpanels. This is a challenge in its own right but it also impacts on the resources available to the ongoing program. Yet, for the longer run, the superconducting effort contributes to an important developing technology with potential applications outside of high energy physics; and for the physics program it provides an avenue for mitigating the power costs of accelerator operations while reaching higher fields and energies.

Another issue that increasingly confronts the U.S. community is the growing vigor of High Energy Physics activities abroad, particularly in Western Europe. The e^+e^- collider PEP has its counterpart in the PETRA facility at DESY in West Germany; the 400 GeV program at Fermilab is paralleled by a corresponding program at the SPS at CERN in Geneva; the pp collider ISR, at CERN, is without counterpart in the U.S.; and the overall level of funding in Western Europe substantially exceeds that supporting the U.S. program. Moreover, there is an increasing level of activity underway and planned in Japan, the Peoples Republic of China, and the U.S.S.R.

At CERN, a $\bar{p}p$ collider facility (540 GeV in the center-of-mass) is now under construction and is expected to come into operation well before the 2 Tev $\bar{p}p$ collider at Fermilab. Counterparts to the high luminosity ISABELLE facility and the fixed target Tevatron program are not presently envisaged for Western Europe. On the other hand, plans for a very high energy e^+e^- collider LEP at CERN (100 GeV in the center of mass in the earliest stage) and for an ep and e^+e^- collider HERA at DESY are now under serious discussion in Europe.

These developments abroad attest to the scientific attraction and importance of High Energy Physics. From the U.S. perspective, they have to be recognized as the inevitable limits which are coming into place on our long established predominance in High Energy Physics. At the same time, they provide welcome opportunities for increased scientific progress on the international scale, through collaboration, allocation of tasks, and friendly competition and cross checking.

As is reflected in the range of accelerator types described above, progress in particle physics calls for variety in tools and approaches. New facility projects are motivated from time-to-time by ongoing developments in the physics itself and in technology. Although it may no longer be feasible for the U.S. and Europe to pursue every new research opportunity in parallel, the sustained vigor of the U.S. program requires that we be at least well represented in the most promising lines of research. In present planning for the coming decade, the U.S. program stakes out the highest energies for the physics of hadron collisions, via the ISABELLE and Tevatron projects. Western Europe will probably emphasize the highest energies for e^+e^- and ep colliders, via the very ambitious and costly LEP and HERA proposals. There are fundamental scientific issues at stake in every one of these areas. In particular, recent advances in our understanding of the weak interactions

strongly indicate that these interactions are mediated by gauge bosons, the Z^o and $W^{\pm}$ particles, with masses somewhat below 100 GeV. The several lines of approach bear on various aspects of these gauge particles: their existence, their production and decay characteristics, and the open possibility that there may be more than one class of such objects.

The e^+e^- collider approach is especially suitable for study of the expected rich variety of Z^o decay processes, including decay channels involving other anticipated (and perhaps unanticipated) objects such as "Higgs" particles, new quark and lepton types, etc. Similarly, the study of high energy ep collisions can extend the search for gauge boson effects to higher mass regions and it opens up new windows bearing on other aspects of the weak interactions. Although the European decisions regarding LEP and HERA have not yet been taken, the U.S. community, in planning for its own program in the coming decade, cannot fail to take into account the possibility of positive European decisions and vigorous implementation.

Several possible new projects, in varying stages of definition, have been proposed for the U.S. program and have been brought to the Subpanel for consideration.

(1) The Stanford Linear Collider (SLC) proposal for an e^+e^- collider at 100 GeV in the center of mass is based on ingenious new technology with promise for later relevance to ultra-high energy devices involving colliding linac beams. The project has provision for one interaction region. Compared to LEP, the SLC project is relatively inexpensive.

(2) The e^+e^- ring contemplated by Cornell at 100 GeV center-of-mass has four interaction regions and involves superconducting rf technology. Again, by the standards of LEP the Cornell project is relatively inexpensive, though much more costly than SLC.

(3) A possible ep facility, being explored by a Canadian consortium and independently by a U.S. group at Nevis Laboratory, Columbia University, was described to the Subpanel. It would involve a 10 GeV electron ring, used in conjunction with a high energy proton beam at Fermilab or BNL.

As with the Tevatron and ISABELLE, either of the above e^+e^- facilities would involve the challenge and opportunities of new technology. For the longer term it is clear that scientific progress rests on the development of advanced technologies, designed to extend the energy, intensity, and flexibility of accelerators and the precision and range of detectors and instrumentation. Much of the R&D effort in the current U.S. program is properly aimed at the short-term goals of existing or imminent projects. However, it seems increasingly important to upgrade the effort devoted to goals of longer range. This issue is addressed in more detail later in the report.

The above discussion lays out some of the major issues that have confronted the Subpanel. High Energy Physics, in its intrinsic scientific dimensions, has accomplished major advances over the past decade. The ongoing program, the newly completed research facilities, and the projects underway, constitute a set of promising ingredients for further advances. On the other hand, budgetary constraints are exacting a serious price in the utilization of existing facilities and pose the risk of foreclosing the timely development of new facilities of major importance. This occurs in the face of increasing research investment and activity abroad.

Finally, an issue that is interwoven among all the others and that was the subject of much discussion by the Subpanel concerns the most important resource of high energy physics research, namely, the people who practice this science. The reduction in the number of accelerator laboratories that has taken place in the past decade and the reduction in utilization of the remaining facilities have combined to limit the opportunities for small groups of people to exercise independence and to mount and carry out experiments in a timely fashion. Moreover, the physics objectives themselves seem increasingly to demand large detectors, large collaborations, and long time scales. These trends are not peculiar to the U.S. program. On the shorter-term view one may have to accept the realities of the situation, in the interest of efficiency and scientific output. For the longer run, however, both efficiency and output depend ultimately on the skills and ingenuity of individual people, qualities that are enhanced by practice and independence. In our long-term planning, provisions have to be made to preserve independence and variety of opportunities, to engage fully the talented pool of high energy physicists at the universities, and to attract new, young talent. How these considerations are to enter into concrete decisions about the overall program, and with what weight, is a very complex problem.

SECTION II

STATUS OF THE FIELD

Introduction

During the decade of the 1970's, there has been remarkable progress in our understanding of the subnuclear world. Of the four basic kinds of forces, at best only two -- electromagnetism and gravitation -- could in 1970 be described in highly fundamental terms. The other two forces -- strong and weak -- were understood only in relatively incomplete and descriptive terms. The high energy behavior of the weak force could not be predicted, although enough was known to give hints of landmark energies at which new kinds of phenomena could be expected to occur. A great deal of information on the properties of the strong force provided by 1970 a body of organizing principles but no fundamental theory. Experiments were just beginning to point the way toward a description of the nucleon in terms of quark constituents, with some promise of simplicity of description at short distances.

In 1980 the situation is quite different. There exist candidate theories for both strong and weak interactions, each formulated at a level as basic as the formulations of electrodynamics and gravitation. The theory of the weak interaction is quantitative and predictive at very high energies as well as at low energies, and has enjoyed a remarkable degree of experimental success. The theory of strong interactions has likewise been successful in accounting for a broad range of strong interaction phenomena.

This revolution in our viewpoint on strong and weak interactions has also been accompanied by much greater confidence in the picture of nucleons as composites of point-like quarks. This picture was very much advanced by the landmark discovery at BNL and SLAC of the J/psi particle in 1974 and its subsequent description in terms of charmed-quark constituents.

The field in 1980 has progressed much further than anyone could have dared to anticipate in 1970, and a central feature of the program of the 1980's will be either to firmly establish the validity of the standard picture or show it to be deficient. If the present theoretical optimism is confirmed by the experimental program of the 1980's, we will have largely accomplished an advance in our understanding of the basic forces of nature comparable to the establishment of the theories of electrodynamics and gravitation.

The Standard Picture

The experimental and theoretical developments of the 1970's have led to a definite theoretical framework. In this standard picture the number and kind of building blocks (six kinds of quarks, three kinds of charged leptons, and three kinds of neutrinos) are specified, as well as the basic forces between them -- electromagnetic, strong, and weak.

The theory of quantum electrodynamics (QED) has been well developed for some time. The predictions of the theory have been tested ever more incisively by experiments at e^+e^- storage rings, so that it is known to be accurate down to a distance scale of order 10^{-16} cm, about one thousandth of the size of a proton.

The strong force acts between quarks and is described by a theory called quantum chromodynamics (QCD). QCD is structurally very similar to quantum electrodynamics (QED). Just as QED implies the existence of photons, QCD implies the existence of similar quanta called gluons. Gluons (like quarks) are only seen indirectly. Recently, indirect evidence for their existence has emerged from experiments at the e^+e^- storage ring PETRA. This will be a major subject of investigation for the PEP storage ring now being commissioned. Earlier evidence for QCD emerged from the lepton scattering experiments carried out at SLAC, Fermilab, and CERN during the 1970's. Experiments on production of lepton pairs at Fermilab and elsewhere have also provided evidence supportive of QCD.

The weak force, which is responsible for radioactivity, is described by a theory similar in many respects to QCD and QED. However, the analogs of photons and gluons, the intermediate bosons $W^{\pm}$ and Z^o, are expected to have large rest masses, of order 100 times the mass of the proton. The theory unifies to some extent the electromagnetic and weak forces at these large energy scales.

The rapid development of this theory in the 1970's occurred as a result of major theoretical breakthroughs and a large body of precision experiments at BNL, Fermilab, and CERN using muon and neutrino beams. The discovery of the J/psi at SLAC and BNL in 1974, and its interpretation in terms of the fourth, or charmed quark, was another important element. A beautiful and delicate experiment at the SLAC linac using polarized electrons also provided important confirmation. By now there is a remarkably consistent and accurate body of data in support of the standard electroweak picture.

The structural similarity of the theories of strong and electroweak forces has suggested a further unification. The simplest such extension involves extrapolation of present theory by 13 orders of magnitude to energies of order 10^{15} GeV, at which point the synthesis of strong and electroweak forces becomes manifest. While the energies required to directly explore this domain are inaccessible, there are observable low energy effects which do occur. In particular, the proton is predicted to be unstable, with a lifetime of the order of $10^{29} - 10^{33}$ years. Experiments with sensitivity in this range are already underway.

Despite these beautiful concepts and encouraging experiments, there are aspects of the electroweak theory which suggest it is incomplete. The most important aspect has to do with the masses of the $W^{\pm}$ and Z^o and of the quark and lepton building blocks. A general theoretical framework which describes the origin of these masses does exist, although there are differing

versions of the details. In any event, it appears inevitable in such frameworks that there exists at least one extra particle (the "Higgs boson") with properties which make it very difficult to detect. The expected mass range is anywhere from a few GeV to 100 GeV.

The actual pattern of observed quark and lepton masses remains totally enigmatic. The understanding of this pattern is one of the greatest challenges for the theorist. Neutrinos may also have mass, a possibility that has been recently revived. Novel experimental techniques on a wide range of energy scales are relevant to the search for effects due to neutrino mass. The AGS, for example, is well suited to undertake these searches.

The number of quarks and leptons is also not understood. The third charged lepton, discovered at SLAC in 1975, and the fifth, or bottom quark, emergent from the Fermilab discovery of the upsilon resonance, were totally unexpected particles theoretically. Thus there is no shortage of profound theoretical problems, even within the standard picture.

Physics in the 1980's According to the Standard Picture

The facilities proposed for the 1980's, both in the U.S. and in Europe, should provide much better and more decisive tests of the standard picture.

The number of crucial tests of QCD is small. The higher energy e^+e^- colliders are especially useful in providing such crucial tests. Fixed target lepton-nucleon scattering and, if possible, ep colliding beams are another class of experiments which provide especially clean tests. The higher energies available in the 1980's at the Tevatron and elsewhere will be especially useful.

The many successful tests of electroweak theory have all been at energies small compared to its natural energy scale, given by the masses of the $W^{\pm}$ and Z^o. The clear central goal is the direct observation of these particles. The large value of the predicted masses make colliding beam facilities a necessity. ISABELLE and the $p\bar{p}$ Tevatron collider are well suited to this task. Electron-positron collisions would be a rich, especially clean source of Z^o's, allowing the accurate determination of Z^o properties. This is a prime motivation for e^+e^- colliders.

Another goal of electroweak theory will be to measure the single parameter of the theory (the weak mixing angle θw) more accurately at low energies as well as at high. This can be done in a variety of ways at all the fixed target machines.

In addition to describing the force laws, the standard picture anticipates the existence of a sixth, top quark, as yet undiscovered. If the mass is less than 100 GeV, it should be found at LEP or some other e^+e^- collider. For masses larger than that, it appears feasible to observe this quark in pp or $p\bar{p}$ colliders such as ISABELLE or Tevatron.

In addition, a third neutrino associated with the tau lepton is anticipated. The Tevatron fixed target program provides about the only opportunity for producing and studying this particle. Such experiments appear difficult, but attempts are already being made and will certainly continue to be pursued.

There is the chance that the single Higgs boson of the standard picture will be discovered if its mass is less than about 100 GeV. This should be possible with e^+e^- colliding beam machines such as LEP. If the mass is considerably larger, the task may have to be left to later generations of accelerators.

Finally, in the standard picture the puzzling phenomena of CP violation is correlated with the equally puzzling issue of the origin of quark masses and mixings. It is necessary to find additional observable effects which test this general idea. Precision measurements of K decay, now underway at Fermilab and BNL, and intense measurement of bottom quark properties at CESR or PEP may provide new information of vital importance.

The Program of the 1980's Without the Standard Picture

It may be fair to say that while the 1970 physicist was too pessimistic about the progress which would be made in the 1970's, the 1980 physicist may be too optimistic. Particle physics has, without fail, encountered major surprises in every new energy regime it has entered. Even within the basic framework of QCD and electroweak theories, unanticipated phenomena may be found. There may be no top quark. There may be rare decay modes of familiar particles into unexpected final states. There may exist new kinds of particles which we cannot even imagine. All machines -- high energy and low; pp, $p\bar{p}$, e^+e^- and ep -- have the potential for providing future surprises. However, very heavy particles may be produced only with high energy colliders. Any new particles with electroweak interactions will be produced at an e^+e^- collider. Any new particles with strong interactions will be produced at a pp or $p\bar{p}$ collider. Who knows what new particles will be produced in an ep collider, for no such machine has ever been built. Each kind of machine complements every other.

Even within the framework of QCD and electroweak theory, it may happen that quarks and/or leptons are themselves composites of simpler constituents -- a possibility which is suggested to some by the proliferation of families of quarks and leptons. Experiments on all three basic kinds of colliders, e^+e^-, ep and pp (or $p\bar{p}$) should be sensitive to compositeness on a very short distance scale.

Finally, we still do not know whether QCD and electroweak theories are really correct or, if correct, only have a restricted range of validity. Remarkable but controversial cosmic ray experiments have hinted at the existence of strong-interaction phenomena at collider energies, phenomena which could signal a radical departure from the picture at present energies. The existence of isolated, fractionally-charged quarks would likewise require, at the least, a major revision of QCD as it now stands.

The weak interaction likewise may not be described within the general gauge-theory framework which is now very widely accepted. Even in the unlikely possibility that the weak bosons do not exist, there is good reason to believe that the natural energy scale of the weak force must lie below 1 TeV, an energy scale accessible to the Fermilab $p\bar{p}$ collider. There are strong reasons to believe this scale in fact to be less than 200 GeV, a regime which is accessible to ISABELLE and especially to ep colliders.

Balance and Diversity of the Program

It should be apparent from the above discussion that diversity of approach is an invaluable asset. Many phenomena are best seen with probes involving electrons, both e^+e^- and ep. Others benefit from the extra center-of-mass energy available in proton-proton or proton-antiproton colliders. Still others require the high intensity and variety of beams in fixed-target accelerator facilities, high energy and low; while yet others, such as the proton decay experiment, do not need an accelerator at all.

Therefore, whether or not the present optimism is vindicated, a balanced and diversified High Energy Physics program for the 1980's will be a rich one and will have a basic impact on our view of the subnuclear world.

Intellectual and Technological Applications to Other Fields

Although the primary justification of High Energy Physics is the drive for a deeper understanding of the fundamental nature of matter there are a number of direct and indirect by-products of the field.

The enormous development of the theory of quantum fields has had major applications to solid state theory and to condensed matter research. The progress of elementary particle physics has provided tools for astrophysics and cosmology, for example in the study of neutron stars and in the extrapolation back to the early stages of the big bang universe. The recent development of a precise theoretical structure has reawakened the interest of many pure mathematicians in physics and led to fruitful exchange and mutual stimulation. The development of a quantitative theory of the strong interactions provides a foundation for nuclear theory. Indeed an increasing fraction of the present program in nuclear physics deals with QCD and electroweak phenomena. This might eventually lead to the prediction of novel nuclear phenomena or new applications.

The experimental accomplishments of the last decade were made possible by a remarkable series of technological developments. Some of these are listed below:

(1) The science and technology of producing, focusing, transporting, accelerating, and storing high energy particle beams has made great progress in the last decade. It is interesting to note that it was the high level of this technology which has made the heavy

ion approach to inertial fusion an exciting and hopeful approach to fusion energy. Indeed, the heavy ion ignited inertial fusion idea was initiated and developed by high energy accelerator physicists. The scanning electron microscope was inspired by methods of high energy particle beam technology and indeed was developed by a high energy physicist. We also note that the physics of particle beams has been applied to research in cancer detection and treatment.

(2) The applications of the synchrotron radiation emitted by the circulating beam in an electron storage ring were developed to a high level because of the importance of this device in high energy physics research. This has led to the controlled use of synchrotron light for a very large and important class of investigations in several fields, solid state physics, surface physics, biology, and micro-circuit fabrication.

(3) The development of particle detectors has continued at a remarkable pace. The resolution and data handling capability of high energy physics research instruments have increased enormously in the last decade. Multiwire proportional and drift chambers were developed and widely used. One important application of these instrumentation techniques is in medical tomography. In addition to the direct application of the detectors themselves, the ideas and system concepts in High Energy Physics are of value for applications. We note that the basic principle of the CAT scanner was developed by a physicist who was a High Energy Physics principal investigator at the time.

(4) The ongoing development of large-scale superconducting magnet projects has been, and continues to be, pioneered by high energy physicists. This technology has led to successful application in the construction of large magnets for the generation of electricity through magnetohydrodynamics (MHD) as well as magnets for fusion energy, both for confinement and for ohmic heating. The development of large-scale superconducting systems may also have important applications to the efficient transmission of electrical energy.

(5) The Winston cone, originally developed for a Cerenkov counter at ANL, has found application in the field of solar energy as a moderately concentrating, nontracking collector. This collector design has potential for good performance at low cost and is currently under industrial development.

High Energy Physics, both experimental and theoretical, may be expected to continue to generate new developments of importance on a wider scale such as those indicated above.

SECTION III

THE CURRENT EXPERIMENTAL HIGH ENERGY PHYSICS PROGRAM

The U.S. High Energy Physics program depends on particle accelerators and storage ring devices located at three national Laboratories supported by the U.S. Department of Energy (Brookhaven National Laboratory, Fermi National Accelerator Laboratory, and Stanford Linear Accelerator Center) and the Cornell University Newman Laboratory of Nuclear Studies supported by the National Science Foundation. Because the various facilities involve different kinds of particles, particle energies and intensities, and targets, the experiments performed at these facilities address many different aspects of High Energy Physics. This section outlines some of the physics activities and opportunities associated with the accelerators operating and under construction in the U.S. High Energy Physics program. Important experimental research supported by the U.S. program, but not associated with high energy accelerators, is also identified.

Fixed Target Program at BNL

The fixed target experimental program at Brookhaven National Laboratory is based on the 30 GeV proton synchrotron, the AGS, which provides not only intense external beams of high energy protons but also secondary separated beams of pions and kaons and very good neutrino beams. Many major discoveries have been made with the AGS throughout its distinguished history. Some of the highlights are: two neutrinos; the "Omega Minus" hyperon; CP violation; the J particle; and the charmed baryon. The physics program now focuses on: studies of the weak interaction via neutrino interactions and K meson decays; the search for neutrino oscillations; hadronic interactions via spectroscopy, dynamics and polarization studies; QED via measurements of muonic atoms with helium nuclei; and a strong program of intermediate energy nuclear physics studying kaon-hypernuclei and K^- - and hyperon-induced atomic X-ray emission.

The AGS has the highest average proton intensity available. It provides up to 10^{13} protons per pulse every 1.2 seconds for neutrino physics and, with a one second flattop, every 2.5 seconds for electronic counter experiments.

Fixed Target Program at SLAC

The present SLAC linac remains the highest energy and highest intensity electron accelerator in the world. The SLAC accelerator also provides positrons and longitudinally polarized photon beams. In normal operation the SLAC electron beam has an energy of 23 GeV, while in the SLED I mode it has achieved energies up to 33 GeV.

The fixed target program is now centered on three facilities -- the electron spectrometers, the multiparticle spectrometer (LASS), and the hybrid bubble chamber facility.

The electron spectrometers have been used in a long and productive program that has led to some of the important discoveries of the past 10 years (viz. the deep inelastic electron scattering and the parity violation measurements). The bubble chamber has had a very productive program of photo-production and hadronic interaction studies and is now being used to study charmed baryon production by monochromatic photons, and also to measure their lifetimes. The LASS spectrometer is being used for a programmatic study of the systematics of strange quark spectroscopy.

Future experimental topics involve more polarized electron scattering experiments to measure parity violation at large q^2, and Møller scattering and very high sensitivity polarized photoproduction experiments.

400 GeV Fixed Target Program at Fermilab

The physics program at Fermilab uses a 400 GeV proton synchrotron. Proton beams of $> 2 \times 10^{13}$ particles per pulse are produced with a repetition rate of one burst each 10 seconds. Proton beams can be directed simultaneously onto several targets, producing secondary beams of hadrons, muons, photons, and neutrinos. As many as 15 experiments can be run simultaneously, resulting in a program of great diversity.

In its early days (1972-74) the program was dominated by the exploration of a new energy regime. Experiments ranged from extensions of hadronic physics at lower energies to searches for quarks and monopoles. As the machine matured, the main thrust of the experimental program focused on the study of hadronic substructure, the interaction of subnuclear constituents with one another, and the investigation of new forms of matter such as charm.

A major discovery of the Fermilab physics program was that of the upsilon resonance, leading to its interpretation in terms of a fifth quark, the b-quark. The substructure of nucleons has been studied by means of muon and neutrino scattering. The structure of pions has been studied by means of dimuon production in pion-nucleon collisions. Static properties of hadrons such as hyperon magnetic moments have been measured. This program has also revealed unexpected dynamical effects such as the polarization of hyperons produced at large transverse momentum. Numerous measurements on the production and properties of charmed particles have been carried out. Notable are direct measurements of the lifetime of charmed particles.

SPEAR

The SPEAR electron positron storage ring at SLAC operates with a center-of-mass energy region between 2.5 and 8 GeV. The research program at SPEAR has produced a large number of spectacular results including the discoveries

of members of the psi family, the charmed particles, the tau lepton, and the production of two-jet hadronic structures. With the commissioning of the higher energy storage ring PEP, the SPEAR facility is now operating 50 percent of the time for particle physics, the other 50 percent going into research with synchrotron light. There remains a rich area of work in the SPEAR energy range that focuses on the systematic study of states involving the c-quark. The recent discovery of the "charmed eta" proves the continuing vitality of the SPEAR program.

CESR

The Cornell electron-positron storage ring CESR, designed to operate at center-of-mass energies between 8 and 16 GeV, obtained its first luminosity in September 1979. During the subsequent 6 months, experimenters at both CESR interaction regions have obtained important results, including confirmation of the three narrow upsilon states and the discovery of what may be a broader upsilon resonance, probably above threshold for the pair production of mesons with b-quarks. The detailed study of states involving b-quarks, both mesons and baryons, including spectroscopy and decay properties, represents a rich and unexplored area to which the CESR facility has an ideally matched energy domain.

PEP

The PEP e^+e^- storage ring at SLAC, which covers the range from about 8 to 36 GeV in center-of-mass energy, is just in the process of commissioning. Although the same energy region has already been given an exploratory study at the German storage ring PETRA, one may expect that increased luminosity and a wide variety of detectors will allow the PEP program to make substantial advances in such areas as tests of QCD, studies of jet structure and fragmentation, measurements of interference between weak and electromagnetic currents, and searches for new particles.

Fermilab 1000 GeV Superconducting Accelerator and Storage Ring

A major new accelerator storage ring complex is being developed at Fermilab by means of adding a string of approximately 1000 superconducting magnets placed directly beneath the present 400 GeV accelerator ring. This allows for a variety of thrusts: large power cost savings can be realized by running the present accelerator up to 150 GeV and then transferring the beams into the superconducting ring for attaining 400-500 GeV (Energy Saver mode); the superconducting ring will also provide the opportunity to reach very high energies by colliding beams head on. This will involve producing and accumulating antiprotons and then simultaneously accelerating these antiprotons and a counter-rotating beam of protons in the superconducting ring to collide with each other at a center-of-mass energy of 2000 GeV (Tevatron I); the superconducting ring also allows for accelerating the primary protons up to 1000 GeV and then extracting them, thereby doubling the energy of the fixed target program involving neutrino, muon, and hadron collisions with high intensities and energies (Tevatron II).

The physics opportunities provided by proton-antiproton collisions in the superconducting ring are unique; nowhere else in the world will collisions at such high energies be available for experimental study. These collisions are expected to provide much new information on constituents of hadrons (quarks), the heavy carriers of weak and electromagnetic forces ($W^{\pm}$ and Z^{o} particles), and the interactions among these particles. Furthermore, this project represents a major thrust into the unknown with good prospects for major discoveries. The higher energies made available by the superconducting ring to the fixed target program make it possible to produce and study particles in new ways not possible with the 400 GeV program and to extend to higher energies many of the studies now carried out at 400 GeV.

ISABELLE

The major authorized project for the future is the 400 GeV on 400 GeV colliding proton machine ISABELLE at BNL. ISABELLE will provide an order-of-magnitude increase in energy available for new-particle production by proton-proton collisions. It represents a major expansion of the frontier of High Energy Physics. An extremely attractive attribute of ISABELLE is its very high intensity which will allow rapid data accumulation as well as exploration of rare processes. ISABELLE will therefore allow searches for the $W^{\pm}$ and Z^{o} particles, possible new members of the quark family, and most important, for the completely unexpected, which historically has accompanied the advent of nearly all new machines.

Non-Accelerator Projects

High Energy Physics never did and never will consist solely of experiments done at conventional accelerators. Much of importance can only be discovered in other ways. Current theories predict that protons live approximately 10^{31} years. Several U.S. experiments will test this prediction. These novel experiments involve very massive underground detectors. They may find proton decay, or reveal surprises such as new cosmic ray phenomena, unstable superheavy particles, or neutrino oscillations. Neutrinos may have mass, and may oscillate, one kind into another. This can be studied in many ways: accelerator neutrinos, intense radioactive sources, beam dumps, the electron spectrum of tritium beta-decay. There already exist hints of effects, and better experiments must be done. Reactors can be used to measure the neutron electric dipole moment and to look for nucleon-antinucleon mixing. Theory suggests the possible existence of unobserved forms of stable matter. Very heavy monopoles may be incident as cosmic rays; very heavy isotopes may be present on Earth. A variety of experiments is called for. Particle physics and astronomy have become closer than ever. Solar neutrinos and neutrino astronomy can tell us as much about neutrinos as about stars.

SECTION IV

ACCELERATOR FACILITIES

This section briefly describes the recently completed accelerator facilities, projects now underway, and some ideas for the future which have been discussed.

RECENTLY COMPLETED FACILITIES

CESR

In the fall of 1977, the National Science Foundation approved an e^+e^- colliding beam facility, CESR, at Cornell University. The design energy was 8 GeV per beam, expandable to 10 GeV. This machine was completed 1 year ahead of the contract period and first luminosity from colliding beams was obtained in September 1979, 2 years after approval of the project. Costs for the storage ring, a large magnetic detector, and computing facility were $400K less than the total budget of $20.7M.

The energy region for which this machine was designed has turned out to be a very interesting one because of the upsilon and related particles, and a productive experimental program is now underway, with the first interesting results already published.

PEP

A major e^+e^- colliding beam facility, PEP, was completed at the Stanford Linear Accelerator Center in April 1980 on time and at cost. This machine was designed for maximum luminosity at 15 GeV per beam and maximum energy 18 GeV, expandable to 24 GeV if a substantial upgrade were to be carried out in the future. This machine was constructed as a joint SLAC/LBL project.

In view of the 2 years earlier operation of the similar machine, PETRA, at DESY, the luminosity of PEP will be of critical importance. It is too early to tell what this luminosity will be but there is expectation that it will exceed that achieved at PETRA.

The experimental program at PEP will begin soon; a number of detectors are installed and others will be completed during the coming year.

PROJECTS UNDERWAY

Fermilab Energy Saver/Tevatron

(1) General

Although the Fermilab Energy Saver-Tevatron I-Tevatron II projects have been defined as three separate construction projects they represent one integrated

program to be accomplished in three phases: the Energy Saver and related 500 GeV programs, the 1 TeV fixed target program, and a $\bar{p}p$ collider program at 1 TeV on 1 TeV. This is clearly an ambitious program involving significant R&D, and it will require vigorous effort by the staff and users of Fermilab for effective achievement. It is one of the most exciting efforts currently underway in the U.S. High Energy Physics program. It is important that the authorization of the two Tevatron projects proceed in a timely fashion in order that the Fermilab management can implement an optimal program utilizing the Energy Saver and Tevatron facilities. It is recognized that surprises may be in store due to the lack of operating experience with major large superconducting magnet and cryogenic systems in accelerator applications. These new challenges will have to be met with vigor and additional R&D support may be required. Schedule risk remains high.

(2) The Fermilab Energy Saver

This project is underway at Fermilab and is scheduled for completion in 1982. After experiencing a number of difficulties in the R&D program for the development of production-quality magnets,the Fermilab personnel believe they will soon be ready to proceed with full production of the 800 cryostatically-encased superconducting dipoles. They have produced some 200 production-quality coil structures which they believe are satisfactory. Recently, a problem occurred with the assembly of these coil structures in the cryostats, which they now believe has been solved so that full-scale production on one shift per day can begin soon. In order to meet the schedule, two-shift production must begin in the fall of 1980.

The Subpanel feels that considerable progress has been made in overcoming the problems of constructing superconducting magnets, but it is concerned that more problems may be encountered which will require additional R&D efforts for their solution. Such problems could result in a delay in completion of the project.

Fermilab has developed a number of critical quality assurance and magnet measurement programs to ensure a satisfactory design. This has been a difficult pioneering effort in an area of critical importance in the development of future accelerators and in the development of other large superconducting magnet and cryogenic systems. Fermilab has also tested strings of magnets and cryostats in a variety of configurations to ensure that their design for magnet protection, controls, cryogenic and other systems will perform adequately. They are in the process of installing a string of accelerator-type superconducting magnets to bring the present 400 GeV proton beam into the Meson Laboratory. This system, operating

continuously with a particle beam in an environment similar to an accelerator, will be another important systems test. Other elements of the Energy Saver, i.e., the refrigeration system, and the rf system, are under construction, and testing and commissioning of major subsystems are occurring.

Operation in the Energy Saver mode is scheduled to begin in 1982 with energies up to 500 GeV, resulting in significant savings in power compared to the present fixed target operation.

(3) 1 TeV Fixed Target Program

The Tevatron I-Tevatron II program will allow the superconducting accelerator to reach proton energies near 1000 GeV with adequate repetition rate and will provide extracted 1 TeV proton beams and experimental area facilities for a 1 TeV fixed target research program.

This facility will provide the highest energy secondary particle beams in the world for many years. The opportunities afforded for experimental investigation of important fundamental issues in the study of the basic constituents of matter are rich and unique.

The research and development work leading to this design has been extensive. A Summer Study was held in 1976, followed in the last year by workshops treating the individual experimental areas. There are many technical papers available on particular studies. At every stage of the work leading to this design report, experimenters and users from the entire national and international particle physics community have participated and contributed.

The 1 TeV fixed target program, Tevatron II, includes:

(a) The construction of a slow-extraction system for 1 TeV protons.

(b) Improvement of the external beams switchyard to 1 TeV and the provision for 1 TeV targeting in the present external experimental area.

(c) The construction of facilities for new secondary-beam enclosures and support facilities in each external experimental area designed to take full advantage of the fixed target physics opportunities of the Tevatron. The total number of secondary beams will not be increased because the new beams will replace existing ones, but the energies and intensities of the secondary beams will be significantly increased.

This project will build on the experience achieved with the Energy Saver program in accelerator, extraction, and proton beam targeting systems.

(4) $\bar{p}p$ Collider

Fermilab plans to use the 1 TeV superconducting ring for beam storage of counter-rotating antiproton ($\bar{p}$) and proton (p) beams. Colliding beam experiments can then be performed in a special straight section, where beams are focused to very small cross section to enhance the interaction rate (low beta section).

The maximum reaction energy of 2000 GeV between colliding particles and the expected high interaction rate expressed by the luminosity $L = 10^{30}\ cm^{-2}\ s^{-1}$ will make this $\bar{p}p$ collider a truly unique facility in the world for many years to come.

The most difficult part of the project is the collection of intense current of antiprotons in 12 bunches with 10^{10} antiprotons in each of them. Antiprotons are produced by bombarding a special target with 80 GeV protons. Because the production efficiency is very low (3×10^{-6}), antiproton accumulation is a slow (5 hours) process. Antiprotons created in the target with energies of about 4.5 GeV are injected into a special precooler ring, where particle momentum spread is reduced by so-called "stochastic cooling." This makes it possible to decelerate the antiprotons to an energy of 200 MeV and inject them into a second special ring (the "cooler ring") where they are accumulated and their oscillations and momentum are further reduced by interaction with an intense electron beam ("electron cooling"). When the required total number of antiprotons (12×10^{10}) has been accumulated, they are transferred to the main proton synchrotron, accelerated to 150 GeV, extracted from the main ring and injected, in the form of 12 bunches, into the superconducting Energy Saver ring. After an equal number of proton bunches (each with 10^{11} protons) has been injected in the opposite direction in the Energy Saver ring, protons and antiprotons are slowly accelerated to the maximum energy of 1000 GeV.

Because of the long accumulation time for antiprotons, the reliability of the accelerator system, especially that of the "cooler ring," is of major concern. There are also new techniques involved, which have been tested to some extent but which need further investigation. The most critical components of this colliding beam scheme are being studied extensively at present. The production target for the $\bar{p}$ has to withstand a beam burst of 1×10^{13} protons at 80 GeV with a burst length of 1.6 microseconds and a beam spot of approximately 0.4 mm diameter. A number of studies have been conducted to address this problem and an appropriate R&D program is planned. Alternatives such as sweeping the beam over the target are being considered.

A version of a 200 MeV electron cooling ring exists at Fermilab including an electron gun for the electron cooling. First electron cooling experiments will be done in the near future. In the same ring longitudinal and transverse cooling experiments with stochastic cooling have already been carried out successfully.

In the main ring, beam storage studies and rf manipulation experiments with the goal of bunching a large fraction of the beam at 80 GeV into 1/13 of the circumference are underway.

In 1979 a Conceptual Design Report of the $\bar{p}$-source was published and submitted to DOE for FY 1981 approval.

Currently Fermilab plans to build two experimental areas for $\bar{p}p$ collisions. Fermilab, together with ANL, LBL, the University of Wisconsin, and the Institute for Nuclear Physics at Novosibirsk, USSR, has formed a collaboration supporting the R&D and construction efforts for the $\bar{p}p$ project.

Such an advanced accelerator project needs commensurate R&D efforts. To date, Fermilab is heavily engaged in the Energy Saver R&D and construction. As a result, the $\bar{p}p$ Collider R&D has slipped. It is hoped that the Collider can soon receive the necessary attention.

In comparison to the CERN $\bar{p}p$ scheme, the Fermilab design has a number of advantages. Due to the larger momentum of the colliding beams (2 x 1000 GeV compared to 2 x 270 GeV) and the smaller emittances, the design luminosity approximately 10^{30} cm^{-2} sec^{-1} can be obtained with one-fifth of the antiproton current. Hence the accumulation time is expected to be 5 hours rather than 24 hours as required at CERN.

There are a number of potential improvements such as increased solid angle in the $\bar{p}$ acceptance, increased target luminosity, reuse of the antiprotons, etc, which could increase the number of $\bar{p}$ in the same collection time and hence increase the luminosity or reduce the collection time and ease operational problems.

The uniqueness of this project and its rich research potential, particularly the very high energy achievable, make it most desirable to proceed expeditiously.

BNL ISABELLE

ISABELLE is an intersecting ring accelerator and storage ring facility designed to collide opposing beams of protons at center-of-mass energies between 60 and 800 GeV. The design luminosity at the "standard" crossing points is 2 x 10^{32} cm^{-2} sec^{-1}. By provision of special focusing elements at the crossing points, the design indicates that a peak luminosity of 10^{33} cm^{-2} sec^{-1} might be achieved.

The accelerator itself consists of two independent rings of superconducting magnets of 50 kilogauss peak field, arranged roughly on a circle of 3834 meter circumference in such a way that the opposing beams can be brought into collision at 6 places around the ring.

The status of the project is as follows:

(1) Civil Construction

ISABELLE construction was authorized in FY 1978. A substantial portion of the conventional (civil) construction is now underway. One-third of the tunnel is almost complete. Work is underway on most of the rest of the tunnel.

Two of the interaction areas are now under construction. The civil construction is on schedule at cost. The tunnel is scheduled to be finished in mid-1981 with the rest of the civil construction to be completed in early 1983. Experience to date indicates that this schedule is realistic.

(2) Standard Accelerator Components

Design and model work is now underway on the accelerator components other than the magnet and refrigeration system, which are discussed below. Some of these items are injection and ejection equipment, rf stacking and accelerating systems, vacuum system, magnet power supplies, and instrumentation and control systems.

(3) Refrigeration System

Extensive work on the refrigeration system has been underway for some time. Heat loads for most major components have been measured (transfer lines excepted) and found to be within design specification. Considerable R&D on some of the critical components such as special heat exchangers, oil removal system, etc., has been carried out. The design for the main cold box for the system is underway. The system is designed to rely on turbine expanders. Small, gas-bearing expanders in test refrigerators have given considerable trouble but, from other experience at BNL, the ISABELLE group feels that the larger turbines for the main refrigerator, which use oil bearings, will be more reliable. This has yet to be demonstrated. No large-scale tests of the cryogenic system have been carried out under realistic accelerator operating conditions.

(4) Superconducting Magnets

The current ISABELLE design calls for 732 dipoles of 5 Tesla (T) field strength and 348 quadrupoles of $60Tm^{-1}$ gradient. The manufacture of the dipole magnets has encountered unexpected difficulty. The magnets produced so far do not reach the design maximum fields or field quality. In response, BNL has instituted an R&D program to analyze the magnet design with the aim of correcting design deficiencies. A major element in this program is the set-up of manufacturing facilities at BNL for the fabrication of full-scale magnets. By experimental study of magnets built with tightly controlled design

variations, supplemented by theoretical analysis, the magnet development group hopes to uncover those features of the current design which result in substandard performance. The tooling available could be capable of manufacturing four sets of dipole coils per month. The plan is to produce at least two sets per month at BNL and ultimately to have most of the magnets produced by industry. This will require careful quality control on the production magnets. In addition to the field strength and training problems, the Laboratory recognizes that the problems of magnetic field quality and quench propagation must also be resolved.

The quadrupole magnets built so far at BNL are reported to be successful in terms of overall performance. Pilot production of quadrupoles at the BNL site is to begin shortly. Fifteen quadrupoles will be manufactured and tested thoroughly before proceeding further with production.

In an attempt to bring more experimental and analytical effort to bear on the magnet problems the organizational structure of the ISABELLE construction staff has been changed and the number of personnel in the magnet program has been increased during the past 2 years almost fourfold, up to 130. Outside help has been sought and some outside experimental or analytical work is being done at LBL and the MIT Magnet Lab. Additionally, the BNL management has set up an internal mechanism for exploring alternative magnet designs, should the problems with the current design prove intractable.

At this time, the BNL management hopes that pilot production of dipoles can begin in mid-1981. Current information from the R&D program is insufficient to assess the likelihood that this goal can be met. If it is not, the ISABELLE project will encounter significant delays.

For achievement of overall project goals, the BNL management may need to enhance technical leadership in a number of areas. In addition, a broader participation in ISABELLE by other BNL staff, particularly from the Physics Department and the AGS, and, perhaps, broader use of personnel from other Laboratories and industry may be necessary. BNL should continue to strengthen the practice of periodic in-depth technical review, involving outside expertise.

Although significant steps have been taken and are being taken by BNL management to solve ISABELLE problems, the very nature of the hardware involved and the lack of experience anywhere with systems of similar technological complexity increase the likelihood of further schedule slippage and the need for additional R&D. In meeting this very difficult challenge, technical leadership will be crucial.

PROPOSALS AND IDEAS FOR NEW FACILITIES

SLAC Linear Collider

(1) Description of the Project

SLAC has proposed to build a novel linear collider device (the Stanford Linear Collider or "SLC"), which would provide electron-positron collisions with an interaction energy (center-of-mass) of 100 GeV and a luminosity of 10^{30} cm^{-2} sec^{-1}. Based in large part on the SLAC linac, the SLC would provide colliding 50 GeV beams rapidly and at moderate cost.

The proponents point out that the SLC could be the first of a new class of electron-positron colliding beam facilities. It now seems relatively certain that circular colliding beam storage rings for electrons would not be feasible at center of mass energies above 300 GeV or so. On the other hand, there are no known fundamental technical limits to the energy which might be reached with linear collider devices until one reaches center-of-mass energies much beyond 600 GeV. The costs of a linear collider will be less than those of a circular machine at some beam energy in the few hundred GeV region. Nevertheless, the costs and power consumption of a linear machine are also likely to be very high. One of the goals of the SLC project is to demonstrate the feasibility of the linear collider concept, and to begin exploration of ways to reduce the expected costs and power.

In the operation of SLC, two microscopic, high-intensity particle bunches -- one of electrons and one of positrons -- are accelerated to an energy of 50 GeV in the Stanford two-mile accelerator after which the two bunches are separated, and transported in separate, reversed-double-bends (like "question marks") so as to be brought into a head-on collision. At the collision point -- where the high energy interactions are studied -- the bunches are compressed by focusing magnets to diameters of a few micrometers in order to achieve the desired interaction rate. Bunch pairs arrive at the collision point at the basic pulse rate of the linear accelerator (180 per second), and after the collision the electrons and positrons are disposed of in a dump.

The SLC requires major modifications and additions to the existing SLAC facilities.

(a) The linear accelerator must be modified to obtain higher electric fields, and thus to reach a peak electron energy of 50 GeV (to be compared with the present value of 33 GeV).

(b) Special sources of both electrons and positrons must be developed and built to provide the two microscopic beam bunches. Each bunch must have a duration of a few picoseconds and a current of a few kiloamperes, providing 5×10^{10} particles (to be compared with the current maximum performance of about 2×10^{9} electrons in a single bunch).

(c) The instrumentation and control system for the steering of the beam along its axis, and for the focusing of the particles along the full length of the accelerator must be upgraded in a major way to permit acceleration of the two particle bunches down the length of the accelerator, without an unacceptable increase in their lateral sizes.

(d) The reverse-bend magnetic channels (which bring the bunches into collision) and the tunnels to house them must be designed and constructed.

(e) An experimental hall and experimental physics apparatus must be provided at the intersection point.

Stanford has proposed to build the SLC in a period of about 2-1/2 years beginning in October 1981 at a cost of $63M (FY 1980).

(2) Appraisal of the Project

The Stanford Linear Collider is a highly imaginative and innovative project which will involve a large number of diverse and novel accelerator developments -- from the realization of the very high-intensity sources of electrons and positrons to the achievement of the microscopic beam sizes at the interaction point. The Subpanel did not find any fundamental difficulties with the concept and proposed design during the short time the design report has been available. It believes that many of the required technical developments can be made in a straightforward manner. Some developments will require a high level of intellectual and technical effort to reach the required performance.

One major concern would be the capacity to accelerate a high intensity bunch along the full length of the accelerator with no significant increase in the effective dimensions of the bunch. Another concern is that relatively minor technical imperfections in each of the several components of the SLC would so reduce the overall efficiency in the acceleration and transport of the electrons and positrons that the SLC would fail to reach the design luminosity -- which some feel is perhaps already marginal. On the other hand, the SLC proponents have pointed out several areas in the system in which their evaluations are conservative and where additional factors of two or three in luminosity may be achieved after further study and development.

In summary, the Subpanel views the SLC as a fascinating and challenging new departure in accelerator technology which might in the near term provide, at relatively low cost, electron-positron collisions at an energy of 100 GeV, and which might well demonstrate an avenue which could possibly reach energies of the order of 1 TeV in the distant future.

The Subpanel has found no basic technical flaws in the SLC proposal and is impressed by the large amount of new knowledge about significant extensions of current techniques that should and would be gained if the project were to proceed. The Subpanel sees important opportunities for making advances in many aspects of accelerator science, and the intriguing chance of exploring methods which might lead the way to the study of electron-positron collision at very high energies.

The Subpanel believes that it is premature to attempt a detailed comparison of the SLC project with the colliding beam storage ring being contemplated by Cornell.

Cornell e+e- Storage Ring

Cornell has produced a preliminary design of an electron-positron colliding beam storage ring which would provide a maximum energy of 50 GeV per beam and a luminosity of 3×10^{31} cm^{-2} sec^{-1} in each of the 4 interaction regions. The project would achieve significant economies of power and size by the use of superconducting rf cavities and would extend the U.S. energy range of electron storage rings significantly beyond the PEP energy (18 GeV per beam), and into the region where the Z^o particle should be produced and where a very exciting field of physics will no doubt be found.

In a high energy storage ring, synchrotron radiation causes energy loss of the circulating electrons and necessitates continuous beam acceleration with powerful radiofrequency systems. Power consumption of these systems becomes one of the dominating aspects of these machines. The optimization of construction and operating costs depends critically on the magnitude of the required radiofrequency power. Superconducting rf cavities offer very large savings in operating costs and allow the optimized storage ring to be built with substantially smaller radius, thereby roughly halving also the capital cost. The Cornell Newman Laboratory of Nuclear Studies is leading the world in the development of superconducting cavities for circular accelerators. The maximum accelerating field of 3 MV/m for the proposed collider is substantially below the maximum field of 4.5 MV/m reached already in their test cavities. The proposed R&D effort in the next 2 years should allow the envisaged accelerating cavities to be tested in the CESR storage ring. Questions of parasitic mode damping, synchrotron radiation effects on the cavities and the economy of a large technical system could then be answered with confidence and might then lead to a formal proposal. This approach seems to be prudent and sensible. Since the proposed storage ring, with the exception of the radiofrequency system, is standard and well understood, such a machine -- after the R&D effort -- could be built with little technical risk. It would be unique in the U.S. and should -- because of its smaller radius -- produce larger luminosity than the proposed 86 GeV x 86 GeV storage ring at CERN in the region below 50 GeV x 50 GeV. The development of large superconducting accelerating systems would have a great impact on all new circular electron and even proton accelerators and may lead to large power savings, and it could have other applications.

ep Colliders

Interest in electron-proton colliding beams in North America has been expressed by a Columbia University group and independently by a group composed of several Canadian institutions. Both groups have considered a 10 GeV electron storage ring tangent to the Fermilab Tevatron ring, providing center-of-mass energies up to 200 GeV.

Since no detailed design of this facility has been presented, only a few general remarks are included here. The combination of high luminosity ($>10^{31}$ $cm^{-2}s^{-1}$) and polarized electron beams required by ep physics introduces severe problems of beam dynamics that should be carefully studied. Other problems requiring investigation are the beam-beam interaction, the stability of high current bunched beams, the depolarizing effects, and the comparison of various possible spin rotation schemes. Experimental information on some of these problems will become available during the next 2 years both in Europe and the U.S.

The possibility of using ISABELLE instead of the Tevatron should be seriously considered. There are potential advantages which compensate for the lower energy of ISABELLE, such as the longer ISABELLE straight section, the exclusive use of one interaction area, and the fact that ISABELLE does not have to supply beams for a fixed target program. All these facts and the overall impact of the ep program on the host laboratory should be evaluated in any future ep proposal.

Higher Energy Proton Accelerators and $\bar{p}p$ Colliders

Studies of multi-TeV proton accelerator/colliders are being pursued at Fermilab. Specifically, the studies are focused on a 5 TeV proton synchrotron/collider, 5 km in diameter and using 8 to 10 T superconducting magnets. A considerable amount of R&D work will be required before such high field magnets suitable for accelerators can become a reality.

SECTION V

UNIVERSITY PROGRAMS AND FACILITY UTILIZATION

Roughly 1,100 experimentalists and approximately one-half that many theoreticians are currently active in U.S. High Energy Physics. The vitality of this community depends primarily on the existence of accelerator facilities which allow one to probe interesting new phenomena, and on effective utilization of those facilities. We discuss here the efficiency of the present program, in terms of both accelerator utilization and manpower.

Universities

The High Energy Physics program depends upon the involvement of physicists at the major educational institutions of the United States, and benefits from the efficiency of experimentation which is possible at large central Laboratories. The diversity of initiatives, competition among ideas, and high quality of talent that results from the participation of University groups lends breadth and originality to the program and is fundamental to it. Most important, the participation of students and University scientists in High Energy Physics is essential to the future of the field, and contributes to the vitality of the University community and the education received by students of all the physical sciences.

There are, however, increasing problems which confront the University groups. It is essential that the University researchers be able to spend a significant amount of time at their home institutions so that they may interact effectively with students, both graduate and undergraduate, and with other faculty. Hence, while the running of an experiment and data-taking must occur at the Laboratories, the design, building, and testing of equipment, and the extensive data analysis required to yield physics results is often most profitably performed at the Universities. This approach has become increasingly difficult, however, for several reasons. First, the computational facilities of the Universities have not kept pace with evolving needs, forcing many groups to reside at the Laboratories long after data-taking is complete as they perform the analysis at Laboratory facilities. Several groups have managed much of their analysis at home institutions very efficiently, using small, but powerful DOE- or NSF-supported computers. The policy of distributed computing, where it is efficient and at a level that is in balance with the major facilities necessary at the Laboratories, should be encouraged. Secondly, the technical complexities of each experiment have grown to such an extent that the size of a typical team of physicists from a University and the time required for the pursuit of an experimental program have steadily grown. Roughly one-third of University research support is used for faculty salary supplements and another one-third for non-faculty personnel, principally post-doctoral fellows. The remaining support available for technical development has dwindled as indirect costs charged by the Universities and the costs of travel have soared.

One recent encouraging development occurred in the handling of PEP equipment funds. A significant fraction of the monies, after being allocated to each approved experiment, were channeled directly to the University groups; this enabled them to participate fully in the construction of the detectors.

While at the present time the bulk of support for High Energy Physics comes from DOE, it should be pointed out that the NSF role in supporting the University groups is vital. In a field such as High Energy Physics, where diversity of ideas and approaches is crucial, diversity and flexibility of sources of support are essential. Central participation by the NSF in the support of University-based high energy science provides this diversity and flexibility. Through its responsibility for the health of basic science in the U.S. and its special mission to nurture scientific education, the NSF is particularly well-suited to play an important role in particle physics at the Universities. At a time of high scientific opportunity in this most basic of sciences, increased NSF support for University-based High Energy Physics is urgent and will pay high dividends.

Fermilab

The 400 GeV fixed target program at Fermilab, with its wide variety of secondary beams, provides opportunity for many different types of experiments. In addition to the 106 experimental physicists employed by the Laboratory, approximately 750 physicists from 135 other institutions perform experiments there. While the physics program is still at the forefront of High Energy Physics research, it has now reached a stage where many of the experiments require a systematic approach utilizing elaborate detectors and extended periods of running time. The execution of experiments has become increasingly difficult, however, due to a shortage of operating funds. The shortage is aggravated by the large R&D expenditures required for development of the Energy Saver. The number of weeks of beam operation per year has decreased steadily from approximately 40 during 1973-1977 to 26 in 1980. In addition, a shortage of detector electronics, rigging, engineering and technical assistance to experimenters, and manpower to install apparatus, has increased the time required to assemble and test experiments. Other cutbacks have also been necessary: the Meson Laboratory, which includes more than 30 percent of Fermilab's facilities, was shut down for about one year, and the 15-foot bubble chamber, a unique facility in this country, has repeatedly been shut down to save money and manpower. The net result is that the number of experiments simultaneously using the beams has decreased from 12-13 during 1976-1978 to 5-9 during 1979-1980. Numerous experiments have been delayed one-to-two years due to lack of beam or adequate support. This is frustrating for senior physicists and is very demoralizing for graduate students and research associates. Further frustration is produced when experiments, first initiated in this country, are outclassed by similar experiments in Europe which proceed more rapidly with superior instrumentation. Fermilab continues to produce excellent physics in the face of these difficulties, but the delays and frustration may severely compromise the attractiveness of the field to young physicists.

SLAC

The community of experimenters at SLAC has grown rapidly in recent years following the approval of the first round of PEP experiments. From a total of approximately 120 active experimental physicists at the laboratory prior to 1977 (including approximately 60 from SLAC and Stanford University), the community in residence has grown to around 270, with 210 coming from outside University groups. The institutions involved number 53, and include groups from Canada, Great Britain, France, Japan, and other countries.

The overall utilization at the laboratory has shrunk steadily over the past 10 years. This is generally a reflection of the pressure on operating funds in the U.S. program and, in recent years, of the need for operations money to prepare for the PEP experimental program. The main brunt of the cutbacks has fallen on the linac physics program, which is now down to 20 percent effective utilization, although the e^+e^- colliding beam physics at SPEAR has also been substantially restricted.

The laboratory has responded in various ways to the reduced operating flexibility. The general maintenance of plant and level of services have been cut. Space is very limited. In addition, the laboratory has instituted programmed "pauses" in some areas and closed down other facilities entirely. The policy of the laboratory has been to approve only those experiments which can be supported well enough to proceed effectively. This has resulted in approval of fewer experiments (for both SLAC and University groups) but satisfactory support and running time for those approved.

BNL

The AGS at Brookhaven has been one of the most successful accelerators; after 20 years of operation there is still a community of 30 physicists within the Laboratory, and approximately 160 users from other institutions who perform experiments at this facility. Thirty-four experiments have received beam time since January 1978. An important feature of the program is that it is still possible to perform interesting experiments with relatively modest detectors and small groups of people. The utilization of the AGS is low with only 25 weeks of operation per year. The operations program has been hit hard recently by a 60 percent increase in power costs. Typically, five-to-six experiments and two test beams are in simultaneous operation; the test beams are in very great demand and typically service 25 groups per year. The availability of electronics, rigging, and other support is limited and often leads to delays in the execution of experiments. The program has been cut back by closing down the 7-foot bubble chamber, delaying the construction of two new facilities, and canceling the development of a rapid-cycling, high resolution bubble chamber. Any further cuts in the AGS operation or support of experiments will further strain the program.

Cornell

Supported currently by the NSF, Cornell has built and operated a series of accelerators over a period spanning more than 30 years. Most recently, the 12 GeV electron synchrotron was shut off for 2 years to permit the construction of CESR, an 8 GeV x 8 GeV colliding beam storage ring. CESR began operation for physics in the fall of 1979 and now serves approximately 100 users from 10 institutions. Services provided to users are minimal and appropriate utilization of this unique facility will be difficult with the present level of support.

Summary

In summary, the utilization of existing accelerators is significantly lower than the optimal level, and support of University groups is marginal. The High Energy Physics program continues to produce excellent physics in spite of these difficulties but would benefit greatly from increased support.

SECTION VI

LONG RANGE RESEARCH AND DEVELOPMENT

The Subpanel has given some consideration to issues in long range R&D both for detectors and for accelerators.

LONG RANGE DETECTOR R&D

Role of Detector R&D

The advances in accelerator facilities in the last decade have been accompanied by a remarkable set of advances in the art and technology of particle detection and measurement. These include the development and universal application of on-line computer control and data acquisition for High Energy Physics; the development of multiwire proportional and drift chambers; the development of calorimetric methods for particle energy measurement; and the development of a number of large, multicomponent, integrated detection systems.

Although the accomplishments of the field, to date, have provided high energy experimentalists with the tools to carry out significant research on the new accelerator facilities that will come into operation in the 1980's, it should be realized that there are research areas of great importance which cannot be addressed without further advances in detector technology. Furthermore, even for problems which can be attacked within the current state of the art, significant improvements in the acquisition rate and quality of experimental information could be achieved by improvements and innovations in detector technology. It is worth noting that these comments apply, albeit to a lesser degree, to research with our existing accelerators.

Opportunities for Detector R&D

At the risk of some oversimplification, the problems facing the detector development of the coming decade can be grouped into two general areas. First, it will be necessary to deal with very high rates of detection, both to handle real events and, perhaps as importantly, to be able to reject background events which may vastly outnumber some special events of interest. Secondly, it will be necessary to have the capability to simultaneously detect and record large numbers of particles produced in the same event.

The list below includes some areas of detector technology which are important candidates for further R&D. This list is meant to be suggestive and certainly is not complete. It is useful to consider the detector field in terms of two general categories -- detector components and detector systems.

(1) Detector Systems

(a) Design of integrated, multi-component, large solid angle detector systems is an important element in the research at colliding beam facilities, and has increasingly become an important aspect of fixed target research.

(b) Data Acquisition Technology - To utilize the high data rates available at fixed target accelerators and in some colliding beam facilities, a great deal of innovative work remains to be done including such matters as hierarchical trigger processors, on-line data reduction, detector system-trigger and computer system interaction, etc.

(c) Data Reduction Technology - This includes off-line computer software (and hardware -- special processors, etc.). This area includes the development of algorithms, system emulators, Monte Carlo methods, etc., required to process efficiently and economically the data produced by high energy experiments.

(2) Detector Components

(a) Particle Identification - The development of various methods of measuring the mass of individual particles via combination of momentum, velocity, and/or energy measurement is an important area. Cerenkov methods, ionization energy loss methods, and shower development methods are among the promising approaches which have been and will continue to be pursued.

(b) Particle Tracking - The need for accurate, rapid registration of charged particle trajectories has been present in High Energy Physics experimentation for many years. It continues to be one of the pacing elements of detector design. Multiwire chambers and drift chambers have developed to a high state in recent years but they will have to be pushed to their fundamental limits to utilize efficiently the new accelerators. Completely new techniques (to high energy physics) such as arrays of "micro" solid state detectors, perhaps produced on the same "chip" which contains their data acquisition electronics, offer significant improvements in accuracy and data rate but require much basic R&D.

(c) Calorimetric Energy Measurement - The calorimetric method (total energy loss measured via sampling) for particle energy measurement has become an important tool in high energy physics research. The method is still in a state of rapid evolution and should and will be the subject of continuing R&D.

(d) Ultra-High Resolution Detectors for Short Lifetime Measurements - The full study of the physics of the new (charm and beauty) heavy quark systems requires the development of new high resolution detectors capable of resolving complex production and decay events with a spatial resolution of a few microns, with electronic trigger and high data rate capability. Promising avenues have been started -- small rapid cycling bubble chambers, and high pressure streamer chambers -- but this field is just beginning.

Conclusion

Detector research is, to some degree, carried on by all high energy physics experimentalists. However, the long range, basic developments which are discussed here are carried out by a relatively small number of Laboratory and University groups. These efforts should be encouraged and supported by the Laboratory and agency administrators.

Long Range Accelerator R&D

The Subpanel has considered the report of the HEPAP Subpanel on Accelerator R&D and endorses their report enthusiastically. The Executive Summary of the report of that Subpanel, which gives its conclusions and recommendations follows.

> Accelerator Research and Development has always been an integral and important part of elementary particle physics research. The physics output has been paced, to a large extent, by the improvement in performance of our accelerators. In the past 50 years, equivalent beam energies available to experimenters have increased by a factor of ten million as a result of accelerator R&D efforts. Not only has performance increased exponentially but unit costs have also been reduced significantly. Even in times of decreased real spending power for the field, significant energy increases were achieved.
>
> Despite our remarkable achievements in unit cost reductions, these reductions have not kept pace with the energy increases achieved. This has resulted in a limitation in the number and diversity of high energy accelerator facilities available to workers in the field. Further decreases in diversity would be necessary to continue frontier research were the pace of cost reduction produced by new accelerator technologies not to increase. This would have a serious negative impact on the scientific productivity of U.S. elementary particle physics. The challenge is clear: we must redouble our efforts to reduce unit costs sharply while

maintaining performance improvements through full exploitation of the potential of our current accelerator technologies and through invention of entirely new technologies and methods.

Historically we have spent approximately 10 percent of our operating resources on accelerator R&D activities. This includes both R&D applied to projects (RDAP) and long range R&D (AARD) which is not associated with a specific project. Today this fraction for all accelerator R&D is 14 percent, reflecting our heavy commitment to superconducting magnet development. The great majority of this is RDAP, as it needs to be . The Subpanel has determined that between 1 percent and 1-1/2 percent of operating resources are now devoted to AARD. If we are to meet the challenge of the future, more of our resources need to be devoted to this activity. We believe that an appropriate level for long range R&D would be up to about 4 percent of high energy physics operating resources.

It is obvious that no level of expenditure for AARD can guarantee that the historical trend in unit cost reduction can continue. Nevertheless, the Subpanel is convinced that without increased effort on AARD significant unit cost reduction is most unlikely. Thus we conclude that increased effort on AARD is an essential investment for the community at this time.

Recommendations

1. The high energy physics community should significantly increase its suppport for AARD. The Subpanel estimates, from an examination of significant areas of this research, that the level for AARD in the high energy physics accelerator field should be increased from about 1 percent to 4 percent of our current operating resources. This increase should be achieved within the existing administrative framework.

2. In reviewing accelerator R&D, the Subpanel has identified certain specific technical areas that should be emphasized in AARD. This list is not presumed to be complete, exclusive, or to indicate relative priorities. Rather, it is intended to be an indication of the challenges of AARD and to

underscore the importance of AARD to the field of high energy physics. This list of specific topics is:

(a) Development of very high field accelerator magnets and the evaluation of the practical limits of this technology. In view of the large scale of this enterprise and its uniqueness, the Subpanel recommends that this AARD effort be carried out as a collaborative effort among the laboratories having capability in this area. The Subpanel feels that this development of high-field magnets should be focused toward a specified accelerator goal.

(b) Development of liquid helium refrigerator systems with goals of improving efficiency and reliability and providing operation at reduced temperatures.

(c) Theoretical and experimental exploration of the limits to microwave linac gradients and to the peak powers that can be delivered in the S- to X-band regions.

(d) Basic physics and device development in superconducting rf accelerators.

(e) Theoretical and experimental studies of basic accelerator phenomena, particularly the beam-beam interaction and other performance-limiting phenomena.

(f) Search for and preliminary development of new accelerator schemes with high performance potential such as laser accelerators or other devices using ultra-high peak power with or without collective effects.

(g) New techniques and devices for manipulating very high power and/or very high energy beams.

(h) The general problem of increasing the brightness of particle beams with emphasis on cooling high energy beams.

(i) Development of new beam diagnostic techniques and devices.

3. Laboratory and University managements, aided by the agencies, should take specific measures to make participation in accelerator R&D practical and more desirable for high energy physics theorists and experimentalists not now so engaged. This could include payments of salary for sabbatical leaves and leaves of absence; and on-the-job training through seminars, summer schools, and the use of specially assembled tutorial and review materials. Also included could be expansion of University-Laboratory collaborations.

4. Laboratory and University managements, aided by the agencies, should take specific measures to facilitate cross-fertilization from other fields such as Plasma Physics, Lasers and Materials Science, and other accelerator-related activities. This could include long-term visits by outstanding individuals in these fields for the purpose of working directly with Laboratory staffs, giving tutorial seminars or lectures series, or participating in extended topical workshops.

SECTION VII

INTERNATIONAL COOPERATION IN HIGH ENERGY PHYSICS

The fundamental importance of High Energy Physics research is exemplified by the fact that many countries have chosen to support major efforts in the field. The Subpanel comments briefly on some of the collaborative aspects between these efforts and the U.S. program.

These international collaborations have benefited all participants. Not only has the scientific research been enhanced, but the intellectual exchange between the U.S. high energy physicists and their counterparts in other countries has often been the leading edge of broader scientific and cultural exchange.

For many years there has been collaboration on an experiment-by-experiment basis at U.S. and European facilities. United States groups have, in collaboration with European groups, carried out experiments at CERN accelerators and, individually, and in collaboration with European groups have carried out experiments at DESY. A similar pattern exists for the use of U.S. facilities by European groups.

The U.S. and Japan have also had a long-standing program of collaboration along the lines discussed above. Recently, the U.S. and Japan have enlarged this program to include a substantial and more formal program of collaboration in High Energy Physics which includes both physics experimentation and collaborative efforts in accelerator research and development. This program has added support, as well as collaborators, and has significantly enhanced the joint projects which are underway.

The U.S. and the U.S.S.R have had a long-standing program of collaboration in the fundamental properties of matter. The major component of this effort has been the program in High Energy Physics. This program is continuing but, for the present, at a reduced level.

Finally, we note that in the past year a substantial collaboration has developed between the U.S. and the People's Republic of China (PRC). Under the terms of this collaboration, U.S. scientists are providing design and review assistance for a 50 GeV proton synchrotron to be constructed in the PRC. In carrying out this collaboration a number of scientists and engineers from the PRC are in residence at leading High Energy Physics institutions in the U.S. In addition to the scientists and engineers specifically concerned with the synchrotron, a number of other experimentalists and theorists have also come to the U.S. for extended periods of study and research. The Subpanel notes that the PRC included the development of High Energy Physics among its top national priorities for the coming decade.

SECTION VIII

THE EUROPEAN PROGRAM IN HIGH ENERGY PHYSICS

The European program in experimental High Energy Physics is centered at CERN and DESY. CERN has 3,600 staff members, a yearly budget of approximately 600 MSF, and roughly 1,800 high energy physicists are involved in the program. The corresponding numbers for DESY are a staff of 1,100 and a yearly budget of approximately 130 MDM. Although DESY is a national laboratory, it is exploited internationally with groups from 11 nations and a total of 400 physicists participating in the program.

At CERN the activities are centered around the fixed target operation of the SPS. The $p\bar{p}$ colliding beam option is scheduled to start operation in the second half of 1981. At present a strong and vigorous program is being carried out at the ISR. However, in order to free funds and personnel for the LEP project, ISR is likely to be phased out with 1983 as the last year of operation. CERN has also a strong program in medium energy physics fed by the PS. Furthermore, CERN has decided to construct a low energy $p\bar{p}$ facility LEAR based on technology developed for the $p\bar{p}$ project. For its future program, CERN, strongly supported by the European Committee for Future Accelerators (ECFA), has decided to construct a large electron-positron colliding ring LEP. The formal proposal to construct LEP will be submitted to the CERN Council in June 1980 with approval expected no later than June 1981. It is intended to start the experimental program during the fifth year of construction.

The program at DESY is at present based on the electron-positron colliding rings DORIS and PETRA with the synchrotron used as an injector and to provide test beams. A study of HERA, a large electron-proton colliding beam facility, with a maximum center-of-mass energy of 314 GeV has been submitted to the German Government. If the project is approved in 1981, the electron ring will be finished in late 1986 and the proton ring in late 1988. HERA will be open to all experimental groups both inside and outside of Europe.

The following discusses the facilities mentioned above in more detail.

(1) CERN

(a) Fixed target operation of the SPS - This machine will be the mainstay of the experimental program at CERN and is hence given high priority. The maximum energy is 400 GeV; however, it is likely that it will be upgraded to 450 GeV with a long spill and maybe to 470 GeV with a short spill by 1982. The intensity, with two batches injected from the PS is now about 1.5×10^{13} protons per pulse (duration approximately 10 s) or 2×10^{18} protons delivered on target in a 10-day period. It will be increased to 3×10^{13} protons per pulse once additional radiofrequency power is available in the SPS in 1981.

The SPS has a very strong experimental program with emphasis on the experiments in the neutrino and the muon beams in addition to the experiments producing dimuons and heavy flavored hadrons. A total of 35 experiments are currently active.

A shutdown, lasting about one year, to install the $p\bar{p}$ facility will start in June 1980. After start-up the available time will be shared between the fixed target and the $p\bar{p}$ running. It is expected that of the order of 25 percent of the available time will be used on the $p\bar{p}$ program.

(b) The $p\bar{p}$ colliding beam project - A considerable effort has gone into this project with the aim of producing $p\bar{p}$ collisions at 540 GeV in the center-of-mass with a luminosity of about 10^{30} cm^{-2} sec^{-1}. The facility will have two interaction regions and is scheduled to come into operation in late 1981. In this project 26 GeV/c protons from the PS are used to produce 3.5 GeV/c antiprotons which are stored and stochastically cooled in an accumulator ring. After accumulating a sufficient number of antiprotons (it takes about 12 hours to accumulate 3×10^{11} $\bar{p}$), the cooled antiprotons are reinjected into the PS, accelerated to 26 GeV (above the transition for the SPS) and transferred, presumably in three bunches, to the SPS. Work has progressed well and is now far along on several aspects of the project. The initial cooling experiment (ICE) was completed successfully in 1978 for stochastic cooling and in 1979 for electron cooling. The accumulator ring is under construction and will be ready for testing toward the end of the year. A series of machine tests in the SPS has succeeded in storing high intensity bunches for periods up to 12-18 hours. The emittance growth observed is consistent with that expected from multiple gas scattering. The civil engineering work on the two experimental areas is well underway. To revamp the machine for $p\bar{p}$ collisions, finish the civil engineering, and install the experiments is a major undertaking. To this end the SPS will be shut down for approximately one year starting June 16, 1980, with the fixed target operation scheduled to resume in June 1981. A total of five experiments have been accepted. Of these, two are large facility type detectors which will be ready to take data at turn-on.

(c) ISR - At present the ISR has a strong experimental program with nine active experiments. The machine performs well with peak luminosities of about 6×10^{31} cm^{-2} s^{-1} in the low-beta intersection region at 62 GeV in the center-of-mass. A simple transfer line has been constructed which makes it possible to operate the ISR for proton-antiproton collisions with luminosities up to 10^{30} cm^{-2} s^{-1} in intersection 8 (which will be equipped

with superconducting quadrupoles to produce a low beta) and about 10^{29} cm^{-2} s^{-1} in the normal interaction regions. This will make it feasible to carry out a proton-antiproton program starting in 1981 using existing detectors.

It is intended to give high priority to the ISR and its program before the operation must be phased out in 1983 to free funds and manpower for the LEP construction.

(d) Low energy physics - The medium energy program is fed by the PS. The emphasis is on antiproton and kaon physics at very low energies. As a spinoff of the $p\bar{p}$ project, CERN will construct a low energy antiproton facility LEAR. Cooled antiprotons from the accumulating ring will be reinjected in batches into the PS, decelerated to 0.6 GeV/c and transferred to a small storage ring LEAR. On the average, $\geq 10^6$ $\bar{p}$ s^{-1} will be available. In LEAR the stored antiprotons can either be accelerated to 2 GeV/c or decelerated to 300 MeV/c. With some extra cooling and an ISR-type vacuum, this could be reduced to 100 MeV/c. LEAR has different modes of operation. It can be run as a stretcher ring with 1000 s spill time delivering an external beam of 10^6 $\bar{p}$ s^{-1}. It can also be run as a storage ring, either as a minicollider ($p\bar{p}$ at 4.4 GeV) or as a source of $\bar{n}$ via the charge exchange reaction $p\bar{p}$ $\bar{n}n$. Compared to present external beams LEAR represents a large step in beam purity, intensity, and duty cycle.

(e) LEP - The proposal to construct a large electron-positron colliding ring at CERN has been well received. The formal proposal will be submitted to the CERN Council in June 1980 and approval is expected no later than June 1981. Phase 1 of LEP is now defined as an e^+e^- machine with a circumference of 30.4 km capable of reaching at least 100 GeV in center-of-mass. It will initially have four interaction regions but the access tunnels to the remaining four are included in Phase-1.

The program calls for collisions in the fifth year of construction, i.e., 1986. This earlier date, compared with the previous schedule, has been made possible by making a lean design including only components needed to reach Phase-1. It has also been decided to use the SPS as an injector instead of building a 22 GeV electron synchrotron. This can be achieved by lengthening somewhat the SPS cycle without any other interference with the fixed target program. (The SPS as an injector is, of course, incompatible with the $p\bar{p}$ mode of operation.)

The energy of LEP can later be extended to 172 GeV with normal conducting cavities or 260 GeV with superconducting cavities. CERN, in collaboration with other institutions in Europe, has launched a strong effort to produce superconducting cavities at 500 Mhz, a frequency suitable for large electron storage rings. Initial results have been very encouraging. The next step will be to test these cavities in operating conditions at DORIS -- i.e., high power test, synchrotron radiation, higher order mode losses, and so on.

It should be noted that, in principle, the protons in the SPS can collide with the electrons in LEP in one intersection region. In the parasitic mode this yields luminosities of the order of 10^{29} $cm^{-2}s^{-1}$, and the electrons presumably will not be polarized. Special dedicated runs with ep might lead to luminosities of the order of 10^{32} $cm^{-2}s^{-1}$. However, since this would stop the entire High Energy Physics program at CERN except for one interaction region, it is not expected to be a prominent mode of operation.

It is clearly premature to speculate what the next step at CERN will be. However, it seems rather natural, as suggested by many people, to install a superconducting proton ring for 5 to 10 TeV protons depending on the status of superconducting magnet technology. This would result in a rather nice kinematical range for ep or $p\bar{p}$ collisions.

(2) DESY

(a) DORIS - The e^+e^- annihilation process can be studied between the mass of J/psi and the mass of the upsilon using DORIS. DORIS has an active experimental program based on complementary detectors. A large facility type detector, ARGUS, is now under construction and is expected to start data taking in late 1981.

A program to upgrade the luminosity in the single ring mode for high energy operation is underway. Higher luminosities will be reached by installing "mini-beta" sections (beta = 3 cm), an electrostatic plate system to separate the beams at injection and an improved sextupole correction system.

DESY, in collaboration with CERN and Karlsruhe, is engaged in a program to produce and test superconducting rf cavities. Karlsruhe has now produced a 500 Mhz cavity with an accelerating field of about 3.6 MV/m. The cavity is expected to be installed in the DORIS ring later this year.

(b) PETRA - PETRA has reached its design center-of-mass energy of 38 GeV. The peak luminosity at 35 GeV in center-of-mass energy is around 5×10^{30} $cm^{-2}s^{-1}$ with an integrated luminosity of about 60-100 nb^{-1} a day. This should be compared to the predicted peak luminosity of about 2×10^{31} $cm^{-2}s^{-1}$ for two bunches on two bunches colliding. Mini-beta sections will be installed in all four interaction regions. The installation will be completed towards the end of 1980 and is expected to increase the present luminosity by a factor of three-to-four.

PETRA has a full experimental program with five large detectors installed in the four experimental areas.

Installing conventional rf cavities in the two remaining long straight sections (leaving room for two new experiments) and doubling the available rf power from a 4 MW to 8 MW would enable PETRA to reach 45 GeV in the center-of-mass. Such a program, or part of it, will be carried out during the next two to three years. To increase the energy beyond 45 GeV requires superconducting rf cavities. It might then be possible to reach 60-70 GeV in center-of-mass energy.

(c) HERA - DESY has proposed to construct a large electron-proton colliding beam facility, HERA, designed to collide either electrons or positrons of 30 GeV nominal energy with protons up to 820 GeV yielding 314 GeV in the center-of-mass and a maximum momentum transfer squared of 98,400 GeV^2. This is equivalent to an electron beam of 52 TeV on a stationary target. The design luminosity of around 10^{32} $cm^{-2}s^{-1}$ is sufficient to explore the region up to 40,000 GeV^2 with both charged and neutral currents. This machine has received strong support from the German high energy community and a project definition endorsed by ECFA has been forwarded to the German Government. A committee has been appointed to evaluate the merits of HERA and other large scientific projects and to establish a list of priorities. The committee is expected to report back to the Government in the fall of 1980.

A program to construct superconducting magnets based on the technology developed at BNL and Fermilab is underway. The construction time for the electron ring is estimated at 5 years and for the proton ring at 7 years.

It is in principle possible, at little extra cost to study e^+e^- collisions using the HERA electron ring. With the rf foreseen for ep operation such studies could be carried out at center-of-mass energies up to 70-75 GeV. Note, that in the standard model e^+e^-, annihilation is dominated by the Z^o-pole in this energy region.

SECTION IX

EVALUATION AND RECOMMENDATIONS

Evaluation

(1) Overall Program

The U.S. High Energy Physics effort, supported for more than 30 years by taxpayers of the U.S. through the Federal Government, has maintained over the years a diverse and exciting program of research and discovery. The DOE, as the main funding agency, supports the three large national accelerator Laboratories, as well as University research groups, while the NSF provides independent support for University groups and also the large, but still University-based facility, CESR. Existence of two funding sources gives the U.S. program an important dimension of flexibility and possibility for innovative alternatives.

The program has a broad spectrum of research with existing accelerators and storage rings at the three major Laboratories and Cornell: a 400 GeV fixed target program with proton-initiated neutrino, muon, photon, and hadron beams at Fermilab; 33 GeV electron-initiated photon and electron beams on fixed targets, and the e^+e^- storage rings SPEAR (3-8 GeV) and PEP (10-36 GeV), at SLAC; the 30 GeV proton-initiated fixed target program of neutrino and hadron physics with the AGS at Brookhaven; the 8-16 GeV e^+e^- storage ring CESR at Cornell. In the next four years, the Energy Saver/Tevatron project at Fermilab will add exciting new physics at TeV energies. By 1986, ISABELLE is expected to provide proton-proton interactions from 60 GeV to 800 GeV. The U.S. program will then be unmatched in the physics of proton-initiated interactions over the whole range up to 2 TeV center-of-mass energies.

The present program is very ambitious with a dependence on new superconducting technology having attendant high risks but very high benefits. Construction using the new large-scale technology of hundreds of superconducting magnets and their associated cryogenics, stretched over several kilometers of tunnel, is a major challenge. Existing budget levels are barely adequate to provide the necessary R&D to sustain orderly construction of both ISABELLE at BNL and the Energy Saver at Fermilab. Each Laboratory has different problems to overcome, with Fermilab further along. It has become clear in the doing that the difficulty of the necessary superconducting magnet technology was underestimated. The problems are greater and more numerous than anticipated. They are largely being solved, step by step, but it must be realized that these accelerators are devices more complicated by far than previous machines. The new facilities will require the concerted efforts of devoted scientists

and engineers at both Laboratories to bring them to reliable operating status for physics. The successful completion of these facilities is the responsibility of the entire national community.

(2) Operations

With Fermilab involved in a major and highly technical construction project, the Energy Saver, the resources of the Laboratory are focused in such a way that the utilization of the existing 400 GeV accelerator for physics research is presently below 50 percent, the generally agreed-upon minimum acceptable level for a viable ongoing program. As the Energy Saver nears completion, further pressures on the 400 GeV program can be foreseen. Every effort should be made to sustain and even augment the 400 GeV running for physics while the Energy Saver is under construction.

Major efforts at the forefront of e^+e^- physics exist at Cornell's CESR and at SLAC with PEP just operational. These operating programs are devoted to exciting and important physics and must be supported vigorously in the coming years.

Fixed target programs exist at SLAC and BNL, as well as at Fermilab. A considerable diversity of physics at lower energies is encompassed by these efforts, although financial pressures have reduced the SLAC linac program to operation of the multiparticle spectrometer (LASS) and the rapid-cycling bubble chamber and have reduced physics running at the AGS to 25 weeks or less per year.

(3) Construction

For the immediate future at Fermilab, the building of the Energy Saver at 500 GeV, followed by the fixed target and $\bar{p}p$ collider at TeV energies, must have the highest priority. The energy-saving aspects of the Energy Saver are important in their own right, but the unique physics opportunities are what force the priority. A fixed target program at 1 TeV will have the highest energy neutrino, muon, photon, and secondary hadron beams in the world, and the $\bar{p}p$ collider will be a unique facility providing 2 TeV center-of-mass energy at moderate luminosity.

At BNL, the construction of ISABELLE must proceed as rapidly as possible. The physics goals of ISABELLE, utilizing proton-proton collisions at roughly 800 GeV cms energy with very high luminosity, are extremely exciting. Detailed study of charged intermediate vector boson production and decay is only one of the prizes awaiting ISABELLE. The present difficulties with magnet manufacture, a serious and unexpected setback in the light of earlier successful prototype superconducting magnets, can only be overcome by an orderly and vigorous R&D program.

(4) Research and Development

The continuing breadth of the field demands ongoing research and development in both advanced accelerator technology and detector development. Some of this R&D is project-specific, with amounts that are governed by relatively short-term requirements. Past experience shows that these needs have been seriously underestimated.

Long-range R&D funds must also be provided on a continuing basis. Past practice has been to allot roughly 1 percent of operating money to this area. The present difficulties with superconducting magnet technology show that economies of effort and funds and more expeditious construction can be realized in the future if more extensive R&D occurs early in the technological innovations necessary for new facilities. This can be accomplished at present funding levels by a slower pace, but momentum is lost and progress delayed by such a solution.

Long-range R&D should be supported at a level approaching 4 percent of the operating budget, as recommended by the HEPAP Subpanel on Accelerator R&D. In the area of detector R&D, it is important to have the involvement of University groups because of their different perspectives and, therefore, possibly different new ideas on the solutions of specific detection problems.

(5) University-Based Research

University-based research groups comprise the large majority of high energy physicists in the country. The vigor of their efforts reflects directly on the success of U.S. high energy physics. In recent years, they have suffered from loss of momentum caused by the reduced operation of existing accelerators, lack of adequate equipment funds, serious increases in necessary travel costs, and erosion of their efforts by inflation, as well as increased overhead charges. The health of the University groups is the health of the field. High Energy Physics will flourish in the future because of the graduate students who now enter the field through the Universities. Adequate support of these groups is essential.

(6) New Opportunities

In the past decade, immense progress in our understanding has come from experiments with electrons. The approximate scaling in deep inelastic scattering of leptons on nucleons, the new families of heavy particles uncovered in electron-positron storage rings, the hadronic (quark and gluon) jets found at higher energies, all demonstrate the power of electromagnetic interactions as an

initiating mechanism. The present and near future program has electrons as a significant component, but at energies below 40 GeV. Because of the anticipated presence of the Z^o at 80-100 GeV, with its associated cornucopia of physics, there is great interest and excitement in going to higher energies with e^+e^-. Similarly, the discoveries in lepton-hadron interactions point the way towards significant new advances in our knowledge of the fundamental interactions with the extremely large energy and momentum transfers available in electron-proton colliding beams.

The overall balance of the U.S. program clearly requires consideration of e^+e^- and ep capabilities at higher energies. Specific suggestions before the Subpanel for new facilities confirm this general view. It appears to be premature to make a decision now on the exact direction such efforts should take. Maturing initiatives can be considered within the next one or two years. Among these the Subpanel is pleased to acknowledge an emerging Canadian initiative for an electron ring at a proton laboratory.

The search for proton decay, predicted by unified theories, is currently underway in the non-accelerator part of the program. Within two or three years, we should learn whether or not the predictions are correct. A positive result will demand a vigorous exploitation of the new physics. The overall program must retain the flexibility to address such exciting opportunities.

In the longer range, there are other opportunities. Clearly, still higher energies will be important. Fermilab has a 5 TeV proton accelerator in mind. The prospects of particle beams at several TeV and of 10 TeV center-of-mass energy $p\bar{p}$ collisions are most attractive, but still remote in time, after the establishment of reliable large superconducting accelerators. SLAC is developing ideas for extremely high energy electron linacs and has discussed two large linacs that give 350 GeV electrons and positrons colliding head on. This intriguing idea evidently requires extensive long-range R&D before a firm conceptual design can emerge. Other projects, perhaps very different from either of these will surely emerge as the field continues and benefits from experience with the new facilities.

Recommendations

The total High Energy Physics program involves large and complex construction projects at two accelerator Laboratories and just completed projects at the others, with underutilization of existing accelerators and warning signs of trouble if present trends continue. The Subpanel considered carefully the implementation of the ongoing program within the constraints of the present

constant effort plan (DOE/OMB Long Range Plan) and also within a plan calling for a 15 percent increase of effort beginning in FY 1982. The recommendations that follow are within the framework of the DOE/OMB Plan. Following those recommendations are some recommendations assuming a 15 percent increase. Finally, a specific recommendation is made on the SLC, the only new construction proposal before the Subpanel.

Recommendations and Comments (base level/constant effort)

Recommendation 1:

The Fermilab 400 GeV accelerator, the newly commissioned PEP storage ring, and the CESR facility at Cornell must be utilized as fully as possible to exploit for physics the large investments already made.

Recommendation 2:

Construction of the Energy Saver and of ISABELLE must proceed with all deliberate speed. Necessary R&D funds must be provided to assure their success.

Recommendation 3:

The Tevatron I and II projects at Fermilab should proceed together. This requires authorization and initiation of Tevatron II in FY 1982.

Recommendation 4:

Options for the future must be kept open. Of great importance are the research and development of the technologies of very high charge-density bunches with low emittance in linear accelerators, very high-field superconducting magnets, and superconducting rf cavities.

Recommendation 5:

University-based groups should receive increased support to assure vitality of their efforts on immediate experimentation and also on detector development for the future.

Recommendation 6:

Accelerator studies and technical research should begin immediately toward the goal of starting the construction of a very large accelerator during the second half of this decade.

Recommendation 7:

A Woods Hole Subpanel or equivalent should be convened in the future to assess the status of construction, of new developments and of new opportunities, probably in 1981 and certainly by 1982.

Comments on the Recommendations:

(1) The constant effort DOE/OMB Plan, at its present level, is very tight, particularly with the two ongoing construction projects. Operations, especially the Fermilab 400 GeV program, cannot be permitted to decline further. CESR and PEP, the front-line U.S. efforts in e^+e^- interactions, must be supported strongly. Exciting physics remains to be done at SPEAR. Inevitably, the program, with a very constrained budget overall, cannot afford to support all components equally. The lower energy, fixed target programs at the AGS and SLAC linac have lower priority. Nevertheless, these programs contain a number of very important experiments providing opportunities for considerable numbers of University users.

(2) As discussed in Parts 1 and 3 of this Section, the development and implementation of superconducting magnet technology at Fermilab and BNL is a major challenge. The Energy Saver will be the first large accelerator with superconducting magnets. It is extremely important to have this project completed as soon as possible to prove out the very complex technology. Adequate R&D funds must be provided to meet exigencies. The same applies to ISABELLE at BNL.

(3) Under the DOE/OMB Plan, beyond Tevatron II there is simply no money for new construction to start in FY 1982. This fixed target program, which will be unique in the world, has overriding priority for construction funds. The facilities at Fermilab, after completion of the Energy Saver-Tevatron I-Tevatron II project, will support an extremely strong and diverse research effort in High Energy Physics.

After significant progress toward completion of the large-scale construction at Fermilab and BNL, new initiatives can be contemplated even under the plan of constant effort. The subsequent Subpanels (Recommendation 7) will confront such issues.

(4) Even though the funding of new construction projects beyond the Tevatron must be deferred at this time, long-range R&D on advanced accelerator technology should be pressed vigorously. Power consumption alone is a compelling argument for the work on superconducting magnets and cavities. Successes in one or more areas would be immensely valuable in expediting U.S. efforts towards facilities of the future.

(5) The needs of the University-based groups are discussed in Part 5 above.

(6) The Subpanel knows of no fundamental impediments to the achievement of electron energies of several hundred GeV and proton energies of 10 TeV or more. The Subpanel is convinced that the scientific value of accelerators in such energy ranges is extremely high.

Accelerators that provide beams of such energies and collisions between such beams are certain to be expensive to build and to operate. Research to minimize costs and to exploit new avenues will be long and difficult. Sustained efforts should be started now if we are to be ready to start such a new project during the second half of this decade.

(7) Now is a difficult time for decisions. On the one hand, we have huge construction efforts with difficulties beyond the normal range. On the other hand, there are exciting physics opportunities involving electrons, but with uncertainties as to the specific facility to implement them. Substantial progress on the advanced accelerator developments should be achieved within the next year or two. The community will then be in a better position to decide on the future thrust of the field.

Modified Recommendations (15 percent increase in effort)

A 15 percent increase in the support level beginning in FY 1982 would make possible major improvements in the program. First of all, the underutilization of existing accelerator facilities could be redressed. The support of research groups, particularly in the Universities, could be made commensurate with the needs of a preeminent scientific program.

Even more important, there would also be an opportunity for new construction initiatives beginning in the first half of the 1980's decade. One very exciting possibility would be an e^+e^- collider operating in the energy range near 50 GeV x 50 GeV. Such a machine would be capable of producing the neutral vector boson (Z^o) in abundance, allowing a rich program to study its decay modes and restoring more balance to the future U.S. program which is currently aimed primarily toward hadron machines. As an example, the proposed SLAC Linear Collider (SLC) might provide one way of doing this physics at a cost which could be acommodated within the early part of the decade in the upper level budget. Alternatively, the Cornell group has proposed a superconducting rf cavity R&D effort aimed toward the construction of a 50 GeV x 50 GeV circular colliding e^+e^- machine. If the R&D effort were successful, construction of this machine might be initiated around FY 1984 and completed a few years later. It is worth noting that a machine with appropriately optimized luminosity in the center-of-mass energy range between the PEP upper limit and the Z^o mass might very successfully compete with the large European e^+e^- facility LEP. Yet another attractive possibility would be an ep colliding beam facility coupled to either ISABELLE or the Fermilab Tevatron. Possible ep facilities, being explored independently by a U.S. (Nevis Laboratory) group and by a Canadian group, were described to the Subpanel. Either would involve a 10 GeV electron ring, used in conjunction with a high energy proton beam at Fermilab.

It is very clear that the scientific balance and productivity of the U.S. High Energy Physics program would be greatly enhanced by the addition of a new facility dedicated to the physics objectives outlined in the e^+e^- and ep submissions made to this Subpanel.

RECOMMENDATION AND COMMENT ON THE SLC PROPSOAL

Recommendation

The Subpanel recommends that no decision be taken on the SLAC SLC proposal at this time.

Comment on the recommendation

The Subpanel studied the proposal to construct a linear collider system at SLAC starting in FY 1982. This is an innovative, detailed proposal that addresses a very important physics goal, the existence and properties of the neutral vector boson, Z^o. Furthermore it addresses research and development questions important to the extension of e^+e^- experimentation to very high energies.

Even though there are major new features in this machine (notably the transmission of small, intense bunches of electrons and positrons and a new regime of beam-beam interactions), the technical subgroup found no reason to doubt the claim that a luminosity in one interaction region of 10^{30} cm^{-2} sec^{-1} can be achieved within the proposed time scale and budget. Particularly attractive aspects of the proposed collider are the relatively low cost, the imaginative use of much well known technology, the realistic possibility of polarized beams; and, finally, its role as a demonstration project for possible very high energy linear colliders.

For the base funding level, there are simply no funds available for new initiatives at this time. For the higher funding level, the Subpanel recommends delaying a decision on the SLC, in spite of the very positive features just described, for the following reasons.

The SLC would most likely compete with the CERN LEP project for the physics of the Z^o. To be in a strong competitive position, SLC or any other U.S. e^+e^- initiative should have a high probability of achieving in a timely fashion luminosity comparable to or in excess of that of LEP in the Z^o energy region. Exploitation of some of the interesting physics at the Z^o may require luminosities in excess of 10^{31} cm^{-2} sec^{-1}. This is true, in spite of the relatively large total e^+e^- cross section, because the branching ratios for many interesting processes are expected to be very small. The proposal states a design luminosity of 10^{30} cm^{-2} sec^{-1}. The proponents, however, believe this to be conservative and discussed a number of factors which could raise the luminosity to 10^{31} cm^{-2} sec^{-1}; but detailed analysis was not provided.

As discussed elsewhere, there is great interest in e^+e^- physics between the high energy end of PEP/PETRA and the Z^0 region. The SLC may or may not be the optimal machine for this energy range. Yet, its approval now would naturally preempt another possibly more useful instrument. An example of an alternative machine has been described in a proposal for R&D funds by the Cornell group. There is a whole complex of advantages and disadvantages to each machine bearing on issues of cost, time scale, luminosity, number of interaction regions, energy spread, beam polarization, and relevance to future directions. In the process of making a decision on the SLC, it is important to consider carefully serious alternatives, such as the possibility of construction of a facility providing both e^+e^- and ep collisions. This should be done in a timely fashion. Nonetheless, a modest delay in a decision on the SLC project has the benefit of permitting a more complete airing of the technical issues and a wider involvement by the U.S. High Energy Physics community.

It will be appropriate to review this situation in one to two years as more information both on technical issues and future funding levels becomes available.

APPENDIX A

Charge to Subpanel

Within the context of changing worldwide high energy physics activities and opportunities, review of the status and prospects of the U.S. program, taking into consideration all aspects of the program, including:

- o Physics progress and achievement of scientific understanding.
- o Physics research programs at Universities and Laboratories.
- o Laboratory facility operations (including program scope, operating effectiveness, utilization levels and user research opportunities).
- o Technology R&D for accelerators and experimental facilities.
- o Progress on facilities recently completed or under construction.
- o Future construction proposals and possibilities.

Develop a general strategy and long-range plan for the U.S. program over the next decade and, in particular, assess the program balance required for physics research, facility operations, technology development, equipment and construction over the period from FY 1982 to FY 1987, and make specific recommendations for FY 1982; all under the following funding constraints:

1. The same guidance as that under which the program now operates; i.e., an average annual funding level for the U.S. program (DOE plus NSF) of $325M in FY 1979 dollars.
2. A funding constraint 10-15% higher than that above.
3. Are there important high energy physics activities which cannot be conducted on a timely basis within the above constraints? What are they are what would be the implications of adding or excluding them?

In an exploratory field such as high energy physics, major scientific achievement rests on the capacity to carry out research at the frontiers of knowledge and technology. Within each of the above funding constraints, assess the prospects and sufficiency of the strategy and long range plan to maintain a forefront U.S. high energy physics program.

APPENDIX B

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1980 Subpanel on Review and Planning

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