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HEPAP SUBPANEL ON THE U.S. HIGH ENERGY PHYSICS RESEARCH PROGRAM FOR THE 1990'S

APRIL 1990

U.S. DEPARTMENT OF ENERGY
OFFICE OF ENERGY RESEARCH
DIVISION OF HIGH ENERGY PHYSICS

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DIVISION OF HIGH ENERGY PHYSICS
WASHINGTON, D.C. 20585

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
LABORATORY FOR NUCLEAR SCIENCE
CAMBRIDGE, MASSACHUSETTS 02139

Dr. James F. Decker
U.S. Department of Energy
Office of Energy Research
1000 Independence Ave. S.W.
Washington, DC 20585

May 11, 1990

Dear Jim,

I am transmitting to you the report of the HEPAP Subpanel on the U.S. High Energy Physics Research Program for the 1990's. The report was submitted to HEPAP and discussed and endorsed unanimously by the Panel at its meeting in Germantown on April 23 and 24, 1990.

The Subpanel interpreted the three budget scenarios specified in its charge as referring to budget averages over the next ten years, with year to year fluctuations permitted. Without that interpretation the Subpanel would have been unable to recommend a strong program under any of the three assumptions. With that interpretation, it was able to recommend a productive but limited program for the constant budget case, and a somewhat enhanced program for the rising budget case.

Although the report does not present detailed budgetary or manpower scenarios, such scenarios were in fact constructed by the Subpanel in order to assess the limitations imposed by funding and demographic considerations. The Subpanel concluded that sufficient funding could be made available for the recommended program in the constant budget case, provided it is possible to deviate from the scenario average in the early years by a positive increment of 10% or less per year; a compensating decrement is projected during the second half of the decade. The Subpanel further concluded that there was adequate physicist manpower to carry out the recommended program, even in the increasing budget case.

The Subpanel reaffirmed that the highest priority in the U.S. HEP program is swift construction of the SSC and appropriate preparation for its optimal utilization. In the following, I would like to offer some comments on the Subpanel recommendations. Recommendations one and two are given the highest priority, in that order. The remaining recommendations are not ordered as to priority.

1. The Subpanel assigns highest priority in the base program to the immediate commencement and speedy completion of construction of the Tevatron Main Injector at Fermilab. The Tevatron collider is the premier U.S. high energy facility, and its full exploitation will keep the U.S. program a world leader for the rest of the decade.

The Subpanel considered how this upgrade could be funded and concluded that the program could find the flexibility to do so without cutting operating budgets provided the early year funding could be increased over the average. They point out that this has been done in the past - construction projects have usually required budgetary peaks.

2. The second major recommendation is for strong exploitation of the existing high energy facilities, to take advantage in a timely way of the many physics opportunities offered by them. Following the detector and accelerator construction and upgrades of the last decade, both in the U.S. and abroad, it makes no sense not to exploit them fully. These facilities offer major physics opportunities for the study of rare k decays, for top quark and other particle searches, and for polarization and other studies of Z^0 physics, to cite a few examples. Indeed, a failure to keep a strong base experimental program alive in the intervening years would seriously degrade the potential for a strong SSC program at the end of the decade.

3. The Subpanel recognized the great importance of e+e- physics. It endorses the physics aims of a B factory and recommends

a vigorous R&D effort to develop the design for such a facility. In the absence of a construction proposal for a B factory, however, it would have been inappropriate for the Subpanel to postpone its other recommendations, given the urgency of the other issues. Under the increasing budget scenario, the Subpanel did find that a B factory should be built once the technology is in hand. Under the constant budget scenario it could not so recommend, given its other recommended priorities. However, if and when the technology is in hand, HEPAP believes that the issue of a B factory should be examined again, with the hope that funds could be found to carry out its construction. I should again emphasize HEPAP's view, and that of the Subpanel, that e+e- colliders will remain an important tool of high energy physics and that they must continue to form an important part of the U.S program.

The remaining recommendations follow:

4. The Subpanel recommends significant enhancements in the support by the Department of Energy (DOE) and the National Science Foundation (NSF) of university groups in the areas of technical infrastructure and scientific manpower.

5. The Subpanel recommends that NSF substantially increase support for its HEP university groups, particularly for equipment.

6. The Subpanel recommends continuation of a vigorous program of R&D at Stanford Linear Accelerator Center (SLAC) for very high energy electron positron linear colliders.

7. The Subpanel recommends that the Division of HEP provide support for the SSC Laboratory physicists' basic research activities that lie outside the SSC project.

8. The Subpanel recommends that both non-accelerator and foreign-based experiments continue to be strongly supported.

9. The Subpanel recommends increased support for generic detector R&D.

The Subpanel was unable to recommend a viable program for the reduced budget case. If significant budget reductions must occur, the Subpanel urges that another Subpanel be convened to advise the DOE on specific actions to be taken.

HEPAP was strongly impressed with this report. The Subpanel clearly made an enormous and devoted effort to understand and clarify the opportunities, needs, and possibilities of the High Energy Program. The members of the Subpanel deserve the heartfelt thanks of the community.

Yours sincerely,

Francis E. Low

Francis E. Low
Chairman HEPAP

FL/en

Columbia University in the City of New York | *New York, N.Y. 10027*

DEPARTMENT OF PHYSICS 538 West 120th Street

April 20, 1990

Professor Francis E. Low, Chairman
High Energy Physics Advisory Panel
Laboratory for Nuclear Science
Massachusetts Avenue - Room 6301
Cambridge, MA 02129

Dear Francis,

Enclosed is the Report of the HEPAP Subpanel on the U.S. High Energy Physics Research Program for the 1990's. The Subpanel attempted to be true to the letter of our charge, and when this was in doubt, to be true to its spirit. How well we have succeeded in looking through our collective crystal ball into the next decade, HEPAP and the DOE should evaluate, but only time will really tell.

The Report comes from the dedicated and hard work of the many members of the Subpanel. Each and every person who served carried a large burden over a long period and gave unstintingly of their time and effort both before, during and after the Williamsburg retreat. I was personally enormously impressed with the effort, the care, and the wisdom of each individual.

The Report stands on its own. I would only add that the ordering of the first two recommendations represents their priorities; the remainder are not prioritized. The recommendations represent the Subpanel's consensus on how best to utilize the funding flexibility that presently exists in the program in order to assure productivity.

This flexibility, as discussed in Section V-D of the Report, is essential. It is important to keep in mind that a number of large experiments and modest construction projects have been undertaken over the last 10 years within the budget shown in Figure V-1 of the Report. Recent examples are the SLD, D-Zero, and L3 experiments, the BNL booster, the Fermilab computer upgrade, and the SLAC Final Focus Test Beam. Such information was used to estimate the flexibility that could be maintained over the next 10 years. As stated in the Report, the Subpanel attempted to identify the portions of the HEP budget historically used for the continuing evolution of the scientific program. Although these total funds are a small fraction of the HEP budget, they provide the flexibility for investment in the future that is crucial for progress in the field.

It was assumed that this amount, very crudely about 10 percent of the total budget, was already optimized and would not vary significantly averaged over the next decade. Without this continued renewal, the vitality of the field would dissipate very quickly.

You have before you the result of the best efforts by the Subpanel in planning for the next decade. At this time I would like to thank, on behalf of the Subpanel, the DOE secretarial staff who assisted us during an arduous period with much of the work. Their substantial help made the report possible.

Last, but not least, thank you for your personal support and help during this process.

Sincerely,

Frank Sciulli,
Subpanel Chairman

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EXECUTIVE SUMMARY

The entire community of high energy physicists looks expectantly to the Superconducting Super Collider (SSC) era. The SSC is the highest priority in the U.S. high energy physics (HEP) program, and physics at the SSC will increasingly become its focus. In this report, the High Energy Physics Advisory Panel (HEPAP) Subpanel on the U.S. High Energy Physics Research Program for the 1990's examines how the National HEP program can go forward vigorously in the period of preparation for the SSC.

The Subpanel concluded early that a viable and productive physics research program in the next decade on a range of promising fronts is essential for this field to continue to attract and educate scientists of great creativity. The Subpanel found that such a program requires both exploiting existing opportunities and undertaking some new initiatives.

The recommendations are based on the "constant budget scenario," which the Subpanel interprets as averaging the FY 1991 budget level over the next decade as described in Chapter VI. For this case, the Subpanel:

1. Strongly recommends the immediate commencement and speedy completion of construction of the Tevatron Main Injector at Fermilab.

2. Recommends strong exploitation of the existing high energy facilities to take advantage, in a timely way, of the many physics opportunities available.

3. Strongly endorses the physics aims of a B factory and recommends a vigorous research and development (R&D) effort leading to a proposal to build such a facility.

4. Recommends significant enhancements in the support by the Department of Energy (DOE) and the National Science Foundation (NSF) of university groups in the areas of technical infrastructure and scientific manpower.

5. Recommends that the NSF substantially increase support for its HEP university groups, particularly for equipment.

6. Recommends continuation of a vigorous program of R&D at Stanford Linear Accelerator Center (SLAC) for very high energy electron positron linear colliders.

7. Recommends that the Division of HEP provide support for the SSC Laboratory physicists' basic research activities that lie outside the SSC project.

8. Recommends that both non-accelerator and foreign-based experiments continue to be strongly supported.

9. Recommends increased support for generic detector R&D.

The Subpanel assigns highest priority to the first of its recommendations. The increased luminosity provided by the Tevatron Main Injector will place Fermilab in an excellent position to discover the top quark. The necessary technology for this project is firmly in hand, and a carefully considered and reliable design exists. The cost of implementing this recommendation, as well as the others, can be accommodated within the constant budget as defined above, provided that sufficient freedom exists to move resources from the second half of the decade to earlier years.

In addition, these recommendations emphasize the continuation of the healthy ongoing program. Over the first half decade, some enhanced operation at Brookhaven National Laboratory (BNL) is expected. In the second half of the decade, much of the Alternating Gradient Synchrotron (AGS) operation at BNL is anticipated to be committed for the Relativistic Heavy Ion Collider (RHIC), and the HEP effort there is expected to diminish. Throughout the decade, including the period of the Main Injector construction, continuation of the strong Fermilab collider and fixed-target programs is crucial. Maintaining optimal utilization of the Cornell Electron Storage Ring (CESR) collider at Cornell is essential. The SLAC program will emphasize exploitation of the Stanford Large Detector (SLD) experiment during the first 5 years with polarization and vertex detection providing complementarity with the high luminosity Large Electron-Positron (LEP) program. R&D looking toward a high energy linear collider and toward a B factory is an important ingredient of this plan.

The health of the HEP university community was of considerable concern to the Subpanel, as were the special difficulties encountered by NSF-supported groups. Added support for university manpower and infrastructure would strengthen university groups and enhance their ability to do research, to invent new experimental tools, and to exploit the opportunities of the SSC. At the same time, it will help draw more students to science.

While these elements of a program based upon a constant budget correspond to a program that has many strengths from which to launch the SSC era, the Subpanel would have also liked to recommend the construction of a very high luminosity B factory. This would provide the field with important balance and strength. Subject to the development of a successful design, the Subpanel felt that a B factory should be built in the context of a rising budget scenario.

Study of several budget scenarios within the reduced budget hypothesis was undertaken by the Subpanel. It was agreed that uniform reduction of all programs by a similar factor was unhealthy, and had adverse implications both before and after SSC turn on. Draconian measures would need to be employed. Consideration of any such steps would require much more deliberation than was possible in the time available to the Subpanel.

In conclusion, the Subpanel reaffirms that the highest priority for the U.S. High Energy Physics Program is the swift construction and implementation of the SSC. During this period, we expect an exciting program of high energy physics. Supported properly, this program will lead into healthy, diverse, and productive science in the SSC era.

I. INTRODUCTION

The Subpanel on the U.S. High Energy Physics Research Program for the 1990's, a subpanel of DOE's High Energy Physics Advisory Panel (HEPAP) which advises the DOE and the NSF, was formed in response to the letter (dated October 4, 1989) to Francis Low, Chairman of HEPAP, from Robert O. Hunter, then Director of the Office of Energy Research of the DOE. This letter was later slightly amended (January 17, 1990) by the Acting Director of the Office of Energy Research, James F. Decker. Together these letters form the Charge to the Subpanel; they are included here as Appendix A.

The membership of the Subpanel was drawn from varied backgrounds within the community of high energy physicists. The 18 members are listed in Appendix B.

The Subpanel first met for an organizational meeting in Washington, D.C. on Tuesday, December 5, 1989. It discussed important organizational questions, and met with Acting Director James Decker to discuss specifics of the Charge. In accordance with the Charge, the Subpanel decided to gain the broadest input possible from the community of high energy physicists to arrive at recommendations to the DOE and NSF.

To this end, a letter was sent to all members of the Division of Particles and Fields (DPF) of the American Physical Society (APS), describing the Subpanel's Charge and inviting written and oral input from the community. Appendix C reproduces this letter.

To collect appropriate input, two to three-day meetings were scheduled at each of the planned and operating high energy physics accelerator laboratories in the country. One half day was spent in open session with the laboratory management to hear its plans and visions for the next decade. At each of the laboratories, an open meeting of the "community of high energy physicists" was convened for an additional half day, with the Chair of the local users' organization presiding. The agenda for the community meeting was mutually agreed between the local users' organization and the Subpanel. The meetings are listed below:

Superconducting Super Collider Laboratory	January 19
Cornell	February 8
Brookhaven National Laboratory	March 1
Stanford Linear Accelerator Center	March 8
Fermi National Accelerator Laboratory	March 19

In addition, one day was specifically scheduled for the Subpanel to hear from U.S. physicists involved in non-accelerator and non-U.S. experiments. This open meeting took place at Cornell on February 9. The agendas of the open meetings are included as Appendix D. Additional information was sought through separate meetings and activities of subcommittees of the Subpanel in order to elucidate specific issues it deemed important.

The Subpanel meetings were well attended; there were at least 16 members present at each. The Subpanel took the opportunity at each of these visits to have a meeting in executive session on the day following the laboratory and community presentations. In some instances, these times were used to discuss specific questions with laboratory management; more often, the time was used to discuss specifics regarding the presentations and discussion heard from the laboratories and from the community.

Copies of the transparencies from the presentations, as well as substantial supporting written materials, were made available to the Subpanel. In response to the request for letters, the Subpanel received over 100 letters from members of the high energy physics community.

The Subpanel met to deliberate the issues between March 31 and April 7, 1990, inclusive. These meetings were held in executive session; approximately half the time was spent in deliberation and the remainder in completing the writing of this report.

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II. HIGH ENERGY PHYSICS: ACCOMPLISHMENTS AND GOALS

A. Introduction

Particle physics or high energy physics seeks to elucidate the origins and nature of matter and of the natural forces that have forged our physical universe.

Great progress has been made toward this goal by using accelerators that fire subatomic particles at one another or into material targets at nearly the speed of light. In these collisions, matter is momentarily heated to extreme conditions similar to those that occurred in the first moments after the Big Bang. Thus, in the laboratory are recreated small-scale simulations of the first epoch, revealing the primordial conditions from which matter and our present mature universe have evolved.

Addressing the wide range of questions in high energy physics requires a variety of probes, energies, and intensities. Present high energy accelerators supply beams of different particles such as protons, electrons, and neutrinos; energies vary from low to the highest technologically available; some questions require the most intense beams while others can be accessed without this extra demand. Every experiment poses its own special demands on technology and it is rarely possible to address more than a fraction of the key questions at any one laboratory.

State-of-the-art electronic devices register and record the results of the experiments, transmitting the information to computerized data banks for subsequent evaluation and study.

The natural environment on earth is quiescent, but elsewhere in the universe energetic events occur, such as supernova explosions, which irradiate the cosmos with high energy particles which can be seen by special detection equipment. Such observations do not use man-made accelerators but rely on the chance arrival of the particles which indicate the occurrence of the phenomena. Other recent examples of such non-accelerator experiments involve detectors sited deep underground where they are protected from most of these cosmic rays. These seek evidence for the natural disintegration of matter, a key to elucidating the ultimate fate of the universe. These detectors also record neutrinos emitted from the center of the sun, or by supernovae, forging links between particle physics, astrophysics, and cosmology.

In addition, fundamental discoveries in high energy physics are stimulating other fields, notably nuclear physics and cosmology, and are developing symbiotic relationships between theoretical particle and condensed matter physics.

The answers to the deep philosophical questions of high energy physics enrich our culture and the opportunity to address them stimulates widespread interest in science. Enrichment of our society through the pursuit of this field extends further. Particle physics has provided many tools and ideas to medicine that are of value both to diagnosis and therapy. Positron Emission Tomography (PET); pion, neutron, and proton cancer therapy; and Computer Assisted Tomography (CAT) scanner technology are outgrowths of high energy physics research. Research in many other scientific disciplines has been enriched by the techniques developed in particle physics. Synchrotron light, a side effect of accelerating particles in circular accelerators, is an important tool in materials science, chemistry, and biology. Numerous ideas and technologies generated in particle physics have become the basis of new industries making use of accelerators, high power tubes, digital computer circuits, and superconducting magnets.

Progress in the next decade and beyond depends not only on the exploitation of outstanding achievements of experiments, theory, and technological innovation in the past, but also on a continued commitment to doing the best science. By exploring some of the most compelling scientific questions and by supporting the most promising means of addressing them, the U.S. program in high energy physics can continue to inspire, educate, and train some of the world's finest minds, to contribute to the wellspring of new technology, and to make historic contributions to one of humanity's most ambitious undertakings.

B. High Energy Physics Today

Over the past two decades, extraordinary progress has been made in the international endeavor to understand the ultimate structure of matter. Experimental discoveries, many at high energy physics laboratories in the U.S., theoretical insights, and technological innovations have enabled great strides toward a unified understanding of matter and energy. The crowning achievement of this work, a greatly simplified picture of the physical world at its most fundamental, is encapsulated in the Standard Model of elementary particle physics.

The Standard Model explains all natural phenomena since the Big Bang in terms of four interactions--the strong, weak, electromagnetic, and gravitational--and three broad classes of elementary particles--quarks, leptons, and the force-mediating particles, gauge bosons.

However, we do not understand the underlying source of the many free parameters in the model, such as the particle masses, the relative strengths and symmetries of the several interactions, nor why nature chose this particular model for the universe. Further, the Standard Model does not address whether its basic constituent particles, the quarks and leptons, are truly elementary or whether they are composed of yet smaller constituents.

1. The Forces

There are four known fundamental forces. On the scale of elementary particles, gravity is so weak that it plays no measurable role in present high energy experiments. The electromagnetic interaction binds negatively charged electrons to nuclei to form atoms and molecules. The strong and weak forces act only over very short distances, and so are not immediately obvious in the world around us. The strong interaction binds three quarks together to form protons and neutrons. The weak force causes some particles and atomic nuclei to be unstable, resulting in certain kinds of radioactive decay (e.g., beta decay). The strong and weak interactions together control nuclear fission and fusion and are responsible for the energy output from the sun and other stars.

At the everyday level electromagnetic phenomena were well described more than a century ago by Maxwell, but electromagnetism at the subatomic level can only be understood when combined with relativity and quantum theory. This was finally achieved in the middle of this century with quantum electrodynamics (QED). The key characteristics of QED are that electromagnetic interactions are mediated by a particle (the photon) and that the basic equations have a mathematical property called gauge symmetry.

The success of QED led physicists to hope that theories with gauge symmetry (gauge theories) might provide the correct description of all the fundamental forces and, ultimately, the means of understanding each of them as different aspects of a single unified force.

By 1970, QED was well established, but there was effectively no theory to explain the strong interaction among quarks. Although experiments had revealed evidence that quarks existed, they had only been observed in clusters. While isolating a single lepton is simple, it has proved impossible to knock an isolated quark free of a proton or neutron--even though, paradoxically, quarks inside those particles appear to behave as if they were free.

A plausible theory to explain this behavior was proposed in the early 1970's: quantum chromodynamics (QCD). QCD has certain profound similarities to QED, although the two theories describe what appear to be very dissimilar forces. Both are gauge theories. Just as QED explains how electrically charged electrons and nuclei are bound together into atoms, QCD describes how particles such as quarks with the property known as "color" (analogous to electric charge) are bound together by gluons to form protons and similar particles (hadrons). While QCD theory successfully describes the behavior of quarks in high energy collisions, their interactions at low energy and the details of the spectroscopy of hadrons (particles containing quarks) fall under an unsolved area of the theory known as "non-perturbative QCD." When this aspect of the theory is better understood it may lead to a fundamental description of nuclear structure.

Another important revolution in particle physics took place in the 1970's. A gauge theory of the weak interaction was developed, built on the QED paradigm. The predicted analogs for

QED's photon were three particles: the W^+, W^-, and Z^o. However, an important and tantalizing difference was that these force-carrying partners for the weak interactions were predicted to be very massive, in contrast to the massless photons and gluons of the electromagnetic and strong interactions. Because of this difference in mass, the electromagnetic and weak forces appear very different at everyday energies, but fundamental similarities are revealed at higher energies: the two theories merge into a single electroweak theory.

In 1973, experiments confirmed the existence of new neutral current processes mediated by the Z^o. Subsequent detailed studies of weak interaction phenomena over a number of years helped to delineate the conditions necessary for W's and Z's to be produced in the laboratory. These led to their discovery in 1983, thereby confirming the theory and allowing the particles to be studied directly.

The QED, QCD, and electroweak theories, built around the photons, gluons, W's, and Z's form part of the Standard Model. A key question, as yet unanswered, is why the W and Z gauge bosons are so massive, when the gauge bosons of QED and QCD are massless. What accounts for the origin of the mass of the W and Z (and indeed of all massive particles) and thereby provides the force that breaks the electroweak symmetry? Current theory suggests that a new mechanism, the Higgs (named after its inventor, Peter Higgs), is responsible for generating the mass of all the fundamental particles. A consequence of this theory could be the existence of new massive particles known as Higgs bosons. For the first time, a theory contemplates the source of mass.

2. The Particles

A complete theory of the universe must explain not only the fundamental forces, but the menu of particles that make up all matter on which the forces act.

In addition to providing spectacular progress toward the ambitious attempt to find a single theory of all the forces, work in recent decades has revealed some exotic and even unexpected kinds of matter. The creation and study of these particles has resulted in the emergence of a pattern of great simplicity: two and probably three generations of particles that, except for the particles' masses and lifetimes, appear to behave in a remarkably similar way. This pattern is shown in Figure II-1.

The first suggestion for the existence of quarks came from the detailed studies of the spectra of new hadrons produced in high energy collisions. Subsequent experiments involving large-angle scattering of lepton beams directly revealed the quarks within the proton and neutron--a modern analog of Rutherford's discovery of the atomic nucleus. The proton is made of two up quarks and one down quark; the neutron is made of two down quarks and one up quark.

In addition to the quarks, the other constituents of matter that so far appear to be fundamental are the leptons. The most familiar lepton is the electron. The electron has an electrically neutral and apparently massless partner, the neutrino, which is not found inside atoms but is created in some radioactive processes.

Figure II-1

THE PARTICLES OF THE STANDARD MODEL

	QUARKS (acted on by strong, weak, and elctromagnetic forces)	LEPTONS (acted on by weak and electromagnetic forces)
first generation	up, down	electron, electron neutrino
second generation	strange, charm	muon, muon neutrino
third generation	bottom, top	tau, tau neutrino

Figure II-1 The particles of the standard model, now believed to be the fundamental constituents of all known forms of matter. The top quark and the tau neutrino have not yet been directly observed.

Nature has repeated this pattern at least once and, it is believed, twice. Mysterious particles first seen in cosmic rays in the 1940's and 1950's were later understood to contain a strange quark, which is a heavier version of the down quark; the emerging Standard Model predicted the existence of a fourth quark, known as the charm quark, which was subsequently discovered in 1974. Another particle that had been seen in cosmic ray experiments, the muon, was later recognized as a heavier version of the electron; in the early 1960's it was discovered that the muon is linked to a different neutrino than the partner of the electron.

Evidence for a third generation of quarks and leptons has recently emerged, with the discovery of the tau lepton and bottom quark. Current experiments provide indirect evidence of the tau neutrino and the top quark, neither of which has so far been observed directly. Recent studies of Z^0 properties demonstrate that there are no more light neutrinos; this implies that there may be only three generations of quarks and leptons.

C. Opportunities for the Next Decade

More than 20 years of experimental results have contributed to the remarkable accomplishment of the Standard Model. No experimental result conflicts with the theory. However, the Standard Model does not explain many of the properties of the fundamental particles and forces. The goal of the next decade of particle physics is two-fold: to test aspects of the Standard Model that have not yet been verified, and to seek phenomena that the Standard Model cannot explain.

As an example, several attractive extensions of current theories postulate as-yet-unseen decay modes of mesons containing strange quarks, kaons, that are forbidden by the Standard Model. Searching for these processes can be done with intense kaon beams produced by proton accelerators.

Nothing in the Standard Model explains why there should be three generations of fundamental particles, nor provides a rationale for the relative magnitudes of their masses and the strengths of their interactions.

Precision measurements of the "Weinberg angle," which governs the relative strengths of the weak and electromagnetic interactions, may expose deviations from the Standard Model. This can be probed rather sensitively in high energy spin-polarized electron-positron collisions.

The concept of symmetry has long played a seminal role in particle physics. A tiny asymmetry in the behavior of matter and antimatter particles, known as CP-violation, was observed in 1964. Although this phenomenon is an essential part of understanding the large-scale asymmetry between matter and antimatter in the universe, its origin is still a mystery. It may arise naturally within the Standard Model of three families of quarks and leptons, or it may be the first manifestation of phenomena that lie outside the Standard Model. Understanding CP-violation is one of the field's key objectives.

Recent results on the physics of B mesons, which contain bottom quarks, suggest that intense studies of these particles may provide particularly sharp insights into the CP problem. This may best be achieved with a high intensity source of these particles: a "B factory." Such studies would complement the current and future program of study of CP-violation using K-decays.

The top quark, required to complete the third generation, has not yet been seen. Knowledge of its mass is crucial for understanding the fundamental properties of matter. Recent experiments have established a lower bound for the top quark's mass; current theory proposes an upper bound that suggests that its discovery may lie within the reach of accelerators in this decade.

Thus, we have the opportunity to discover the missing links in the Standard Model or even to expose its limitations. The experience and intuition emerging from the program of this decade will focus attention onto the most profound challenge for the next: unravelling the symmetry-breaking force in the electroweak interaction, which is the source of the masses of the W and Z bosons and maybe of the masses of all fundamental particles. The means by which nature achieves this is presently hidden but theoretical developments flowing from recent experiments show that this mechanism--whether it be caused by the interactions of massive Higgs bosons or by some completely new phenomenon--is within the reach of the SSC.

We are assured that a new level of understanding of the physical world will emerge from research at the SSC and from the complementary programs of a healthy field. We are on the threshold of understanding beyond the Standard Model, leading to the theory incorporating quantum gravity and deeper unification of all the forces.

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III. THE PRESENT U.S. PROGRAM

The U.S. program in HEP research covers a broad spectrum of experiments extending from studies of low energy interactions to those at the highest energies in the world. Also, an active R&D program on new research techniques is being pursued. The experimental program is carried out at accelerators at the four major U.S. laboratories, at laboratories abroad, and with a variety of non-accelerator particle physics experiments. Detector R&D is mainly, but not exclusively, aimed at developing techniques for exploiting the full potential of the SSC. Accelerator R&D is directed both at refining existing methods and searching for new techniques that can take us beyond the current energy and beam-intensity limits. In addition, other preparations are being made for the SSC experimental program. In this chapter we review the current status of these various components of the U.S. program.

A. The AGS Program at BNL

The AGS program at BNL is centered around the study of rare kaon decay modes, the study of hadronic physics, and a new measurement of the anomalous magnetic moment of the muon (muon g-2). The BNL AGS is entering its fourth decade as a high energy physics research facility. In spite of its age, its high duty cycle and its intense beams of 30 GeV protons make it a unique facility for studying certain aspects of the Standard Model and searching for phenomena beyond.

The performance of the AGS has steadily improved over the years and it now routinely has an external beam of 1.4 times 10^{13} protons per pulse (with a 1 second spill and a 2.4 second repetition rate). The Booster presently under construction will act as an injector to the main ring. It has three primary functions: to increase the proton beam intensity by a factor of 4, to increase the polarized proton beam intensity by about a factor of 20, and to provide AGS beams of fully stripped gold ions for the heavy ion program. The Booster is expected to be operational in 1991.

A program to study rare, or infrequent, decays of kaons was undertaken in the early 1980's in order to study predictions of the Standard Model in a precise manner. It was made possible both by advances in the technology of particle detectors and by the ability of the AGS to provide large fluxes of charged and neutral K mesons. Experiments study flavor-changing neutral currents, search for decays involving interactions beyond the Standard Model, and search for low mass scalar particles whose existence is suggested by a diverse group of models. In addition, these experiments observe and measure the properties of allowed decay processes, adding greatly to our knowledge of the Standard Model, and test for the presence of CP-violation in modes heretofore unobserved.

The rare kaon decay program has almost completed its first round of experiments. Several new limits have been set that exclude new interactions and new particles, improving sensitivity over previous work by orders of magnitude. Significant numbers of events from allowed modes have also been collected, yielding improved parameterizations of the interactions leading to these decays. In addition, a great deal of experience in utilizing the large beam fluxes and data rates, and in understanding presently limiting backgrounds, has been accumulated.

Proposals for upgrades and new experiments that extend the sensitivity of these studies are in preparation. All involve utilizing the increased intensity to be provided by the Booster upgrade. It is anticipated that this second round of experiments will be substantially complete by the mid-1990's.

The program of hadronic physics includes studies of hadron dynamics and light quark spectroscopy; the theme is that of a varied experimental program to investigate non-perturbative QCD, including the phenomenon of color transparency in elastic p-p scattering inside nuclear matter, the physics of rare large-angle exclusive reactions, searches for exotic hybrid mesons, and systematic studies of mesonic states with masses between 1.0 and 2.4 GeV. Other experiments search for six quark states and strange-quark matter, and study the spin dependence of inclusive and exclusive scattering amplitudes employing the AGS polarized proton beams.

The third facet of the AGS program is an experiment aimed at improving the determination of the muon g-2 value by a factor of 20. Such a result would measure the weak interaction contribution to the anomalous magnetic moment with 20 percent accuracy. If the measured value differs significantly from that expected, it would indicate the presence of physics outside the Standard Model.

In the last few years, the AGS has been used to accelerate heavy ions (oxygen and silicon) to about 15 GeV per nucleon for studies of nuclear phenomena at high nuclear densities and temperatures. This program is the forerunner of physics at a proposed new collider, RHIC, which would use the AGS as an injector. Although the fixed-target proton program is expected to diminish as RHIC begins operation, the AGS will retain its capacity for high intensity proton running, and could readily be exploited for this purpose if the physics warrants it.

B. The CESR Program at Cornell

Studies of the properties of the b-quark system have proven to be a rich source of new insight into both the strong and weak interactions. The CESR e^+e^- storage ring on the Cornell University campus, which is optimized for a center-of-mass energy range of 9 to

11 GeV, is the world's premier laboratory for these studies. B-flavored mesons and many of the upsilon $b\text{-}\bar{b}$ bound states were discovered at CESR. In addition, many of the characteristics of the upsilon system, the B mesons, charmed particles, and tau leptons have been determined. CESR has provided a large fraction of our current understanding of the Cabibbo-Kobayashi-Maskawa (CKM) matrix which describes the transitions among the various quarks induced by weak interactions; physicists there have recently observed the weak transition from b to u-quarks as well as corroborated the surprisingly large $B/\bar{B}$ mixing, first seen at DESY.

Since the first operation of CESR in 1979, the storage ring and the detectors have continuously evolved. The luminosity of CESR has increased to 10^{32} cm^{-2} sec^{-1}, corresponding to a production rate of 25,000 B meson pairs per week, the highest level of any e^+e^- storage ring in the world. Plans exist to improve the RF system, modify the machine optics, improve the positron source, and increase the number of bunches, with an ultimate goal of a fivefold increase to a luminosity level of 125,000 B meson pairs per week. The detection capabilities have also improved. In 1990, the second generation CLEO-II detector, with a high resolution cesium iodide shower counter, was commissioned. While the specific goal of CLEO-II is the complete reconstruction of thousands of B mesons, it is also well suited for inclusive measurements, upsilon spectroscopy, and tau physics.

The CESR/CLEO-II physics program will concentrate on further studies of B meson decays and the better determination of the CKM matrix elements. Branching ratios for rare decays will be measured, and searches for forbidden decays will be carried out. Parameters that are important for Standard Model CP-violation searches, such as the $B\bar{B}^*$ cross section and branching ratios for $B \to \psi K_s$ and $B \to \pi^+\pi^-$, will be measured and searches made for non-Standard Model CP-violations. In addition, an active program of accelerator studies using CESR to address beam dynamics problems associated with high luminosity e^+e^- B factory designs will continue.

C. The Fermilab Program

The Fermilab Tevatron Collider is the highest energy colliding beam accelerator in the world. Its success required the development of superconducting magnets and the first integration of such magnets into a reliable accelerator system. The collider and its major particle detector, Collider Detector at Fermilab (CDF), have operated spectacularly well. A second large detector, D-Zero, with properties complementary to those of CDF, will be installed in the summer of 1991. Collider experiments include searches for the expected top quark, for other new particles (like supersymmetric particles), and for new phenomena associated with non-Standard Model origins, like compositeness of quarks. In addition, precision measurements of strong interaction and electroweak phenomena are carried out. The collider provides an excellent training ground for experimentation at the SSC.

Several important results have already emerged from analyses of CDF data. For example, the mass of the top quark has been shown to exceed 89 GeV, and the masses of the hypothetical "squark" and "gluino" (particles predicted in theories that extend the Standard Model by incorporating supersymmetry) must exceed 73 GeV.

If there are heavier gauge bosons, replications of the known W and Z, CDF data show that their masses must exceed about 400 GeV. Searches for manifestations of quark compositeness establish that the characteristic mass scale exceeds about 1 TeV. The cross section for the production of jets of hadrons has been measured over seven orders of magnitude, extending to transverse momenta of about

400 GeV. These data, as well as data on prompt photon production and on the production of jets in association with the W and Z gauge bosons, permit sensitive new tests of perturbative QCD. Data from CDF provided a determination of the mass of the Z with an accuracy which had not been expected in hadron collider experiments. Measurements of the mass of the W from CDF are competitive with those from the UA2 experiment at CERN. After further data are accumulated, the error on the W mass determination should be reduced to about 100 MeV, permitting a precise determination of the Weinberg angle.

A rich and diverse program of fixed-target experiments operates at Fermilab with the highest energy beams in the world. Among the achievements are the recent precise measurement of the CP-violation parameter ϵ'/ϵ and of sensitive upper bounds for several rare decays of the K^0_L. Another experiment is now determining the phase difference between η_{+-} and η_{00}, an important test of CPT invariance. A search will begin soon for the decay $K^0_L \rightarrow \pi^0 e^+ e^-$ with sufficient sensitivity to observe this process at the level expected in the Standard Model.

Photon and hadron beams at Fermilab are employed to produce charm and bottom hadrons for experiments that study their decays. Active silicon vertex detectors are used in a set of large spectrometers that observe decays in flight. Present data sets contain over 10,000 fully reconstructed charm decays, and the next generation of experiments, beginning now, will collect up to 100,000 fully reconstructed decays. The most precise determinations of the lifetimes of several charm mesons have been made at Fermilab, new D^{**} mesons have been identified, and limits have been placed on $D^0/\overline{D^0}$ mixing. Among the goals of current experiments on bottom production are determinations of cross sections and lifetimes, as well as measurements of exclusive and rare decay modes.

The study of strong interaction processes in fixed-target experiments at Fermilab continues to provide essential information fully complementary to that accessible at hadron collider facilities. Prompt photon production is under investigation with the expectation that measurements will extend the reach to transverse momenta of 12 GeV. This experiment will determine the gluon structure function over a wide range of values of the parton fractional momentum and test perturbative quantum chromodynamics through next-to-leading order in perturbation theory. An experimental program that has no collider counterpart is the study of scattering processes involving polarized protons at Fermilab.

Deep inelastic lepton scattering experiments are being carried out with the world's highest energy muon beam. Studies are being made of nucleon and nuclear structure functions at the smallest values of fractional momentum yet accessible, and important investigations are being carried out of quark fragmentation and of quark propagation in nuclear matter. Neutrino scattering experiments designed to measure structure functions and the Weinberg angle completed data taking in 1988. The analysis of this sample of more than 10^6 events is nearing completion.

D. The SLAC Program

SLAC has a long and successful history in e^+e^- physics. It started with the Stanford Positron Electron Asymmetric Ring (SPEAR), which now runs as a synchrotron radiation source, continued with the Positron-Electron Project (PEP), and led to the Stanford Linear Collider (SLC), the first accelerator to achieve particle collisions using the linear collider principle, a technology that is necessary for future e^+e^- colliders at very high energies. The Mark II detector has now been operating with collisions at the SLC for about a year and has collected about 500 Z events. The collaboration has reported results on the basic properties of the Z boson, including a precise measurement of its mass; a limit of three varieties of light neutrinos; limits on several types of new particles, including heavy quarks and neutral leptons; and properties of hadronic Z-decays. The commissioning of the LEP collider at CERN in late 1989, with the associated four major detectors, presents the SLC with severe competition in luminosity.

The Mark II experiment will continue to run through the summer of 1990, exploiting two newly installed vertex detectors--a precision drift chamber and a silicon-strip device. During 1990, the polarized electron beam in the SLC will be commissioned, providing another capability unique to the SLC. This project involves a polarized electron gun, a spin rotation system, and polarimeters.

The new SLD detector will be installed in the SLC in the fall of 1990 and begin taking data with the polarized electron beam in 1991. The SLD detector is notable for its excellent calorimetry and particle identification and for its silicon vertex detector with two-dimensional readout, the first such detector to be used at a collider. The experiment will take advantage of the very small SLC beam spot, the small diameter beam pipe, and the polarized electron beam of the SLC machine to pursue a physics program that includes the measurement of the left-right polarization asymmetry and studies of $B/\overline{B}$ mixing. The left-right polarization asymmetry allows an independent measurement of the Standard Model parameter, the Weinberg angle, with more sensitivity than other asymmetries at the Z pole. This measurement makes use of nearly all the Z decay final states and is largely unaffected by the various experimental and theoretical systematic uncertainties.

The Time Projection Chamber (TPC)/2-gamma experiment at PEP has been upgraded with a precision vertex detector and is now waiting to take 1 to 2 fb^{-1} (femptobarn) of data. This experiment will open up a qualitatively new domain of two-photon physics. Studies of

B-physics, QCD jet phenomena and tau-physics will also be pursued. SLAC has recently approved the PEP Gas Jet Spectrometer System (PEGASYS) experiment, which proposes to study coherent processes in QCD, formation zone phenomena, color transparency, spin transfer reactions, and precision QED tests. This effort is a joint high energy and nuclear physics project, and waits formal approval from the nuclear physics community.

The precision electron scattering spectrometers in End Station A, together with the high energy polarized beams developed for the SLC, will open up new opportunities for the study of structure functions, shadowing, and nucleon form factors at high momentum transfer. During the building and commissioning of the SLC, this program has been put on hold. Proposals are now in hand for several experiments, and studies of color transparency, QCD tests, inelastic scattering in nuclei, and measurements of $e - \pi$ scattering are all in preparation. A new experiment is being proposed to study the spin-dependent structure function of the neutron by scattering polarized electrons from a polarized He^3 target. This study will give a direct measurement of the quark spin content of the nucleon, which could be important in the eventual understanding of the interesting polarization phenomena under study in proton-proton collisions.

The laboratory is very active in accelerator physics studies. It has been an intellectual center (with Lawrence Berkeley Laboratory (LBL)) for a high luminosity, e^+e^- storage ring facility (B factory). A strong effort is focussed on the design of such a machine with asymmetric beam energies, on the R&D program to prove out the accelerator physics issues, and on the evaluation of a comprehensive experimental program, drawing heavily on the widespread national interest in such a facility. SLAC is also the world leader in the area of high energy e^+e^- linear colliders. A vigorous R&D program is in progress, and enjoying real participation of Japanese, Soviet, and European accelerator physicists.

E. U.S. Participation in HEP Programs at Accelerators Abroad

Strong international collaboration has always been the rule in HEP. Such collaboration has benefitted the science while being a positive element in international relations. Many important discoveries have been made by collaborations of researchers from different countries. It has long been common for facilities built in different countries to be shared. For example, in the early 1970's when the Intersecting Storage Rings at CERN became operational, there was no comparable U.S. accelerator operating or being planned. A number of U.S. research groups started to focus their activities at CERN to take advantage of this unique laboratory. Subsequently, U.S. physicists have mounted experiments at a number of accelerators abroad: TRISTAN at KEK and HERA at DESY, neither of which has a direct counterpart in the U.S.; and LEP at CERN, which has unique luminosity capabilities at the Z. At the present time, about 14 percent (or 230 researchers) of the U.S. high energy physics community are conducting research at accelerators abroad. A comparable number of foreign researchers are actively using U.S. facilities.

U.S. activities abroad range from major efforts initiated and led by U.S. physicists, through programs that have major U.S. participation, to smaller efforts with a single U.S. group collaborating on an experiment with a number of overseas groups. The wide range of opportunities for participation in programs at laboratories outside the U.S. enables the U.S. community to maintain a broad program of research without duplicating expensive facilities.

There are approximately 130 U.S. researchers at the LEP Collider at CERN, where they are studying high energy e^+e^- annihilations at the Z resonance. These experiments started running in the fall of 1989. A number of important results have already been presented, including the determination that there are only three varieties of light neutrinos and the exclusion of the existence of a standard Higgs particle with mass below 24 GeV. In addition, a number of properties of the Z particle have been measured, verifying Standard Model predictions to high precision.

There is also a strong U.S. participation of about 40 researchers in the study of e^+e^- collisions at the TRISTAN collider at KEK. These experiments test the Standard Model in the energy region where the electromagnetic and weak interactions are of comparable strength. In addition, there are about 50 U.S. participants in the HERA program at DESY, primarily concentrated on the ZEUS experiment. This unique accelerator, which should become operational in 1991, will probe the proton structure at the distance scale of order 10^{-18} meters, an order of magnitude improvement over current measurements.

International cooperation will certainly continue in the future and will stimulate the intellectual health of the field. New collaborations include B-decay studies at the CERN proton-antiproton collider and studies of polarization effects in proton-proton collisions at the UNK accelerator at the Serpukhov Laboratory in the Soviet Union. In addition, foreign collaborators have both contributed to and benefitted from research at U.S. accelerators, and many researchers from outside the U.S. have expressed their intention of participating in the SSC experimental program.

F. Non-accelerator Physics

The spectacular progress in HEP has been a direct result of the development of new and more powerful accelerators and the tools to exploit them. Yet, there has always been a class of crucial experiments in our field that have not used accelerators. An early example was the discovery of parity violation in nuclear weak decay. In the past decade we have seen a trend toward larger scale and more ambitious non-accelerator projects.

The success of electroweak unification and the development of QCD gave strong impetus for trying to unify the weak, electromagnetic, and strong forces. The simplest such theory is SU(5), which predicts instability of the proton at a measurable level. This prediction inspired the construction of several large underground experiments to search for evidence of proton decay. These experiments succeeded in setting limits on proton decay, thereby ruling out the simplest SU(5) theory. Variations of this theory, which are consistent with these limits on proton decay, are still being pursued.

Other predictions of Grand Unification include the existence of superheavy magnetic monopoles and the possibility of finite-mass neutrinos leading to neutrino oscillations. The search for monopoles is underway with both small and large-scale experiments, and neutrino oscillations have been sought both in accelerator and non-accelerator experiments, each of which explore different possible mass regions.

One of the most dramatic results in non-accelerator physics over the past decade has come from the observation that the flux of neutrinos arriving from the sun is less than predicted by standard solar models. This "solar neutrino problem" was first observed in an experiment using ^{37}Cl and looking for the production of ^{38}Ar from solar neutrino interactions. Only about one-third of the expected flux level was observed. This result was confirmed recently in the Kamiokande Proton Decay detector using a totally different technique. The predicted range is on reasonably firm ground as it is determined from well known nuclear reactions. This implies the effect may come from some, as yet, undiscovered property of neutrinos. New experiments to pursue these questions are now being mounted.

Another notable, though unanticipated, result of the proton decay experiments was the observation of neutrinos from the supernova 1987a. Two experiments observed a burst of neutrinos at the same time and several hours before an optical signal was visible. This result has profound consequences both for our understanding of the physics of the gravitational collapse of stars and, at the same time, has led to significant information on the properties of neutrinos. A future observation of the collapse of a star that is nearer the center of our galaxy could yield further information about neutrinos.

Work in non-accelerator physics is often on the interface between particle physics and nuclear physics, cosmology, or astrophysics. The total effort in this area grew in the early 1980's, and currently involves about 190 researchers, costs about 2 percent of the total budget for HEP, and represents about 15 percent of the university program in particle physics. Overall, non-accelerator physics plays a crucial role in elementary particle physics by addressing specific problems which cannot be studied using accelerators, and, in general, by bringing diversity to the program.

G. Status of the SSC

A bold step beyond existing accelerator facilities, the SSC will provide opportunities for research extending far into the 21st century. In experiments at the SSC, searches will be made for new particles and new phenomena--both predicted and unexpected--and precise measurements will be made to probe the validity of our current understanding of particle physics. The major goal of SSC experimental studies is a complete elucidation of the nature of the breaking of the symmetry between the weak and electromagnetic interactions. Complex phenomena associated with the symmetry breaking are expected theoretically on the TeV mass scale for interactions among quarks, leptons, and the W and Z gauge bosons. One possibility involves production of the Higgs boson. If there is no such particle with mass less than approximately 1 TeV, then there must be new strong forces between the gauge bosons, perhaps manifest as enhanced production of gauge boson pairs. Whatever the mechanism, it will be essential to conduct a thorough exploration of the mass region up to approximately 2 TeV. The origin of the mass of quarks and leptons may also be revealed.

The first year in which SSC construction money was allocated was FY 1990, a crucial one for the project. A temporary office has been established south of Dallas, Texas, near the Ellis County site, providing laboratory and office space for the rapidly expanding staff. A "footprint" was proposed and approved by the DOE, and the supplemental environmental impact statement is being prepared. A draft of the revised design and cost estimate was presented to the DOE in January 1990. The architect-engineering and construction management firm has been selected and contract negotiations are in progress. Finally, and most important, procedures have been established to formulate the initial SSC experimental program with a widespread and enthusiastic response of the scientific community.

The present SSC design has taken into account the results of detailed design studies carried out since the non-site-specific Conceptual Design Report of March 1986. In particular, it has now been decided to raise the injection energy from 1 to 2 TeV and the magnet aperture from 4 to 5 cm. A slightly modified lattice design and layout required an increase of the circumference from 52 to 54 miles. To improve the flexibility of the experimental program, a by-pass configuration is foreseen for the interaction regions and the size of the experimental halls has been increased. These changes should not only guarantee high reliability during the commissioning and early operation of the SSC, but also add flexibility for later additions and upgrades. The Report of the 1990 HEPAP Subpanel on SSC Physics emphasized the need for a flexible and reliable facility at 20 TeV for decades to come and concurred with the Laboratory in the logic of making these changes. The Subpanel report was subsequently accepted by HEPAP in its meeting of January 12, 1990. The decision not to compromise the energy and to aim for an initial luminosity of $10^{33}cm^{-2}sec^{-1}$ with the potential for later increases was also supported by all other groups which considered these issues (ref: Ad Hoc SSC Physics Committee (11-12/89), the SSC Scientific Policy Committee (12/89), the SSC Users Organization (12/89), and the Universities Research Association (URA) Board of Overseers (11-12/89)). The changes mentioned above will be part of the Site-Specific Conceptual Design, which will be submitted, together with its cost estimates and schedule, to the DOE in May 1990.

The current schedule from the SSC project calls for the 200 GeV Medium Energy Booster to be ready to provide test beams in 1996 and for the SSC project to be completed by the end of 1998. It is important to have a strong and ambitious experimental program from the start.

The schedule for the experimental program calls for receiving Expressions of Interest (EOI) in SSC experiments in May 1990. Several collaborative groups, some including substantial international participation, are preparing EOI's. A review of these EOI's by the Program Advisory Committee will take place in the summer of 1990. It will allow the Laboratory to plan its technical and infrastructural support. Preparation of the experimental program requires extensive test beam facilities. CDF and D-Zero at Fermilab each required about 4 to 5 beam-years in various test beams before the commissioning of the detectors. For SSC experiments, even larger demands are expected. Test beam facilities will be provided by both existing accelerator laboratories and the SSC Laboratory itself.

The project is now well on its way. The Laboratory will provide the scientific community with one of the greatest instruments for basic scientific research, and will be a scientific center of excellence.

H. Accelerator R&D

Progress in particle physics is limited by the capabilities of its instruments, particularly accelerators and detectors. Advances in these instruments are often followed by important discoveries. Among the accelerator developments that have significantly impacted particle physics are strong focusing (the basis of all modern accelerators), storage rings for colliding beams, stochastic cooling, superconducting magnets, and high power klystrons. This trend is expected to continue as the various high performance accelerators (B-, ϕ-, τ/C- factories), linear colliders and futuristic ideas (laser, plasma wakefield devices) are proposed and implemented. Accelerator R&D is currently being performed at all of the national laboratories and at some universities.

Many other fields of science and technology have been affected by accelerator development. Synchrotron radiation facilities are now common research tools for solid state physicists and biologists, and are widely used by the semiconductor industry. In addition, various technologies have been pushed by the specific demands of high energy accelerators to new performance levels. Examples are superconducting magnets, vacuum, and RF technology.

The maximum energy of proton storage rings is determined by the magnetic field strength that can be produced. R&D focused on the superconducting magnets for the SSC has been critical in determining the SSC parameters. This R&D includes metallurgy of NbTi, mechanical properties of high-field magnets, field quality of superconducting magnets, and cost optimization for large-scale production.

Beam dynamics is another area addressed by accelerator physics research. While the dominant motion, that of single particles, is linear and well understood, most circular accelerators work in regimes where additional dynamics affect performance. Both non-linear and multiple-particle effects are important for determining the ultimate SSC luminosity, the configuration of the upgraded Tevatron collider, and B factory parameters. Topics in beam dynamics are under active investigation using theory, simulations, and experiments on operating accelerators. For example, important design information was drawn from the non-linear dynamics experiments performed at the Tevatron and the beam separation experiments at the Tevatron and CESR.

One of the greatest accelerator research challenges is the energy limitation of circular electron accelerators that arises from synchrotron radiation and the need to make up the associated energy losses with radio frequency (RF) power. While the development of superconducting RF cavities has relaxed this constraint, linear colliders are required to achieve electron-positron collisions well above LEP energies. Thus, linear colliders are regarded as one of the key ingredients in the future of HEP programs.

The SLC, the first attempt to achieve electron-positron collisions with a linear collider, has been operating for about a year. Continued accelerator R&D has led to significant performance improvements of the SLC. While experience with the SLC will influence future linear collider designs, there are other issues that must be addressed for a higher energy linear collider. These issues are closely coupled and include power sources, accelerating structures, beam brightness preservation, and collision point parameters. Research in all these areas is in progress and a number of prototype systems are being pursued. These include the Final Focus Test Facility, being developed at SLAC, and the RF power source studies of SLAC and Livermore. The Final Focus Test Facility will use the low emittance, high charge electron bunches from the SLC, to produce very small ($\leq$ 50 nm) beam spots and learn how to control and manipulate them optically. This facility will be built and experimentally exploited by an international collaboration of U.S., Japanese, Soviet, and European physicists.

In the longer term, energies beyond those of the SSC could become important for particle physics. This is addressed by research in novel and advanced accelerator concepts, such as laser, and wakefield or plasma accelerators, that could reach ultra-high energies. At present, the focus of this advanced accelerator work is concentrated on the basic ideas and proof-of-principle devices. Examples of this area of activity include the Accelerator Test Facility at BNL, the Test Facility at Argonne National Laboratory (ANL), and the plasma research at the University of California, Los Angeles (UCLA). Such concepts could provide the basis of the accelerators after the SSC.

I. Detector R&D

The development of new detector techniques is often a prerequisite for advancing the physics frontiers. For example, special detector techniques had to be developed for high-rate fixed-target experiments, such as rare kaon decays and charm studies. Detector electronics and methods of recording data have undergone tremendous evolution in the past 25 years. Detector R&D has assumed even more prominence as high energy physicists have begun to address requirements for detectors for the SSC. Experimental conditions at the SSC will be extremely demanding. It will be necessary to record events at a very high rate--there will be 10^8 interactions per second with bunch crossings occurring every 16 nanoseconds. In addition, SSC detectors must be capable of isolating extremely rare processes. The detectors must operate effectively over long periods in a high level of radiation.

A program for generic detector R&D was started by the DOE in late 1986 in response to requests for support to address SSC-related detector issues. Proposals for detector R&D were reviewed by an international committee. In FY 1987, eleven projects were funded at a total of about $0.5M. By FY 1989, the program had grown to 49 projects in 38 U.S. institutions with a total funding of $6.3M. Important areas of R&D being funded include the following:

- Front end, triggering, and data acquisition electronics
- Warm liquid calorimetry
- Scintillator-based calorimetry (fibers, plates)
- Silicon pixel detectors
- Silicon microstrip tracking
- Straw tube and radial wire chamber tracking
- Scintillating fiber tracking
- Computer simulation of detectors

With the SSC now moving rapidly into the construction phase, the emphasis has shifted towards consideration of detector subsystems (e.g., tracking or calorimetry). The main differences between generic R&D and SSC subsystem R&D are that (1) the latter is more focused towards building a detector using the results of previous generic R&D and (2) the collaborations must involve multiple institutions, often with industrial participation. The SSCL began a program of major detector subsystem R&D with a call for proposals due in October 1989. Thirty-eight proposals requesting a total of $43M were submitted, with approved proposals funded at a total level of about $10M. Some generic R&D is also being funded in FY 1990. Sixty-eight U.S. scientific institutions, representing virtually all universities and national laboratories with experimental high energy physics programs, are participating in SSC detector R&D. There is also some non-U.S. participation.

The generic R&D effort has evolved into a broad and successful program resulting in significant detector developments that otherwise probably would not have occurred. However, as detector R&D for the SSC concentrates more on the design and fabrication of experiments, the generic program will merge with the subsystem R&D program and, from FY 1991, will no longer be funded through the SSCL. The subsystem R&D program will continue through FY 1991 after which such work will probably be funded as part of the approved detectors. Second-round SSC experiments and other future experiments will likely benefit from ongoing generic detector R&D.

J. The University Program

The role of the universities in HEP has undergone several changes during recent years.

As HEP developed after World War II, research based at universities played a central role. Through the 1970's small teams of physicists, mainly from universities, designed, built, and executed experiments--typically in less than 3 years. Analysis of these experiments typically took about a year. Students, who were often involved in this entire process, could obtain their degrees 4 to 5 years after entering graduate school.

Detector fabrication for these projects largely took place on university campuses. Since new experiments were being constructed while older ones were being completed, the workload was rather continuous, and an infrastructure of engineering and technical support was built-up. Funding was most often directed from the agencies to the universities doing the work.

In the last two decades, with the advent of collider physics and the increased size of fixed-target experiments, the pattern has changed. The time between conception and first publication grew from a few years to perhaps double that time; group sizes grew, with several collaborations having more than a hundred members; and detector sizes, complexity, and costs grew, necessitating organization and funding to be concentrated in the laboratories. Also, in this period, a greater fraction of the university community became involved in experiments at laboratories outside the U.S.

Currently, roughly three-quarters of the high energy experimentalists (40 percent of whom are graduate students) are based in the universities. The roles played by these people in the present programs depend on the particular experiment being pursued, with major differences between those engaged in small fixed-target experiments and those in large collider experiments. While university groups build pieces of a large apparatus, those pieces are usually parts of a much larger whole. Because of the erosion of technical infrastructure, university physicists now usually contribute in those areas that require more modest engineering and technical support. These include simulation, detector R&D,

electronics specification, data acquisition and analysis, and design at the detector-component level. University groups also often provide a large fraction of the manpower to execute experiments and run tests at the accelerators. Finally, the Ph.D. thesis process plays a major role in the detailed analysis of the data.

A primary role of university groups is the education of the next generation of physicists to insure the continuation and future vitality of the field.

IV. INITIATIVES FOR THE 1990's

There are a number of compelling physics questions that will be addressed during the time before the SSC becomes operational. These include the search for and the likely discovery of the top quark, the clarification of the nature of CP-violation, investigations of the intricacies of the Standard Model and, perhaps, glimpses of what lies beyond the Standard Model. There will also be a continuation of measurements of the CKM matrix elements, the determination of more precise nucleon structure functions, and more detailed tests and investigations of QCD, both in the perturbative and non-perturbative domains. Important advances in our understanding of the relationship between particle physics and cosmology may well emerge from powerful new detectors that are just becoming operational.

A number of initiatives have been suggested to facilitate these investigations. Fermilab has proposed to replace its Main Ring with the Main Injector, a new accelerator in its own tunnel. Both SLAC and Cornell are investigating designs for high luminosity e^+e^- storage rings with center-of-mass energy near 10 GeV (B factories) for investigating CP-violation in B meson decays. Other groups have expressed interest in similar devices with center-of-mass energies near 1 GeV (phi-factories) and 4 GeV (tau/charm factories). BNL is studying adding a "Stretcher" to the AGS which would improve the duty cycle of the machine and permit more sensitive studies of weak interaction phenomena. SLAC plans to develop a design for a TeV scale e^+e^- Linear Collider. A number of other, smaller, advanced accelerator R&D projects have also been suggested. The success of the SSC initiated generic detector R&D program has led to suggestions for continuation of that research. New non-accelerator experiments would increase the sensitivity of searches for dark matter, magnetic monopoles, and high energy cosmic-ray sources;

powerful new detectors will search for supernova neutrinos, neutrino oscillations, and address the solar neutrino problem. In addition, there have been proposals to improve the infrastructure and calculational tools of theoretical physics.

This section contains brief overviews of a number of the initiatives presented to the Subpanel and considered in its various scenarios. They illustrate the range of opportunities available for the coming decade.

A. The Fermilab Main Injector

1. Introduction

During its last running period, the Tevatron collider attained a peak luminosity twice its design goal, delivering for the run an integrated luminosity of 9.6 pb^{-1} (picobarn). Fermilab has already embarked on an ambitious program of accelerator improvements to further increase this. A number of upgrades are expected to be operational prior to 1993. These upgrades should give an overall improvement in luminosity of roughly a factor of 6.6.

Fermilab has proposed a construction project to further increase the collider luminosity by yet another factor of 5 by 1995. The main improvement relies on the replacement of the Main Ring with a new Main Injector in a separate tunnel. In addition to providing a peak luminosity of 5×10^{31} cm^{-2} sec^{-1}, the Main Injector project would increase the intensity for Tevatron fixed-target running by a factor of 2, allow for the creation of a high intensity facility with 120-150 GeV proton beams, and remove Main Ring backgrounds from the collider regions. The 120 GeV proton beams would provide year-round opportunities for both kaon and neutrino physics and would also permit the year-round operation of test beams. The physics possibilities of the Main Injector program are explored in more detail in the following sections.

2. Collider Physics

The luminosities obtainable with the Main Injector would allow the collection of 1 fb^{-1} of data during two years of collider running. During this period, the Tevatron will likely remain the highest energy collider in the world. Thus, the data sample would significantly probe the high energy frontier. The number of possible new discoveries include:

a. Discovery of the Top Quark

The CDF group has recently set a lower limit on the top quark mass of 89 GeV. Since the production cross section falls rapidly with mass, large integrated luminosities are required to extend this limit significantly. In addition, the small cross sections require that harsher cuts be imposed to reject background. Such effects further increase the luminosity required for discovery of the top. In a 1 fb^{-1} data sample the accessible top mass will be extended to around 200 GeV. Since this is near current theoretical upper bounds, it is quite likely that the top will be discovered at the upgraded Tevatron.

b. Vector Boson Pair Production

A sizable sample of W-γ events are expected with this large integrated luminosity. These can be used to limit the W anomalous magnetic moment. Standard Model predictions for

WW, WZ and ZZ production indicate each collider experiment would see a few such events; an observed excess would signal new and unexpected physics.

c. New W's and Z's

A one fb^{-1} data sample would provide sensitivity to new vector bosons up to masses in excess of 1 TeV.

d. Compositeness Structure

With such a large data sample, contact interactions up to the 1.8 TeV mass scale would be visible.

e. Other Heavy Particles

Supersymmetric and technicolor particles with masses up to 250-300 GeV would be accessible to Tevatron experiments.

In addition, the Tevatron will produce large samples of W and Z bosons, B mesons, and direct photons. These events will allow precision tests of the Standard Model in both the strong and electroweak sectors. The implementation of high resolution vertex detectors is expected to further improve the ability to study heavy flavors at the collider.

3. Fixed-Target Physics

a. The Main Injector Fixed-Target Program

The Main Injector would provide opportunities for research with very high intensity, but moderate energy, beams. Certain categories of physics can be uniquely explored with high repetition rate proton beams which will become available at 120-150 GeV, and intensities up to 3 x 10^{13} protons per pulse. Such operations would be available either with the collider or with the 800 GeV fixed-target programs in operation. Certain studies of CP-violating kaon decay and of neutrino physics would benefit greatly.

A Fermilab kaon facility would have some advantages of better energy resolution over a lower energy kaon factory, and improved discrimination against background for rare decays with π^0's in the final state. Also, it has often proved easier to obtain higher detector acceptance at higher energies. The two topics of ϵ'/ϵ, and of the CP-violating rare mode K_L to π^0 e^+e^- could be studied with increased sensitivity.

According to the Standard Model, ϵ'/ϵ is unlikely to be zero. This measurement should be pursued with higher sensitivity. The present round of experiments will have errors of order 10^{-3}; sensitivity to ϵ'/ϵ of 5 x 10^{-5} is expected with a dedicated experiment at the Main Injector.

The decay K_L to π^0 e^+e^- or π^o $\mu^+\mu^-$ is of particular interest in that CP-violation contributes in the lowest order. In the Standard Model, the expected branching ratio is about 10^{-11}, but the effective value of ϵ'/ϵ for this mode is of order unity. Present experiments are sensitive to branching ratios of about 10^{-9}, whereas a dedicated experiment in progress at the Tevatron should have a sensitivity of 10^{-11}. The Main Injector should provide for a much more definitive study.

At the Main Injector, the flux of neutrinos above 10 GeV is sufficiently high to anticipate precise measurements of the Weinberg angle and the performance of sensitive oscillation experiments. In particular, the search for the oscillation of ν_μ to ν_τ can be extended in its sensitivity to small mixing angles by nearly two orders of magnitude over the current limit. At these higher energies, the tau lepton from the charged current vertex can be directly observed.

Very long base-line experiments are also possible in such a beam. These can detect mixing in the ν_μ to ν_e channel with a sensitivity in ΔM^2, the difference in the squares of the neutrino masses, about 100 times better than the current best limits. Already existing or planned underground detectors might be exploited for such a measurement.

b. The Tevatron Fixed-Target Program

The 800 GeV physics program would benefit importantly from the planned Tevatron upgrade. The number of available protons should increase substantially. The Tevatron, as the highest energy fixed-target facility in existence, could extend its physics capability, particularly in studies of heavy quark production and decay and in studies of deep inelastic lepton scattering.

Experiments in place, in both photon and hadron beams, could study particles containing charmed quarks. The upgrade will make possible the observation of about 10^6 fully reconstructed charmed particles. Suppressed charm decays and the phenomenon of $D\bar{D}$ mixing are candidates for first observation.

Experiments to study particles containing bottom quarks are promising, but require continued R&D for the development of vertex detectors to operate in this high rate environment. The development of triggers and event filters based on vertex recognition are very important. In these fixed-target experiments, on the order of a few thousand bottom quark particles might be reconstructed through the full B to D decay chain. The separate lifetimes of charged and neutral B mesons would be observable. There are also experiments geared specifically toward production of B mesons with the subsequent decay to two-body final states. As the Main Injector comes into operation, there will be a natural coalescing of experimental efforts around the most promising techniques.

The Tevatron intensity increase will permit accurate measurements of structure functions from high energy neutrino and muon interactions; here one issue is the observation of the decrease of the strong coupling strength at short distances. This could be demonstrated by precise measurements at three or four different distances. This is equivalent to the determination of the QCD parameter Λ to a precision of 10 MeV. Structure functions in the very low x region, important for accurate predictions of rates at the SSC, can be probed effectively. With the neutrino beam, a determination of the Weinberg angle could be much more precise. This, coupled with future precise determinations of M_W and M_Z from collider experiments, can severely constrain or make visible contributions from physical processes outside the Standard Model.

Finally, the improved Tevatron provides to smaller experiments the added flexibility associated with higher intensity; these include studies with polarized protons striking polarized targets and precise studies of the rare decays and static properties of hyperons.

B. CP-Violation in the B Meson System

1. B Factory Initiatives at Cornell and SLAC

The Standard Model permits large CP-violating asymmetries in neutral B meson decays. If asymmetries were not found at the expected levels, it would be evident that the CP-violation in K-decay involves new physics outside the Standard Model. To observe the CP-violation in B decay requires experiments which are about two orders of magnitude more sensitive than those now operating. This is the primary motivation behind plans being developed in U.S. laboratories, and around the world, to design a B factory.

Although this CP-violation question is the physics that motivates the B factory, there is an extraordinarily rich program of other physics available with such a high-luminosity electron-positron collider. Precise measurements of the quantities in the CKM matrix, for example, require comparable luminosity. For studies of the weak decays of the charmed quark and the tau lepton, the B factory provides a sensitivity that is typically one to two orders of magnitude beyond present experiments. The study of light quark and gluon spectroscopy with two-photon collisions also benefits from the luminosity increase provided by such a machine.

Currently, the CESR machine at Cornell is the highest luminosity electron-positron collider in the world. It operates principally at a center-of-mass energy of 10.57 GeV, at the Upsilon (4s) resonance. This resonance has many advantages for the study of B mesons, including a high production rate and a particularly clean final state. The largest sample of B-$\bar{B}$ events collected in a single running period at CESR is about one-half million; a decisive experiment on CP-violation in B decays requires at least one hundred million events. To obtain this extra factor of over one hundred requires electron and positron rings filled with hundreds of closely spaced bunches of particles. In order to keep these bunches separated, all plans for B factories use two rings: one for electrons and one for positrons.

For a symmetric B factory, the beams have the same energy. Since the B mesons are produced practically at rest, the time evolution of the decays cannot be measured. This makes it impossible, while running at the Upsilon (4s) resonance, to measure the CP-violating asymmetry in decays to CP eigenstates. At a symmetric B factory, this asymmetry can only be measured above the resonance, where the B production rate is substantially lower. An asymmetric configuration involves unequal energies of the two beams. In this case, the B mesons move at about half the speed of light and the CP asymmetry can be measured while running on the (4s) resonance. The asymmetric collider is estimated to need 4 to 10 times less luminosity.

On the other hand, this advantage may be offset by more difficult accelerator problems associated with asymmetric collisions. There are some aspects of a symmetric collider that make it simpler than an asymmetric collider. First, all electron-positron colliding beam experience is with equal beam energies, and beam-beam experiments can be performed in existing colliders and interpreted with confidence. Second, equal beam energies simplify the optical design of the interaction region. Third, the vertex detection requirements are not as stringent, which simplifies the interaction region masking and shielding.

The CESR staff at Cornell is working on a variety of designs for a B factory that would use the existing tunnel and some components from the existing facility. Their present thinking is to build a machine which can reach a luminosity of $10^{34} cm^{-2} sec^{-1}$ running as a symmetric machine (i.e., with 5.1 GeV energy in each beam). As a second phase, they would upgrade the machine to operate as an asymmetric collider. They currently believe that the asymmetry feature adds too many unknowns to the already challenging task of building and operating a collider of such high luminosity, so that the approach should be staged. The CLEO collaboration now working at CESR is planning the detector upgrades needed for the B factory.

SLAC is working on a machine design for an asymmetric B factory to be placed in the existing PEP tunnel and would use many PEP components. In the view of the SLAC/LBL team, the lower luminosity needed at an asymmetric collider makes it likely that the CP-violation will be observable sooner with such a machine. They currently plan to proceed directly to an asymmetric machine, with typical energies 3.1 and 9 GeV for the two rings. A team of physicists and engineers from SLAC and LBL are working on a conceptual design for such a B factory, for which they expect a design luminosity of $3x10^{33} cm^{-2} sec^{-1}$. There is also a working group from the two laboratories and 20 other institutions that is studying the design of a detector for such an asymmetric machine.

Both the Cornell and SLAC teams expect to complete conceptual designs within the next year. Besides the machine design effort, there are extensive R&D programs at Cornell and SLAC to test the major concepts behind the B factory, including experiments in the existing CESR and PEP rings. The estimated costs for the B factory lie in the range of $100-200 million, although detailed estimates must await the completion of machine designs.

There are also design efforts for asymmetric B factories at Novosibirsk in the USSR; DESY in Germany; CERN, the European laboratory in Switzerland; and KEK in Japan. All six laboratories are sharing research results in the accelerator physics needed to complete these designs. There is significant Canadian interest in the construction of a U.S. B factory.

2. Observing CP-Violation with B Mesons at a Hadron Collider

The main advantage of using hadron collisions as a source of B mesons is the high rate of B production. For example, with the Main Injector, the Tevatron should produce 2×10^{10} $b\bar{b}$ pairs per year. At the SSC, 10^{12} $b\bar{b}$ pairs per year are expected for a luminosity of $2 \times 10^{32} cm^{-2} sec^{-1}$. However, efficiencies for triggering and tagging the $b\bar{b}$ events reduce the number of $b\bar{b}$ events by a very large factor. Further detector studies and tests are needed to determine whether this technique will allow a sufficient number of $b\bar{b}$ events to be detected to measure CP-violation.

Two groups, BCD at Fermilab and the SSC, and P238 at the CERN SPS Collider, have proposed to examine these problems. The detector systems require aggressive, dedicated R&D efforts over the next few years to evaluate the feasibility of measuring CP- violation in B decays at a hadron collider.

C. Upgrades for the AGS

The BNL AGS is the leading center for the pursuit of new physics at the intensity frontier. Rare kaon experiments at the AGS can probe mass scales in the 100 TeV range for lepton flavor-violating interactions, and can do important studies of CP-violation and of second order electroweak interactions. With the completion of the 1.5 GeV Booster in 1991, the AGS will be able to produce approximately 10^{19} 30 GeV/c protons per week. In combination with upgraded beamlines and detectors, this will permit these experiments to be pushed between one and two orders of magnitude beyond current levels, and will allow other high-precision tests of the Standard Model, such as a new measurement of the muon anomalous magnetic moment.

To double the proton intensity and to improve the duty cycle from 40 percent to almost 100 percent, BNL has begun design studies of a 30 GeV storage ring. The AGS would be run in rapid cycling mode, injecting 6×10^{13} protons into this "Stretcher" ring every 1.2 seconds. Decoupling the extraction from the acceleration functions would also yield improvements in the beam microstructure and in machine reliability. As a result, instantaneous rates would remain constant or decrease while the average number of protons per second increased by a factor of 2.5. Overall, the experimenters anticipate gains of 3 to 5 in the number of useful protons per second. This gain puts virtually no new demands on detectors, and thus can be realized by all experiments using slow extracted proton beams. The mass reach for virtual particles would be increased by 30-50 percent (e.g., to over 200 TeV for an interaction mediating $K_L \to \mu e$), while studies of second order weak processes could move from discovery to measurement status.

The potential also exists for a further, similarly beneficial, factor of two in integrated intensity through full exploitation of the AGS for proton running. The AGS complex could be run twice as many weeks per year for protons without interfering with other programs or compromising essential maintenance.

A proposal by the TRIUMF Laboratory in Vancouver, to the Canadian government, has been made to construct a 100 μamp, 30 GeV proton synchrotron facility (KAON). Such a machine would yield about a factor of ten more protons per hour than the AGS with a booster and stretcher. Early approval and an aggressive construction schedule could result in operation by 1997. Construction of such a machine would logically lead to the continuation of an AGS type physics program in the next century.

D. TeV Scale Electron-Positron Collider Development at SLAC

High energy electron-positron collisions have provided a clean and decisive probe of fundamental physical processes. They offer a surgical tool to explore new physics and to perform precision tests on old physics. The scaling laws for costs of e^+e^- storage rings are such that, for energies beyond LEP II, linear colliders are the more practical technical option.

SLAC is working on the advanced accelerator physics questions associated with this new class of machines. SLC, the centerpiece of this program, is the only operating linear collider in the world. It provides an experimental platform for studies of beam dynamics and beam control, central issues to the success of a high energy linear collider. The SLC will also be used as an injector for a new beam line--the Final Focus Test Facility--which will exploit the very small emittance of the SLC beam to study large demagnification optics where performance depends on the precision with which one measures and compensates for irreducible errors. This unique facility has attracted worldwide interest; a collaboration of Soviet, European, Japanese, and U.S. scientists has formed to build, commission, and exploit this facility.

To achieve the high energies required of these colliders, efficient, high power RF power sources have to be developed. SLAC is testing new 11.4 GHz klystrons, crossed field amplifiers, RF pulse compression, and magnetic pulse compression technologies. High power test results will be available from these programs during the next year; after that the most promising technologies will be selected for engineering optimization. Basic research on RF power production will continue beyond this selection since the leverage of technological innovation in this area has a major influence on the design and cost optimization of future e^+e^- colliders.

Novel accelerator structures are being tested. The new linear colliders require a new kind of structure on which the wakefield effects caused by the multiple high current bunches are essentially eliminated. This is achieved by damping all but the fundamental accelerating mode of the cavity. High power testing over the next year will lead to the selection of one design approach for the development of engineering prototypes of the optimal accelerator structure.

An Engineering Test Accelerator, of 10m length at 1 GeV energy, is being proposed to test the acceleration and control systems so crucial for the high energy linear collider. This facility will also allow experiments on beam dynamic studies of the beam cavity interactions and the development of better controls. The experience gained in this facility will help in system integration and optimization for the high energy collider machine.

The accelerator physics efforts will be focussed on a specific design of a collider with luminosity greater than 2.5 10^{32} cm^{-2} sec^{-1} and a center-of-mass energy of 500 GeV. Both luminosity and energy will be expandable, and the study will be non-specific as to site. The goal is to produce a conceptual design report within 5 years. Such a design effort forces studies of system tradeoffs, realistic evaluation of the technologies, and proper attention to system integration issues.

E. Other "Particle Factories"

In addition to proposals for B factories, which were discussed in Subsection B, plans were presented to the Subpanel for a detector at a tau-charm factory, and for the construction of a ϕ-factory.

The tau-charm factory is an electron-positron collider operating at an energy between 3-4.5 GeV and with a luminosity of 10^{33} cm^{-2} sec^{-1}. The physics motivation is based on the experience of SPEAR at SLAC and other machines operating in this energy range. Currently, the full exploration of this physics is limited by numbers of events. The physics menu includes precision measurements of the tau-lepton and the tau-neutrino masses, and the tau-rho parameter. In addition, detailed studies of charm particles would be possible, including doubly forbidden Cabibbo decay modes, rare and forbidden decays, D_0-$\bar{D}_0$ mixing, and pure leptonic decays of D^+ and D^o's.

A tau-charm factory might be built in Spain. The Spanish Federal and Andalusian governments have indicated that the necessary funds could be made available, assuming technical assistance is provided by CERN and possibly other European laboratories. A U.S. group has expressed interest in collaborating on an experiment at such a facility.

A phi-factory is an e^+e^- collider with a center-of-mass energy of 1.02 GeV (the mass of the ϕ-meson) with a luminosity of 10^{32} cm^{-2} sec^{-1} or more. This would allow the production of 10^9 ϕ's in a typical 1 year run.

The main physics motivation is the unique opportunity the ϕ-meson provides for studying CP-violation in the K-system. The ϕ-meson decays 34 percent of the time into a pair of neutral K mesons. The quantum numbers of the ϕ-meson require this to be a K^0_L-K^0_S pair, giving an extremely clean experimental handle.

In addition, tests of CPT invariance, the study of various radiative decays of the ϕ-meson (in particular into the pseudoscalar η and η' mesons), and investigations of the scalar a- and f-mesons would be performed.

A group at UCLA is preparing a design for a ϕ-factory with the hope of proposing its construction. Similar efforts are going on in Frascati (Italy), KEK (Japan), and Novosibirsk (USSR). The estimated total cost of the accelerator and detector is about $60M.

F. Non-accelerator Physics/Physics at Accelerators Abroad

Non-accelerator physics has emerged as a major new scientific endeavor, addressing fundamental questions on the borderline of particle physics, astrophysics and cosmology. There is a large diversity of experimental efforts with major activities directed towards a few fundamental issues. New experimental initiatives may be expected over the next few years.

One of the most intriguing problems in particle astrophysics involves the nature of dark matter, the unobserved matter which is assumed to make up most of the mass of the universe. There are strong suggestions that dark matter is of a non-baryonic nature, meaning that it is different from the building blocks of our natural world; it may well consist of a yet unknown elementary particle species. There are many conjectures of candidate particles for this matter which may be experimentally testable. However, such tests generally pose extremely challenging technical problems. New techniques to detect these particles directly are being developed. One special class of experiments is based on the assumption that the dark matter particles are annihilated within the sun leading to high energy neutrinos. The detection of these neutrinos would provide indirect evidence for this hypothesis.

Grand Unification Theories (GUT) postulate the unification of the strong and electroweak interactions. The energy for this Grand Unification is expected to be of order 10^{15} GeV, an energy range only accessible to non-accelerator experiments.

The experimental consequences of Grand Unification are being sought in new experiments on unexplored decay channels of the proton (e.g., the SOUDAN underground experiment), an ambitious search for Grand Unified Monopoles (MACRO at the Gran Sasso laboratory in Italy), and long base line searches for neutrino oscillations. These new detectors will soon come into operation. Others are still in the planning stage.

The observations of a deficiency of neutrinos from the sun has stimulated both a great deal of theoretical and experimental work to resolve the puzzle. The effect might be due to undiscovered properties of neutrinos or possibly indicate a problem with the theoretical understanding of some of the fusion reactions in the sun. This has prompted new projects including two experiments which use Gallium as detector material to determine the rate of low energy neutrinos coming from the primary pp reaction in the sun. Another experiment uses heavy water (SNO in Canada) to measure reactions in order to determine whether different neutrino flavors convert from one to another.

Finally, there are various indications in extensive air shower arrays that cosmic point sources may exist which produce high energy cosmic γ-rays up to 10^{15} eV or more. These candidate sources are binary star systems and could indicate unknown acceleration mechanisms of hadrons in those systems. These binary stars may represent the main source of very high energy cosmic rays. To generate photons with such high energies, hadronic production of π^0's are necessary. The observation of these high energy γ-rays is important both from the particle physics and astrophysics standpoint. New experiments are just coming into operation and others are planned or proposed. If such sources of high energy particles exist, both neutral and charged pions will be produced. The charged pions yield very energetic neutrinos. These are also being sought in present and future underground and underwater experiments. The detection of these neutrinos are fundamental to the underlying physics of the stellar collapse.

Many new non-accelerator initiatives are likely to be based in other countries, consistent with past experience. Initiatives in other nations, at existing or new accelerator laboratories were presented to the Subpanel, and others are likely to come forward over the next decade. For example, countries considering or planning new accelerator facilities are: Canada, Spain, USSR, and Japan.

G. Theoretical Initiatives

Many open problems in theoretical particle physics are exciting, challenging, and important. These are tied directly to the goal that motivates the entire field: to explore and understand the structure of the physical world at its most basic level. Intimately related to ongoing and prospective experimental activities are a number of topics that would benefit from systematic, concentrated theoretical analysis. A partial list includes computations of higher order contributions for electroweak and strong interaction processes; calculations of hadronic matrix elements of weak currents; systematic investigations of promising signals for top, Higgs, and supersymmetry; studies of weak decays and the physics of the weak mixing matrix; analyses of hadron structure functions and fragmentation; and investigations of hadronic jet phenomena. Concerted theoretical effort along these lines is essential for full exploration of the implications of existing data and for effective design of future experiments and facilities.

A committee of theorists and experimenters was established in 1989 by the DPF, APS to examine the need for theoretical work in areas which have clear contact with experiments and to suggest mechanisms by which theoretical research of this kind can be fostered. The DPF committee has made a number of recommendations including support for long-term workshops on important topics in phenomenological particle physics, for focussed visitor programs at the national laboratories, and for summer programs at the laboratories aimed at introducing theoretical graduate students to problems of current phenomenological interest. The DPF committee further recognized the need to commit funds for graduate students, postdoctoral, and faculty positions for individuals whose research deals with problems of experimental interest.

A collaboration of phenomonologists, lattice gauge theorists, and theoretical and experimental physicists with extensive experience in computer software and hardware propose to design and construct (with considerable industrial collaboration) a 1 teraflop computer optimized for lattice QCD. By performing quenched calculations on very large lattices (e.g., 128^4) and full QCD calculations on somewhat smaller lattices (e.g., 64^4) the machine would be able to provide a variety of important strong interaction physics results. Some examples are: (a) calculation of weak matrix elements in K, D, and B decays to better than 25 percent precision, (b) a variety of hadron spectroscopy calculations able to guide future glueball search experiments, and (c) studies of the quark-gluon plasma with controlled lattice-spacing and finite-volume errors. The total cost of this project is estimated to be about $30M.

H. Detector R&D

The development of the appropriate detector techniques is crucial for high energy physics experiments. Some examples of specially developed detector methods are wire chambers for charged particle tracking, silicon microstrip vertex detectors, ring imaging Cerenkov counters, and detectors to search for magnetic monopoles. The experiments running today would not be possible without past detector R&D. Experiments at future accelerators and upgrades of existing detectors to allow more sensitive measurements require detector R&D done now. The DOE supported a very successful program of generic R&D for SSC detectors. Now that the SSCL is concentrating on the design and construction of detectors for the SSC, it will no longer be supporting generic detector R&D. It has been proposed that generic detector R&D should continue at the level of $2.5-5M per year. Some examples of areas for future generic R&D funding might be silicon drift detectors, new calorimeter techniques, and high-rate data acquisition systems.

V. RESOURCES

A. Interaction of SSC and HEP Resources 1990-2000

1. Impact of SSC on the Rest of the HEP Program

One of the major challenges of the next decade for the U.S. HEP program will be to phase in smoothly what will undoubtedly be an SSC-dominated program at the turn of the century. The challenge lies in the fact that while the SSC program will be making continually larger demands on HEP resources, it will not commence physics research until the end of this decade. Accordingly, the health and future of the U.S. HEP program is very much dependent on providing exciting research opportunities for the entire U.S. HEP community and appropriate training facilities for graduate students, postdoctoral fellows, and junior faculty during the next decade.

Probably the main impact will come from the large effort required to build the SSC detectors, an effort that must start now in order to be ready at SSC turnon. There will be an additional impact due to the human and material resources required to staff and operate the SSCL.

It is convenient for the purpose of this summary to consider four areas of possible impact: financial, manpower, construction facilities, and test beams. Furthermore, to discuss the impact of detector construction, one may want to identify four time phases, devoted mainly to: (1) generic detector R&D, small prototype construction, and small-scale beam tests; (2) design of detectors, fabrication of full-size prototypes, and large-scale beam tests; (3) building and calibration of complete detectors and software development; and (4) the installation and commissioning of detectors, and debugging of software in a complete detector environment.

It is reasonable to assume that each of these phases will take approximately 2-3 years and that they will overlap each other.

a. Financial Impact

It is clear that the financial impact will be dictated to a large extent by the actual funding policy of the DOE. Given the official policy of "new" money for the SSC program, there should be minimal impact during phases (3) and (4). It is probably reasonable to expect that a significant fraction of phase (1) will be funded by the existing HEP program, since many of these efforts cannot be clearly identified as unique to the SSC and will frequently be a part of the ongoing experimental program. This total effort will probably need to be at the level of a few million dollars per year for the program to proceed at the optimum rate. The Subpanel's recommendation for funding generic detector R&D is formulated, at least partly, in response to the need for this effort. Regarding phase (2), the major part of the funding is expected to come from the SSC equipment funds, outside the HEP base program. Some HEP resources (physicists' time, some technical support, and some operating funds) will be redirected from the ongoing HEP effort. This phase will require significant engineering effort, which is not presently available at the universities. The recommended increase in infrastructure would be of great value here.

Regarding the SSCL itself, the Subpanel assumes that the construction funds will be incremental to the current HEP program. Some ongoing physics research by the SSCL staff which lies outside the SSC project is anticipated during the SSC construction phase. It is reasonable that some of the funding for such efforts come from the HEP base program.

b. Manpower Impact

The details of the manpower projections are discussed in Section B and Appendix E. For the purpose of this discussion, the Subpanel assumes that roughly half of the U.S. high energy physicists will have SSC research effort as their dominant activity by the time SSC starts producing collisions. In the intervening years, the Subpanel estimates a growth of about 2 percent per year in the U.S. HEP population. Thus, there can be expected to be about 35 percent diminution of U.S. manpower in the existing activities at currently operating laboratories, laboratories abroad, and non-accelerator experiments.

This projected shift will be rather gradual. The Subpanel expects the first two phases of detector fabrication to present some of the biggest challenges. The main reason for this assessment is that those stages are sufficiently removed from actual physics output so that existing university groups cannot undertake those activities as their sole effort. Graduate students and postdoctoral research associates will be involved in and will contribute to this stage of detector development, but at the same time, they also have to participate in the ongoing physics research. For these reasons, the initial phase of detector construction will rely heavily on additional postdocs doing this work part of the time, along with professional engineers and technicians. The latter resources are in very short supply in university groups today.

The last two phases will necessitate full-time involvement of a sizable fraction of the U.S. HEP population. They will occur at a time close to potential physics output so that graduate students and postdocs would naturally participate in these activities full time. Thus, the Subpanel anticipates that there will be a significant shift of HEP personnel from current activities to SSC effort during that time.

The projected growth of the SSCL technical staff will undoubtedly have some impact on the ongoing program. On the other hand, this impact may not be as large as the projected numbers might indicate superficially. There are several reasons for this. First, some fraction of the SSCL staff--particularly technical personnel--will come from outside HEP itself. Second, one can expect significant influx of people from abroad, drawn by the opportunities the SSC will provide. Third, some fraction of the scientific staff, especially younger people, will continue their prior research. Finally, the Subpanel expects larger retention in the field of graduating students who will be induced to remain because of the new challenges and opportunities provided by the SSC.

c. Fabrication Facilities

The Subpanel expects that the fabrication facilities existing at the present HEP laboratories will be heavily used for SSC detectors. They will undoubtedly be augmented by similar resources at other non-HEP laboratories whose personnel might want to be involved in various SSC activities. Universities, with upgraded infrastructures, could provide significant resources toward fabrication of these detectors. In addition, the Subpanel expects much heavier use of large-scale fabrication facilities available in U.S. industry. Finally, some of the detector components will undoubtedly be built abroad. For optimal detector fabrication, it is highly desirable or even essential that there be intellectual involvement in the SSC research at the institution where detectors are fabricated. The Subpanel also expects that the present U.S. research laboratories will play some part in construction of SSC accelerator components. In spite of an anticipated lack of large-scale detector fabrication facilities at the SSCL during the next decade, the Subpanel does not anticipate significant shortage of resources in this area.

d. Test Beam Facilities

There probably will not be any test beam facilities at the SSCL until the second half of this decade. Accordingly, the bulk of the beam tests that will be needed during the design and fabrication of the SSC detectors must be carried out at the existing U.S. high energy laboratories. Some of the preliminary tests, mainly during phase (1), may also be done adequately at the existing low or medium energy facilities. During phases (2) and (3), the great majority of tests will have to be carried out at BNL and Fermilab. There could be significant problems if test beam facilities are not continuously available at Fermilab. The availability of the Main Injector at Fermilab may be crucial to a satisfactory solution of this problem.

The experience obtained with construction of recent large-scale detectors shows that a great deal of test beam time is required for their design, fabrication, and calibration. The SSC detectors will also require considerable technology development during early stages (phases (1) and (2)), which will put additional strains on existing test beam facilities.

B. Manpower Considerations

At present, groups are forming and actively working to define SSC experiments. These activities are expected to grow substantially as the SSC approaches commissioning. Given this, and the needs of the base program outlined in this report, the Subpanel has estimated both the manpower required and the manpower available.

The manner by which the Subpanel arrived at the manpower resources in HEP research during the next decade is described in Appendix E. From this effort, the Subpanel obtained a picture of the presently available manpower. These observations are summarized briefly by the following:

1. The number of senior scientists, presently about 940, has remained approximately constant since 1985.

2. The number of postdoctoral research associates, now about 330, has increased by approximately 3 percent per year since 1985.

3. The number of Ph.D. students, presently about 580, has increased by approximately 6 percent per year in this period.

These results indicate that HEP research activities have continued to attract large numbers of young physicists, reflecting the intellectual excitement of the field.

The results of the various scenarios for the HEP program studied by the Subpanel and the manpower needs anticipated by the SSC as the decade progresses, indicate that toward 1999 approximately 2,150 U.S. physicists will be needed to maintain a healthy HEP program. The present number in the U.S. found in the surveys, is approximately 1,850. The Subpanel concludes that the number of HEP scientists should increase by approximately 300 toward the end of the decade. This represents an increase of less than 2 percent per year, which is consistent with the average growth in the physics community over the last 15 years as shown in Figure 2 of Appendix E. Such growth can be accommodated by future positions available at the SSCL and by a moderate increase in the university population, given adequate funding.

This analysis agrees with the Subpanel's sense of a strong and healthy HEP program near the end of the decade, in which approximately 50 percent of the community is involved in SSC experiments and 50 percent in other experimental programs. Ultimately, the actual mix will depend on the intellectual attraction of the scientific enterprises and the judgments of the community's physicists. The Subpanel concludes that, given the uncertainties on any extrapolation of this kind, the community of physicists will be sufficient to ensure a compelling SSC program and an exciting complementary program over the next 10 years and beyond.

C. The Role of the Universities in the 1990's

University-associated personnel comprise about three-quarters of the scientists in the field. During the 1990's they will continue to participate in the ongoing experimental programs at the existing laboratories and gradually increase their activities related to preparations for the SSC. A large fraction of the new activity will be engineering and technical. Increased demands will be made for activities appropriate for university groups, including the design, specification, simulation, testing, and calibration of detectors. Much of this work, especially that requiring test beams, will closely resemble that done for fixed-target experiments. Thus, opportunities and responsibilities for university groups should increase over the next several years.

The Subpanel found strong arguments and general enthusiasm for continuation of an active research program during construction of the SSC. The conclusions of our manpower survey, described in Section B, indicate that the population of physicists is expected to match the need. But there are other questions regarding the health of the university community which are addressed below.

The Report of the HEPAP Subpanel on Future Modes of Experimental Research in High Energy Physics (Treiman Subpanel Report) noted the deterioration of university mechanical and electrical shop facilities. The number of groups with automated design and fabrication tools, as well as those with adequate engineering and technical support staff, has decreased. This has a deleterious effect on the ongoing program, and could seriously hamper the development of SSC detectors. Effective utilization of the manpower and scientific capabilities of the university groups in bringing up SSC experiments must be addressed.

The first recommendation of the Treiman Subpanel Report offers a solution: "(a) A program specifically aimed at upgrading instrumentation and research facilities at HEP universities should be sponsored by DOE and NSF. . . . (b) In parallel, we recommend a program specifically aimed at rebuilding the technical support staffs associated with university groups." The Treiman Subpanel Report recommended an increase in support for these areas totaling about $10M per year.

Other issues relevant to the health of the universities arise due to the length of the SSC construction period. One issue is that this time interval is significantly longer than that normally required for graduate training leading to a Ph.D. degree. Another is that, for junior faculty involved in detector fabrication, there will be little opportunity to demonstrate the ability to do the independent, innovative HEP research that is required for promotion.

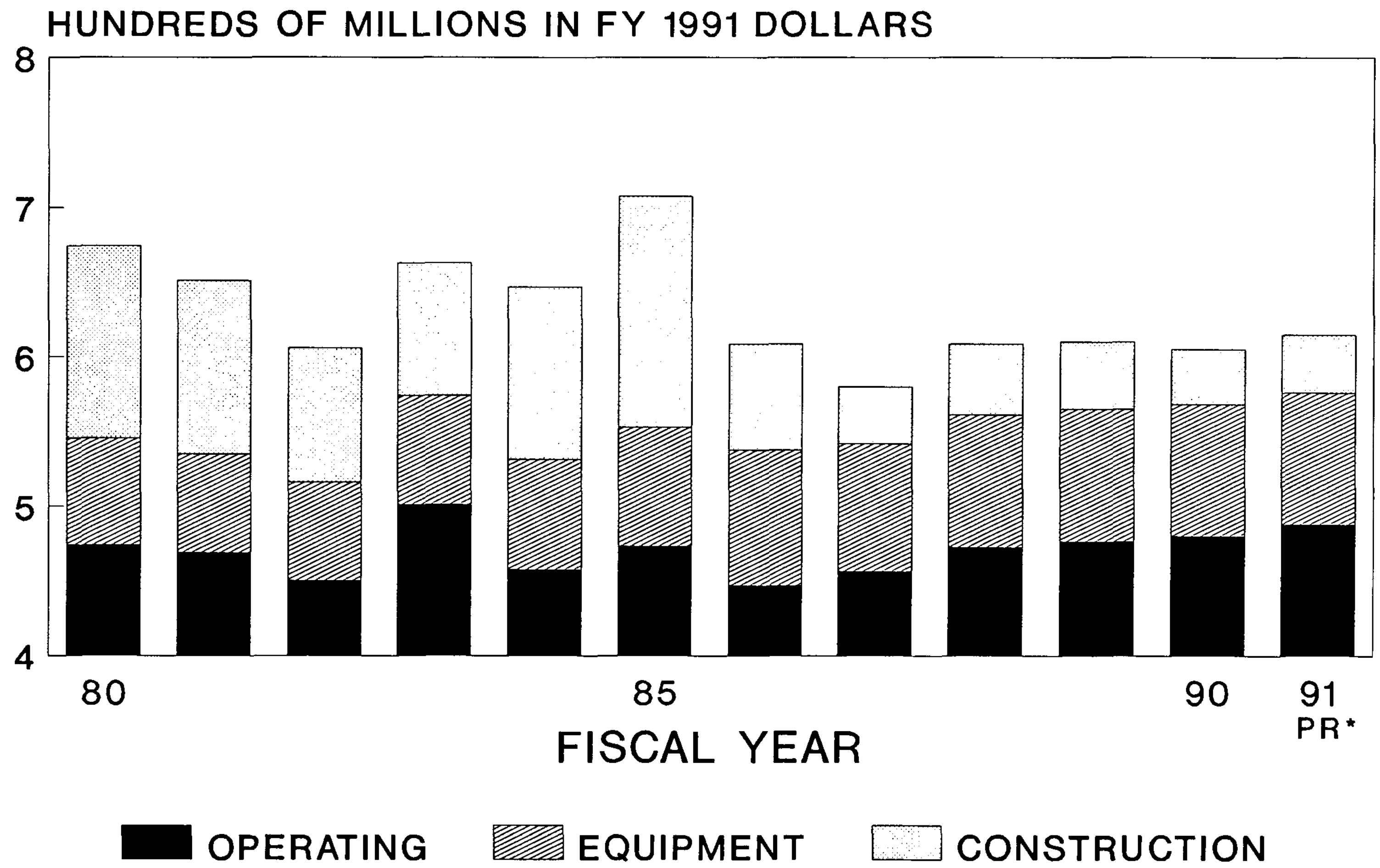
These concerns are assuaged by the existence of a vital ongoing research program. This program will give young physicists the opportunity to actively participate in forefront research while making essential contributions to the design, fabrication, and testing of the SSC detectors.

D. Budgetary Considerations

The support of the U.S. HEP program over the past 10 years is shown in Figure V-1 (in 1991 dollars) for operating, equipment, and construction funds. The field has been supported at (FY 1991 dollars) an approximately level budget of $620M per year (DOE) and $50M per year (NSF), while the population of physicists has increased over the same period (see Appendix E). During this period there have been several line item construction projects at the major laboratories (the AGS Booster at BNL, CESR upgrade at Cornell, the SLC at SLAC, and the Computer Center and Linac at Fermilab) which have required sizable temporary upward excursions in the funding level. In addition, a number of large detectors have been fabricated during that time (CDF and D-Zero at Fermilab, CLEO II at Cornell, SLD at SLAC, the LEP detectors at CERN, ZEUS at DESY, and AMY at KEK).

Figure V-1

HIGH ENERGY PHYSICS FUNDING



SSC not included

* PR = President's Request

at DESY, and AMY at KEK) to exploit the physics opportunities at the accelerator facilities and several large experiments have been built to study particle physics questions without accelerators (MACRO, SOUDAN II, IMB, etc.).

The Charge to this HEPAP Subpanel requests advice on planning a viable and productive HEP program under a series of budget assumptions: (1) the budget will remain constant at the level of the President's budget request for FY 1991, (2) the budget will increase in real dollars at the rate of approximately 1 percent per year, and (3) the budget will follow a profile which, when averaged over the 1990's, is 5 percent below the FY 1991 level in real dollars. The FY 1991 President's budget request for HEP is $621M and for NSF $52M. In response to the Charge, the Subpanel considered scenarios with the present funding level and scenarios with the levels enhanced or reduced by $300M over the decade. The Subpanel interpreted this Charge as imposing a ceiling on the total expenditure during the 10-year period but allowing upward and downward fluctuations from year to year.

To study the effects of the above budget scenarios on the physics program, the Subpanel attempted to identify the portion of the HEP budget historically used for the continuing evolution of the scientific program. This was done by separating the budgets of recent years into two parts: (1) the amount necessary for support of the existing base of scientists and the continued operation and exploitation of existing facilities, and (2) the amount used to support the growth of scientific manpower, new experiments, and the construction of new facilities. Obviously, such a division is quite subjective. To estimate the total monies applicable to new initiatives, the Subpanel added to the second category funds which should become available as experiments end later in the decade. Although these total funds are a small fraction of the HEP budget, they provide the flexibility for investment in the future that is crucial for progress in the field. This amount was taken as the available funding in the constant budget scenario, and varied up and down by $300M in the two extreme scenarios.

A list of projects and initiatives was next developed to be set against the amount established above. This list was compiled from the many verbal and written representations. It can be usefully divided into three categories.

1. Initiatives targeted at the basic scientific manpower of the field, the university physicists: their number, their support, and their ability to shape and carry out the experimental program.

2. Current and proposed programs at U.S. and foreign accelerators, whose funding demands were determined from proposals and presentations to the Subpanel.

3. Major new facilities: the descriptions of these were drawn from the proposals presented to the Subpanel and from workshop studies.

The total funds required to address the complete list established here far exceeded the level of programmatic funding available. From these initiatives, we identified broad categories to represent a healthy physics picture in the 1990's. This is discussed further in Section VI.

Following discussions by the Subpanel, models were constructed which attempt to satisfy the constraints of the three scenarios of the Charge. We feel that despite the uncertainties inherent in this procedure, we established that the programs recommended below are possible within the appropriate funding level when averaged over the 10-year period. Strict adherence to the year-by-year constraints of the scenarios was not possible, unless fluctuations were allowed to accommodate line item construction projects.

VI. RECOMMENDATIONS AND COMMENTS

In this section, the Subpanel presents its response to the Charge of developing a viable and productive U.S. HEP program under three budget scenarios. The Subpanel's recommendations for a scenario in which the FY 1991 budget is maintained, on average, throughout the decade are presented first, with its judgment of what the effect of those recommendations would be. The recommendations and effects of a 10-year budget with an average increase of 1 percent per year and with a decrease of 1 percent per year are then presented.

The Subpanel interpreted the Charge of "constant budget scenario" as referring to total expenditure over the 10-year period under study, i.e., it did allow for departures from the FY 1991 funding level in any one year, provided that the total 10-year sum satisfied the overall constraint. There are two main reasons for this procedure. First, the Subpanel realized that it is impossible to have any significant new initiatives while preserving the health of the base program unless this flexibility exists. Second, the Subpanel felt that the only possible way to plan a healthy program is to be able to map out a budget scenario for a 5-10 year period with a certain latitude on the spending limit in any one year.

The Subpanel would like to stress that its recommendations cannot be divorced from these budgetary assumptions. It felt that a healthy program could not be generated within the constraints of a rigid year-to-year constant budget without the flexibility to transfer funds from one year's allocation to another.

The highest priority in the U.S. HEP program is swift construction of the SSC and appropriate preparation for its optimal utilization. Since the initial HEPAP recommendation of 1983, new physics results, technological developments, and progress made on the project itself all reinforce the case for the SSC.

A. The Constant Budget Scenario

In arriving at its recommendations for the constant budget scenario, the Subpanel recognized that new initiatives require some judicious redistribution of resources. Some resources will become available naturally over the course of the next 5 years, as some experiments at all of the laboratories reach a natural and successful conclusion, while others coalesce around a common goal. The Subpanel notes with concern that it was unable to accommodate several attractive opportunities to pursue a broad range of forefront research under this scenario.

Under the constant budget, the Subpanel will recommend only one major construction project, the Fermilab Main Injector. The Fermilab Tevatron will remain the highest energy accelerator in the world for most of the decade. Construction of the Fermilab Main Injector will provide a significantly higher collider luminosity. This will likely lead to the discovery of the top quark and the elucidation of its properties. The experimentation with high luminosity will provide important experience in preparation for work at the SSC. In addition, the very high intensity 120-150 GeV protons from the Main Injector will also allow a new program of fixed-target initiatives.

The Fermilab 800 GeV fixed-target program includes important experiments addressing a variety of issues such as hadron structure and heavy quark production and decay. The intensity increase will enhance these. Many experiments will successfully conclude data taking by the mid-decade while others will likely consolidate around common goals so that a net contraction is likely.

To address the competitiveness of the SLC physics program in the LEP era, some members of the Subpanel and several accelerator physics consultants met at SLAC for a one-day technical review of the SLC accelerator program and the Laboratory's preparations for improved luminosity. The group concluded that the Laboratory goal of 10^5 Z^0's per year with 40 percent polarization was credible, but the projected target date of late 1991 was somewhat optimistic. The proposed physics program with the SLD experiment and the polarized electron beam was also reviewed; it was concluded that precision measurements of the Weinberg angle through the left-right asymmetry and studies of B mixing would be competitive with LEP if the machine performance goals were achieved. The Subpanel concluded that it was important to vigorously pursue the SLC/SLD program to the mid-decade subject to the successful implementation of the luminosity and polarization upgrades of SLC.

SLAC is a major center for accelerator R&D both in the area of high luminosity (the B factory) and high energy linear e^+e^- colliders. There is a substantial international effort on the design of high energy e^+e^- linear colliders in Japan, USSR, Europe, and in the U.S. At present, SLAC is the leader in this effort.

The Subpanel emphasized the importance of full exploitation of the physics opportunity offered by the kaon rare decay program at BNL. This program has already achieved unprecedented sensitivities to lepton flavor-changing and other non-Standard Model decays. The AGS, which is already the source of the highest available kaon flux, will soon benefit from a fourfold increase in intensity. This upgrade will significantly enhance the reach of the facility for non-Standard Model physics and make possible much improved measurements of Standard Model parameters.

Since the focus of AGS activity is anticipated to shift to the study of relativistic heavy ion collisions after the middle of this decade, the Subpanel stressed the need for adequate and timely support of the kaon program. To realize fully the available opportunities, adequate support should be given to the construction of the upcoming generation of beams and detectors, and the operation of the AGS should be strongly supported during the first half of the decade. In this way, the Subpanel believes, the kaon program can be substantially completed as BNL enters the RHIC era. However, since the AGS will retain the capacity for fixed-target proton running, it could be exploited economically later, if the physics warrants it.

The Subpanel anticipates that the new opportunities for B physics at Cornell will result in a long and active program. CLEO II, a second generation detector optimized for B physics, was recently completed and is now taking data. Further luminosity increases are planned for CESR, which holds the luminosity record for e^+e^- colliders. R&D for a B factory, now well underway, has become the focus for the longer term.

The Subpanel recognized that foreign-based and non-accelerator physics provide essential opportunities to increase the diversity of our field. We have anticipated the possibility of a new initiative in one or more of these areas, and we believe that even under a constant budget scenario resources must be available to take advantage of these possibilities. In addition, we recommend continued support of the existing non-accelerator program at roughly the current level and believe it is important to provide for some modest enhancements to existing detectors at both U.S. and foreign accelerators.

Generic accelerator R&D has been an active area of research in the past several years. This activity is important for the advancement of the field, for the much needed education of new accelerator physicists, and for the conception of the next generation of high energy accelerators. The Subpanel believes the existing program of generic accelerator R&D should continue in the coming decade.

In constructing the recommended program under the assumption of a constant budget, the Subpanel emphasized exploitation of existing facilities and strengthening the university programs.

The Subpanel strongly endorsed the physics potential of the B factory, and recommended a vigorous R&D program leading to a proposal to build such a facility. It also would give high priority to construction of such a machine, if a design luminosity sufficient for exploring CP-violation could be demonstrated and if the funding level permitted.

It is impossible to reconcile a constant budget scenario, however, with two large construction projects and responsible support of existing programs. Each of the large projects, the Fermilab Main Injector and the B factory, would provide excellent science and received strong support from the Subpanel. The Main Injector has a complete design and should proceed immediately. The B factory will not be ready to be built until satisfactory design and construction plans are complete. For these reasons, construction of the B factory is not in the constant budget scenario. This compelling physics opportunity could be regained in the rising budget scenario or if the agencies (DOE, NSF) were successful in obtaining incremental funds.

We proceed now to discuss our specific recommendations for the constant budget scenario:

1. The Subpanel strongly recommends the immediate commencement and speedy completion of construction of the Tevatron Main Injector at Fermilab.

   The construction of the Fermilab Main Injector allows proper exploitation of the Tevatron's unique energy reach by significantly enhancing its intensity. The Main Injector guarantees that the Tevatron will remain the premier high energy collider facility in the world in the pre-SSC era. As just one example, it allows the exploration of the full mass range for the top quark favored by present day theory. In addition, this new facility presents important new opportunities in the area of fixed-target physics, including very high intensity beams from the Main Injector. These Tevatron capabilities are important to position the U.S. HEP community for the optimal future exploitation of the SSC.

2. The Subpanel recommends strong exploitation of the existing high energy facilities to take advantage, in a timely way, of the many physics opportunities available.

   The next decade offers many diverse physics opportunities to reap rewards of the modest and well-planned improvement programs of the past few years at existing U.S. accelerator laboratories. The Fermilab Tevatron, thanks to the U.S. previous investment in superconducting magnet technology, is the highest energy accelerator-collider complex in the world and, with the Main Injector, will be the optimum instrument to probe the energy frontier in the pre-SSC era. The BNL AGS, augmented by its booster injector nearing completion, provides the most intense kaon beams in the world, and thus offers unique ways of probing the Standard Model and exploring what lies beyond. The world's first linear collider, the SLC at SLAC, will soon begin a unique program of high energy e^+e^- collisions with polarized beams. The recently completed upgrade of CESR at Cornell, accompanied by its sophisticated new detector, will provide the best means of studying a whole spectrum of issues in B physics, a field of rapidly increasing importance.

Each of these facilities offers unmatched opportunities for new breakthroughs and will remain at the cutting edge of the field at least into the second half of this decade. Relatively small incremental funds can significantly increase their operating time and allow the physics community to properly exploit them. We strongly urge taking advantage of these unparalleled opportunities.

3. The Subpanel strongly endorses the physics aims of a B factory and recommends a vigorous R&D effort leading to a proposal to build such a facility.

   A high luminosity B factory would allow precision studies of new manifestations of CP-violation, a fundamental problem in particle physics. There are large, enthusiastic communities of accelerator and experimental physicists committed to this physics goal at Cornell, LBL, SLAC, and collaborating universities. The existing CESR and PEP storage rings are well suited for experimentally solving the demanding problems associated with high luminosity colliders. We encourage cooperation among the interested communities so as to establish the strongest possible R&D program. If the R&D is successful and a B factory with adequate luminosity is designed, we hope that the agencies could provide additional support for construction.

4. The Subpanel recommends significant enhancements in the support by DOE and NSF of university groups in the areas of technical infrastructure and scientific manpower.

We note the deterioration of the technical infrastructure of the universities, and reiterate Recommendation 1 of the Treiman Subpanel Report. That recommendation proposed creation of a program sponsored by DOE and NSF specifically aimed at upgrading this technical support base. This base includes, but is not limited to, professional engineering and technical support personnel, state-of-the-art instrumentation, design tools, shop equipment, and computer and networking equipment.

The intellectual excitement of the field has led to a gradual growth in the number of high energy physicists over the past 15 years. The funding of university groups has not kept pace with this growth. To bring up SSC detector systems and efficiently execute the ongoing program, this growing base of high energy physicists must be better supported.

5. The Subpanel recommends that the NSF substantially increase support for its HEP university groups, particularly for equipment.

   HEP research is supported by both the DOE and the NSF. This system provides a valuable flexibility; the DOE is the lead agency for the field, providing most of the support for HEP laboratories, while the NSF supports about 35 percent of the university physicists and the Cornell accelerator facility. Nevertheless, for some time a significant disparity has existed in the levels of support of university groups between the two agencies. In particular, sufficient equipment funds are not available for NSF-supported investigators, and this lessens their contributions to collaborations in such areas as engineering resources, experimental equipment, and computational power. This situation has not been rectified since the Treiman Subpanel Report recommended in 1988 "an increase of a least $5M in the annual level of NSF support to enable the existing university groups to exploit their potential."

University groups do the major fraction of the physics at the HEP laboratories, and they will be major users of the SSC. The recommended increase in NSF support would have an enormous leverage for producing science and training scientists. We urge the NSF to recognize this.

6. The Subpanel recommends continuation of a vigorous program of R&D at SLAC for very high energy electron-positron linear colliders.

   As has been evident throughout the past 20 years, electron-positron and hadron colliders bring different strengths to the study of elementary particle physics. We expect that a (1-2) TeV e^+e^- collider will provide a clean, incisive probe of the physics at that energy. Linear colliders are the only known route to such high energy e^+e^- collisions, and SLAC, by virtue of its successful pioneering work with the SLC, is the world leader in linear collider development. The research necessary is demanding and requires a systematic attack on a broad frontier. Continuing operational experience with, and improvements of, the SLC for accelerator R&D are important parts of that research; elements of the SLC will surely be central to future accelerator experiments. A substantial international effort is being focused on the design of high energy e^+e^- colliders, and groups have joined to develop a new facility at SLAC to create and to study very small (50 nm), intense electron beam spots.

7. The Subpanel recommends that the Division of HEP provide support for the SSCL physicists' basic research activities that lie outside the SSC project.

   It is essential that physicists at the SSCL have the opportunity to contribute to and participate in the intellectual excitement of active research during the period of construction of the SSC. The

Subpanel recognizes that pressures on the HEP budget may preclude full support.

8. The Subpanel recommends that both non-accelerator and foreign-based experiments continue to be strongly supported.

   Non-accelerator and foreign-based accelerator experiments have become important parts of the U.S. HEP program. The non-accelerator experiments cover a wide range of particle and particle-astrophysics questions that do not require accelerators, and the foreign-based experiments use the unique accelerators in other countries. Together they have brought new physics opportunities to U.S. physicists and have diversified the program. We expect these areas will continue to be important components of the program, and we believe that support for these activities should continue at roughly the present level. We note that these activities represent an important component of international collaboration in HEP.

9. The Subpanel recommends increased support for generic detector R&D. The future of our field depends on nurturing innovative ideas in detector technology. Generic R&D is essential to accomplish this.

B. The Rising Budget Scenario

All of the recommendations for the constant budget scenario are again endorsed for the rising budget scenario. For the reasons stated in the previous section, the Subpanel did not recommend the construction of a B factory under a constant budget. Under the scenario of a budget rising an average of 1 percent per year, the Subpanel concluded that the B factory should be built, assuming a successful accelerator design.

The highlight of the physics program at the B factory would be the study of CP-violation in the B meson system. The mystery of

CP noninvariance is one of the central problems of particle physics. The prospect of making the first observation of CP-violation in B decays has motivated a large community of physicists in the U.S. and the rest of the world to work toward a B factory design.

Under the rising budget scenario, the breadth of the present physics program is maintained. HEP in the U.S. would continue to be at the forefront of the field throughout the 1990's, while the SSC is being built.

C. The Lower Budget Scenario

In the past decade, budget limitations have prevented full utilization of accelerator facilities. At the same time, support for physicists in universities has not kept pace with the increasing requirements. Consequently, the Subpanel stressed that, within the constraints of a constant budget, existing facilities must be exploited and university programs must be strengthened. In addition, the Fermilab Main Injector was deemed necessary to maintain the Tevatron as a forefront facility through the latter half of the decade. Even under the constant budget scenario, the Subpanel could not accommodate several attractive research opportunities on a broad range of compelling issues. This was a cause of serious concern.

In its modified Charge, the Subpanel was asked to consider consequences of a budget that would "follow a profile which, when averaged over the 1990's, is 5 percent below the FY 1991 level in real dollars." The Subpanel examined several possible options for achieving such a profile.

One possibility considered was a uniform across-the-board reduction in program activities. The Subpanel concluded that by the middle of the decade such a uniform reduction would exacerbate current problems of underutilization of accelerator facilities. It would also force cutbacks in the university programs at a time when they need strengthening in anticipation of the SSC. This alternative was deemed totally unacceptable.

The Subpanel next examined options that preserved the vitality of one component at the expense of one or more major elements of the recommended program. In particle physics, progress in our understanding requires a diversity of probes and experiments. (For example, results from low energy neutrino scattering experiments impact directly on measurements at the highest energy colliders.) Restriction of this breadth will undoubtedly impede progress. By virtue of the exploratory nature of particle physics, one runs the risk of inadvertently eliminating the most crucial elements of the program.

Some possibilities considered by the Subpanel entailed removing one or more of the major programmatic elements of HEP. A step of this kind would be drastic and premature, and was deemed unacceptable to the Subpanel at this time. If significant budget reductions occur, then we urge that another subpanel be convened to advise the DOE on specific actions to be taken.

The Subpanel concludes that the reduced budget scenario represents an accumulated loss in vitality by the end of the decade that would leave the field poorly positioned to pursue research at the SSC or elsewhere. Each of the considered alternatives constitutes an unhealthy program.

Department of Energy
Washington, DC 20545

October 4, 1989

Professor Francis E. Low, Chairman
Laboratory for Nuclear Science
Massachusetts Institute of Technology
77 Massachusetts Avenue, Room 6-301
Cambridge, MA 02139

Dear Francis,

With the approval of the SSC as a construction project, the field of high energy physics enters an era characterized by continuity, transition, and preparation. With physics results from the SSC not expected until around the year 2000, there is a clear need for continuity in the existing, or base, research program in high energy physics. However, the magnitude of the SSC project and the enormous new physics capabilities it will make available will significantly change the structure of the field. Such changes impose a transition from the existing program to the new SSC-era program and require careful preparation to be in a position to maximize the physics productivity of the national high energy physics program.

To achieve an orderly transition while maintaining appropriate continuity in the base program and preparing for the SSC era will require careful, realistic, and extensive planning by DOE, NSF, and the high energy physics community. It is to this end that we now seek your help.

We need to plan the broad outline of the HEP base program for the decade of the 1990's including the universities, existing accelerator centers, non-accelerator experiments, and U.S. involvement abroad in both accelerator based and non-accelerator experiments. The plan must be structured to allow the base program to carry out viable and productive research while the SSC is being constructed and be in a position to exploit the physics opportunities thereafter.

HEPAP is requested to provide advice regarding this planning based on the following assumptions:

1. the SSC will be built on a schedule resulting in physics around the year 2000; and

2. DOE support for the HEP base program through the year 2000 will follow one of the budget scenarios listed below:

   a. the budget will remain constant in real dollars (i.e. inflation allowed for) at the level of the President's budget request for FY 1991,

b. the budget will increase in real dollars at the rate of approximately 1 percent per year, or

c. the budget will follow a profile which, when averaged over the 1990's, is 10 percent below the FY 1991 level in real dollars.

Specifically, HEPAP is asked to address the following issues under each of the above budget scenarios:

1. The relative importance and appropriate balance:

   a. between operations and major upgrades at a given laboratory, and

   b. among the proposed major upgrades and new facilities at the various laboratories.

2. The interface of the base program with the SSC; e.g., the implications for operations at existing facilities as present users begin to prepare for experiments at the SSC, and funding procedures for SSC-related research activities.

3. How to maintain or increase the strength and vitality of the university groups so they are able to effectively carry out their programs of research and education.

4. The relative importance, within the base program, of advanced accelerator R&D needed to explore new physics areas not addressed by the SSC.

5. Overall program balance as appropriate to address the most significant physics issues, including accelerator facilities in the U.S., the use of accelerator facilities abroad, and non-accelerator experiments both in the U.S. and abroad.

In view of the importance of these issues and of HEPAP's recommendations, you should seek broad input from the High Energy Physics community. I would appreciate submittal of HEPAP's report to DOE by April 30, 1990. This would allow its recommendations and conclusions to be considered by the Department in formulating its FY 1992 budget.

Sincerely,

Robert O. Hunter, Jr.
Director
Office of Energy Research

Department of Energy
Washington, DC 20585

JAN 17 1990

Professor Francis E. Low
Laboratory for Nuclear Science
Massachusetts Institute of Technology
77 Massachusetts Avenue, Room 6-301
Cambridge, MA 02139

Dear ~~Professor Low~~ Francis:

After meeting with the HEPAP Subpanel on the U.S. High Energy Physics Research Program for the 1990's chaired by Frank Sciulli, I have reviewed the Charge to the Subpanel as stated in Bob Hunter's October 4, 1989, letter to you. I wish to make one change in the Charge. Budget scenario 2.c. is changed to read, "the budget will follow a profile which, when averaged over the 1990's, is 5 percent below the FY 1991 level in real dollars." The change is in the percent reduction, which in the original Charge was 10 percent. We feel that 5 percent is more appropriate and will lead to a more useful report from HEPAP. The remainder of the Charge stands as originally written.

We very much appreciate the time and effort that you, Frank Sciulli, and the members of the Subpanel are devoting to this study.

Sincerely,

Jim Decker

James F. Decker
Acting Director
Office of Energy Research

cc:
F. Sciulli
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APPENDIX B

HEPAP SUBPANEL ON THE U.S. HIGH ENERGY PHYSICS RESEARCH PROGRAM FOR THE 1990'S

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APPENDIX C

HEPAP SUBPANEL ON THE U.S. HIGH ENERGY PHYSICS RESEARCH PROGRAM

FOR THE 1990's

December 18, 1989

Dear Colleague:

The enclosed letter from the Director of the Department of Energy (DOE) Office of Energy Research to the Chairman of the High Energy Physics Advisory Panel (HEPAP) requests assistance in formulating a plan for the structure of the High Energy Physics (HEP) program in the decade between now and first physics from the Superconducting Super Collider (SSC). The details of this charge may be revised by the new Acting Director; any such revision will be made available to the community when we receive it. A HEPAP subpanel has been appointed to provide a response to the charge, and I have been asked to chair it. The membership of the subpanel is included as the second enclosure.

While there was a subpanel with a similar charge approximately one year ago, chaired by Stan Wojcicki, there are substantial differences between the two subpanels. Since the Wojcicki Subpanel submitted its report in early 1989, we have come some considerable way in making a start on construction of the Superconducting Super Collider (SSC). Also, we are now being asked to recommend plans based on different, very specific, funding scenarios for the DOE HEP research program exclusive of the SSC.

The next decade promises to be a crucial one for our community. We must be sure that the ongoing HEP research program continues to produce first class physics results while the SSC is being built and beyond. At the same time we must see clearly mechanisms by which the ongoing research program, including possible new inititives, and the SSC reinforce each other during this period. The ongoing program and the SSC then must combine in a complementary fashion at the end of the decade to form a single high energy physics effort for the country. To achieve these goals will require careful, thoughtful planning.

It is important that the community of high energy physicists take the time and make the effort necessary to consider these matters in depth, to communicate their interests and their wisdom, and thus contribute to the planning. The Subpanel is to serve as a vehicle to air the thoughts of our physicists, and to facilitate the achieving of a broad consensus for direction over the next 10 years.

To this end, we have scheduled a series of five "town" meetings over the next three months at which all members of the community are invited to participate. These town meetings will be held in conjunction with Subpanel meetings to be scheduled at the SSC Laboratory, Fermilab, SLAC, Brookhaven, and Cornell. The dates for these meetings are contained in the third enclosure. Information about housing and travel to the laboratories may be obtained from the laboratory Users' organization.

A specific period (about a half day) during each of these two-day meetings will be set aside for the laboratories to present their plans and interests for the decade. In addition, at least a half-day will be available for community discussion of the issues (the town meeting). The meeting at Cornell will include a session particularly directed toward non-accelerator research and research at non-U.S. laboratories. The town meetings will be organized by the Subpanel with the help of the chairs of the local Users' Committees. While these meetings are located at our accelerator laboratories, it is intended that they be used to discuss the issues from a broader perspective than that of any individual laboratory. Presentations addressing the issues from the perspective of the entire community's future needs are especially invited.

It is important that you, as a contributing member of our community, be heard on the issues. Please contact the Chair of the appropriate Users' Committee or me regarding presentation time at any of these meetings. Another extremely useful method of communicating with the Subpanel would be to write directly to me, in care of Earle C. Fowler, Subpanel Executive Secretary, ER-223, Division of High Energy Physics, Washington, D.C. 20545; or by bitnet: DOEHEP @ BNLVMA. (Indicate on the message that it is to me for the HEPAP Subpanel.) Earle will see that all members of the Subpanel are provided copies.

The next 10 years will provide great opportunities for high energy physics, but they will also be critical for the future of our science. It is important that we reach a reasoned consensus on how to remain on the forefront of our science during the 1990's as we approach the onset of the SSC. It is also important that you participate. We look forward to hearing from you.

Best wishes,

Frank Sciulli

Frank Sciulli
Professor of Physics
Columbia University
Chairman of the Subpanel

3 Enclosures

APPENDIX D

Agenda

HEPAP Subpanel on the U.S. High Energy Physics Research

Program for the 1990'S

SSC Laboratory

Dallas, Texas

January 19, 1990

Friday, January 19

8:30 a.m.	Overview of the SSC	R. Schwitters
9:45 a.m.	Experimental Program and R&D Status and Plans	M. Gilchriese
10:30 a.m.	Break	
10:45 a.m.	Test Beam Needs and Plans	J. Bensinger R. Stefanski Fermilab Presentation
11:30 a.m.	SSC Research Program Needs	F. Gilman R. Schwitters T. Kozman
12:30 a.m.	Lunch in executive session Organizational matters Discussion of Charge	 E. Fowler DOE
2:00 p.m.	Town meeting General Comments on Needs for Success at the SSC	D. Cassel
2:15 p.m.	The L* Detector	H. Hofer
2:30 p.m.	The General Purpose Solenoid Detector	T. Kirk
2:45 p.m.	The BCD Detector	N. Lockyer
3:00 p.m.	The EMPACT Detector	M. Marx
3:15 p.m.	A Detector Focussing on Fast Calorimetry	L. Sulak
3:30 p.m.	Executive Session Discussion of Lab presentation	

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Agenda

HEPAP Subpanel on the U.S. High Energy Physics Research Program

for the 1990'S

Cornell University

February 8 and 9, 1990

Thursday, February 8

8:00 a.m.	Breakfast & Tours (CLEO Experimental Area) Wilson Laboratory	
8:45 a.m.	Bus to Statler Inn (Statler Amphitheater)	

LABORATORY PRESENTATIONS

9:00 a.m.	CESR B Factory Upgrade - Introduction	K. Berkelman
9:30 a.m.	What a B Factory Can Do	N. Mistry
10:15 a.m.	Detector Issues	D. Hartill
10:40 a.m.	Break	
11:10 a.m.	Accelerator Issues	M. Tigner
12:10 p.m.	Summary & Conclusions	K. Berkelman
12:30 p.m.	Lunch-Executive Session	

CESR COMMUNITY TOWN MEETING

1:45 p.m.	The CLEO II Collaboration & Experiment	R. Kass
2:00 p.m.	The CLEO II B Physics Program	E. Thorndike
2:20 p.m.	τ Physics and Y Spectroscopy with CLEO II	T. Skwarnicki
2:35 p.m.	The Syracuse B Factory Workshop	M. Goldberg
2:45 p.m.	Canadian Views on B Factories	M. Ogg
2:55 a.m.	Los Alamos HEP Experiments	D.H. White

3:15 p.m.	Open Discussion		
3:45 p.m.	Adjourn		

Friday, February 9

FUNDING PATTERNS

8:30 a.m.	DOE and NSF FY 1991 Budget Request		J.R. O'Fallon D. Berley
9:00 a.m.	DOE		P.K. Williams
9:15 a.m.	NSF		D. Berley

NON-ACCELERATOR PHYSICS

9:30 a.m.	Astronomy-Astrophysics Survey Physics		B. Sadoulet
9:45 a.m.	I.	Dark Matter Searches	B. Sadoulet
10:05 a.m.	II.	Solar Neutrino Physics	G. Beier
10:35 a.m.	III.	Macro/Gran Sasso Monopoles, WIMPS, Neutrino Astronomy	G. Tarle
10:55 a.m.	Break		
11:10 a.m.	IV.	High Energy Gamma Rays	C. Hoffman
11:30 a.m.			D. Lamb
11:40 a.m.			M. Cherry
11:55 a.m.			M. Abashian
12:05 p.m.	V.	DUMAND-Neutrino Astronomy	J. Learned
12:15 p.m.	VI.	Fly's Eye (UHE Gamma Rays)	E. Loh/J.Cronin
12:30 p.m.	Lunch-Executive Session (with Marcel Bardon)		

RESEARCH AT ACCELERATORS ABROAD

2:00 p.m.	Report from the Tau/Charm User Group	J. Brau
2:30 p.m.	L-3 Status and Plans	P. Piroue
2:40 p.m.	ALEPH	S.L. Wu
2:50 p.m.	OPAL Status and Plans	G. Snow
3:00 p.m.	DELPHI Status and Plans	T. Meyer
3:10 p.m.	ZEUS Status and Plans	T.Y. Ling
3:20 p.m.	AMY Status and Plans	S. Schnetzer
3:30 p.m.	Open Session	
3:50 p.m.	Break	
4:10 p.m.	Subpanel Executive Session	
6:00 p.m.	Adjourn	

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Agenda

HEPAP Subpanel on the U.S. High Energy Physics Research Program

for the 1990's

Brookhaven National Laboratory

March 1, 1990

Seminar Room

Physics Department, Building 510

Thursday, March 1, 1990

8:30 a.m. Executive Session, Room 2-160, Building 510

LABORATORY PRESENTATIONS

Chairman: T.L. Trueman

9:00 a.m.	Major Issues: HEP, RHIC, Funding	N.P. Samios
9:30 a.m.	AGS Program Overview, Future Plans for HEP at the AGS, SSC-Related Activity	T.L. Trueman
10:15 a.m.	Proton Opportunities at RHIC	S. Ozaki
10:25 a.m.	Facilities Overview (Existing, Booster, Stretcher, Upgrades)	D. Lowenstein
10:45 a.m.	The Need for the Stretcher	A.J.S. Smith
10:55 a.m.	Break	
11:10 a.m.	Support for SSC at BNL (Test Beams, Detector and Electronic R&D) D-Zero	H. Gordon
11:30 a.m.	Electroweak Physics of the Future at the AGS	W. Marciano
11:55 a.m.	Future Rare Neutral K Decay Experiments	J. Ritchie
12:10 p.m.	E787 K -> $\pi\nu\bar{\nu}$	D. Marlow
12:30 p.m.	Lunch for Subpanel in Executive Session, Room 2-160	

COMMUNITY MEETING AT BNL

Chairman: B. Lee Roberts

2:00 p.m.	User Overview, Muon g-2 Experiment, Upgraded Charged K Experiment	B. Lee Roberts
2:25 p.m.	QCD Physics of the Future at the AGS	R. Jaffe
2:55 p.m.	New Spectroscopy Experiment at the AGS	A. Dzierba
3:10 p.m.	Advanced Accelerator R&D	W. Willis
3:40 p.m.	Break	
3:50 p.m.	Phi Factory	D. Cline
4:20 p.m.	University Infrastructure/R&D and the SSC	F. Gilman
4:30 p.m.	Treiman Panel Report	A.J.S. Smith
4:45 p.m.	Open Discussion	
6:00 p.m.	Executive Session, Room 2-160	
6:30 p.m.	Adjourn	

Agenda

HEPAP Subpanel on the U.S. High Energy Physics Research Program

for the 1990's

Stanford Linear Accelerator Center

March 8, 1990

Thursday, March 8

8:00 a.m.	Executive Session (Orange Room, Central Laboratory)	

LABORATORY PRESENTATIONS

Auditorium

8:30 a.m.	Welcome and Introduction	B. Richter
9:00 a.m.	SLC - The Machine	T. Himel
9:30 a.m.	Discussion	
9:35 a.m.	SLC - The Physics Program	C. Baltay
10:00 a.m.	Discussion	
10:05 a.m.	Break	
10:25 a.m.	The B Factory - Physics	J. Dorfan
11:05 a.m.	The B Factory - Machine	A. Hutton
11:30 a.m.	Discussion	
11:35 a.m.	The Next Linear Collider - Physics	D. Burke
12:05 p.m.	The Next Linear Collider - AARD	R. Ruth
12:30 p.m.	Discussion	
12:35 p.m.	Lunch-Executive Session (Orange Room)	
2:05 p.m.	Summary, Scenarios and Budgets	B. Richter

COMMUNITY MEETING

Auditorium

3:00 p.m.	High Energy Physics at LBL	P. Oddone
3:30 p.m.	The importance of a well supported diversified High Energy Program	W. Busza
3:50 p.m.	The status of the University Infrastructure	C. Baltay
4:05 p.m.	Need of even-handed funding policy for SSC and non-SSC related projects	J. Matthews
4:15 p.m.	Need to maintain the funding of near-term projects to continue the flow of young physicists	D. Kofler
4:30 p.m.	The next ten years	G. Trilling
4:40 p.m.	Tau/Charm Factory	M. Perl
4:45 p.m.	The need for generic detector R&D	S. Shapiro
4:55 p.m.	CP Violation, B Factories and the SLAC User community	D. Hitlin
5:10 p.m.	The importance of the SLAC HEP fixed-target physics	S. Rock
5:25 p.m.	Support for particle theory	R. Peccei
5:40 p.m.	A B Factory at the CERN SPS	P. Schlein
5:55 p.m.	Open Discussion	

Agenda

HEPAP Subpanel on the U.S. High Energy Physics Research Program

for the 1990's

Fermi National Accelerator Laboratory

March 19, 1990

Monday, March 19

8:00 a.m.	Executive Session (Central Laboratory) 1 West Laboratory Presentations, Auditorium	
Chairman:	John Peoples	
8:30 a.m.	Welcome and Overview	J. Peoples
8:45 a.m.	Tevatron Luminosity in 1993	G. Dugan
9:15 a.m.	The Main Injector and the Neutral K and Neutrino Physics Experiments that can be done with it	S. Holmes
9:55 a.m.	Break	
10:15 a.m.	The Evolution of the Fermilab Charm and Beauty Physics Program during the 1990's	J. Spalding
10:55 a.m.	Precision Electroweak Measurements and the Search for the Top in CDF and D-Zero	M. Shochet
11:35 a.m.	The Fermilab Budget Requirements to Achieve its Goals for the 1990's	J. Peoples
12:05 p.m.	FNAL SSC efforts	D. Green
12:15 p.m.	New Direction for Accelerator Education	M. Month
12:30 p.m.	Discussion	
1:00 p.m.	Lunch-Executive Session (1 West) Community Meeting: Chairman, Ray Brock	
2:30 p.m.	University Infrastructure and Generic R&D	B. Hollebeek
2:45 p.m.	Introduction	R. Brock
3:00 p.m.	Future of Kaon Physics at FNAL	G. Gollin

3:15 p.m.	Future of Heavy Quark Physics at FNAL	J. Russ
3:30 p.m.	Future of Neutrino and Muon Physics at FNAL	S. Mishra
3:50 p.m.	Future of Spin-Dependent Structure Function Physics at FNAL	D. Underwood
4:05 p.m.	Health of University Programs	L. Pondrom
4:20 p.m.	Conclusion	J. Christenson
4:35 p.m.	Break	
4:55 p.m.	Argonne High Energy Physics Program	T. Kirk
5:20 p.m.	General Discussions	
6:20 p.m.	Executive Session (1 West)	

APPENDIX E

Manpower Study

A. SURVEY PROCESS AND OBSERVATIONS

In order to determine the number of scientists working in the various areas of HEP in a manner that allows us to establish the reliability of the numbers, we carried out two independent surveys.

In one survey, at the Subpanel's request, the HEP staff at the DOE requested from a reliable senior investigator at each university or laboratory a list of physicists in their institution working in HEP related projects regardless of funding source. The information was requested in such a manner that one could determine who were senior scientists, junior postdoctoral research associates, and students, as well as their various research activities. Results from this survey and a similar survey carried out in 1985, are presented in Table 1 (1985 survey) and Table 2 (1989 survey).

In a second, independent survey, the Subpanel requested a similar list of physicists from the spokesperson or knowledgeable senior physicist of every experiment at both U.S. and non-U.S. accelerators and in non-accelerator high energy experiments. It also requested an estimate of the fraction of research time spent by the experimentalists on each activity so that an estimate of full time equivalents (FTE's) could be determined for each activity. This information was requested for every year between 1985 and 1989. A summary of this detailed survey is shown in Table 3. Using this and the programmatic scenario developed and discussed by the Subpanel for a rising budget, the manpower needs for the HEP program, including the SSC, were projected over the next decade. The results of this work are discussed in Sections B and C of this Appendix.

TABLE 1

HEP Manpower Statistics
Based on HEP 1985 Census

Category	Arg.	Brook.	Cornell	Fermi	L.B.L.	SLAC	Others	Univ.	Total
Senior Exp. (vis.)*	21 (1)	44 (0)	24 (0)	100 (36)	41 (0)	73 (7)	0 (0)	532 (0)	835 (44)
Res. Assoc. Exp. (vis.)*	7 (0)	3 (0)	5 (0)	28 (0)	6 (0)	15 (0)	0 (0)	176 (0)	240 (0)
Students Exp*.	2 (0)	1 (0)	12 (0)	0 (0)	25 (0)	25 (0)	0 (0)	421 (0)	486 (0)
Senior Theo. (vis.)	6 (0)	8 (0)	6 (0)	23 (0)	21 (0)	10 (2)	14 (0)	501 (0)	589 (2)
Res. Assoc. Theo. (vis.)	4 (0)	2 (0)	4 (0)	10 (0)	2 (0)	9 (0)	1 (0)	109 (0)	141 (0)
Students Theo. (vis.)	4 (0)	0 (0)	11 (0)	0 (0)	8 (0)	22 (0)	0 (0)	335 (0)	380 (0)
Senior Acc. Phys. (vis.)	6 (0)	67 (0)	17 (0)	91 (6)	32 (0)	47 (0)	0 (0)	11 (0)	271 (6)
Res. Assoc. Acc. Phys. (vis.)	2 (0)	4 (0)	4 (0)	4 (0)	0 (0)	2 (0)	0 (0)	5 (0)	21 (0)
Students Acc. Phys.	1 (0)	1 (0)	6 (0)	0 (0)	1 (0)	2 (0)	0 (0)	19 (0)	30 (0)
Senior Exp. Non Acc. (vis.)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	72 (0)	72 (0)
Res. Assoc. Exp. Non Acc. (vis.)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	7 (0)	7 (0)
Students Exp. Non Acc.	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	29 (0)	29 (0)
TOTAL	53 (1)	130 (0)	89 (0)	256 (42)	136 (0)	205 (9)	15 (0)	2,217 (0)	3,101 (52)
Ave. Age Senior Exp.	46.5	45.9	47.6	41.3	51.2	44.5		47.5	46.4

* These rows refer to accelerator based experiments

(numbers in parentheses represent visitors)

TABLE 2

HEP Manpower Statistics
Based on HEP 1989 Census

Category	Arg.	Brook.	Cornell	Fermi.	L.B.L.	SLAC	SSC, LAL+	Univ.	Total
Senior Exp. (vis.)	26 (1)	44 (0)	24 (0)	81 (53)	44 (0)	82 (0)	16 (0)	572 (0)	889 (54)
Res. Assoc. Exp. (vis.)	3 (0)	2 (0)	5 (0)	34 (0)	9 (0)	18 (0)	13 (0)	192 (0)	276 (0)
Students Exp.	3 (0)	3 (0)	14 (0)	0 (0)	18 (0)	24 (0)	3 (0)	537 (0)	602 (0)
Senior Theo. (vis.)	6 (1)	8 (0)	5 (0)	25 (0)	14 (0)	9 (0)	13 (0)	496 (0)	576 (1)
Res. Assoc. Theo. (vis.)	4 (0)	5 (0)	4 (0)	12 (0)	8 (0)	10 (0)	8 (0)	136 (0)	187 (0)
Students Theo. (vis.)	3 (0)	0 (0)	6 (0)	0 (0)	11 (0)	14 (0)	0 (0)	355 (0)	389 (0)
Senior Acc. Phys. (vis.)	10 (0)	74 (0)	20 (0)	87 (2)	25 (0)	56 (0)	29 (0)	50 (0)	351 (2)
Res. Assoc. Acc. Phys. (vis.)	0 (0)	2 (0)	4 (0)	6 (0)	0 (0)	1 (0)	8 (0)	7 (0)	28 (0)
Students Acc. Phys.†*	2 (0)	0 (0)	6 (0)	9 (0)	1 (0)	6 (0)	1 (0)	76 (0)	101 (0)
Senior Exp. Non Acc. (vis.)	6 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	3 (0)	73 (0)	82 (0)
Res. Assoc. Exp. Non Acc. (vis.)	1 (0)	0 (0)	0 (0)	0 (0)	0 (0)	0 (0)	2 (0)	37 (0)	40 (0)
Students Exp. Non Acc.	0 (0)	0 (0)	0 (0)	0 (0)	7 (0)	0 (0)	2 (0)	55 (0)	64 (0)
TOTAL	64 (2)	138 (0)	88 (0)	254 (55)	137 (0)	220 (0)	98 (0)	2,586 (0)	3,585 (57)
Ave. Age Senior Exp. in 1985	43.5	50.1	46.3	42.5	54.0	46.8		50.4	47.9

† all the 9 Fermilab students are from Universities

* 3 out of the 6 SLAC students are from Universities other than Stanford.
Hence they double count with the 76 under universities. The total of 101 is really 89.

TABLE 3

Statistics on U.S. Physicists in Various Areas of Activity from 1985 to 1989
Based on Committee 1990 Study

Activity	Category	1985	1986	1987	1988	1989
BROOKHAVEN	Total (FTE)	322 (251)	344 (268)	300 (223)	303 (207)	436 (254)
	Fac. + Senior R.A.	143	152	125	111	147
	Post. Docs.	44	50	45	43	45
	Ph.D. Students	64	66	53	53	62
	Ph.D.s Granted	8	11	11	10	14
CORNELL	Total (FTE)	92 (66)	104 (75)	100 (71)	90 (64)	104 (74)
	Fac. + Senior R.A.	12	14	13	12	14
	Post. Docs.	20	23	22	20	24
	Ph.D. Students	34	38	36	32	36
	Ph.D.s Granted	5	3	9	7	7
FERMILAB FIXED TARGET	Total (FTE)	431 (328)	499 (372)	499 (376)	544 (404)	557 (407)
	Fac. + Senior R.A.	163	195	196	203	203
	Post. Docs.	73	77	77	81	86
	Ph.D. Students	92	100	103	120	118
	Ph.D.s Granted	4	8	9	17	17

The middle 3 rows in each laboratory activity are full-time equivalents (FTE). For the first (total) rows, the number in parentheses are FTE. The number to the left are total people, for which there is some double-counting.

TABLE 3 (Cont'd)

Activity	Category	1985	1986	1987	1988	1989
FERMILAB COLLIDER	Total (FTE)	228 (191)	260 (215)	266 (231)	322 (276)	342 (280)
	Fac. + Senior R.A.	133	146	149	158	153
	Post. Docs.	26	32	36	45	48
	Ph.D. Students	32	37	46	73	79
	Ph.D.s Granted	0	0	4	5	7
SLAC	Total (FTE)	543 (437)	555 (438)	580 (430)	538 (371)	486 (343)
	Fac. + Senior R.A.	245	247	252	202	185
	Post. Docs.	84	86	74	64	58
	Ph.D. Students	108	105	104	105	100
	Ph.D.s Granted	9	24	21	21	11
Non U.S. Acc.	Total (FTE)	187 (138)	209 (162)	232 (186)	246 (206)	261 (223)
	Fac. + Senior R.A.	72	75	84	92	96
	Post. Docs.	27	36	39	43	49
	Ph.D. Students	39	51	63	71	78
	Ph.D.s Granted	4	2	3	3	6
Non Acc.	Total (FTE)	76 (61)	104 (98)	143 (124)	177 (154)	215 (192)
	Fac. + Senior R.A.	31	57	65	74	84
	Post. Docs.	17	19	25	36	44
	Ph.D. Students	13	22	34	44	64
	Ph.D.s Granted	4	5	4	5	8
SSC Detector R&D	Total (FTE)					150 (45)
	Fac. + Senior R.A.					45
	Post. Docs.					0
	Ph.D. Students					0
	Ph.D.s Granted					0

There is a remarkable agreement in detail between the two surveys as shown in Table 4. Both surveys indicate that there has been an increase in the number of HEP experimentalist participants in accelerator based programs between 1985 and 1989; namely, an increase of about 13 percent over 4 years, or 3.5 percent per year. This increase was distributed among the various categories as follows. The number of senior members (defined to be either faculty or permanent staff) appears to increase by about 8 percent, but a more detailed analysis described later may indicate a smaller growth. The junior postdoctoral research associates have increased by about 14 percent, and students have increased by about 26 percent. The total increase matches well with the total HEP scientist yearly increases, which have been occurring since the middle-to-late seventies shown in Figure 1. These increases speak to the excellent health of the HEP program; students have shown and continue to show a strong desire to participate in HEP experiments.

The DOE survey also shows that there has been a dramatic rise in the number of theory postdoctoral research associates (+33 percent) which has outpaced both the change in senior theorists (-2 percent) and in theory students (+2 percent).

The number of graduate students working in accelerator physics has increased substantially between 1985 and 1989. A survey of laboratory- and university-supported research programs shows a total of 30 students in 1985 and 89 in 1989. However, the HEP community is competing with other communities for this resource, and only about 40 percent of these accelerator physicists entered the high energy accelerator programs in recent years. If this trend continues, such accelerator research activities will generate approximately 7 Ph.D.'s per year in high energy accelerator physics in the next several years.

Non-accelerator HEP experiments have shown a rise in the number of junior postdoctoral research associates and graduate students.

TABLE 4

Comparison of Laboratory and DOE Survey

Category	1985			1989		
	Labs	DOE	Labs FTE / DOE	Labs	DOE	Labs FTE / DOE
Total Exp.	1,803 (1411)*	1,561 [44]†	.90	2,336 (1,626)	1,767 [54]	.92
Senior Exp.	768	835 [44]	.92	843	889 [54]	.95
Post Docs.	274	240	1.14	310	276	1.12
Students	369	486	.76	473	602	.78

* ( ) FTE

† [ ] Foreign visitors, mainly Fermilab

These results, combined with our study of how the experimental program may change towards the end of the decade, lead us to the following conclusions about manpower needs for the next decade and how these needs can be accommodated by a growth scenario compatible with what has occurred over the last decade.

B. Manpower Needs for the Next Decade

The present number of experimentalists in HEP, as determined from Table 3, consists of 1,626 FTE researchers working in accelerator based experiments and 192 working in non-accelerator based experiments. The total of 1,818 is rounded to 1,850 to be slightly more in accordance with the DOE surveys. In our scenario discussions we assume 1,850 to be our present base population.

The scenario of experimental activity during the decade used by the Subpanel was based on the rising budget case. This presents the largest need for physicists and, hence, is adopted as the most conservative. It assumed the two construction initiatives recommended by the Subpanel and strong exploitation of existing facilities now, with a considerable falloff, overall, in these activities in the second half of the decade.

Under this scenario, the Subpanel estimated that approximately 1,100 FTE's would be required to carry out the ongoing non-SSC program effectively at the end of the decade. About 750 scientists in the base program will have shifted their focus to SSC-related work.

SSC activities in 1999 are expected to require approximately 950 FTE's, leading to a shortfall of approximately 200 by the end of the decade if there were no growth. The projected need for an additional 300 scientists by 1999 could be met by the growth scenario described below.

C. Projected Growth in HEP

Figure 1 shows the number of accelerator based U.S. physicists versus year between 1964 and 1989. For comparison purposes, we show the same information for CERN (ref: High Energy Physics in Europe and in the United States: Comparison on Non-scientific and Non-technical Topics. Christian Roche, March 15, 1988. Resources Given to High Energy Physics in 1986 in the CERN Member States EF/CR/001. Christian Roche, May 1988). The U.S. curve shows a significant drop in the early 1970's. In contrast, the CERN curve has had a steady growth rate throughout this period. As a result, by 1984, the CERN program had about 1.5 times the number of scientists in the U.S. program.

In attempting to project the future for the U.S., there is some ambiguity in the interpretation of the falloff in the early 1970's. An optimistic (pessimistic) extrapolation yields approximately a 2.8 (1.5) percent per year average growth. In order to understand the present growth better, we have analyzed the 1985-89 period in detail. In Figure 2 we show the data of Table 3 (from 1985-89), in the categories of senior, junior postdoctoral research associates, and graduate student FTE's. The number of senior FTE's is approximately constant, at least within the 2 percent year-to-year fluctuations; the research associates have increased by approximately 3 percent per year, and there has been a significantly larger growth in the number of graduate students, approximately 6 percent per year.

The 300 additional scientists needed over the next decade, out of a present population of 1,850, represents an average growth of only 1.6 percent per year, well within the growth limits just discussed and presented in Figure 1.

The placement of these scientists could be accommodated in new positions as follows:

1. The Subpanel expects that the growth of the SSCL will generate

Figure 1

NUMBER OF ACCELERATOR-BASED EXPERIMENTALISTS IN U.S. AND CERN

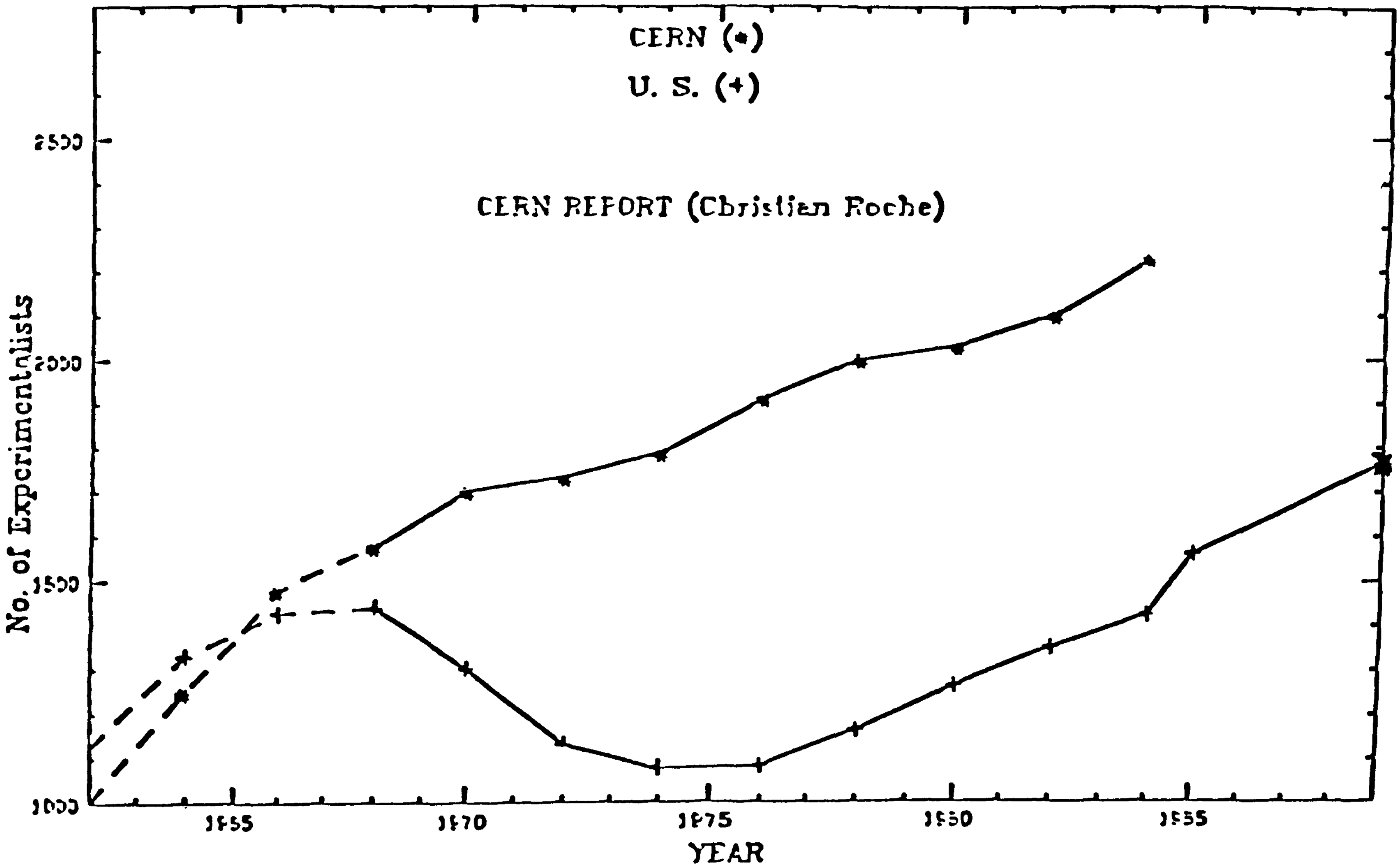
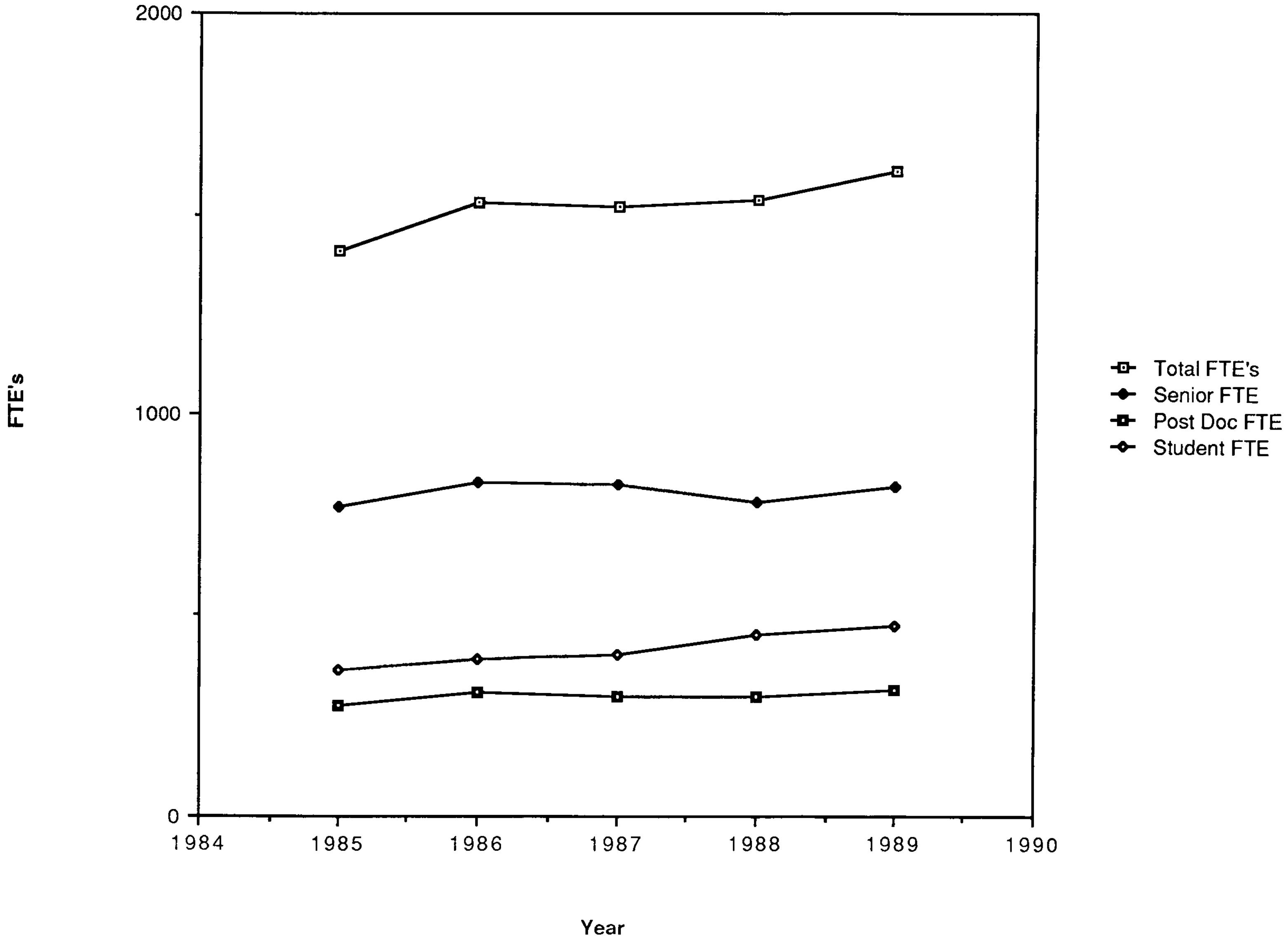


Figure 2

FTE's vs. Year

Accelerator-Based



approximately 200 (100 FTE's) new Ph.D. level positions over the next 5 years above the 25 presently there. We assume no growth after that.

2. The Subpanel anticipates that approximately 35 new Ph.D. level positions by 1995 and 100 Ph.D. level positions by 1999 will become available in the university program. This is based on the constant funding scenario contribution to the University program. We assume that they will work fully on the SSC; hence, they also contribute 100 FTE's.

In addition to the above, we would to like to comment that there will be large losses in the HEP field due to retirements. We estimate this to be 89 by 1995 and an additional 147 by 1999 (using 65 as the retirement age) based on the age distribution shown in Figure 3. A later retirement age will reduce this number. This loss can be replenished by the present pool of young scientists in the program.

Both the new growth and the retirements can be filled by the present production rate of students. As presented in Table 4 and shown in Figure 4, the number of Ph.D. students in 1989 is 473 and the number of Ph.D.'s granted is 70 and increasing. Our best estimate is that the HEP field will produce 425 new Ph.D.'s over the next 5 years. This is consistent both with the number of students in the program and the production level of Ph.D.'s. Assuming the traditional retention rate of 50 percent, we can expect to replace approximately 215 of the 324 vacancies expected to occur during the next 5 years. These vacancies, as described above, come from retirements (89), new faculty positions (35), and the SSC needs (200). It is expected that the difference (109) could be filled by physicists from outside this U.S. HEP program.

In conclusion, this study indicates a healthy U.S. HEP program with an adequate present manpower level. It is reasonable to expect a growth rate for the 1990's approximately equal to that of the 1980's and this study indicates that such a growth rate can accommodate the exciting SSC experiments, within a healthy scientific program over the next decade.

Figure 3

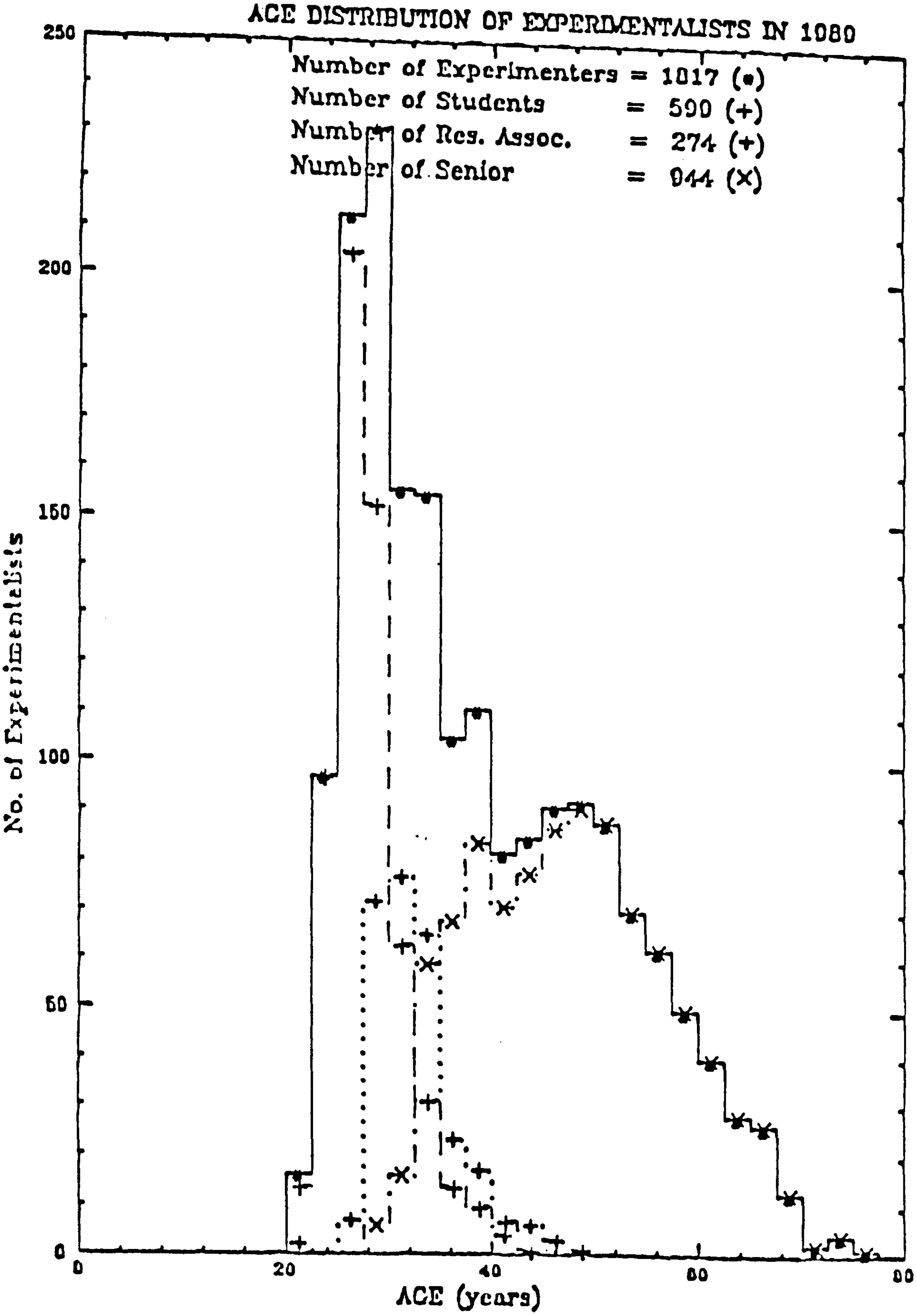
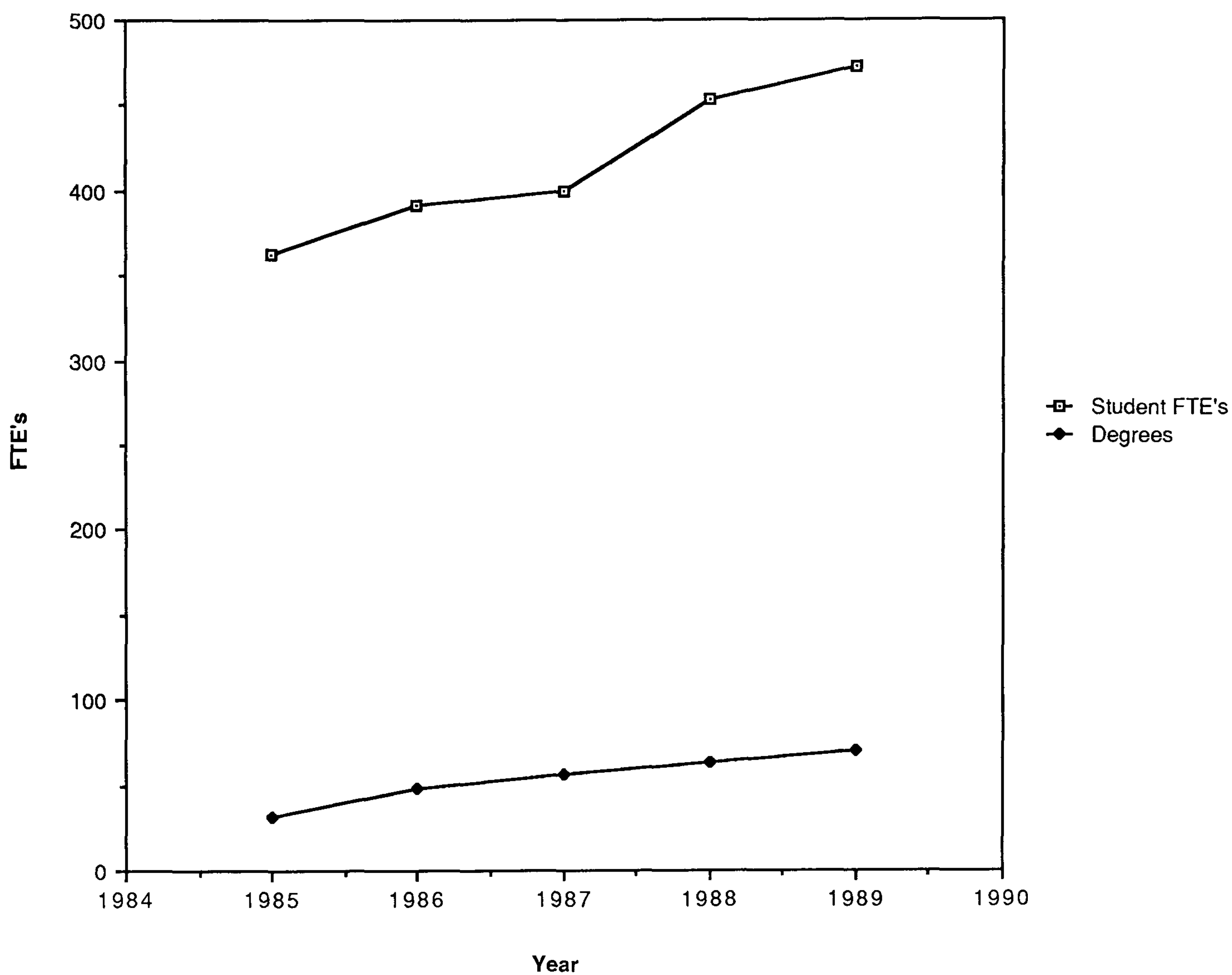

ACE DISTRIBUTION OF EXPERIMENTALISTS IN 1080
Number of Experimenters = 1017 (•)
Number of Students = 590 (+)
Number of Res. Assoc. = 274 (+)
Number of Senior = 944 (X)
No. of Experimentalists
250
200
150
100
50
0
0
20
40
60
80
ACE (years)

Figure 4

Student Population

Accelerator-Based



APPENDIX F

List of Abbreviations

AGS	*Alternating Gradient Synchrotron*
APS	*American Physical Society*
BNL	*Brookhaven National Laboratory*
CAT	*Computer Assisted Tomography*
CDF	*Collider Detector at Fermilab*
CERN	*European Laboratory for Nuclear Research*
CESR	*Cornell Electron Storage Ring*
CKM	*Cabibbo-Kobayashi-Maskawa Matrix*
CLEO	*CESR Spectrometer System*
DESY	*Deutsches Elektronen Synchrotron*
DOE	*Department of Energy*
DPF	*Division of Particles and Fields*
GUT	*Grand Unification Theories*
HEP	*High Energy Physics*
HEPAP	*High Energy Physics Advisory Panel*
KEK	*National Laboratory for High Energy Physics*
LBL	*Lawrence Berkeley Laboratory*
LEP	*Large Electron-Positron Collider*
NSF	*National Science Foundation*
PEP	*Positron Electron Project*
PET	*Positron Emission Tomography*
QCD	*Quantum Chromodynamics*
QED	*Quantum Electrodynamics*
RHIC	*Relativistic Heavy Ion Collider*
SLAC	*Stanford Linear Accelerator Center*
SLC	*Stanford Linear Collider*
SLD	*Stanford Large Detector*
SPEAR	*Stanford Positron Electron Asymmetric Ring*
SSC	*Superconducting Super Collider*
SSCL	*Superconducting Super Collider Laboratory*