EXECUTIVE SUMMARY

CONCLUSIONS AND RECOMMENDATIONS

The goal of particle physics is to understand the nature of matter at its deepest level, to answer the questions: What are the fundamental building blocks that make up the universe? What laws of nature determine their interactions at any time and place in the universe?

Twentieth century civilization has inherited a long and rich scientific tradition that began with the Greek philosophers. From the chemists of the eighteenth century who explored the behavior of atoms, to the physicists of the twentieth century who unlocked the secrets of the nucleus, scientists have probed nature in ever finer detail in search of its basic constituents and fundamental physical laws.

Each step along the way has given human beings a new way of looking at the universe, and each has, in time, led to new possibilities for applying basic knowledge for the benefit of humanity. The discovery of atoms, for example, turned alchemy into chemistry and gave us control of the elements. During the twentieth century, physicists went a step further and revealed that the chemical elements are assembled from still more elementary building blocks--the protons and neutrons that make up the nucleus, and the electrons that orbit around them. This work led to advances in medicine, electronics, computing and many other technologies. More recently, particle physicists have discovered that the particles in the nucleus are made up of even more fundamental constituents: the quarks. Who knows where this discovery will lead?

One thing is certain: over the years, many beautiful--and often surprising--experimental results have revealed a remarkable simplicity that underlies not only the world around us, but phenomena anywhere in the universe, at any time in its history. Indeed, as we have been able to study collisions between particles at higher and higher energies, we have begun to see patterns in their interactions that give us reason to hope that one day all interactions can be described by a single, unified theory.

The desire to understand the laws that govern the behavior of matter and energy, space and time, has inspired us to build particle accelerators and detectors that are marvels of scientific imagination and technological ingenuity. Over the past forty years, many important new discoveries have been made by experimental collaborations at high-energy particle accelerators. In addition, these accelerators have made it possible for human beings to experimentally study the early history of the cosmos. A combination of astrophysical and accelerator-based inquiries has enabled physicists to retrace the history of the universe back to less than a billionth of a second after the Big Bang. To understand the unimaginably hot and dense conditions that existed at that time, we must understand the physics of elementary particles. The synergy between astrophysics and particle physics enables us to hone our comprehension of our universe and our theories of the fundamental forces in nature. There is growing evidence for a deep connection between the physics of the infinitely small and the largest structures in the cosmos.

The pursuit of particle physics has opened the door to important new questions. Recently, for instance, experimenters at Fermilab announced evidence for the top quark, believed to be the last member of a family of six such particles. But, a priori, no one could predict what its mass would be. No one can explain why it weighs orders of magnitude more than its siblings. And no one has found a meaningful pattern in the masses of any of the quarks or elementary particles that have been discovered. Today, we are challenged to understand the origin of mass and other equally compelling issues: Why does there appear to be more matter than antimatter in the universe--a fact that ultimately enabled stars, planets, trees, and human beings to exist? And what makes up the 90% of the mass of the universe that we know exists but have not been able to see?

Answering these questions, and looking deeper and deeper into the heart of matter, requires experiments with particles of higher and higher energies. We must build on our work at today's cutting edge in moving on to tomorrow's frontiers. The high-energy frontier has historically provided the most direct path to new breakthroughs. This was the motivation for the Superconducting Super Collider (SSC), which the high-energy physics community expected to lead to the next big step in understanding. Throughout the past decade, the community endorsed the construction of the SSC as the highest priority in the U.S. particle physics program.

There are scientifically compelling reasons for exploring particle collisions at the energies the SSC would have reached. There was also a strong conviction that we could master the technological challenges to its successful construction and operation. Moreover, its design incorporated an important potential for future growth to higher energies and more intense beams, which promised a unique opportunity extending well into the twenty-first century, to address some of the most important questions about the nature of matter.

Cancellation of the SSC is a severe loss to worldwide high-energy physics. It is an especially severe loss to the American community, which now faces the challenge of creating a new vision for the future--one that continues to place scientific excellence at the core, minimizes the damage to the careers of young physicists from the loss of the SSC, maximizes the efficiency of research, and is acceptable and affordable to the American people.

This subpanel was charged with developing and characterizing a strategy to realize that vision; a strategy that will continue to address the most important scientific questions, ensure the U.S. a position among the world's leaders in this field, and look to more effective international collaborations to realize our scientific goals. In addition, we were asked to suggest how best to use the assets and accomplishments of the SSC project.

Conclusion 1

We have inherited a great tradition of scientific inquiry. The field of particle physics has made dramatic progress in understanding the fundamental structure of matter. Recent discoveries and technological advances enable us to address such compelling scientific issues as the origin of mass, the underlying cause of the preponderance of matter over antimatter, and the nature of the invisible matter that accounts for up to 90% of the mass of the universe.

Recommendation 1

As befitting a great nation with a rich and successful history of

leadership in science and technology, the United States should continue to be among the leaders in the worldwide pursuit of the answers to fundamental questions of particle physics.

Status of the Field

The current complement of U.S. high-energy accelerator laboratories is world class. It provides university students, faculty, and national laboratory physicists with access to experimental frontiers. These accelerators and detectors at the Fermi National Accelerator Laboratory (Fermilab), the Stanford Linear Accelerator Center (SLAC), Brookhaven National Laboratory (BNL), and Cornell are now addressing basic questions of particle physics and will continue to do so for some time. With the completion of scheduled upgrades in 1999flthe Main Injector at Fermilab, the B-factory at SLAC, and the Cornell Electron Storage Ring-II at Cornell--the U.S. will maintain frontier capabilities, and the potential to make significant discoveries, during at least the next decade. In addition, there are significant U.S. particle physics research efforts that do not rely on accelerators. These domestic resources are complemented by unique opportunities abroad for U.S. scientists to conduct research, just as many foreign collaborators contribute substantially to the work in this country. With adequate support to upgrade and operate these accelerators and detectors and to respond to new experimental initiatives, the U.S. high-energy physics program will remain a world leader for a decade or more.

However, we must look beyond the horizon of this program if the U.S. is to remain productive, responsive, and a world leader in this field. To achieve this goal, two additional elements are essential:

> We must continue to collaborate in international scientific endeavors that are exploring or will explore new high-energy frontiers, whether in the United States or abroad.
>
> We must create opportunities at laboratories and at universities to open new research frontiers through a strong program of advanced accelerator and detector research and development. Technical advances generated by this program, together with advances in scientific understanding from ongoing experiments, will be important guides in making choices and setting priorities for the longer-term future.

The U.S. high-energy physics program is severely challenged as it faces FY1995. At the same time as its scientific potential has been growing, the program is being strained by the budget reductions that have occurred in recent years (see Figure 1). The loss in annual research support--that is, in the annual appropriation of operating and equipment funds--from FY1990 to the Presidentfls budget submission to Congress for FY1995 is about $135M in current dollars, when inflation is included. This represents about a 20% reduction in buying power.

With the termination of the SSC, additional funds were lost to the entire high- energy physics program, both from that project and from the Texas National Research Laboratory Commission (TNRLC). This loss has led to a scarcity of research opportunities and positions for young high-energy physicists. This situation is further aggravated because a number of new experimental research groups were formed throughout the nation in anticipation of the SSC. As a result, many careers are in jeopardy, and many young people believe that future prospects are bleak for continuing this historic scientific quest.

Strategies and Opportunities for the Future

The Tevatron at Fermilab is now the highest energy accelerator in the world. Ten years from now, the Large Hadron Collider (LHC) at the European Laboratory for Particle Physics (CERN) will offer a unique prospect for advancing to the highest energy frontiers. The LHC, planned to begin operation at CERN circa the year 2003, will be a high-performance proton-proton collider with seven times the energy and up to 100 times the luminosity of the Tevatron. Research and development in the U.S. and in Europe have capitalized on rapid change in electronics technologies to create particle detectors that promise to meet the stringent demands of the LHC environment. The LHC will open new windows to discovery and present important opportunities to confront questions posed by current experiments and theories.

Besides providing U.S. physicists with access to new scientific territory, participation in the LHC would benefit the U.S. in other ways beyond the life- span of the accelerator itself. Successful construction and operation of the Tevatron, as well as research and design for the SSC, have provided U.S. scientists and engineers with the world-class experience and knowledge needed for the design, implementation, and operation of large detectors and superconducting accelerators. This priceless human resource would enable the U.S. to make important contributions to the LHC; furthermore, sustaining this collective expertise over the next decade will be invaluable in keeping the U.S. at the forefront of proton accelerator technologies for the long-term future. In addition, helping to build the LHC, and developing strategies and mechanisms needed for global cooperation on large science projects, would further strengthen our credibility as a capable host for such projects, in all fields of science.

The LHC project is still in its design stage, so the time is appropriate for the U.S. to initiate participation. In June of this year, CERN will seek approval from its member states for the LHC project. CERN desires interregional collaboration in the design and construction of the accelerator and detectors.

While the LHC will be a great step on the energy frontier, it will not answer all the important questions. To ensure the long-term future of the field, a number of diverse approaches to advanced accelerator and detector research and development should be encouraged. The technology of the LHC does not exhaust the possibilities for proton storage rings. Preliminary examination indicates that it may become practical to build a proton collider with beams of up to ten times the energies of the LHC, using technology that could be developed during the next decade. For the U.S. to maintain its place among the leaders of the world high-energy physics community, it will be important to participate in regional or global collaborations to carry out the research and development required for such a future machine. Investigation also should be pursued of a possible expansion of the Tevatron/Main Injector complex at Fermilab as an intermediate step; possibilities may emerge that offer new scientific opportunities, thereby enhancing fruitful research in the U.S. at a hadron collider.

Our experience over the past four decades has provided ample evidence of the great importance of probing nature with both electron and proton collisions. Electron-positron colliders offer new and uniquely important experimental opportunities. An international consensus has been forged on the physics goals and parameters of a future linear collider. A worldwide effort is under way to develop the understanding of accelerator physics and the technology needed to build the next-generation

electron-positron collider, beyond the currently operating Stanford Linear Collider (SLC). This international research and development program is focused on formulating conceptual designs, validating the technology, and generating reliable cost estimates in the second half of this decade. Prototype accelerators are now under construction. For the U.S. to maintain a leading role in the development of a future-generation linear collider, it is important to support research and development at U.S. laboratories and universities involved in this work.

For the longer term, it is important to investigate novel acceleration systems, as well as techniques for accelerating particles other than protons and electrons. Many intriguing ideas are being discussed, but much work remains to be done before it will be possible to determine which, if any, are practical for high-energy physics research.

Conclusion 2

To sustain excellence in the U.S. high-energy physics program for two decades and beyond, three elements are essential:

a flexible, diverse, and dynamic ongoing research effort to address scientifically compelling questions. This implies strong support for university groups, effective use and timely upgrades of domestic accelerators, and an active program of nonaccelerator-based inquiries.

vigorous studies to develop and master the technologies for future accelerators and detectors, and

significant participation at the highest energy frontier, for which the best current opportunity beyond the Tevatron is through international collaboration on the LHC at CERN.

When we consider what funding profiles are necessary to realize this vision, two important points emerge clearly. The first is that the greatest immediate need is to revitalize the current program to serve as a healthy and balanced base on which to build a future. This is the essential starting point for any successful strategy to realize our vision. The second is that an effective American presence on the high-energy frontier is essential for the long-term vitality of the U.S. program. The first requires a temporary budget increase over the next three years. The second can be fit in under a constant- level-of-effort budget over the long term, without making a large immediate demand for funding support (see Figures 2 and 3). What is needed is an early flgofl decision by the U.S. government to enable scientists to plan effectively for participating in the research and development toward construction of the accelerator and detectors at the LHC.

The build-up in sizable U.S. spending levels as part of the LHC collaboration can be phased in as the current commitmentsflto the construction of the Main Injector at Fermilab and the B-factory at SLAC, and to the operation of the Alternating Gradient Synchrotron (AGS) at BNL for high-energy physicsflwind down. These reductions in budgetary obligations of the U.S. high- energy physics program were anticipated in planning for the start of physics research at the SSC. They now provide important flexibility to meet long-term needs, so that with modest budgetary commitments, the U.S. can realistically support a world-class high-energy physics program. An effective collaboration in the LHC will offset in part the lost research opportunities of the SSC and keep the U.S. high-energy physics program at the energy frontier into the twenty-first century.

Conclusion 3

A temporary and modest bump of $50M/year in the total funding for three years from FY1996 through FY1998, followed by a return to a constant- level-of-effort budget at the level of the President's proposed FY1995 budget, as shown in Figures 2 and 3, would revitalize the ongoing research program and sustain it through the construction years of the two upgrades at Fermilab and SLAC. Within that budget profile, it would be possible to reverse the FY1995 cut in the research (operations plus equipment) budget, permitting its restoration to the FY1994 level in buying power and, thus, the productive use of investments made to date; and also to initiate significant participation in building the LHC, with the level of commitment growing slowly until FY1997 and reaching its full level in FY1999.

Recommendation 2

The subpanel recommends that the federal government commit itself to a budget for the Department of Energy's High Energy Physics program that provides constant-level-of-effort funding plus a $50M/year bump for three years, starting in FY1996, to implement the following program:

Productive use of existing domestic facilities and their ongoing upgrades, including support for the university-based researchers, and flexibility to pursue new ideas.

Significant participation in the LHC accelerator and detectors, both to provide research opportunities at the energy frontier and to ensure that U.S. physicists remain integrated in the international high-energy physics community.

Enhanced effort in accelerator research and development, in preparation for a strong role in creating the accelerators of the next century.

Recommendation 3

Given the above three-year supplement and a commitment to support at no less than constant-level-of-effort funding thereafter, the subpanel recommends that the U.S. government declare its intention to join other nations constructing the LHC at CERN and initiate negotiations toward that goal. Participation in the LHC should be endorsed with a timely decision of support. This will enable the high-energy physics community in the United States to take full advantage of this opportunity and to maintain momentum in the collaborations that have been forming in the hope of applying to the LHC the expertise and technology developed for the SSC and its detectors, and of sharing in its discoveries.

The subpanel foresees U.S. expenditures for the LHC starting in FY1995, at the level of $(5 to 10)M in FY1995, and $(10 to 15)M in FY1996, with larger expenditures thereafter. Starting in FY1999, support of approximately $60M per year would result in a total of about $400M at the end of FY2003. We expect that a large fraction of this sum would be spent in the United States on building, for instance, special magnets and equipment for the interaction regions and the large

detectors. (The figure of $400M is introduced simply to indicate the scale of possible total involvement under this budget assumption.) Such a commitment would constitute a serious, effective, and important U.S. investment of great value to both the U.S. and the Large Hadron Collider program.

The LHC is not only an important scientific opportunity, but also an important step in what we expect to be a growing trend in international collaboration on construction of large scientific instruments. The experience gained from this collaboration should thus strengthen the U.S. potential to be a host country for other international scientific endeavors.

As this discussion emphasizes, it is vital to have predictable funding for research projects that are large and costly multi-year commitments. This approach contrasts with recent history, in which such projects have been funded through the annual appropriations process, without prior Congressional authorization.

Conclusion 4

The subpanel emphasizes the importance of future major high-energy physics construction projects being fully authorized at the start of the project. This process, although it does not guarantee full funding by the government, can be important in building the support in Congress that is essential for the success of a large project. Full authorization at initiation of a construction commitment will bring needed strength to the role of the U.S. as a reliable partner prepared to undertake and complete long-term commitments. It can also help to ensure that projects proceed efficiently and expeditiously.

We also endorse the conclusion, emphasized in the report on "Science, Technology, and Congress: Organizational and Procedural Reforms" (February 1994) by the Carnegie Commission on Science, Technology and Government, that multi-year funding would greatly facilitate the planning of large projects and increase their operating efficiency. For the high-energy physics program, a budget cycle of two or more years would provide major advantages, because experiments depend primarily on large and complex instruments that take many years to build, and research generally involves long-term collaborations.

Recommendation 4

The government should give serious consideration both to restoring earlier practices of full authorization at the start of major scientific construction projects and to introducing budget cycles of two or more years.

The subpanel also considered implications of a flat, FY1995 constant- level-of-effort, budget without the supplement.

Conclusion 5

We emphasize that the main purpose of the temporary $50M budget bump is to strengthen the existing program. Without a three-year, $50M/year supplement in FY1996, the current U.S. program would suffer continued damage. The programfls scope and flexibility would be further diminished, and ongoing commitments would be stretched out. This

conclusion is independent of U.S. involvement in the LHC.

We do not believe that this problem should be addressed by continued proportional budget decreases at each of the laboratories and in each area of the program. We do believe that new priorities would have to be set that would likely call for sacrificing important parts of the U.S. program, in order to preserve quality and productivity in what survives. The inevitable consequences will be continued loss of vitality in the current program and further discouragement to the new generation entering the field.

As argued earlier, the commitment to initiate effective collaboration at LHC has little impact on the need of the current program for roughly $50M, up front, to redress its needs. Also, as Figures 2 and 3 show, the planning assumption of a constant-level-of-effort budget for the future provides sufficient flexibility for effective LHC involvement. This budget scenario may necessitate some delays in making a sizable U.S. contribution to the LHC, and may reduce the total commitment, but it should not close the door. We must find a way to do the most important things; the LHC is one of them.

Recommendation 5

If there is no three-year, $50M/year bump in the budget, the subpanel recommends that the Department of Energy appoint a special subpanel of the High Energy Physics Advisory Panel to review the current U.S. high- energy physics program (preferably jointly with the National Science Foundation) and recommend appropriate changes and sacrifices.

However, the Subpanel still believes that joining the LHC collaboration is of sufficient importance that the U.S. should commit to doing so under a constant FY1995 level-of-effort budget, and should enter negotiations toward that goal.

Managing the Program

The potential of the U.S. to make historic contributions in high-energy physics must be realized in a way that uses the public's money and other resources as efficiently as possible. The subpanel has identified aspects of the program that should be reviewed by the Division of High Energy Physics of the Department of Energy, in collaboration with the high-energy physics community. These include governance, within the field and by the Department of Energy, with regard to the setting of priorities, the phasing of construction, the balance of resources and representation between universities and laboratories, opportunities for young scientists with new ideas and initiatives, and rewarding superior scientific performance. Mechanisms for interagency support for the interdisciplinary field of particle astrophysics should also be reviewed.

The subpanel has not been in a position to focus on such issues, but we believe they deserve consideration by the Department of Energy, with National Science Foundation and community involvement. There is also need for an assessment of what the community perceives to be a growing administrative burden, beyond what is needed for proper accountability, and an excessively bureaucratic application of Environmental, Safety & Health (ES&H) regulations that is neither risk-based nor adequately evaluated for its contribution to safety.

Using SSC Assets

On behalf of the entire high-energy physics community, the subpanel wishes to acknowledge the extraordinary generosity of the State and the people of Texas in connection with the SSC. Although world science has now lost the SSC, it is vital to remember that not everything that went into the SSC has also been lost. Both intellectual and physical assets remain. The research and development that were carried out are still available to science and industry, through the literature and through the experience of many excellent scientists and engineers. Most of these people are now--or will soon be--applying their knowledge and experience in industry and in government laboratories, or engaged in research and teaching in universities or at the high-energy physics laboratories. The physical assets consist primarily of equipment on the Ellis County site.

Conclusion 6

The subpanel understands that a vigorous process for encouraging and reviewing proposals for on-site use of buildings and equipment is now proceeding under a cooperative agreement between the Department of Energy and the Texas National Research Laboratory Commission (TNRLC). These uses could be educational, medical, scientific, or commercial--or some combination of these. The subpanel applauds this effort to make good use of the on-site investment.

One possible use of the buildings and equipment is a superconducting magnet laboratory, which might be the center for U.S. participation in the CERN LHC project. The subpanel anticipates difficulties with such a use. The powerful cadre of scientists and engineers that came together to design, assemble, and operate the SSC has now been widely dispersed. Prospects for rebuilding and maintaining a scientific and technical staff of the highest caliber, far from a high-energy accelerator laboratory, are, in our judgment, not good.

Recommendation 6

Proposals for a scientific mission for the former SSC site should undergo stringent peer review. The review should call upon international experts in relevant areas of science to judge the proposals on their scientific and technical merit, feasibility, and cost-effectiveness compared with other possible avenues for doing the same work.

Concerns about the vitality of a superconducting magnet laboratory for high- energy physics that is geographically separated from an accelerator laboratory will have to be weighed in evaluating proposals for such a laboratory on the former SSC site. The subpanel believes that these concerns weigh against such use.

Disposition of movable equipment will depend upon agreements between the Department of Energy and the State of Texas, which may involve the resolution of financial claims of the State. The subpanel does not presume to comment on issues outside our scientific and technical expertise in high-energy physics. However, if some of the equipment will not ultimately

be used on site, it could be used elsewhere to the benefit of the U.S. high-energy physics program. The subpanel urges all parties to decisions regarding the disposition of SSC assets to recognize that a timely resolution will make it more likely that they will be put to good use.

In Conclusion

We have presented a vision for the future in very general terms. It is too soon after the cancellation of the SSC to be more specific. At this stage, that is just as well: on the time scale of decades required to plan such large basic scientific endeavors at the frontiers of the unknown, we may expect big surprises. We have charted a path that, within its budgetary outlines, preserves a healthy balance and flexibility.

There is no way of predicting what scientific discoveries and technical breakthroughs may occur during the next decade. Such advances might lead us to seek to initiate another major international construction project before completion of the LHC. Additional funding would then be required. On the other hand, failure to maintain support for a constant level of effort in the future could deny the U.S. the opportunity to continue its tradition of success in the field of high-energy physics.

The U.S. high-energy physics program has long been a source of dramatic scientific progress and national pride, and a symbol of international collaboration. Building on the superb quality of its institutions of higher learning and research, and the generous support of the American public, the U.S. program has been a world leader at the frontiers of discovery. If our recommendations are implemented, we believe it will remain so well into the twenty-first century.