

The SSC: A machine for the nineties



The scale of the Superconducting Super Collider (if 5-T magnets are used) is shown in comparison with the Tevatron at Fermilab (the smallest ring) and LEP at CERN (the somewhat larger ring). All three rings are superimposed to scale on the environs of Washington, DC. Note that the SSC is about the size of the Washington Beltway. This NASA photograph was made in November 1982 from an orbiting Landsat.

With 30-erg protons colliding with 30-erg protons, the Super Collider will help resolve some of the open scientific questions concerning the nature of elementary particles.

Sheldon L. Glashow and Leon M. Lederman

In July of 1983, the High Energy Physics Advisory Panel of the Department of Energy recommended that the highest priority be given to construction of a very large accelerator, the Superconducting Super Collider. The recommended energy per beam of this accelerator is 20 TeV, or 20 000 GeV—this is a macroscopic energy of about 32 ergs for each proton in the beam. Head-on collisions of protons against protons will thus make 40 TeV available in the center of mass, more than 60 times the energy available at the present CERN collider and 20 times that to become available at the Fermi National Accelerator Laboratory in the near future. The committee urged, moreover, that this facility be completed and available for physics research within about a decade. The solemnity of the advice was underscored by the simultaneous recommendations that all other proposals for high-energy accelerators not be approved. This included both the Colliding Beam Accelerator, in which the Brookhaven National Laboratory had invested considerable effort, and the Dedicated Collider, a proposed expansion of the Fermilab complex.

Often in the history of elementary-particle physics, high energy has been the key to new discoveries. In the middle of the 19th century, experiments at energies of several electron volts led to the discovery of emission and absorption spectra, and to the realization that the atom is a structured system. In the early part of this century, x-ray experiments at energies in the keV range revealed the inner structure of the atom and led Henry Moseley to the concept of atomic number in 1913. The evolution of nuclear physics, from the discovery of the atomic nucleus itself in 1911 to the mature discipline of modern nuclear

science, began with experiments at energies on the order of MeV, first with particles available from naturally radioactive sources, but later from small and specialized machines such as Van de Graaff accelerators. Another factor of 1000 in energy was required to expose the mysteries of the subnuclear world. The Bevatron at Berkeley, operating at a center-of-mass kinetic energy of 2.7 GeV, first produced and detected antiprotons in 1955. In 1977 the upsi- π particle (sign of the fifth quark) was discovered at Fermilab, where the center-of-mass energy was 27.5 GeV—ten times greater than the energy available a generation earlier.

In these comparisons of available energy, the relevant criterion is center-of-mass energy rather than beam energy. The largest beam energy now available worldwide is generated at the Fermilab accelerator known as Tevatron II. Its beam energy is at present 800 GeV, but, because it operates in a fixed-target mode, the available center-of-mass energy is only 40 GeV. On the other hand, the CERN proton-antiproton collider has about 300 GeV in each beam; so it has 600 GeV available energy, which is precisely what allowed the discovery of the W and Z particles in 1983.

Not all great discoveries have depended on access to the largest possible energies. The J/ψ particle, the tau lepton, the upsi- π , and bare charm were all discovered in the US during a time when the CERN Intersecting Storage Rings were clearly ahead in the high-energy sweepstakes. Skill, imagination, persistence and experience are vital research ingredients; a high usable luminosity also helps. It is for this reason that fixed-target physics is complementary to colliders. However, particle physics has reached the point where even good old American know-how cannot compete with the higher energies that colliders can make available. The CERN collider, having confirmed the central predictions of the electroweak theory, has certainly earned the Nobel prize for European high-energy physics (PHYSICS TODAY, January, page 17)—the first in many

years (see the photo on page 30). But, more importantly, Europe has shown the way to the style of research required to address the open questions—the hadron collider furnished with integrated and sophisticated detectors.

The development of colliding-beam physics began at Stanford University, which operated an electron-electron collider in 1963. Electron-positron colliders followed at Frascati, Orsay and Novosibirsk. The CERN Intersecting Storage Rings, the first hadron-hadron collider, was commissioned in 1971. A series of electron-positron rings have operated at SLAC, Orsay, Frascati, Novosibirsk, Hamburg and Cornell University. However, with the CERN $p\bar{p}$ collider, the field will remain wide open to European research until Tevatron I becomes effectively operational at Fermilab. The table on page 31 summarizes the present and planned accelerator inventory.

We have come a long way in our search for an understanding obtained from what are now four generations of postwar accelerators. We have identified six quarks, which are the constituents of all hadrons, that is, all particles that interact via the strong force. We have evidence for six leptons. We have a partially unified electroweak theory and a very promising theory of the strong (color) force, quantum chromodynamics. The data support the idea that the forces are described by a quantum field theory obeying gauge invariance. We have now identified all the corresponding gauge bosons, the carriers of the forces: photon, $W^\pm$, Z^0 and the gluons. All of this constitutes a powerful synthesis called the Standard Model (see the box on page 32).

It has been said that the Standard Model offers a complete and correct description of all observed phenomena on Earth, and perhaps in the universe. Such things have been said before: of the clockwork universe of Robert Boyle, and of the great syntheses of Isaac Newton and James Clerk Maxwell. More recently (in 1947), George Gamow wrote:

...We have now much sounder reasons for believing that our ele-

Sheldon Glashow is Higgins Professor of Physics at Harvard University; an elementary-particle theorist, he is one of the architects of the unified theory of electroweak interactions and has introduced the concept of charmed quarks. Leon Lederman is director of Fermilab; a high-energy experimenter, he found the upsi- π , a hadron containing the bottom quark.



The Nobel prize award ceremony in Stockholm on 10 December 1984. From the left (front row) are Carlo Rubbia and Simon van der Meer, who shared the 1984 physics prize for discovery of the W and Z. Among those in the second row are the 1979 Nobel prize winners, whose electroweak theory predicted the W and Z. To the left of Abdus Salam (wearing white headdress) are Sheldon Glashow and Steven Weinberg.

A glossary of high-energy physics jargon

J/ ψ The compromise name for a particle discovered simultaneously at Brookhaven (J) and SLAC (ψ) in 1974, and which was soon interpreted as the bound state of a charmed quark and a charmed antiquark. The charmed quark was suggested by J. D. Bjorken and Glashow in 1964.

τ lepton The third charged lepton (after the electron and muon), discovered at SLAC in 1976.

γ The discovery of three closely spaced particle states at Fermilab in 1977 by Lederman and collaborators was soon interpreted as the

bare charm Since the J/ ψ contains both charm and anti-charm, the intrinsic properties of the quarks are hidden. A state containing a charmed quark with, say, an anti-up quark, exposes the quantum numbers of charm and represents bare charm. (The γ is an analogous system containing bottom quarks.)

W,Z The force carriers of the weak force, discovered at CERN in 1983.

mentary particles are actually the basic units and cannot be subdivided further. Whereas allegedly individual atoms were known to show a great variety of rather complicated chemical, optical, and other properties, the properties of elementary particles of modern physics are extremely simple; in fact they can be compared in their simplicity to the properties of geometrical points. Also, instead of a rather large number of "indivisible atoms" of classical physics, we are now left with only three essentially different entities: nucleons, electrons and neutrinos, and in spite of the greatest desire and effort to reduce everything to its simplest form, one cannot possibly reduce something to nothing. Thus it seems that we have actually hit the bottom in our search for the basic

constituents from which matter is formed.

Such hubris has never survived for long. Pions were found in 1947, strange particles came soon afterwards, and the 1960s saw a virtual population explosion of new "elementary particles."

True, the Standard Model does explain a very great deal. Nevertheless it is not yet a proper theory, principally because it does not satisfy the physicist's naive faith in elegance and simplicity. It involves some 17 allegedly fundamental particles and the same number of arbitrary and tunable parameters, such as the fine-structure constant, the muon-electron mass ratio and the various mysterious mixing angles (Cabibbo, Weinberg, Kobayashi-Maskawa). Surely the Creator did not twiddle 17 dials on his black box before initiating the Big Bang, and its glorious sequela, mankind. Our present theory is incomplete, insufficient and inelegant, though it may be long remembered as a significant turning point. It remains for history to record whether, on the threshold of a major synthesis, we chose to turn our backs or to thrust onward. The choice is upon us with the still-hypothetical SSC.

A giant step

Several arguments point to the necessity of a giant step in accelerator construction. Theorists and experimenters have settled on the parameter of greatest importance, the center-of-mass energy of 40 TeV. The High Energy Physics Advisory Panel, after long and agonizing debate, accepted this and recommended the SSC over all competing proposals. Theorists are generally agreed that new phenomena must certainly rise up and be counted at the SSC, perhaps discernible only as dim shadows at the CERN or Fermilab colliders. Indeed, experimenters fortunate enough to work at the CERN collider have already reported a handful of rare "monojets"—curious events that do not appear to be explainable in terms of the Standard Model. Through a tiny window, we may be seeing the new and confusing phenomena that may well require the SSC for their unraveling. Besides the arguments of experiment and theory, there is history: A great leap forward in physics occurs, regularly, at each significant jump in energy; in our projections for

The high-energy hadron accelerators

Machine	Location	Type	Beam energy (TeV)	CM energy (TeV)	Status
SPS	CERN	Fixed target	0.4	0.03	Operating
Tevatron II	Fermilab	Fixed target	0.8	0.04	Operating
ISR	CERN	pp collider	0.03	0.06	Discontinued
CBA (Isabelle)	BNL	pp collider	0.4	0.8	Cancelled
SpS	CERN	pp collider	0.32	0.64	Operating (1984/5)
Tevatron I	Fermilab	p $\bar{p}$ collider	>0.9	>1.8	1986
Dedicated Collider	Fermilab	p $\bar{p}$ collider	2.0	4.0	Not recommended
UNK	USSR	p $\bar{p}$ collider	3.0	6.0	1993?
SSC	?	pp collider	20	40	Our dream

the 1990s, multi-TeV physics is where the new action will be.

The major parameters of the SSC, 40 TeV of center-of-mass energy and "... up to 10^{33} particles/cm²sec of luminosity," are imposed by the scientific goals. The power and the incompleteness of the Standard Model conspire to indicate a domain of energies where data are sure to resolve the dilemmas blocking progress in particle physics. The objective is the "1-TeV mass scale." This means that collisions should easily be capable of exploring this mass scale—for example, by producing a suspected new particle with a mass of a few TeV. The colliding protons are, however, complex objects, composed of quarks and gluons; it is the hard collisions of the pointlike constituents that are relevant. The Standard Model gives us the motions of the constituents that share the momentum of the colliding protons. The net effects are known in great detail and can be found in a compendium¹ of cross sections for anticipated new physics processes. Because of the momentum sharing, one must divide the energy of the protons by about ten; so 40-TeV protons will permit one to explore the mass range up to 2-4 TeV.

The luminosity is a measure of the number of collisions of a particular kind that take place per second. The consensus of a very large number of workshops and seminars (as well as the paper¹ by Estia Eichten and his collaborators) is that an energy of 40 TeV and an integrated luminosity of 10^{39} cm⁻² will make possible a detailed exploration of the 1-TeV mass range.

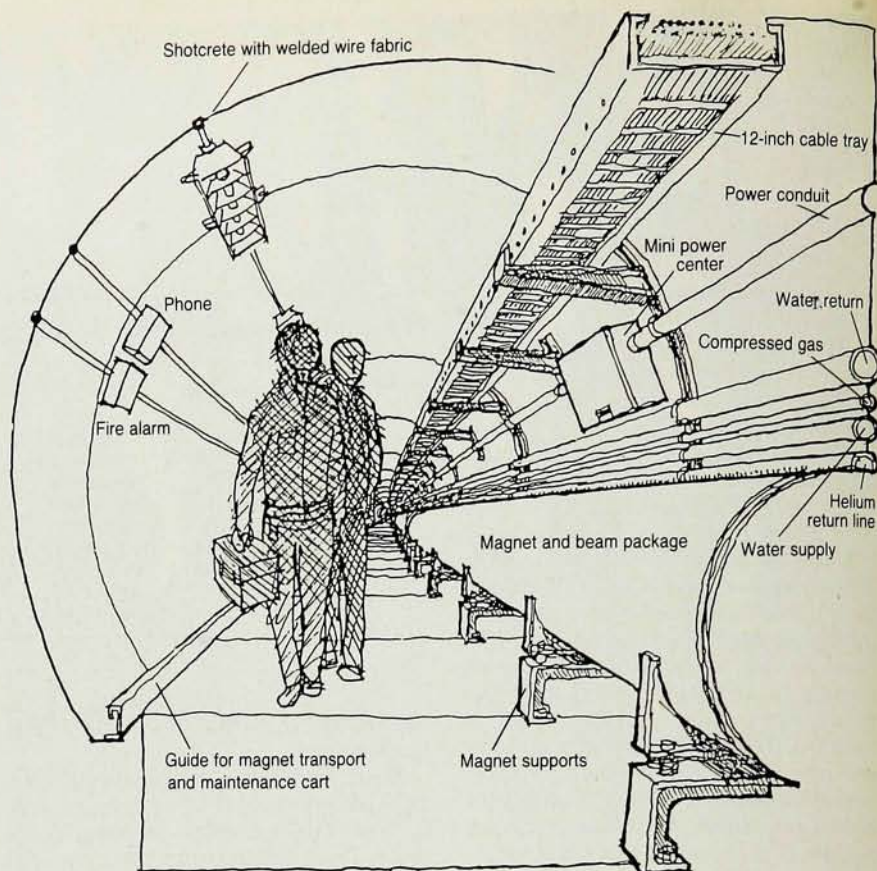
Now what are we looking for? While we can list specific problems here for which we seek some resolution, we must acknowledge history's lesson that

surprises have been our most frequent lot.

While we believe that a unified theory of the strong, weak and electromagnetic forces must be correct, confirming it and filling in the details require data to resolve some unanswered problems.

► The major problem has to do with the symmetry of the triumphant electroweak theory—namely, that it is broken, and, in particular, that the W and Z masses are large whereas the very proper photon has zero mass. This problem has been treated by postulating the "Higgs boson," a particle whose mass and interaction properties are not specified by the symmetry. It is a study of this solution to the mass problem that points to the 1-TeV scale, below which some new phenomena, some new physics, must show up. The problem is more general: In all honesty, we have no real insight into the ultimate origins of the masses of any of the basic particles. They are simply parameters in the Standard Model, and so we come again to the 17-parameter problem. The 1-TeV mass scale comes in because of the close involvement of these parameters with the electroweak symmetry—the so-called Higgs sector.

► Perhaps we have gone astray to assume that the quarks and leptons are really primordial. Perhaps they are composites of more basic "prequarks." If so, a new force must exist, and unification must remain incomplete until we understand this. Significant probes of the pointlike nature of electrons, muons and quarks must be at least at the 1-TeV scale, as suggested by the success of the Standard Model. Here we could add that we do not understand why there are two "Xerox copies" of the first generations (u, d, e,



ν_e) and we do not know if there are additional generations.

► Symmetry has been the guiding light of our world view, yet there is a long-puzzling violation of CP invariance in weak interactions. Here again, there is a connection of some kind to the Higgs boson and therefore, again, to the 1-TeV scale.

To address these and other problems, a very large number of theoretical papers have attempted to extend the Standard Model. Many new concepts have been introduced employing key words such as "technicolor," "hyperco-

lor" or "supersymmetry." These often elegant ideas are unencumbered by any experimental facts. The SSC is needed to provide data that will guide physics to a true understanding.

Finally, we should note a profound novelty in the state of particle physics in the SSC era—namely, the new conjoining of this subject with cosmology and the data from that great accelerator-in-the-sky, the early universe. The 40-TeV machine will allow us to study matter in a state equivalent to 10^{-16} sec after the Big Bang; the data obtained may prove crucial to an under-

standing of how we all got here.

Perhaps we have convinced you that the "desert"—a large energy domain above the W mass containing no new physics—is a mirage. Perhaps you will concede that startling new discoveries are likely to be made with the SSC. Still, you may argue against it. Particle physics is no longer "relevant," you may say. The Standard Model is all we know and all we *need* to know for technology, and indeed for all of science but cosmology and elementary-particle physics itself. Our discipline, the critic continues, seems to have turned in upon itself and no longer relates to the rest of the scientific endeavors of mankind. It is a kind of art for art's sake, though far more costly to pursue. Again, the critic asks, who needs the SSC?

Why do we need an SSC?

We respond in several ways—in terms of challenge, spinoff, pride, and duty.

Challenges. Consider Arthur, an intelligent alien from a distant planet, who arrives at Washington Square (New York City) and observes two old codgers playing chess. Curious Arthur gives himself two tasks: to learn the rules of the game, and to become a Grandmaster. Elementary-particle physics resembles the first task. Condensed-matter physicists, knowing full well and with absolute certainty the rules of play, are confronted with the

The Standard Model

The fundamental particles of the Standard Model are six quarks and six leptons. The quarks come in three "colors," R,Y,B. The particles interact via forces, described in quantum theory as gauge-invariant fields whose quanta are bosons having spin 1. The electroweak force is carried by the photon, Z^0 , W^+ and W^- . The strong (quantum chromodynamic) force is mediated by gluons. The fourth force, gravity, is not encompassed in the Standard Model. A fifth force, responsible for breaking the symmetry of the rest, is deleted from the tables below out of a sense of ignorance.

	1st generation	2nd generation	3rd generation
Quarks	u d	c s	t b
Leptons	e ν_e	μ ν_μ	τ ν_τ
Forces	Carried by		
Electromagnetic	Photons		
Weak	W^+ , W^- , Z^0		
Strong	Gluons		

second task. Most of modern science, including chemistry, geology, and biology since the fall of vitalism, is of the second category. It is only in particle physics and cosmology that the rules are only partly known. Both kinds of endeavor are important—one more "relevant," the other more "fundamental." Both represent immense challenges to the human intellect.

Challenge has another aspect. As physicists pursue higher energies and the ever finer structure of matter, the task becomes more difficult. The construction of great machines, elaborate detectors and powerful data-handling techniques brings us to the cutting edge of modern technology. Our workers and our factories will be compelled to confront all but insuperable technical obstacles. Meeting these challenges will make American industry better able to compete, produce and flourish in an increasingly technological society.

Spinoff. The following arguments are well known to our colleagues in physics. They are used, and validly so, by all fields. Indeed they are strengthened by cross fertilization. High-energy physics contributes its share to the benefits that physics brings to society.

The design, construction and operation of a large accelerator in a cost-effective manner demands technological innovation that can be of considerable value elsewhere in our society. Intense study of superconducting mag-

nets can be important to many socially relevant technologies: super-rapid transit, energy-storage systems, electrical power transmission, for example. The accelerator laboratories substantially advanced the technology, which had grown out of basic materials science. The SSC will require excavation of a very large tunnel; search for cheaper tunneling techniques will produce patents that may prove important for sewage systems, subways, and the like.

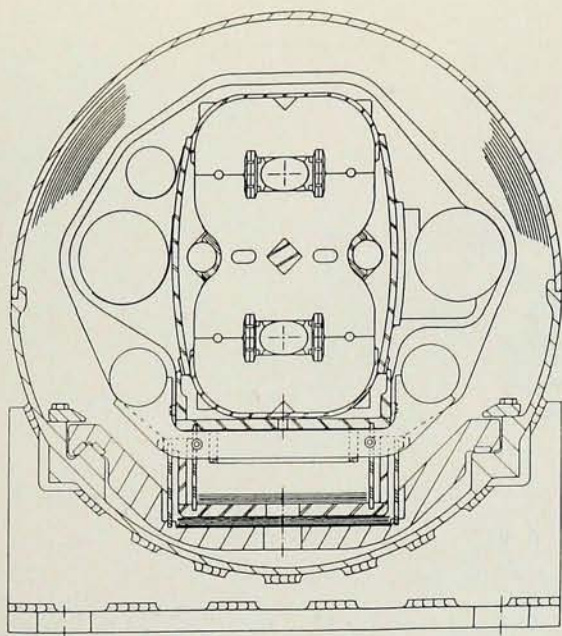
History offers many examples of past successes: Developments originating in particle physics have had an impact on computers and computer science, cryogenics, copier technology, medical diagnostics and treatment, synchrotron light sources, industrial and medical accelerators, and petroleum exploration and recovery—to name only a few examples. We should also note that the highest-priority devices in both nuclear- and materials-science programs (as described elsewhere in this issue) are high-energy electron accelerators that are derived from the high-energy accelerators pioneered at Cornell and Stanford.

In addition to new and improved technologies, particle physics yields highly trained scientists accustomed to solving the unsolvable. They often go on to play vital roles in the rest of the world. Physicists trained in our discipline can be found in large numbers outside it, happily and gainfully em-

ployed, but doing something else and doing it well. One of "us" recently won the Nobel prize in chemistry for the discovery of the genetic repressor. Another won it in medicine for the invention of the CAT scanner. Andrei Sakharov, who explained the origin of matter in the "hot Big Bang," went on to win the Nobel peace prize in 1975. Many of us have become important contributors in the world of energy resources, neurophysiology, arms control and disarmament, high finance, defense technology, and molecular biology. There is even an occasional artist or author.

High-energy physics continues to attract and recruit into science its share of the best and the brightest. If we were deprived of all those who began their careers with the lure and the dream of participating in this intellectual adventure, the nation would be considerably worse off than it is. Without the SSC, this is exactly what would come to pass. We acknowledge that other components of fundamental physics have equally valid claims, but let's think of the entire activity: Can we have fundamental physics without this subject and its scientifically compelling next step?

Pride. Although pride is one of the seven deadly sins, we are proud of the successes of our predecessors, and proud of our country, which has generously supported the study of the most fundamental structure of matter.



Design for the Texas Accelerator Center 3-T superferric magnet. The magnet has two beam channels that are magnetically independent. Eight turns of 10 kiloamps are used to drive the field in each channel. The 3-T magnet was called design "C" in last year's Reference Design Study.

Physics is an international discipline and has operated in a competitive-collaborative mode since Galileo. However, true collaboration requires rough equality. Yet, most of the recent discoveries in particle physics were made abroad. Gluons were first found in Germany, where the PETRA collider holds the record for e^+e^- collisional energy. CERN, after triumphantly revealing neutral currents in 1973, went on to discover the W, the Z, and the top quark, and now presents us with a bewildering array of anomalies. Many of our colleagues approach us to find out "what's up?" in particle physics. The usual answer has been, "Construction has just started on the HERA machine, a giant electron-proton collider," or, "CERN's Large Electron-Positron Collider, a 27-km ring near Geneva is underway," or, "The CERN collider has restarted brilliantly," or, "PETRA is now running at 47.6 GeV," or, "Europe will surely convert the LEP tunnel to a large hadron collider in the 1990s" or, "Perhaps we will have the SSC by 1994." More and more, American accomplishments either recede in to the past perfect or dangle in the future conditional while the Europeans pursue the present indicative. Of course, as scientists, we must rejoice in the brilliant achievements of our colleagues overseas. Our concern is that if we forgo the opportunity that SSC offers for the 1990s, the loss will not only be to our science but also to the broader issue of national pride and technological self-confidence. When we were children, America did most

things best. So it should again.

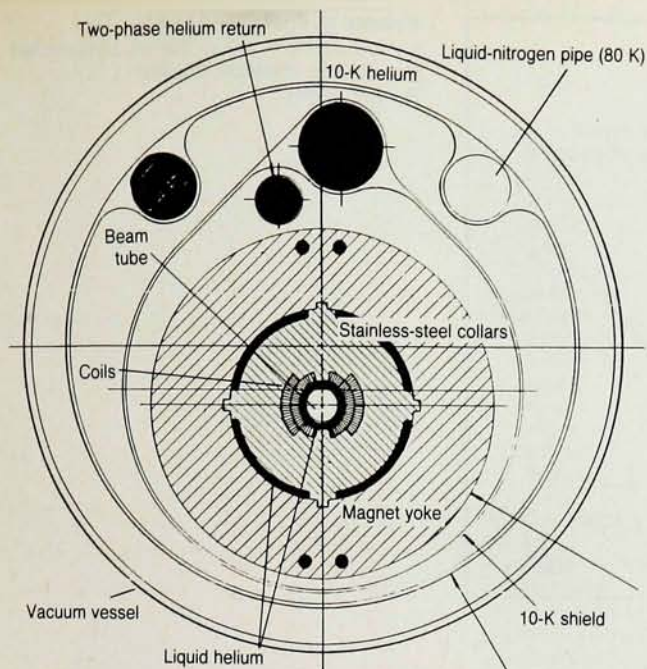
A sense of duty. This motivation for the SSC is the most difficult to explain, but it is the driving force of the particle physicist. Faith in the underlying simplicity of nature—quite unjustified, to be sure—has time and again led to discovery. We are amazed at the incredible successes of 20th-century science, and at its enormously positive (and, regrettably, sometimes negative) effect upon everyday life. The universe astonishes us by its very comprehensibility. In this we find our call: Being born upon an obscure planet located at the rim of a middling galaxy among a hundred billion galaxies of an aging universe, it is our sacred duty to know its deepest secrets, as well as we are able. Dolphins and chimpanzees can be made to speak, after a fashion. Yet, only humans will look at the stars with wonder and find it necessary to understand just what they are and how they work and why we are here to see them. No better mousetrap or wrist tv here—just the triumph of human imagination. It is simply *the need to know* that compels us to build a bigger and better accelerator and to approach an understanding of the mother of us all—the Big Bang—and its curious by-product, the matter of which we are made.

Colleagues are careful to insist that particle physics at the high-energy frontier can have no direct effect upon our technology. If this turns out to be true it will be the first time in the history of science. But what can we say with certainty about technologies of

the future? Human society, aided by science, should be better able to cope with the vicissitudes of life on Earth than the dinosaur's could. Yet, they flourished for 300 million years. One may hope that our species may do at least as well. We cannot argue reasonably that the pure physics of today will not become essential to the technology of a distant tomorrow.

What, then, is the SSC?

As now conceived, SSC is a double ring of superconducting magnets in an underground tunnel 90–180 km in circumference. (See the illustration on page 32.) It will have four or six interaction regions where counter-rotating protons make head-on collisions to generate 40 TeV in the center of mass. In January 1984, about 150 physicists and engineers from throughout the US gathered at the Lawrence Berkeley Laboratory and, by May, produced the "Reference Design Study." This was intended to narrow the uncertainties of costs and schedule and to determine the most fruitful topics to be addressed by R&D. The reference design breaks no dramatic new ground in fundamental accelerator science. It is unabashedly a scaled-up and improved version of Fermilab's superconducting Tevatron, making use of both the experience developed there and the intensive R&D in magnet technology now taking place at Brookhaven, Fermilab, LBL, and the Texas Accelerator Center. Other systems that will be improved are cryogenics and computer controls, both of which will be applied



Preliminary design for a 6-T superconducting magnet, being developed by a Brookhaven, Fermilab, Lawrence Berkeley Lab collaboration. This design, denoted "D," is a compromise between design "A" for a 6.5-T magnet and design "B" for a 5-T magnet, both of which were considered in last year's Reference Design Study. The vacuum vessel in this drawing has a 53-cm diameter.

on an unprecedented scale. The enormous size of the SSC is illustrated on page 28.

Work on superconducting magnets began in accelerator labs after the discovery of hard superconducting materials, in about 1960. Soon, very large magnets were built for spectrometers and other instruments. In the late 1960s, laboratories started to develop pulsed superconducting magnets, adding the complication of rapidly varying fields. At Fermilab this R&D work began in 1972-73 and benefited from the experience of many laboratories, most notably the Rutherford Laboratory in England, and LBL and Brookhaven in the US. The construction of a ring of superconducting magnets began in July 1979. The entire 6-km-circumference ring of over 1000 magnets was cooled to 4.5 K in May 1983. In July 1983, acceleration of protons was achieved. Experiments were started in October (PHYSICS TODAY, March 1984, page 17) and today 800-GeV beams are being delivered routinely to eight targets. There is every indication that a superconducting machine will be a reliable and efficient device.

The Tevatron has raised the available energy from 400 to 800 GeV (the goal is 1000 GeV) and, at the same time, has reduced electrical power consumption by a factor of four. The basis of confidence in the feasibility of an SSC rests on the success of the Tevatron accelerator.

The reference design envisions an accelerator complex involving a "...central office-laboratory building,

sized to accommodate 3000 full-time and visiting scientists and staff, injector facilities consisting of a 200-m linear accelerator, a 1.2-km low-energy booster and a 6-km high-energy booster, designed to provide a 1-TeV beam of protons suitable for injecting into the SSC main ring." The two concentric rings of superconducting magnets provide the place where counter-rotating protons would be accelerated (each to 20 TeV), stored, and then brought into collision in the interaction regions. Typically, storage times in excess of 10 hours are expected.

Two magnet strengths are being considered in the current R&D program: a "low field" 3-tesla magnet, which would imply a ring circumference of 180 km, and a "high-field" 6-T magnet, requiring a tunnel of 90-km circumference (see the illustrations on pages 34 and 35). The two magnets also embody different philosophies: The 3-T design depends heavily on the shaping of iron pole pieces to define the quality of magnetic field that guides the protons; in the 6-T design, the current-carrying superconducting wires determine the field shape, as in the Tevatron.

The reference design estimates a need for thousands of magnets of the bending and (quadrupole) focusing types, and for a massive cryogenic system to bring and hold these at liquid-helium temperatures. Managing a system of this scale will require an instrumentation and control system of impressive proportions.

The Reference Design Study and

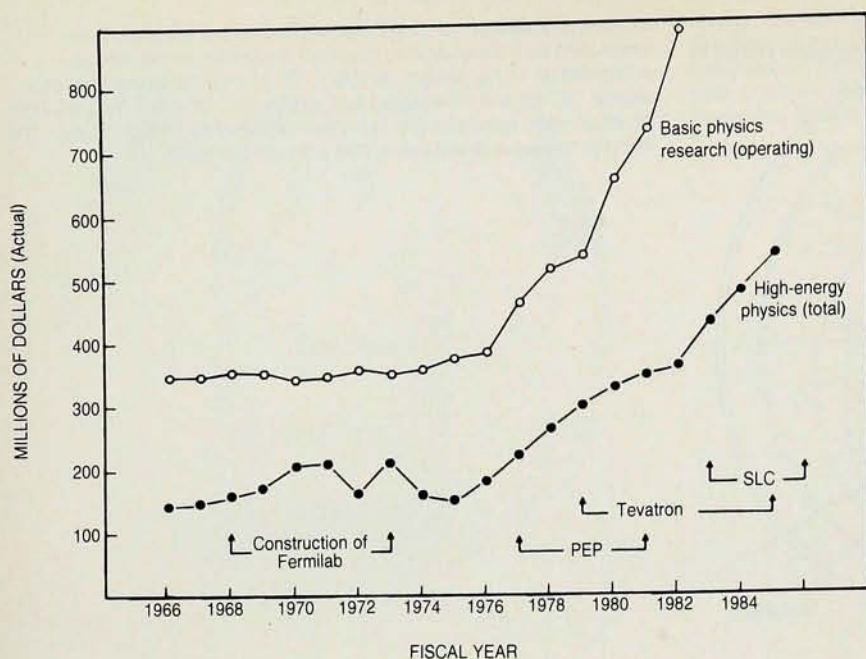
subsequent workshops have to date uncovered no obstacles to our plan to construct such a machine. What provides the unprecedented technological hurdle is the scale of the project. The major challenge is in technology and engineering, although some physics issues do remain. What is needed, the experts concluded, is about three years of hard work to design options and to invent and test cost-saving and reliability-enhancing ideas. The objective is to have the machine ready for physics experiments by about 1994.

Some of the uncertainties that must be removed before the definitive design is made center about the following questions:

- What is the minimum magnet aperture that will work? The magnet aperture of the Tevatron is about 7 cm, which yields a "good-field" region of about 4 cm. With a higher-energy injector to the SSC, the required aperture is smaller—and this saves much money. The definition of a "good field" also needs precise specifications.

- What is the best magnetic field value and construction style? RDS looked at three magnet styles and fields. These have since been reduced to the two styles discussed above. After suitable tests on several models of each design, a decision must be made and the chosen design improved during exhaustive system testing.

- Can interaction regions be clustered for economic sharing of support facilities, or must they be uniformly distributed around roughly the 100-km circumference?



Federal budgets for physics. The vertical axis is in actual dollars, uncorrected for inflation. (Source: Physics Survey, 1984)

The community has also examined and will continue to study other options such as $p\bar{p}$ collisions, ep collisions, and fixed-target applications. These, however, are not in the mainstream of the current effort.

International collaboration?

High-energy physics has a long tradition of active and intimate international collaboration. This is illustrated by the pioneering creation of CERN, the European consortium that operates the very large accelerator complex near Geneva. European and American teams continue to use one another's facilities easily and frequently. We have formal exchange agreements with Japan, China and the Soviet Union.

In 1975, high-energy physicists organized the International Committee on Future Accelerators. ICFA had two missions: to use its best efforts to enhance communications and minimize duplication of frontier facilities, and to look ahead to the time when the resources required for the next energy level would require worldwide collaboration. Recently, this grass-roots movement was supplemented by promulgations coming down from the Economic Summit. Here the leaders of the industrial nations (European Economic Community, Japan, US, Canada) have selected a number of scientific and technical fields and have in effect committed themselves to the intellectual prosperity of these fields, while urging that a coherent, collaborative, long-range plan be presented. The

London Summit of June 1984 was followed by a high-energy physics meeting in Brussels in July. There, a committee was established to examine the problems of international collaboration in the long-range planning of new facilities. Recognizing this to be a two-to-three-year process, the Committee requested an interim report by June 1985.

The US community, moderately optimistic about SSC as a result of the decision of the Secretary of Energy (in August 1984) to proceed with the R&D plan, nevertheless took the summit message very seriously. Our European colleagues are in the midst of an ambitious program of accelerator construction at CERN and DESY (in Hamburg). When this program is completed in 1990, they will have expended close to \$2 billion on capital facilities, including detectors. (Here we have tried to do the accounting on the US system where, for example, salaries are included in the cost estimations.) After that, it is conceivable that Western Europe could join with other countries to make contributions to the SSC. Some of the candidate nations that might assist by accepting construction responsibilities are Japan and Canada. Other modes of collaboration are also possible. Obviously there are risks and, not so obviously, vast sums of money are unlikely to be saved by the host country. Nevertheless, as facilities escalate in cost, both financial and intellectual, there is widespread recognition that a coherent international plan makes sense. We look to the Sherpas to guide the

summiters toward such a plan over the next few years.

What will it cost?

The cost of the SSC was carefully estimated by RDS at about \$3 billion (1984 dollars). This estimate includes a contingency fund, suggested by DOE, of about 20%. One must add to this the cost of detectors (\$0.4–0.8 billion) and preoperations (\$0.1–0.2 billion).

Much has been learned from CERN's 600-GeV (center-of-mass) collider about the required properties of a general-purpose detector. The Tevatron Collider Detector Facility at Fermilab will cost \$60 million. Estimates for scaling this 2-TeV detector to 40 TeV run as high as \$200 million. Special-purpose detectors designed for more specific researches are usually far less expensive.

The track record of high-energy physics construction projects is very good. In view of the essentially conservative technology and in view of the now characteristically exhaustive scrutiny of cost estimates for major projects, it is extremely unlikely that overruns will be incurred. On the other hand, two to three years of R&D could produce significant savings. International collaboration may further serve to reduce the cost to the US taxpayer. The largest previous high-energy-physics construction, Fermilab, cost about \$900 million (in 1984 dollars) on the same basis. Thus we are facing an almost fourfold increase for a facility of the 1990s over the amount spent for the accelerator of the 1970s.

In view of the universal increase in

1953

1965

1980

2000

Princeton
Brookhaven
Berkeley
Caltech
Carnegie-Mellon
Carnegie Inst.
Chicago
Columbia
Cornell
Harvard
Illinois
Indiana
Maryland
Michigan
MIT
Ohio State
Penn
Purdue
Rochester
Stanford
Washington U.
U. of Washington

Argonne
Berkeley
Brookhaven
Caltech
CEA
Cornell
PPA
Stanford

Brookhaven
Cornell
Fermilab
SLAC



demand for more sophisticated equipment, this is not out of line. It's just a very large sum of money. After construction, and assuming an eventual constriction of the funding for the rest of high-energy physics, the budget for high-energy physics can return to its pre-SSC level. The annual operating budget of the new facility has been estimated at about \$200 million. To this, one would have to add about \$50 million for detector and machine improvements. High-energy physics in 1985 is supported at about \$600 million. Clearly, the new machine will draw major effort and resources from the ongoing program. There doesn't seem to be any reason why the US should not be able to afford this and other research facilities. It is crucial, of course, that these pass stringent tests to determine their scientific value.

There is a concern that so large a project will adversely affect other deserving physics programs. A study of the funding history over the past 25 years does not support this concern, as the graph on the opposite page shows. Although we cannot predict the future pace of science funding, it is likely that the public and its representatives in government will continue to appreciate good science as a necessarily increasing proportion of the Federal budget. But this esteem will not persist if the scientific community loses its vision and exuberance and becomes fretful and divisive. Yes, we have national budget deficits, and yes, we have urgent social problems. But if we have faith in the enduring future of the nation, then

basic research must go on with reasonable stability, and at a pace that is perceived to be viable by the young scientist.

The high-energy-physics community has been reasonably responsible in recommending the termination of older but still scientifically useful facilities to provide funding for accelerators that could address more crucial issues. The figure on this page illustrates the termination of major facilities over the past three decades.

At each stage, we were compelled by increasing cost and complexity to assemble larger teams and to stretch experiments over longer durations—the idyll of a backyard accelerator is now for very few, and commuting across the country is the norm. No one likes this, and it does require enormous attention to ensure that we are still attending to our students, our junior faculty, our replacements. For example, until recently, universities have been the training grounds for accelerator builders and facility directors. As accelerator technology has become much more sophisticated, individuals have become specialized as accelerator physicists and particle physicists. It is difficult to generate accelerator leaders and experts, as the number of accelerators decreases and they become divorced from the universities. The SSC is one more step in this process, but we see no alternative that preserves the scientific vitality, no, the *validity* of the activity. It is our opinion that high-energy physics must go in this direction or terminate the 3000-year-old quest

for a comprehension of the architecture of the subnuclear world.

Modern particle physics has a rich heritage, following a track—consciously or not—set out for us by the ancient philosophers. This enduring quest has produced several great intellectual revolutions, two in this century: one in the perception of space time and causation (relativity), the other in the understanding of the behavior of matter at the atomic level (quantum theory). We must also note the gradual establishment of a scientific basis of technology and the interdependence of sciences via both the overlapping content and the spread of instruments of science. Particle physics has as its intellectual neighbors, on one side cosmology, on the other side nuclear physics. The techniques and devices evolved in the accelerator laboratories have found ready applications in other fields of science. Symmetrical benefits have been received, both intellectual and technological, from all the subdisciplines. However, the thing we all share, above all else, is the sense of wonder and awe at the distance we have covered toward comprehension of our universe.

We are now asking our fellow physicists to join us, however vicariously, in a very great adventure: nothing less than a giant step in the continuation of our collective ambition to strive for a deeper understanding of that nature within which humankind is embedded.

Reference

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