Reviews of Accelerator Science and Technology
Vol. 1 (2008) 259–302

World Scientific
www.worldscientific.com

The Supercollider: The Pre-Texas Days

A Personal Recollection of Its Birth and Berkeley Years

Stanley Wojcicki

Department of Physics, Stanford University,
382 Via Pueblo Mall, Stanford, CA 94305, USA
sgweg@slac.stanford.edu

This article describes the beginnings of the Superconducting Super Collider (SSC). The narrative starts in the early 1980s with the discussion of the process that led to the recommendation by the US high energy physics community to initiate work on a multi-TeV hadron collider. The article then describes the formation in 1984 of the Central Design Group (CDG) charged with directing and coordinating the SSC R&D and subsequent activities which led in early 1987 to the SSC endorsement by President Reagan. The last part of the article deals with the site selection process, steps leading to the initial Congressional appropriation of the SSC construction funds and the creation of the management structure for the SSC Laboratory.

Keywords: Accelerator; collider; superconductivity; SSC; CDG; superconducting magnets.

1. Introduction

This article is the first in a two-part account of the history of the Superconducting Super Collider (SSC), or the Supercollider for short. Its story starts in the early 1980's, when initial stage-setting events for the SSC took place, and runs to the end of 1988, when the activities of the Central Design Group (CDG), responsible for the oversight and coordination of the early SSC efforts, started being phased out and the SSC focus shifted from Berkeley to Texas. The account of the years from 1989 to 1993 when the SSC was terminated, is the subject of a companion article [1]. This combined account relies to a large extent on numerous relevant documents and contemporary articles, but is also very much shaped by my personal recollections.

The SSC was to be the biggest and costliest basic research instrument ever constructed. The proposing high energy physics (HEP) community argued persuasively that such a large scale instrument was essential for attacking the frontier problems in particle physics. Because of its unprecedented size and cost, the SSC generated a lot of controversy both in scientific circles and among the public at large. Thus the history of the SSC has to involve not only science and technology, but also politics and public relations.

I do not attempt to provide a detailed technical description of the SSC and of the technical challenges that were faced, and to a large extent met, during its brief history. Instead, I try to present to a rather general scientific audience my recollections of the scientific, technological and political background which led to the SSC proposal, of the efforts to make the SSC a reality, and finally (in the second article) of the events that led to its demise. I had strong involvement with and in the SSC over most of that time; in such circumstances development of some biases is probably unavoidable. I have tried to maintain objectivity in this article, but some prejudices may flavor this account and the reader might keep that in mind [2]. In writing this article I have profited from discussions with many colleagues involved with the CDG.

My association with the SSC had two distinct phases. During the period discussed here, I was intimately involved in SSC, and my perspective is one of an insider. On the other hand, during the Texas days, most of the time my involvement was from outside, in my role as a High Energy Physics Advisory Panel (HEPAP) member and later as its chair. I did spend eight months at the SSC Laboratory (SSCL) on my sabbatical in 1993, just before the SSC cancelation, and obtained additional insight from that vantage point.

The story and the lessons of the SSC might be especially timely today, since the international HEP community is proposing to build a new facility, the International Linear Collider (ILC). There are many parallels with the SSC — in the cost, the technological challenge, and the difficulty of its realization. Study of what went wrong, but also what went right, with the SSC might be instructive as we grapple with how one might best achieve the HEP goals.

2. The Early 1980's

It is quite common today in federally sponsored research for the funding agencies, in deciding on allocation of funds, to rely heavily on advice from one or more peer review groups specially constituted for this purpose. In HEP in the US, the principal body providing this function is HEPAP. It was initiated in 1967 — with Victor Weisskopf, a professor of physics at MIT and former director-general (DG) of CERN, as its chair — as an advisory group to the Atomic Energy Commission (AEC), the federal agency responsible for the stewardship of the major part of the US HEP program. This advisory structure carried over to the Energy Research and Development Administration (ERDA) as AEC was phased out, and more recently to the Department of Energy (DOE) as that department absorbed ERDA. The other major US HEP funding agency, the National Science Foundation (NSF), has always had access to HEPAP deliberations and recommendations, but it was only in the last decade that both agencies assumed formally the responsibility for HEPAP membership and activities.

HEPAP has typically 15–20 members, each serving for 3 years, and meets 3 or 4 times a year. Its membership is composed mainly of relatively senior US HEP physicists, with a few members from abroad and from related fields or industry. HEPAP, as a chartered federal panel, has to meet in public. That requirement makes the debates open to all interested parties and thus restrains the discussion of controversial issues. This feature and its infrequent meeting schedule means that HEPAP is not ideal for debating and deciding on recommendations for the more complex and controversial issues.

The mechanism for dealing with such topics is the appointment of HEPAP subpanels, with a very specific charge and a more limited term of appointment for its members. Each subpanel, after its deliberations, which can be held in closed sessions, writes a report that is presented to HEPAP, which discusses it and can accept it or reject it (so far always the former, if I remember correctly).[a] Most often the main issue presented to the subpanels has been long range planning, frequently focusing on relative priorities of new accelerator facilities being proposed. The first of these subpanels was formed in 1974 and was chaired by Weisskopf. Such a subpanel, named the Subpanel on Long Range Planning for the US High Energy Physics Program, was formed in 1981 with George Trilling, a professor of physics at UC Berkeley, as its chair, to address the anticipated key issues in the field in the 1980's.

Before discussing the charge of this subpanel and its recommendations, it is useful to recall the overall situation in HEP in the early 1980's. The mood among the US HEP community at that time could be characterized as one of malaise. The successes of the 1970's — the concurrent J/ψ discovery at Brookhaven National Laboratory (BNL) and Stanford Linear Accelerator Center (SLAC), the τ lepton discovery and elucidation of the charm sector at SLAC, the startup of Fermilab several years ahead of SPS (400 GeV proton synchrotron) at CERN — were things of the past. The community became used to being pre-eminent in the field, but the future could not promise that this would continue. All indications were that the torch of leadership was passing to Europe, where it might well stay for the forthcoming decade, with the US lagging a poor second behind [3].

The European e^+e^- high energy collider, PETRA, was completed a year ahead of its US competitor, PEP, and was the first one to provide conclusive evidence for gluons. CERN SPS physics was eclipsing that from the Fermilab accelerator, both in neutrino physics and in high energy muon scattering studies. CERN was about to succeed in achieving high energy proton–antiproton collisions in its SPS and thus very likely to observe both W and Z

[a]This practice has changed in recent years. HEPAP now receives the almost final version of the reports some time before its formal meeting and its members can provide initial feedback to the Subpanel.

bosons (predicted by the Standard Model of particle physics, which had passed all the other experimental tests to date). In contrast, Fermilab was involved in an ambitious program to convert its accelerator to a superconducting fixed target accelerator [4] and eventually to a 2 TeV center-of-mass energy superconducting collider. This was, however, a long range and risky endeavor which might or might not succeed. The construction of the CERN LEP collider, which would be a Z factory and a means of studying e^+e^- interactions at energies beyond the Z, was to be initiated in 1984. The ambitious SLAC Linear Collider (SLC) project, however, was still in an R&D phase and its complexities led many to doubt whether its design goals would even be approached.

Simultaneously, serious strains and cracks were developing in the US HEP structure. US HEP at that time was effectively based on three principal DOE-supported accelerator laboratories: BNL, Fermilab and SLAC (with an important smaller effort at Cornell, supported by NSF). This reflected the fact that the accelerator facilities, to be on the frontier, had to be so big and hence so expensive that they had to be few in number and built on a regional (or even national) basis. By early 1980's, with HEP budgets fixed (or decreasing in terms of purchasing power), many in the community questioned whether the three-lab model was viable for the long term future.

A significant factor responsible for the difficult situation in US HEP was a mismatch between the expected and the actual funding. In 1978 an agreement was reached between the Office of Management and Budget (OMB) and DOE as to the levels of funding for HEP in the future. Those levels, plus ones a few percent above and below, were given to the HEPAP subpanels as the yardsticks for planning future programs. Since the actual appropriations invariably turned out to be lower than in even the lowest scenarios, it is not surprising that the recommended and generally adopted programs could not be fitted into the appropriated budgets.

This dilemma was brought to the fore by the issue of ISABELLE, a p–p collider with 800 GeV total energy in the center of mass [5]. This collider, using superconducting magnets, was proposed by BNL in 1976 and endorsed by a HEPAP subpanel [6] in 1977. The project was included by DOE in its budgetary submission and approved by Congress in 1978. By 1981 the conventional construction, including the tunnel, was essentially finished but on the technical front serious problems developed [7]. When the project was first proposed, the superconducting technology for accelerator magnets was still in its infancy. BNL gambled on the so-called "braid" superconducting cable for the magnets. Even though successful in few early "handmade" prototype magnets, the cable did not appear to be adequate for mass production and thus the technical side of the project was delayed. The braid was originaly developed at BNL, so there was probably some sentimental attachment to it. An alternative, called Rutherford cable, was developed in that laboratory and had great promise, but no significant data were available as yet at that time. Fermilab started an effort, using this cable, to upgrade its 400 GeV synchrotron to a superconducting accelerator, with initially a fixed target capability and later evolving toward a proton–antiproton collider. Besides the technical concerns, there was an additional question whether these two facilities were sufficiently complementary to warrant construction of both of them within the limitations of the projected HEP budgets.

In trying to understand the sociological background in US HEP in the early 1980's, one must also look at the overall national scene. The late 1970's were years of malaise also on the overall national scale. The oil embargo, high inflation and high interest rates, the Iran hostage situation, all contributed to a great deal of self-doubt in the US population at large. Undoubtedly it was this atmosphere and Reagan's promise to reverse it that played a major role in Carter's defeat in 1980.

George Keyworth was appointed as Reagan's science adviser and Director of Office of Science and Technology Policy (OSTP). He is best known as an advocate of the Strategic Defense Initiative (SDI), known as Star Wars. He believed in bold steps in scientific initiatives and was concerned that the US was squandering resources by adhering to "entitlement" programs for various labs rather than trying to optimize the program for the best science by setting priorities [8, 9]. It is not surprising that he believed that ISABELLE was a mistake and one should strive for a more ambitious project.

It was in this climate that DOE convened the Trilling Subpanel. As expected, its charge dealt with the optimization of the US HEP program [10]. But it also emphasized the request for a recommendation regarding ISABELLE; three different budgetary levels were put forth as the framework for specific recommendations. The most relevant parts of the charge are quoted below:

> "Develop a strategy and long-range plan for the U.S. Program over the next decade under the following funding constraints:
>
> 1. The DOE/OMB agreement of 1978, which is equivalent to a DOE level of $423M in FY 1982 dollars.
> 2. A level of 10 percent lower.
> 3. A level of 10 percent higher.
>
> ...submit an interim report which evaluates the priority of ISABELLE in each of the three constraint cases, taking into consideration the physics potential of the facility, the funding and schedule required to complete construction, and competing needs and opportunities...."

The deliberations of the Trilling Subpanel took up a large part of 1981. Even though the Subpanel members do not represent their regions or their home laboratories, it is difficult in such deliberations to completely divest oneself of one's more parochial interests, whether they be connected with one's scientific area of interest or with the laboratories hosting one's experimental program. Thus it is not surprising that there would be a strong difference of opinion on the most controversial issue, i.e. ISABELLE. The final report represented a carefully worded compromise; it was not finalized until January 1982.

In retrospect, three Subpanel recommendations and suggestions [11] are especially important in their relevance to the topic of this article:

(1) Regarding ISABELLE, the Subpanel concluded that in the middle level scenario "construction of this scientifically valuable facility would have to be abandoned in its present scope."
(2) In the case of termination of the ISABELLE project, the report gave a recommendation: "Start by the mid-1980's on a new high energy construction project of more modest financial scope." Some possible projects were mentioned in the report.
(3) The Subpanel also addressed the issue of long range planning within HEP, pointing out that some felt disfranchised in this currently HEPAP-dominated process. The Subpanel suggested that it would be appropriate if "DPF (Division of Particles and Fields) on its own made a larger effort in the planning area. This effort might involve such activities as sponsorship of appropriate workshops...."

This last recommendation, made without much discussion or controversy, had the most immediate impact. Charles Baltay, a Physics professor at Yale and an incoming (January 1982) chair of the Division of Particles and Fields (DPF) of the American Physicsal Society (APS), took this message to heart and got the DPF to organize (and funding agencies to sponsor enthusiastically) a planning workshop in the summer of 1982 in Snowmass, Colorado. The previous HEP workshops tended to address physics at a specific facility and thus were sponsored by single laboratories. This one was organized quite differently: different physics topics, deemed as worthy of investigation, were examined from the point of view of how effective different facilities would be in addressing them. This extremely useful exercise provided a yardstick by which different potential facilities could be measured. A possible downside was that some would accept the results unequivocally, forgetting that a lot of these conclusions were highly model-dependent.

A multi-TeV machine was a topic of two previous workshops in the late 1970's, sponsored by the International Committee on Future Accelerators (ICFA) [12, 13]. ICFA was chartered by the International Union for Pure and Applied Physics (IUPAP) with a charge to organize workshops, exchange information and hold joint studies on a Very Big Accelerator (VBA). The VBA was conceived in the 1950's not only as an ambitious future facility requiring global participation in construction but also as a means of bringing together scientists from different nations to work jointly on a scientific project and thus further the cause of peace [14]. But little concrete progress was made, and even at the time of the ICFA workshops the theoretical motivation for the needed energy was not well established and the required technology was still far from proven. The Snowmass '82 workshop was thus the first one where a collider in the 20 TeV energy/beam range was discussed in some detail [15] and with the hope that such a facility might be feasible. From the theoretical point of view, this was the machine called for. The Standard Model of particle physics was getting established as an excellent paradigm for explaining phenomena in the sub-100 GeV energy region. But the model was incomplete and there were strong theoretical arguments that new phenomena must occur in the TeV or below constituent energy range.

From the experimental point of view, a machine probing this energy would represent a huge leap forward, and addressing those challenges was the main focus of the accelerator working group at the workshop. Several ideas were put forth as to how this energy regime could be attained. They spanned semi-conventional approaches based on high field magnets ($\sim$10 T) to (hopefully) lower cost approaches based on lower field ferromagnets with superconducting coils. Maury Tigner, who was the coordinator of this group, summarized the outcome of the discussions [16]: "...we were able to give some hope to the idea that a well designed and concentrated R/D program, elaborating much further on technologies we now possess, might bring a 20 TeV facility within our national reach." He highlighted three central challenges for this R&D program:

- Achievement of virtual automation of superconducting magnets, accelerator housing and other accelerator component manufacture and installation;
- Achievement of a thorough understanding of the field–cost relation for superconducting magnets;
- Achievement of a thorough understanding of the luminosity–aperture–energy relation.

The outcome of the Snowmass '82 workshop was a strong desire and enthusiasm for exploring the multi-TeV region in the next generation of hadron colliders. Many individuals became strong proponents for such an action; Leon Lederman was one of the strongest and most vocal advocates of building a multi-TeV hadron collider and at the same time terminating the ISABELLE construction.

3. Formation of a New Subpanel

Several disparate factors generated the driving force for another subpanel, not even a year after the previous subpanel's report was finalized. There was an awakening enthusiasm for a bold new initiative, lingering unease about what to do about ISABELLE (renamed now the Colliding Beam Accelerator, CBA) and fear that US HEP was losing its pre-eminence. Keyworth shared all of these sentiments and was probably quite influential in creating this subpanel, hoping that it would suggest killing CBA and starting on a multi-TeV collider. The charge of the Subpanel [17] verbatim, was:

> "Consider and make recommendations relative to the scientific requirements and opportunities for a forefront United States High Energy Physics Program in the next five to ten years. Make specific recommendations with respect to possible new construction items for FY1985. In its deliberations the subpanel should consider opportunities that may be available later as it weighs its specific recommendations. The report of the Subpanel should include a definite recommendation concerning the proposed Colliding Beam Accelerator at Brookhaven National Laboratory. The Subpanel should estimate the program funding required to realize the scientific opportunities associated with the recommendations, and should give the relative priorities of the actions it recommends."

This charge differed dramatically from those given to the previous subpanels. There was no funding constraint within which the recommendations had to fit; rather, the Subpanel was being invited to specify the funds needed to restore a forefront US program. The reference to "possible construction items" was also rather open-ended; traditionally subpanels reacted to proposals from the laboratories rather than initiating something on their own. Finally, one might contrast the word "definite" in front of CBA recommendation with "...evaluates the priority of ISABELLE...." in the 1981 charge.

I was recruited to be the chair of this Subpanel in the fall of 1982, via a phone call from Bill Wallenmeyer, Director of the Office of High Energy Physics in DOE. I was a somewhat curious choice for this role. I had never been a member of HEPAP or of any of its subpanels. But I had been involved quite extensively in various committees dealing with science policy decisions and specific US HEP programs: I had served as a chair of both the Fermilab and SLAC Program Advisory Committees, chaired one of the subgroups of the recently concluded DOE comprehensive review of the US HEP university program

(TACUP), and was a member of a recent committee to review the *raison d'être* of both the LBL and Argonne HEP programs. There was probably a desire to have someone in that chair position young enough to feel as if he or she had a stake in the outcome of the process and in the future of the US HEP program.

The Subpanel had 17 members, and its membership represented an effort to include all segments of the US HEP community [18]. There was one non-HEP member, Arthur Kerman, a respected nuclear physicist from MIT. There were two Europeans on the Subpanel. One was John Adams, a well-known British accelerator physicist and a former CERN (DG). The second was Carlo Rubbia, the leader of the UA1 experiment at CERN, probing for the first time high energy p-pbar collisions with the goal of discovering the predicted carriers of the weak force, charged W and neutral Z bosons. At that time Rubbia had a joint appointment at CERN and on the Harvard faculty. Jack Sandweiss, as the HEPAP chair, attended all the Subpanel meetings. An important and intentional difference in the composition of this Subpanel was that there were no members from the DOE proton accelerator laboratories. The purpose was to exclude people with potential conflicts; the drawback was exclusion of some expertise on accelerator matters.

The schedule outlined for the Subpanel's work was very tight. After an organizational meeting in Washington on February 25/26, 1983, there were to be three meetings at Fermilab, BNL and SLAC, with a final meeting at Woods Hole, Massachusetts, on June 5–11, 1983, to write the report. There was a full HEPAP meeting scheduled for June 29/30 to discuss this report. As usual, the timeline was driven by the US budgetary schedule.

From the very beginning it was clear what the main issues were going to be [19]:

(1) Does one press for a bold new initiative?
(2) What should be the recommendation on the CBA?
(3) How does an intermediate construction project (*à la* the CBA) interact with the potential new initiative? Is it needed for the health and vitality of the US HEP program?

Thus the deliberations of the Subpanel focused to a large extent on these three issues.

4. New Subpanel Activities and Related Events

The first, organizational, meeting in Washington created the structure for the Subpanel's work and included presentations from Alvin Trivelpiece (Director of DOE Office of Energy Research), Keyworth and Marcel Bardon (Head of the Physics Division at NSF). The tenor of those presentations can be summarized in a few lines taken from my summary of that meeting written for the record and for the benefit of the members unable to attend [20]: "...the message conveyed was quite clear and unequivocal: the Panel should ignore any political, geographical or institutional issues and concentrate on making a recommendation based on objective scientific issues. In addition, the budgetary questions should not drive our recommendations; we should concentrate on what is needed to reestablish American leadership and preëminence in the field during the next decade.... Dr. Keyworth emphasized that he will support good, sound scientific judgement that is presented in a well-reasoned way...on the specific issue of CBA, it was stressed that what is needed is a firm unequivocal decision, arrived at on purely scientific grounds...we should not shy away from 'thinking big' when writing a recommendation."

I divided the Subpanel membership into three task forces, each charged with acquiring and evaluating information on a specific topic relevant to the overall charge of the committee. However, putting all this information together and arriving at recommendations would be the responsibility of the whole Subpanel. The three task forces and their chairs were: Accelerator Technical Task Force (Maury Tigner, professor at Cornell, chair), CBA Physics Task Force (H. H. Williams, professor at University of Pennsylvania, chair), Overall Program Task Force (C. Baltay, chair). "CBA Physics" was somewhat of a misnomer, since that group was not only to look at CBA physics but also to compare its potential with that of other possible intermediate time scale machines. The Program Task Force was to estimate future trends in the field and evaluate risks, benefits and costs of possible scenarios. It was also charged with appraising HEP plans abroad and evaluating US HEP manpower data.

It was important to get input from all members of the community. I later wrote a letter [21] to all DPF members describing the charge and our plans and inviting them to express their opinions by writing to the Subpanel. The four lab directors (Leon Lederman, Fermilab; W. K. H. Panofsky, SLAC; Nick Samios, BNL; and Boyce McDaniel, Cornell) were invited to make presentations to the Subpanel describing programs at their laboratories and giving their vision of the future. They were evenly split on the CBA, with Panofsky and Samios advocating its continuation. Former and current HEPAP chairs (Weisskopf; Sid Drell, Deputy Director at SLAC; and Sandweiss) were also invited to present their views at subsequent meetings. I was charged also with writing to John Adams (absent from this first meeting) and asking him to prepare a document outlining the European plans for the next 5–15 years.

The general format of the laboratory meetings — Fermilab, Brookhaven and SLAC — was similar but with somewhat different emphasis at each lab. Naturally, at Brookhaven the focus was on the CBA — its physics reach, relevant technical issues, and costs. The meeting at SLAC was the last laboratory meeting, so considerable time was spent on discussing the issues stated in the charge. At the evening town meeting there was an opportunity for Q&A between the audience and the Subpanel members.

Several new ideas and concepts were put forward during the Subpanel's deliberations. Fermilab proposed construction of a dedicated collider (DC) — a 2 TeV per beam p-pbar collider based on Tevatron-style magnets and using proven technology [22]. It would have better reach than the CBA in a number of different areas. BNL put forward a new cost estimate for completion of the CBA, somewhat lower than the previous ones: US$218 million with the first colliding beams in October 1987. They also suggested that the CBA machine could be a stepping stone to a high energy collider in the 10–30 TeV range to be built at or contiguous to BNL — a "Sandatron" — using the CBA as an injector [23]. SLAC presented the possibility of an e^+e^- linear collider in the 1 TeV range [24], but it was clear that such a machine required considerably more R&D than a high energy pp machine.

A number of relevant and important events took place during the first half of 1983, in parallel with the Subpanel's work. The most important ones were:

(1) DPF workshop at Berkeley (February 28–4 March) on detector issues in a high radiation environment, which would be expected either at the CBA or at a multi-TeV hadron collider.
(2) DOE review of the CBA technology and costs. On the whole it was quite positive; the magnet problems appeared to have been solved, mainly through adoption of Fermilab Rutherford cable, with Bob Palmer playing a key role here; the cost and schedule projections were reasonable. The new management appeared to have the situation well in hand.
(3) Workshop on R&D for a 20 TeV Collider, held at Cornell form March 28 to April 2. Organized by Maury Tigner, it had as its goal the identification of the most important R&D issues relevant to the construction of a 20 TeV collider [25]. A rough construction cost estimate was also made for such a machine — US$1.72 billion. This cost, however, included no R&D, no preoperation, no detectors, no contingency and no escalation.
(4) Discovery of the W and Z bosons [26–29] at CERN in 1983.

The Subpanel received over 200 letters from the community addressing the issues spelled out in the Subpanel's charge. Almost without exception, they supported the idea of building a high energy collider; many of them felt that was essential. A flavor of these opinions is well represented by the following quote from one of the letters: "We should place as the highest priority of the American program the design and construction of a super high energy (>20 TeV) hadron collider."

The comments regarding the CBA varied, however. The majority of the letters were against it; some, mainly from the traditional BNL users, argued for its construction. The two quotes below, one pro and one con, are representative of these two sets of opinions:

> "I believe the CBA should be built and be ready for experiments before the end of 1987. The physics potential justifies it. . . ."

> "The decision to build an intermediate energy machine, with the concomitant delay in acquiring a truly high energy collider, is a decision to remain second to European physics for all or most of the rest of our careers."

The possibility of international collaboration on the high energy collider did not play a prominent role in the Subpanel's deliberations. Up to that time collaboration on an international scale in the exploitation of facilities was the accepted norm, with the accelerator construction done on a national or regional basis. Even LEP, by far the largest accelerator facility planned up to that time and potentially providing a tunnel for a VBA-like machine, was always seen as an exclusively European construction project.

The Subpanel was well aware of the situation abroad and the plans for accelerator facilities in other regions. It was partly their ambitious plans (like LEP) that made many believe that the US must have its own frontier energy facility. Sir John Adams wrote a very thoughtful memo describing his analysis of the pros and cons of going ahead with the CBA and/or a large hadron collider from the point of view of its impact on the international science [30]. He pointed out that the proposed SSC would be a significant perturbation on the currently planned program on the worldwide scene. I want to quote two excerpts from this memo on issues that turned out to be quite prophetic and are also very relevant today for the ILC. The first one dealt with the cost estimates: "I note with dismay the tendency which has grown up in recent years to err on the side of optimism. This daring approach can have unfortunate consequences which put at risk the future of large physics communities if the optimum turns out to be unrealistic... governments lose confidence in the competence of the planners and their laboratories." And on the long term planning: "... predicting government reactions several years ahead is an uncertain art particularly since it may be another government in power by then which is not usually committed to the promises of its predecessor."

The perceived need to restore US eminence in HEP through the construction of a frontier collider facility dominated the Subpanel's attitudes toward all the issues in its charge. It also dominated some influential public opinion, as indicated by an editorial at that time in *The New York Times* [31], titled "Europe 3, US not even Z-Zero." I quote several characteristic sentences from that editorial: "The bad news is that Europeans have taken the lead in the race to discover the ultimate building blocks of matter.... European accelerators have established a better record of success than any of the three American laboratories... American accelerators should be designed to win or not be built at all.... The 3–0 loss in the boson race cries out for earnest revenge. The physics team needs to try harder, and coach Keyworth should reward any sensible new strategy with management's full support."

This sentiment came through quite clearly in a meeting I had with Keyworth in May, arranged at the suggestion of Jim Leiss, Associate Director of the DOE Division of High Energy and Nuclear Physics. The meeting was also attended by Doug Pewitt and Al Trivelpiece. Quoting from the notes I wrote right after the meeting: "GK stressed the political difficulties in getting a new project approved... he said that the key word is *competition*, as the thing that the Congress will understand. Hence a project that will put us ahead (e.g. "desertron") will stand a better chance of being approved than CBA, which will only make us equal.... He is sure that a lot of people in Congress would rather spend that money on things like biomedical technology." Keyworth was clearly enamored with the possibility of a new multi-TeV collider. He offered to fly to Woods Hole for the Subpanel's last meeting but I managed to dissuade him from it, arguing that it would prejudice the integrity of Subpanel deliberations.

Before that meeting I spent about half an hour alone with Pewitt discussing the funding prospects. Quoting again from my notes: "DP stressed the political realities today. He said I should be aware that nobody in Washington can really 'deliver' a 3-billion dollar package." I mention this because some members of the HEP community (as well as some outside it) rather naively thought that all one had to do was to convince Keyworth about the value of the project. It is my opinion, however, that the Subpanel members were not influenced (at least not significantly) by such considerations and evaluated the issues on the grounds of physics potential and technical feasibility.

The Subpanel met at Woods Hole on June 5–11 for what was supposed to be the final meeting. However, it became clear, rather early, that the process still had not converged sufficiently. The Subpanel had already agreed in its earlier meetings to recommend initiation of a vigorous R&D effort on the high energy collider (in the 10–20 TeV per beam energy range), with a goal of being able to proceed to construction as soon as possible. It also agreed that in spite of its significant scientific potential, the DC proposed by Fermilab would be too much of a distraction both to the Tevatron effort and to the recommended R&D program on the multi-TeV collider. But there was a pronounced division on the CBA issue, mainly regarding the impact of the CBA on the multi-TeV collider program. The CBA straw votes oscillated between 10 to 7 and 9 to 8 against continuation. There were only two or three members who still appeared somewhat flexible on this issue.

Several of us were very concerned about a potential serious rift in the community, regardless of which recommendation the Subpanel adopted. To forestall this, it was suggested (I recall by Dave Jackson) to invite the four lab directors to Woods Hole, brief them on the current status of deliberations, and make a plea for their active help in keeping the community together after the report was made public. We did this, meeting with each director individually, and it probably did help at some level. One very important point (not discussed to any significant extent previously) was made by Panofsky, who expressed concern that the rest of the scientific community might react very negatively to an expensive new HEP initiative and that one had to do whatever possible to forestall that.

We had to schedule another meeting to resolve the CBA situation and finish the Subpanel's work. It turned out to be possible to delay the HEPAP meeting until early July and find several contiguous days beforehand that would suit the great majority of the Subpanel members. The final meeting of the 1983 Subpanel was scheduled for June 30–July 2 at Nevis Laboratories of Columbia University.

5. The Subpanel's Recommendation

An initial straw vote on the CBA at Nevis showed no significant change of opinion among the Subpanel members. The decision was made to report a negative recommendation and state that it was decided by a small majority (10 to 7 against on a final vote). It was also decided to state that in spite of an agreement on most of the facts regarding the CBA, different Subpanel members drew different conclusions from these facts. These conclusions, essentially arguments pro and con the CBA, were to be included in the report.

The rest of the meeting was devoted to writing the report, focusing especially on potentially politically sensitive comments about the CBA. There was some discussion of the name for the proposed new collider. The adopted name, Superconducting Super Collider (SSC), was proposed by David Jackson.

There were five recommendations [32] in the report: on the SSC, on the CBA, on the DC, on the ongoing program, and on the need for support of technology research and development. The main ones, on the SSC and the CBA, stated:

> "The Subpanel unanimously recommends the immediate initiation of a multi-TeV high luminosity proton–proton collider project with the goal of physics experiments at this facility at the earliest possible date"; and
>
> "By a majority vote, the Subpanel recommends that the Colliding Beam Accelerator (CBA) project at Brookhaven not be approved."

Immediately after the CBA decision was made, I had to call Nick Samios and inform him of it. It was not a pleasant task. Nick appeared stunned by the decision. Since the report was still being drafted I did not elaborate to any great extent on that or any other recommendation but did say that the decision on the CBA was final.

The discussion regarding international collaboration on the SSC in the Subpanel's deliberations was rather limited. The need to restore energy frontier research in the US was viewed as one of the strong arguments for the SSC. The parameters of the SSC were chosen so that it would complement the existing and proposed facilities around the world. It was felt that the energy chosen had to be significantly higher than what could be achieved in a potential hadron collider in the LEP tunnel at CERN. The report strongly endorsed the customary international collaboration in HEP experiments, ending with the following statement: "... the U.S. should welcome foreign involvement in exploitation of new U.S. facilities, in either a general way or for a specific capability."

Toward the end of the meeting, the two European members of the Subpanel, John Adams and Carlo Rubbia, asked that the report state that they dissociate themselves from the statements about restoration of American pre-eminence in the field. A statement to that effect was included in the report [33].

We were able to achieve a high level of confidentiality about the result of the deliberations, at least for a few days afterward [34]. *The New York Times* published an article on July 2, 1983, under the byline of Walter Sullivan, titled "Panel of experts deadlocked on next U.S. atom smashers," which stated that the panel "could not agree on what to recommend" and that its "noncommittal report" would be discussed by full HEPAP on July 11.

One important event occurred before that forthcoming HEPAP meeting. It was announced by Leon Lederman in a cable [35] to the physics community. The cable stated: "At 3:27 AM today, July 3, we successfully accelerated protons to an energy of 512 billion electron volts in the Fermilab superconducting energy saver." This was a new record for man-made acceleration of elementary particles and provided an important boost to the credibility of the Subpanel report regarding the technical feasibility of the SSC.

At the HEPAP meeting the discussion of the Subpanel report was started with my presentation as the Subpanel chair. As expected, there was some concern about the risk of going "for broke" with the SSC. Bob Palmer of Brookhaven, a HEPAP member, referring to the projected SSC costs, argued: "That is a very optimistic number. And it's not promising that Congress will appropriate it. I am appalled at the kind of risk you're recommending." During the following floor comment period, several prominent physicists — Nick Samios, Sam Ting, T. D. Lee and Bob Marshak — expressed their dismay at the recommended cancelation of the CBA, which they saw as a facility with very promising physics prospects. In the end, however, the argument for a united front and aggressive pursuit of the energy frontier prevailed. All HEPAP members voted in favor of acceptance of the Subpanel report with all of its recommendations [36].

With the benefit of hindsight one can speculate about the scientific impact that the CBA would have had if it had been completed. My opinion is that its exploitation would have been much more difficult than anticipated. The experience with detectors from both the Tevatron and the LHC suggests that the technology available in the 1980's and early 1990's would not have been able to handle the luminosities required for frontier investigations. On the other hand, cancelation of the CBA, a half-completed project with no apparent technical difficulties, established a very bad precedent and raised the bar for the level of readiness that would be required in the future for project construction approval.

On July 12, 1983, Jack Sandweiss, as HEPAP chair, formally submitted the Subpanel report to Dr. Alvin Trivelpiece, stating that HEPAP unanimously endorsed all of the Subpanel's recommendations. The concern that construction of the SSC might negatively impact federal support of other scientific research was addressed in the transmittal letter [37] as follows: "HEPAP realizes that this project demands effort and resources unprecedented for a single tool in basic research. It would constitute a new national commitment that should signal the intention of the U.S. to strengthen its support of basic science in all its aspects. As a community of high energy physicists, we would not wish this project to have a negative impact on support of other branches of science." In light of future developments, one cannot overemphasize this last point. The SSC would be a project an order of magnitude greater in scope and cost than the others put up for consideration (CBA, DC). The construction price tag was about ten times the annual support level of HEP and hence would require doubling the HEP budget during SSC construction. Thus it was likely that scientists in other fields might be concerned that it would have a negative impact on funding levels in their fields.

The letter also endorsed the steps that the Subpanel identified as being required in the next stage: "It is clear that a first phase of project planning, cost definition and R&D, followed by a second phase of construction, must be a broad-based national effort, centrally managed from the outset. HEPAP concurs with the Subpanel belief that a successful approach will call for an innovative collaboration between the universities, the national laboratories, and industry with the Government. HEPAP recommends that steps to define this process be initiated immediately."

6. Pre–Central Design Group Events

The US HEP community had unequivocally spoken in favor of the SSC. What remained was to make it a reality. One view of the enormity of this challenge was summarized in this paragraph of a July 1983 article in *Science* [38]: "The community is gambling its future on a program for which there is no explicit proposal, no design, no site, no research and development plan, no management plan, no management team, no director, no budget, and no guarantee of long-term federal support. Moreover, the supercollider is an enormous extrapolation from current experience." From an interview for that article I was quoted: "The course is bold, risky, and perhaps foolhardy."

Doug Pewitt, speaking at BNL, cautioned the HEP community not to underestimate the challenge [39]: "A machine of the order of 10–20 TeV challenges us on multiple fronts — technically, politically, and financially... any commitment to proceed with a next-generation accelerator will require that all those factions — the Executive Branch, Congress, the physics community, the science community in general — agree on its importance... such an initiative is possible only with greater national commitment of resources to basic research... the path through Congress can hardly be smooth."

In spite of these great challenges (or maybe because of them) things moved surprisingly quickly after the HEPAP recommendations. Already on August 11, 1983, Al Trivelpiece, in a letter to Jack Sandweiss [40], requested additional input from HEPAP, specifically "advice and recommendations on the content and implementation of the FY1984 R&D effort preliminary to the Superconducting Super Collider." The mechanism was to be a new "HEPAP Subpanel to consider and provide advice on these matters."

The Subpanel was formed within days, with Panofsky as its chair. There was no formal charge but the guidelines presented to the Subpanel by the Chair included recommending allocation of funds (to become available for the SSC as a result of CBA termination) to institutions interested in SSC R&D and providing a consultative process among the participants. The Subpanel held its only meeting on September 7–9, 1983, where it heard presentations from various groups proposing a total program of US$28.3 million. The Subpanel made an allocation recommendation [41] to DOE and strongly recommended that "an interim line manager be selected on as rapid a time scale as possible."

The preliminary allocation for FY84 to BNL for the CBA work was US$23 million. To initiate SSC work on a fast track, DOE wanted to reprogram US$18 million of these funds to the SSC R&D work. This required agreement from the chairs of the four congressional committees (appropriations and authorization, House and Senate) dealing with science research. At the request of Rep. William Carney, in whose district BNL was located and who not surprisingly was very unhappy with the suggested CBA termination, the House Science and Technology Committee held a hearing on October 19 on the HEPAP recommendations and DOE's plans for their implementation. DOE plans were announced in letters from DOE Secretary Donald Hodel, sent to the four committee chairs on October 18. Al Trivelpiece, Jack Sandweiss and I were asked to testify at that hearing, via both written testimony prepared beforehand and discussion during the hearing [42].

The hearing started with Rep. Carney showing the CBA construction already completed, emphasizing the large scale of the project. He stated that the appropriations for the CBA by Congress to date were largely based on previous HEPAP recommendations, in 1974, 1975 and 1977: "We have gone along with it because you asked us to." But now that the high energy community has reversed itself in an unprecedented way, "How can I get a consensus from American taxpayers that this is a prudent step?" The questions afterward, however, were not hostile and were focused more on the feasibility of the SSC within a reasonable cost envelope, rather than on the recommended termination of the CBA. We tried to make the point that one cannot extrapolate the SSC cost from the Tevatron or CBA magnets since an optimized design and additional R&D would reduce the cost significantly. Following the HEPAP recommendation a reasonably strong consensus had developed that the CBA could not be revived, at least not as a proton–proton collider, and hence that issue was pretty much moot; the important question was whether one should embark on SSC R&D. After the hearing, I was told by a staff member that it had gone well; this was confirmed in a letter to Jack and me from Trivelpiece a few days later, thanking us for our efforts. Finally, the few written "softball" questions I received from the committee afterward confirmed that general impression. The initial congressional hurdle appeared to have been passed over successfully [43].

To obtain a more reliable estimate of the SSC cost, the laboratory directors chartered in January 1984 a Reference Design Study, sponsored jointly with URA and DOE. This study, led by Maury Tigner and held at Lawrence Berkeley Laboratory (LBL), brought together some 160 experts in the field of accelerator physics. The goal was a site-independent review of the technical and economic feasibility of the SSC. It used three different superconducting magnet designs, with different magnetic field strengths. The study, completed in May 1984, concluded that no major obstacles existed; cost estimates for a 20 on 20 TeV collider ranged from US$2.7 to 3.05 billion, depending on the type of magnet chosen. This estimate was in 1983 dollars and was for the collider complex alone [44].

The community's interest in the SSC was demonstrated by a number of workshops relevant to the SSC design, construction and utilization. The major ones were at the University of Michigan on accelerator physics issues [45], at BNL on cryogenic systems for the SSC, at Texas Accelerator Center on the fixed target option, on the p-bar p option [46] for the SSC (jointly organized by Argonne and the University of Chicago), and at LBL on electroweak symmetry breaking. In parallel there was a series of meetings examining principal issues connected with doing experiments at the SSC — called Physics at the SSC (PSSC) [47]. The culminating effort of all these activities was a DPF-sponsored workshop on the design and utilization of the Superconducting Super Collider, held from June 23 to July 13, 1984 in Snowmass, Colorado [48].

There was a very important theoretical paper [49] by E. Eichten, I. Hinchliffe, K. Lane and C. Quigg, "Supercollider Physics," published in *Reviews of Modern Physics* in 1984. This work used parton distribution functions to calculate cross sections at different collider energies for various processes of interest. It provided firm phenomenological footing for the next generation of calculations and rationalized the argument for a high luminosity, multi-TeV collider.

A top priority task was establishment of the management structure for the SSC activities. A real concern was that if one started this process from scratch with a new legal entity, much time would be lost. The optimum seemed to be for the Universities Research Association (URA)[b] to perform this management task as an addendum to its contract. The Executive Committee of the Council of Presidents urged Guyford Stever, President of URA, to submit a proposal to DOE for organizing the SSC R&D effort. A letter to that effect was sent [50] by Stever to DOE Secretary Donald Hodel on July 14, 1983. Subsequently a Memorandum of Understanding (MOU) was drafted under URA auspices and signed by seven entities with heavy involvement in HEP and with planned involvement in the SSC.[c] It stated: "The Signatories of this Memorandum, all having serious commitments to the future of high energy physics, agree to provide a climate for development of an effective organization designed to operate and manage the R&D and conceptual design of the SSC project by the DOE."

A Board of Overseers (BOO) for SSC R&D and conceptual design would be created which would have "principal responsibility and authority for management oversight of the R&D and conceptual design phases as described by the DOE." This arrangement was designed to avoid any potential conflict of interest between Fermilab and the SSC effort. The Board was to be composed of "eleven members chosen from the community with significant interests in the SSC program." It was to "reflect the legitimate interests in this program on the part of universities, national laboratories, industries, and government." The initial membership[d] included seven members affiliated with the seven signatories of the MOU and four additional members from outside HEP, mainly industry with potential SSC interest. At the Board's first meeting on March 23, 1984, Boyce McDaniel was elected as the chair. Also at that meeting a subcommittee was formed (Bjorken (chair), Cronin and Trilling), charged with addressing the issue of the selection of the Director of the R&D effort [51]. Based on this arrangement, in March 1984, DOE appointed the URA to manage the R&D and design studies for the SSC (Phase 1 of the SSC).

The Director's search committee reported on its progress at the next three meetings. A number of

[b]URA was started in 1965 as a consortium of 25 US research universities to provide a nationwide management organization for Fermilab. By 1983 it had grown to 54 universities, including some from abroad.

[c]The signatory institutions were: Associated Universities, Inc (operator of BNL), Cornell University, Stanford University (operator of SLAC), Texas University Consortium (operator of Texas Accelerator Center, TAC), URA (operator of Fermilab), University of California (operator of LBL) and University of Chicago (operator of Argonne National Laboratory).

[d]The initial members were: J. D. Bjorken (Fermilab), J. Cronin (University of Chicago), J. Deutsch (MIT), H. Furth (Princeton), J. Hulm (Westinghouse), B. McDaniel (Cornell), G. Pake (Xerox), S. Treiman (Princeton), G. Trilling (LBL), S. Weinberg (University of Texas, Austin) and S. Wojcicki (Stanford).

outside individuals were consulted and DOE input was obtained on the top candidates. Based on these reports and extensive discussions within the Board, the Board decided, at its fourth meeting on May 12, 1984, to offer the position of the Director of the Central Design Group to Maury Tigner, and he accepted [52]. The CDG was to be the national organization charged with coordinating the SSC R&D work and the related activities. I was asked by the Board and Maury to assume the position of the Deputy Director, and I agreed to do so.

Before the CDG could get to work, it needed a home. Proposals were solicited for offers to host the group. Nine different proposals were received, including ones from the major accelerator laboratories. The choice was up to Tigner, who recommended LBL as the one which offered the most advantages and the fewest negatives [53]. LBL had a strong accelerator physics group and several of its members could be counted on to contribute to the CDG work. It had a strong technical group with involvement in superconducting magnets focused mainly on the task of cable development. It had an intellectually stimulating atmosphere and was an attractive place, thus facilitating recruitment. The experience of carrying out the Reference Design work there was quite positive. The laboratory and its Director, Dave Shirley, were eager to host the CDG and were willing to provide the necessary space (including 20 parking spots). Even though CDG home would be at LBL, it would be an independent entity, reporting to the Board of Overseers rather than the LBL Director. Both the Board and DOE approved this choice.

During May 8–11, 1984, DOE performed an in depth review of the Reference Design Study [54]. This review supported the cost estimates given in that document. Based on it, DOE Secretary Don Hodel authorized the release of US$20 million for the SSC R&D [55]. The work of the Central Design Group was officially launched.

7. Central Design Group (CDG) Overview

The CDG was to oversee and coordinate all the SSC activities and control the SSC R&D budget until SSC Laboratory's management structure was established. It would perform some of the tasks in-house and coordinate and direct the R&D efforts at national laboratories and universities. Its work could be grouped into several categories:

- R&D and design in technical areas. The main focus was the magnet development but equally important were efforts in other technical systems and in accelerator physics.
- Conventional systems work. The principal activity was definition of required facilities for the future accelerator laboratory and preparation of the site requirement document for the anticipated site competition.
- Interactions with the government agencies, both the executive and the legislative branch. This involved budgetary issues, reviews, preparation of required documents, congressional testimonies and personal interactions.
- Interactions with the general public and the high energy community (both in the US and abroad).

The starting point for CDG's work was definition of the principal design parameters for the SSC. As discussed earlier, there was a strong conviction in the US HEP community, which crystallized at the Snowmass '82 workshop, that the main motivation for this facility had to be exploration of the physics beyond the Standard Model. The scale of electroweak symmetry breaking indicated the need to probe the 1 TeV energy scale at the constituent level. This argument motivated the 1983 HEPAP recommendation of a multi-TeV hadron–hadron collider. The paper by Eichten *et al.* [49] provided tools for quantitative estimates of different processes as a function of energy. The consensus opinion in the community was coalescing around 20 TeV as the energy in each beam and a luminosity of 10^{33} cm^{-2} s^{-1}. There was, however, no clear threshold that had to be attained. The high luminosity required pretty much excluded an antiproton–proton option.

Tigner and I drew up a formal proposal for primary physics parameters of the SSC, which was presented to the BOO for their approval [56]. The suggested parameters were:

- Proton–proton collider
- 20 TeV in each beam
- Luminosity of 10^{33} cm^{-2} s^{-1}
- six experimental areas

The Board approved these goals but also added the following statement: "The CDG will continue to perform the studies. . . which examine tradeoffs between primary physics design goals (energy, luminosity, and number of experimental areas) and total project costs. . . ." This statement is worth noting in light of the subsequent debate in 1989 about possible energy reduction to limit the cost increase.

The CDG was going to be led by the Director, Maury Tigner, who had two Deputy Directors — Dave Jackson, responsible for operations, and myself, responsible for external relations. Bob Matyas from Cornell served as Management Advisor to the Director. There was an External Advisory Group, headed by Paul Reardon from BNL. The CDG activities were divided into several departments, headed respectively by:

- Accelerator Physics — Alex Chao, SLAC
- Superconducting Magnets — Victor N. Karpenko, formerly from Livermore Lab
- Accelerator Systems — Peter Limon, Fermilab
- Injector, Steve Holmes — Fermilab (somewhat later)
- Conventional Systems — Jim Sanford, BNL
- Management and Administration — Tom Elioff, LBL
- Detector R&D unfilled initially

The Directorate members and the Department Heads formed the SSC Steering Committee, which was to have (as charged by Tigner) the responsibility to "sell" the SSC and make it successful. The first meeting was on October 10, 1984. About a year later, the group was enlarged and renamed the CDG Coordinating Committee, providing a forum for discussion of and decisions on various CDG-overviewed activities.

An early task undertaken by the CDG was the preparation of the Phase I (R&D and design) schedule and milestones. This was based on rather optimistic budgets, doubling each successive year. The most important early milestones were preparation of Site Parameters Document (April 1985), Magnet Design Type Selection (October 1985), Conceptual Design Report, CDR (March 1986) and Initiation of the Magnet Systems Test (October 1986) [57]. The hoped for construction start was to be in October 1987.

The CDG started its activities at LBL in October 1984 with only a few members — its telephone list dated October 22, 1984 had seven scientists and two supporting staff members. The CDG existed as a separate entity for over four years, until the end of 1988, when SSC laboratory was founded and the CDG became for about a year one of SSCL's Divisions. The amount of work that was accomplished during the CDG's existence is truly impressive. There is no doubt that Tigner deserves the lion's share of the credit for its achievements. He set the tone for the organization, provided inspired leadership, and set an example by working long days, frequently seven days a week. CDG goals were achieved with a minimum amount of bureaucracy and in a stimulating intellectual atmosphere, fueled and supported by the weekly "Hadrons and Cheese" Friday seminars. In this brief summary, one cannot possibly do justice to all the work that was done, and in the following sections I will only be able to briefly describe the CDG's most important activities and accomplishments.

8. Magnet Design and Other Technical Activities

The superconducting magnets were going to be the heart of the SSC; they were the largest and most expensive subsystem of the collider and also the most challenging. Their design determined the other parameters of the machine, in both the conventional and the technical areas. Thus the main priority among the CDG activities was the R&D on the magnets and selection of the design to be used. This was a very intense effort, involving a large number of institutions. Remarkable progress was made in that area during the lifetime of the CDG; I limit my discussion to a few highlights.

When CDG activities started, superconducting magnet technology could be characterized as becoming mature [58, 59]. It had been over a year since protons were first accelerated to 512 GeV in the Energy Saver and the Tevatron was slated to run soon at 800 GeV in the fixed target mode and 900 GeV in the collider mode. BNL solved its magnet problems and had a successful systems test. DESY Laboratory in Hamburg, Germany, was about to start construction of a new electron–proton collider, HERA, with the proton ring based on superconducting magnet technology, with magnets built by industry. It was clear that the next generation high energy hadron colliders would be based on superconducting magnets but their optimum design was uncertain. The goal of CDG magnet work was to improve the technology as much as possible and define the design.

In 1984 this technology was based on the Rutherford cable utilizing niobium–titanium (NbTi) filaments embedded in a copper matrix. The shape of the field was determined by placement of the coils made with this cable; the structure of the positioned coils gave the name "cos θ" to these magnets. Such magnets were the basis of both the Tevatron / Energy Saver and HERA, and they were designed for a B field around 4.5 T. Two other general concepts were also being put forward. One was a superferric magnet; most of the field would be generated by the iron magnetized by a superconducting coil [60]. The field would be only 2–3 T but less conductor would be required. The other concept was based on a higher field magnet using a niobium–tin superconductor. But this design was still in the development stage and was viewed as unrealistic for the SSC.

By 1984 the only viable options appeared to be superferric and cos θ NbTi magnets. But even here there were distinct options. In the Tevatron magnets, the iron surrounding the coil was at room temperature; in the HERA design it was held at liquid helium temperature. Both HERA and Tevatron magnets required only one superconducting channel — for protons in HERA and a common channel for both p-bars and protons in the Tevatron. A proton–proton collider would need two separate channels with opposite magnetic fields. There were two options for such a machine: housing these two channels in the same cryostat (2-in-1 design), as proposed by Bob Palmer of BNL, or separately (1-in-1 design).

The main players in the superconductiing magnet effort were Fermilab, BNL, LBL and Texas Accelerator Center (TAC). The last was a new organization, created near Houston with support from George Mitchell (a Texas oilman), by Russ Huson and Peter McIntyre, both former Fermilab employees, who wanted to build a research center focusing on accelerator issues. In addition, there was very important work at the University of Wisconsin on superconducting cable under the leadership of P. Larbalestier.

The most difficult part of the magnets was the ends; a high fraction of the cost was in the connections there. Thus, in the early stages of the R&D, relatively short (~1 m) magnets were built so as to understand and optimize the most difficult elements. Cost optimization of the machine, however, would eventually require magnets significantly longer than those used in the Tevatron. Initially, three different magnet types were worked on [61]:

- Design A — 2-in-1, cold iron, high field (6.5 T); pursued at BNL and LBL;
- Design B — 1-in-1, warm iron, high field (5 T); pursued at Fermilab;
- Design C — 2-in-1, low field (3 T, superferric); cold iron; pursued at TAC.

Subsequently, work on designs A and B indicated that a better option would be a 1-in-1, cold iron, high field (6.5 T) magnet, called design D. The main focus of the magnet activities at BNL, LBL and Fermilab shifted toward the work on that design. In parallel, the TAC group started working on design C*, a 1-in-1 version of design C. Early in 1985, Tigner appointed a Technical Magnet Review Panel (TMRP) to review and help guide the magnet R&D program. This panel, chaired by Alvin Tollestrup of Fermilab, was composed of senior members from each institution working on the magnets as well as additional scientists without direct institutional involvement in this work.

Naively, one might view the magnet development work as having purely technical scope. However, there were also strong political overtones. The state of Texas was very much interested in hosting the eventual SSC Laboratory and even offered, to that end, to consider providing the site and conventional facilities at no cost to the federal government. Adoption of a low field magnet, requiring a larger diameter machine and hence a larger overall site, would help Texas in the future site competition by reducing the total number of proposals satisfying the site requirements.

The work on superconducting cable was being carried out through a collaborative effort between DOE, universities and industrial concerns, with the University of Wisconsin playing a leading role. The manufacturing process was quite complex and was based on many years of development work [62]. Since later on new, as-yet-undeveloped, higher-temperature, ceramic superconductors were being suggested for the SSC, it is useful to review briefly the complex manufacturing process. Several thousand NbTi 1/8″ diameter rods are stacked into billets within a copper matrix. The billets are about 75 cm in height and 30 cm in diameter. They are then drawn into wire, about 0.8 mm in diameter, with original rods becoming fine filaments about 2–3 microns in diameter. The resulting wire is then wound in a helical pattern into a cable about 9 mm wide.

One of the most important parameters of the wire is its current-carrying capacity at the operating field and temperature. The cable used for the Tevatron had a capacity of 1800–2000 A/mm^2 at 4.2 K and 5 T. The Reference Design had assumed that with additional R&D one could raise that to 2400 A/mm^2. The R&D during the initial years of the CDG resulted in filaments with a capacity up to 2750 A/mm^2, allowing 6.5 T magnets in the CDR design. Besides the work on the wire, new cable winding machines had also been developed in the laboratories and the acquired know-how transferred to industry.

There were a number of other technical R&D activities that were essential for producing the collider design. Among the most important ones were the accelerator physics calculations which provided important guidelines for the design not only of the magnets but also of the accelerator as a whole. Accelerator physics studies were a key input determining the aperture of the magnets. In the SSC the projected lifetime of the beam was of the order of 1–2 days, limited mainly by proton–proton inelastic interactions at the collision points; for the projected circumference of the SSC of about 80 km, the protons would execute typically about 10^9 revolutions after injection. Ray-tracing of individual particles over such a long time would clearly not be possible. Thus other calculational methods had to be developed for this problem [63, 64].

The method used in the SSC design relied on the concept of linear aperture, i.e. the size of the physical region in which the motion of the particles around a closed orbit is approximately linear. Because of nonlinear effects, the actual aperture required would be larger than this linear aperture. To obtain the relationship between the two, one has to take into account such effects as irregularities in the magnetic field, the size of beams, variations in the injection position, alignment errors and correction schemes.

The quality of the magnetic field around the orbit executed by the protons is determined mainly by the dipoles, which are the most numerous components in the lattice. This quality is characterized by the size of the multipole components, i.e. deviations from the perfect dipole field. Their effect can be reduced or eliminated by correction coils; the optimum design of the machine will then be a compromise between the effort and cost required to reduce the magnetic field inhomogeneities and their variation from magnet to magnet and the complexities of the required correction coils.

The SSC was to be the first proton machine in which synchrotron radiation from the circulating protons would be significant [65]. At the design energy and luminosity each beam would radiate about 9 kW in synchrotron power. This would have a positive effect of increasing the luminosity during the first day after injection due to synchrotron damping. On the negative side, the heat from the absorbed radiation would have to be removed, placing additional load on the cryogenic system. In addition, the energetic photons upon striking the beam pipe walls would desorb gas molecules "frozen" on those surfaces. If they are not removed, pressure would build up in the beam pipe, shortening the beam lifetime. To investigate these effects, several experiments were performed at the National Synchrotron Light Source at BNL to study the desorption due to synchrotron radiation.

The design of the cryogenic system was another major challenge addressed in the early years of the CDG. The SSC would be by far the largest cryogenic system held at the liquid helium temperature of 4.3 K. To keep operating costs low it was essential to reduce the heat leakage into the magnets from its surroundings. This was achieved by having an intermediate shield at liquid nitrogen temperature and optimizing placement of the insulation and support mechanism. Measurements of heat leakage in a 12 m test cryostat confirmed preliminary estimates that the heat leakage could be kept at 0.25 W/m, roughly a factor of 5 below the Tevatron value.

All of the above activities, plus measured performance of magnet prototypes, formed the input for the selection of the magnet design type. To evaluate all of this information, Tigner appointed several different task forces. The Aperture Task Force, composed of worldwide experts in this area, evaluated all the relevant experimental and theoretical results with a view to understanding whether the apertures proposed were adequate [66]. The Cost Comparison

Task Force had as its charge obtaining detailed cost estimates of all magnet-dependent SSC subsystems for all of the candidate magnet types. It received input on the cost issue both from the proponents of different magnet styles and from industrial firms contracted by the CDG for cost evaluation and, based on this input, made its own independent cost evaluation. The Operations and Commissioning Task Force was to look at operational issues connected with each candidate magnet type.

A Magnet Selection Advisory Panel (MSAP) was then appointed to review the reports of all the task forces as well as the final report [67] of the previously mentioned TMRP, and to put forward a recommendation on the magnet type to be selected. The Panel was chaired by Frank Sciulli, a professor of physics at Columbia University, and included experts on large technical systems, superconductivity technology, accelerator design and operation, use of accelerators for particle physics and/or underground construction or who had experience with complex science and technology policy issues. In addition, three industrial consultants, with expertise in the technology of superconductivity and magnets based on it, were appointed to advise the Panel.

The Panel met at Berkeley on August 25–28, 1985, to hear presentations from proponents and reports from various CDG analyses. The meeting was also attended by representatives of all four R&D centers as well as representatives from DOE and URA. The final report, dated September 9, 1985, was unanimous (including the three consultants) in recommending a high field, cos θ, cold iron, single-channel magnet, i.e. design D, as the basis for the SSC. The basic arguments for the cos θ design were its well-understood behavior and predictable cost. The high field was favored for operational and cost reasons. Maury Tigner accepted this recommendation.

The possibility for future upgrades of the machine was not one of the criteria given to the Panel. Some people argued for the largest possible tunnel so as to have the option in the future to go to higher energies by using next generation high field magnets. Tigner did not find this argument persuasive, arguing that the scientific and technical uncertainties in predicting the future some 15 years in advance were so large that it would not be wise to depart from the optimum design for those reasons [68].

Even though the magnet selection process was finished and recommendation made and accepted, the issue was not put to rest. The decision did not sit well with the TAC or with the Texas Congressional delegation. A rather far-ranging House Energy and Water Subcommittee hearing was held on October 29, 1985, part of which was devoted to the magnet selection process [69]. Joe Barton, Representative from the Texas 6th Congressional district, tried through his questions to raise doubt about the quality of the selection process, focusing mainly on the tunneling costs, but did not appear to make any significant impact on the subcommittee members.

At the time of the MSAP meeting, TAC had completed and measured only relatively short superferric magnets. A 28 m magnet was being produced by General Dynamics Corp but was not finished. A few months later, a finished magnet was delivered to TAC and cooled down; a number of measurements of the quality of its magnetic field were then performed. The TAC leaders, F. R. Huson and P. M. McIntyre, found the results so promising that on April 7, 1986, they wrote a letter [70] to William Wallenmeyer, stating: "This new information leads us to the conclusion that the superferric design is the best and least expensive choice for the SSC... project cost can be reduced by US$1 billlion compared to the high field design."

This letter triggered a request from Al Trivelpiece to Jack Sandweiss, as HEPAP chair, to convene a subpanel with a charge to review the new information from TAC to determine whether the evidence was sufficient to warrant reopening the magnet selection process. This new subpanel "on Review of Recent Information on Superferric Magnets," chaired by Burt Richter from SLAC, met twice, on April 24 and May 8–10, 1986, when it heard presentations from TAC and had discussions with members of TAC and CDG. Its deliberations on the status of the TAC magnet R&D program and on conventional cost issues led to its report [71], dated May 20, 1986, drawing three conclusions:

(1) The TAC magnet R&D program is still about two years behind the high field magnet program. The TAC magnet is not ready for industrialization.
(2) Cost uncertainties in TAC technical components are large, and superferric magnet costs cannot be

estimated with confidence at the present state of R&D.

(3) We do not believe that the new information presented warrants reexamination of the CDG magnet-selection decision.

The magnet selection issue was finally settled; design D magnet was the focus of subsequent R&D work [72] and the basis for the CDR.

9. Conventional Facilities Plans

It was anticipated from the time of the 1983 HEPAP Subpanel that there would be a nationwide competition for the SSC site. This process would be organized and directed by the federal government but the CDG was to play an important supporting role. The first CDG task in this area was the preparation of the "Siting Parameters" document, scheduled for completion in April 1985. Preparing this document and directing the development of conceptual design for the conventional SSC facilities were the main tasks of the Conventional Facilities Department of the CDG during the first two years.

The conventional facilities of the SSC could be divided into five components: the main SSC tunnel for the 20 TeV storage rings, the injector complex consisting of several lower energy accelerators, the interaction regions, the laboratory campus and the laboratory infrastructure (e.g. roads, power distribution). Because of its length, some 80–90 km, and the requirement that the tunnel be at least 20 below the surface over its whole circumference, the first component was most expensive and was the one that imposed the most restrictive conditions on the suitability of a given site.

Based on the above components of the SSC, a "Siting Parameters" document was prepared for DOE [73]. The function of the report was summarized in its abstract: "The principal objective of this technical advisory is to delineate for the DOE those factors governing the choice of a site for the SSC essential to the creation of a laboratory." The report addressed only the technical issues; the definition and execution of the site selection process, including the preparation of the formal "Site Selection Criteria" document, was going to be the responsibility of DOE.

The "Siting Parameters" document used the accelerator parameters suggested in the Reference Design Study and considered a facility based both on 3T and 6T magnets. It summarized the technical criteria that DOE might use in evaluating the candidate sites for the SSC and addressed the requirements on the topology and geography of the site and the auxiliary conditions, like availability of power and water, proximity to large population centers, ease of access via public transportation, etc. Thus, for example, the required overall surface area under lab control was estimated at 11,000 acres; the main ring tunnel should be level within 1°; up to 2000 gal/min of water should be available and up to 250 MW of electrical power, with separate feeds. The geology of the site had to have been extensively characterized; man-made excessive noise and vibration should be avoided. The environmental issues had to be addressed so that compliance with National Environmental Protection Agency (NEPA) standards could be achieved.

The next step in the work of the Conventional Facilities Department was selection of an architectural and engineering (A&E) group of companies to assist in developing the conceptual designs for the laboratory campus and interaction region buildings, injector complex, utility distribution, required roads, etc. This group was also charged with evaluating different tunneling methods in common use and estimating the costs of tunneling in various soil conditions. The RTK joint venture (Raymond Kaiser Engineers, Tudor Engineering Company, Keller & Gannon-Knight) was selected for this task.

The underground component of the SSC was going to be a major part of the project in terms of effort, cost and time. Thus expert advice and oversight during the design and construction phase appeared beneficial. To that end, Tigner appointed an Underground Tunneling Advisory Panel (UTAP) with the advice and under the leadership of Bob Matyas. A related effort was getting the US National Committee on Tunneling Technology of National Research Council to initiate a study of the contractual relationships between different parties in large underground engineering efforts. At the request of DOE, who sponsored the study, the SSC was used as an example of such an effort. The subcommittee appointed for this task was composed of 12 individuals from all over the US, with expertise covering all aspects of such projects. Their report [74], "Contracting Practices for the Underground Construction of the Superconducting Super Collider," became a bible of the tunneling industry and is still in wide use today. Several members of the CDG Conventional Facilities Department made major contributions to this work.

10. The Conceptual Design Report, CDR (1986)

The work described above formed the basis for the CDR, scheduled to be completed in April 1986. This would provide an up-to-date vision of what the SSC would look like, what its performance would be, how long it would take to build it, and what it would cost. It would be the basis of the proposal to DOE to build the SSC. The initial budgetary assumptions were much more optimistic than what was eventually realized. Instead of doubling every year, the annual appropriation for the SSC R&D for the first three years remained constant, around US$20 million. In spite of that, the realized progress was impressive across the board. The full length magnets were still a few months away at the time of the CDR but the experience obtained from the shorter magnets gave one confidence in the successful conclusion of that R&D program.

The actual preparation of the CDR was the focus of the CDG members and its visitors during the last few months of 1985. Six US national laboratories and 13 universities were involved. In addition CERN, DESY and KEK (Japanese HEP laboratory) contributed. Dave Jackson accepted the monumental task of serving as the editor of the report. Consisting of a 712-page main volume and 4 attachments, the report [75], described the physics at the SSC, the relevant experience with previous facilities, the R&D programs over the previous three years, the technical design, the relevant accelerator physics issues, the conventional facilities required, the technically driven construction schedule and a cost estimate down to WBS level 4. The basic parameters were: proton–proton collider, 20 TeV per beam, and design luminosity of $10^{33}\,\mathrm{cm}^{-2}\,\mathrm{s}^{-1}$. The CDG also made a study of the pbar-p option (advocated probably must strongly by Leon Lederman) and found that the resulting relatively small cost savings did not warrant a factor of 10 loss in luminosity and additional operational complexity wich would decrease reliability [76]. Since the site was yet to be chosen, a variety of different realistic sites were considered for which efficient construction methods would exist. The design was based on 16.54-m-long magnets, a peak field of 6.6 T and a 4-cm-diameter aperture. The last was the result of extensive accelerator physics studies and overall cost optimization.

The collider would have an injector complex consisting of four accelerators. A linac would accelerate H^- ions to 600 MeV (kinetic energy), which would be stripped at injection into the low energy (8 GeV/c) booster (LEB). Subsequently, the medium energy booster (MEB) and high energy booster (HEB) would bring the protons to energies of 100 and 1 TeV, respectively. The 1 TeV protons would then be injected into the collider rings. The HEB would be a superconducting machine to minimize its power usage and its circumference; the other preaccelerators would operate at room temperature.

The main collider tunnel would be in the shape of an oval with a circumference of 82.944 km. Two semistraight sections of the oval would house injection and utility regions as well as six interaction halls. The main campus buildings would be near the injector complex. The parameters of main interest to the experimentalists were: 4.8 m separation between bunches, 1.4 average number of interactions per bunch crossing at peak luminosity, luminosity lifetime of about 1 day and injection time of about 1 hour. Four test beams (up to 1 TeV energy) would be available from the HEB.

The cost estimate for the total accelerator complex was US$3.01 billion (in 1986 dollars). The average contingency was 21.4% of the base cost, rather low by the standards used today, especially considering the significant increase in scale over previous machines. These costs did not include detectors, R&D, preoperations or escalation. This was the standard costing practice for DOE accelerators in 1986. Furthermore, no site acquisition costs were included (on the assumption that the site would be provided free by the proponents). The construction time was estimated to be six-and-a-half years.

The CDR was delivered to DOE on April 1, 1986, and underwent an extensive review on April 26–30 at LBL involving 83 reviewers and consultants [77]. The conclusion was: "The DRC [DOE Review Committee] concludes that the design set forth in the CDR is technically feasible and properly scoped to meet the requirements of the US high energy program in the period from the mid-1990's to well into the next century.... The DRC finds that the SSC CDR cost estimate is credible and consistent with the scope of the project." A parallel review of the costs was performed by the Independent Cost Estimators (ICE) group. They agreed with the general cost estimate but expressed concern that some of the estimates and the level of contingency might be on the optimistic side.

To obtain an estimate of the potential cost of the initial detector complement and associated computing, three advisory panels were appointed. The first one, called the Detector Cost Model Advisory Panel, chaired by George Trilling of Berkeley, had as its charge definition of a likely complement of detectors that would be able to appropriately respond to the physics needs a decade hence. The second one, chaired by Roy Schwitters of Harvard, was asked to estimate the cost of such a set of detectors. The third one, chaired by Stu Loken of LBL, was to estimate the associated computing costs [78]. This work was completed by April 20, 1986. The total cost estimate obtained for detectors from these efforts was in the US$558–865 million range, and for computing it was US$70 million. The shortcoming of this process was that it did not allow for optimization of the physics output per dollar in either the choice or the design of the detectors [79].

11. The CDG and Government Agencies

Interaction with government entities and individuals was a very important part of the CDG activities. One key element of these interactions was the organization of, preparation for, and participation in reviews. DOE held both quarterly and annual reviews of the CDG, lasting typically two or three days. They covered the full spectrum of the CDG activities, focusing on technical work for the collider itself, but also covering budgets, detector R&D and various outreach activities. In addition, there were other reviews of major pieces of work, such as the Conceptual Design Report.

Another important activity was providing information, in either written or oral form, to various government agencies. There was a constant flow of information from the CDG to DOE about the work being done and the results. There were regular monthly and quarterly reports detailing work done and budgetary status. Trip reports from international travel kept DOE informed about our activities in the international collaboration area. There was the regular annual testimony at the Congressional authorization and appropriation hearings (about 2–4 per year), generally given by Maury Tigner. Briefing papers and SSC fact sheets were prepared for distribution to the Members of Congress and Congressional staff. There were other hearings when specific issues came up, such as after the magnet selection decision, where CDG members were asked to testify. There were periodic interactions with DOE officials outside of reviews. In addition, there were many interactions with members of Congress or members of their staff, generally at their request but also frequently initiated by the CDG.

The CDG was often asked to prepare briefing documents. Early in 1985 at the request of the OSTP, the National Academy was asked to provide a briefing on "Scientific Frontiers and the Superconducting Super Collider" and I was asked to chair the panel to prepare that report [80]. Subsequent to that I gave oral briefings on the substance of the report to G. K. Keyworth, NSF Director E. Bloch, DOE officials and staff of the relevant Congressional committees. There were also CDG briefings about the SSC to the National Science Board of NSF. In later years, as the SSC was receiving more attention at higher levels of the federal government and beginning to be treated more seriously, we were frequently called on by DOE to provide information for briefing books for various officials about different aspects of the project: physics goals, technical details, technological spinoffs, international situation, budgets and schedules, etc.

12. The CDG and the US HEP Community

From the very beginning of the SSC, the US HEP community exhibited great interest in it and great enthusiasm about its physics potential. The CDG has actively worked with the community, with three main thrusts in mind:

- Involving members of the community in the SSC design and R&D activities;
- Enlisting the community to generate grassroots and Congressional support for the project;
- Working with the community to ensure the maximum possible state of readiness to do physics with the SSC upon its completion.

The appropriations for the first four years (FY84–FY87) of SSC R&D stayed constant at about US$20 million/year; that level was certainly inadequate for the effort that was anticipated at the beginning of the CDG. Thus the CDG activities had to rely heavily on the community participation. There were a number of long term visitors at the CDG who came for a few months or a year, both from the US and from abroad, and made important contributions to various technical or planning issues. Experts in various areas contributed generously through their membership in various task forces that were being organized all the time.

One of the mechanisms for communicating with the community and other interested parties was *The Lattice*. This was a bimonthly, 8–12-page newsletter published by the CDG under URA auspices.[e] It described the main happenings at the CDG, provided relevant news about the SSC R&D results and a calendar of upcoming events, and listed the recent SSC reports published.

A formal way to get the community involved in planning for physics at the SSC and at the same time form an advocacy group for the SSC was the creation of the Users Organization of the SSC (UOSSC), open to all scientists with a professional interest in the SSC [81]. This organization was formed in the spring of 1987 by the Division of Particles and Fields of the American Physical Society at the request of the CDG and the community as a whole. The first meeting of the UOSSC was held on May 20–21, 1987, at LBL.[f] Lee Pondrom of the University of Wisconsin was elected the first chair of the UOSSC. On July 16, 1987, as part of the Workshop on Experiments, Detectors, and Experimental Areas, held in Berkeley, a town meeting was organized at which Pondrom outlined the planned activities of the organization and Brig Williams (Penn) and Satoshi Ozaki (KEK) discussed the ongoing detector R&D activities.

Numerous periodic workshops were an efficient means to both keep the community informed and solicit their input. They were generally organized by a combination of DPF, national laboratories, universities and the CDG. To give a flavor to these activities, I will mention here some of the larger and/or more important ones:

- Workshop on Triggering, Data Acquisition and Computing for High Energy/High Luminosity Hadron–Hadron Colliders, held November 11–14, 1985, at Fermilab.
- 1986 workshop in Snowmass, Colorado on the "Physics of the Superconducting Supercollider" [82], attended by 264 people. This was the first major workshop after publication of the CDR, so it gave participants a chance to critique it and also use the parameters to start thinking seriously about generic detector designs. At this meeting a request was formulated for a separate program to fund generic detector R&D.
- Workshop on Experiments, Detectors, and Experimental Areas, held July 7–17, 1987, in Berkeley [83]. Over 260 people attended, with 24 of them from outside the US.
- Summer Study on High Energy Physics in the 1990's, held June 27–July 15, 1988, in Snowmass, Colorado [84]. This study, with 536 participants, had a somewhat broader scope, examining anticipated opportunities in high energy physics in the 1990's, both at the SSC and at other facilities.
- Workshop on Triggering and Data Acquisition for Experiments at the SSC at the University of Toronto, Canada, held January 16–19, 1989.
- Workshop on Calorimetry for the SSC, held March 13–19, 1989, at the University of Alabama, Tuscaloosa, Alabama. The choice of this location was part of a broad effort to generate and/or increase interest in the SSC in areas without traditionally large involvement in HEP.

Early in 1987, the CDG (in consultation with DOE, NSF and the community) established a Generic Detector R&D Coordinating Office [85]. Its mission was to coordinate and focus the national effort on generic (rather than detector-specific) detector R&D relevant to the SSC and maintain appropriate contact with similar research being done elsewhere in the world. Full utilization of luminosities in the 10^{33} range required sophisticated detectors with hitherto-unavailable capabilities [86]. Thus generic R&D was needed to identify the problems and find solutions. M. G. D. Gilchriese, a professor at Cornell, was

[e]The initial name, *SSC Newsletter*, was found objectionable by the DOE on the grounds that it might imply a stronger commitment to the SSC than DOE was prepared to make at that time. The more innocuous name *The Lattice* was accepted as a compromise.

[f]There was an earlier, informal meeting of potential SSC users in May 1985, at LBL, organized by the CDG, but no formal organization was formed at that time.

appointed the Detector R&D Coordinator. In addition, an International Advisory Committee, under the chairmanship of H. H. Williams from the University of Pennsylvania, was appointed to provide advice and guidance to the Coordinator regarding this R&D program.

The program was to be proposal-driven, with the proponents making submissions to funding agencies, which generally would refer them to the CDG for recommendation. The Coordinator, assisted by the Advisory Committee, would examine them and advise the funding agencies regarding their potential support and its level. The Coordinator would also make periodic recommendations to the funding agencies regarding the required funding level for these activities, periodically publicize the R&D required in hitherto-neglected areas, and organize workshops as deemed necessary.

At its first meeting in April 1987, the Committee considered 31 tasks requesting US$3.7 million and recommended funding 11 of them with a total sum of US$545,000. DOE followed this recommendation. The program grew in the following years: in FY88, US$2.25 million were disbursed (US$8.9 million were requested and a US$3.4 million funding level was recommended); in FY89, US$2.9 million were allocated for new efforts and US$3.4 million for continuation of ongoing ones. During the existence of the CDG, 7 national labs, 29 universities and 8 industrial concerns were involved in the program. R&D in many diverse areas was supported; some of the topics studied were radiation-hardened electronics, pixel detectors, triggering and data acquisition, computing, large mechanical systems (e.g. for calorimeters) and the test beams required [87]. Once SSC Lab was established, the program would be absorbed by the Lab and would gradually transform itself into specific detector design and R&D.

13. The CDG and the General Public

To make the SSC a reality, the project had to enjoy significant popular support so as to get the required Congressional approvals. Accordingly, from the very beginning of the CDG's existence there was a concerted effort to inform the general public about the SSC and to create enthusiasm for it. The sending-man-to-the-moon project in the 1960's, which fired up the general population's imagination, was a model to emulate.

One of the means toward that end was publication of various brochures describing SSC in general or focusing on some of its specific aspects. Some of the brochures were:

- "To the Heart of Matter," a general description of the SSC and its physics motivation, first published in 1984. Later, two updated versions were printed, when the CDR was finished and when SSC Lab was established in Texas.
- "Supercollider R&D, The First Two Years," published in December 1985, describing the initial SSC R&D, its goals and achievements. This brochure was geared toward industrial concerns which potentially might be involved in that work.
- "The Superconducting Super Collider," a leaflet distributed to the states interested in hosting the SSC, describing SSC physics motivation, the accelerator complex and its potential impact on the community.
- "SSC Detectors — Looking to the Future," describing the challenges presented by the future SSC detectors, also aimed at potentially interested industrial enterprises.

These booklets were put together by members of the CDG and LBL staff, with Rene Donaldson, CDG editor/publisher, playing a major part in producing the publications. We were helped in these and other public outreach efforts by the prizewinning science writer K. C. Cole. In 1988, Chris Quigg, theory group leader at Fermilab, who replaced J. D. Jackson as Deputy Director for Operations in May 1987, and Leon Lederman compiled a book titled *Appraising the Ring*, which put together short statements of support for the SSC by various individuals prominent in science or industry [88].

In parallel, there was a concerted effort on the part of the whole HEP community to give presentations about the SSC in various forums: colloquia, schools, conferences, etc. Fermilab Director Leon Lederman was one of the most active scientists in promoting the SSC through numerous talks and articles. Following up on the request from AAAS, we organized a symposium on the SSC at their annual meeting in the spring of 1985, with Lederman, Jackson and Tigner as the speakers. The CDG made a strong effort to contact physics teachers about the SSC by sending "To the Heart of Matter" to all science teachers in the country. We also organized an exhibit about HEP and the SSC at the Annual Science Teachers Convention on April 7–10, 1988, in St. Louis. A traveling exhibit about the SSC was prepared and shown at various science centers through collaboration with the Association of Science and Technology Centers.

There was also a broad effort to get articles published in various popular journals and newspapers about the SSC either by members of the HEP community or by journalists, based on interviews. Some of the popular articles written by the HEP community were:

- "Elementary Particles and Forces," by Chris Quigg, in *Scientific American*, April 1985.
- "To Understand the Universe," by Leon Lederman, in *Issues in Science and Technology.*
- "The Superconducting Supercollider," by J. D. Jackson, Maury Tigner and S. Wojcicki, in *Scientific American*, March 1986, describing the SSC and its physics goals.
- "The Case for the Supercollider," by J. Cronin, in *Bulletin of the Atomic Scientists*, May 1986, pp. 8–11.
- *Science* article "Elementary Particle Physics and the SSC," by Chris Quigg and Roy Schwitters, March 1986.
- *American Politics* article on the SSC in July 1986 by Roy Schwitters; it was reported to have made a significant positive impression on Secretary Herrington.

These outreach activities intensified significantly during the site selection time, 1987–1988. Officials in interested states wanted to become better-informed about the nature and scientific goals of the SSC, its site requirements, and what the SSC would mean to a given state if the SSC was located there. As a national group, the CDG could not express favoritism toward any single proponent, but if requested we did go to various events connected with the site selection all over the country and tried to answer all the questions. During that time the CDG also produced a short videotape discussing the SSC and geared to the parties interested in hosting the SSC [89].

A very important issue was interaction with the non-HEP scientific community. There was an understandable concern there about potential negative impact of the SSC on other sciences. Very simplistically, the reaction of other scientists fell into two broad categories: some, like Phil Anderson, a Nobel laureate and a professor of theoretical physics at Princeton, Jim Krumhansl, a condensed matter theorist at Cornell, and Rustum Roy, a materials scientist at Penn State, argued strongly against the SSC, being convinced that it was bound to hurt their fields; others supported the SSC's scientific goals and hence its construction, provided that this would not impact adversely the support for their own fields. I was on the APS Council for three years during the CDG days; the Council shied away from any positive statements about the SSC, because a number of its members expressed these fears. I discuss this issue in more detail and provide some specific examples in the following sections. This potentially negative reaction was a serious concern of the CDG and the BOO; an effort was made to alleviate the concern and emphasize the SSC's scientific value through colloquia, popular articles, rebuttals of unjustified anti-SSC articles, etc. John Deutsch, one of the BOO members and a chemist by training, emphasized frequently the need for such intensified efforts.

As the SSC project became better known [90] and began to look like it might materialize, the industrial interest intensified. We tried to mobilize potential industrial participants in SSC construction to help with getting the SSC approved. The example of NASA and the International Space Station, where industrial support in getting Congressional approvals was very helpful, was a good model. An important meeting along those lines was the National Symposium on the SSC held on December 3–4, 1987, in Denver, Colorado [91]. This was during the site competition, so interest from industry and state officials was very high. There were about 600 participants.

It was always planned to fabricate the SSC superconducting magnets in industry. To accomplish this, the technology in this area, developed in the national labs, had to be transferred to industrial concerns. The CDG and DOE formulated a three-phase Magnet Industrialization Program to accomplish this, the three phases being Technology Orientation, Tooling Design and Magnet Preproduction, and Magnet Production. Phase I was initiated by the publication of an announcement in the *Commerce Business Daily* on July 21, 1988. During Phase I, representatives from participating companies had the opportunity to visit national laboratories and interact there with the magnet experts. The initial Technology Orientation meeting was held at the CDG on January 9–10, 1989.

The next big industrial meeting, organized by a consortium of industrial companies, national laboratories, universities and government agencies, was held in New Orleans on February 8–10, 1989 [92, 93]. It focused on physics, technology and politics relevant to the SSC. DOE Secretary Herrington addressed the audience at that meeting.

We tried to get a professional assessment of the impact of HEP research on innovation, new products, and new industrial activity. This was a topic that frequently came up in Congressional debates and various articles. Such study, focused on benefits from CERN contracts, was performed at CERN in 1984, and found that for every franc spent by CERN on high technology contracts, three francs of secondary economic benefits were generated [94]. The methodology used was frequently questioned, so we initiated interaction with two US economists, David Mowery of Carnegie Mellon University and W. Edward Steinmueller of Stanford, to investigate the general question of economic benefits coming from basic research. They submitted a formal proposal [95] to DOE for such a study. The proposal was funded and the resulting report received moderate attention in Washington but had no major impact on the SSC. The main results of the research were published in a journal and that article [96] has been cited frequently.

14. International Collaboration Issues

The issue of potential international collaboration on SSC construction is quite complex. When one looks superficially at the global situation in the area of science and technology in the early and mid-1980's, one might surmise that the situation was ripe for significant collaboration. At the 1982 Versailles summit meeting of the heads of state of seven principal industrial nations, Mitterand proposed exploration of a possibility of closer cooperation between the major industrial powers in the area of science and technology. The first step was to set up working groups in 18 project areas, with lead countries identified in each. The US would serve as the lead country in 5 areas, including HEP, this to be chaired by Alvin Trivelpiece [97]. The activities of these groups were endorsed by the heads of state at their meeting two years later [98]. However, the lofty goals, as expressed in the relevant resolutions, were submerged, at least as far as the SSC was concerned, by practical and parochial considerations having to do with perceived needs for regionally based programs and preservation of the existing labs.

Early in the SSC days there was resentment in some circles that the SSC decision was made unilaterally by the US and that the process completely bypassed ICFA.[g] However, the facts were that while ICFA did organize workshops and did discuss issues, it was never involved in planning any construction project and, in my opinion, was not structurally designed to do that. At the May 1984 ICFA meeting at KEK in Japan, it was formally recognized that ICFA should not be a body for planning joint accelerator undertakings but rather should focus its efforts on sponsoring workshops, facilitating coordinated research and development efforts in key HEP technologies and organizing worldwide inclusive meetings on future plans for regional facilities. The key term of reference was modified to read: "... to promote international collaboration in all phases of the construction and exploitation of very high energy accelerators" [99].

The SSC started and evolved as a US domestic project and one of its expressed goals was restoration of US pre-eminence in HEP. This made the subsequent efforts to obtain contributions from abroad to its construction difficult, but not impossible. To understand the complexities involved, one has to look at the two most important potential contributors, Western Europe and Japan, where situations were quite different.

When the SSC was proposed, the two large HEP laboratories in Western Europe were initiating major accelerator projects. CERN in Geneva was starting construction of a large electron–positron collider, LEP, in a tunnel 27 km in circumference. It was scheduled for completion in spring 1989 and CERN

[g]There is some controversy as to how damaging this was, especially with the Japanese HEP community. It is my opinion, based on numerous trips to Japan and conversations on this topic with a wide spectrum of Japanese HEP physicists, that it was not very relevant. The only person I ever heard complaining about this issue was Yamaguchi, chair of ICFA at the time, who voiced his unhappiness about this at the 1987 ICFA seminar held in Serpukhov. But, for a somewhat different point of view, see Refs. 2 and 14.

construction funds were committed through 1992. The other laboratory, DESY in Hamburg, was in the process of constructing the world's first electron–proton collider, HERA, scheduled to be finished around 1990.

The long term plans for the future of CERN provided additional complication. Already, when the LEP collider was being designed the possibility was kept in mind that the tunnel could eventually be used for a hadron–hadron collider. Very little work had been done on this possibility prior to 1984, CERN being very much occupied with LEP. The idea was, however, revived very shortly after the HEPAP recommendation for the SSC, spurred by the substance of this recommendation. A March 1984 workshop at Lausanne studied the feasibility of hadron colliders in the LEP tunnel but it was almost entirely devoted to physics issues [100]. At the May 1984 meeting of the AAAS, CERN DG Herwig Schopper described plans to build a 5-on-5 TeV proton–proton collider by adding current technology superconducting magnets on top of the low field LEP magnets. He argued that the price tag would only be US$500 million and invited the US to participate [101].

As subsequent developments showed, that cost figure was completely unrealistic and the idea of building and running the collider contemporaneously with LEP operation was problematic on operational grounds. But it was true that a Large Hadron Collider (LHC) in the LEP tunnel was an obvious next step for CERN to take once the physics at the LEP was fully exploited. Thus the SSC, or any other high energy hadron collider elsewhere, was viewed by many as a threat to CERN's future and it was only natural that the DG of CERN would be concerned. A few quotes from Schopper's prepared testimony [102] before Congress on April 7, 1987, illustrate this: "The Committee of Council is of the opinion that a hadron collider in the LEP tunnel should be seriously considered as the next step in the exploration of the microcosmos... hadron collider in the LEP tunnel would cover the interesting energy range at a fraction of the projected cost of the SSC." And, later on, during the SSC discussion: "...if we would propose such a project of that size, it would not go through because the emphasis is on cost efficiency, cost effectiveness."

On the whole, however, Europeans did express support for the SSC, indicating at the same time that the likelihood for significant European contribution to the SSC construction was slim. Volker Soergel, DG of DESY, in his prepared testimony [103] for the same hearing, wrote: "I think SSC is a great project and I hope very much that a machine with those characteristics can be realized.... For financial collaboration, I am not as optimistic that a sizable amount of money for these big projects can come from abroad." The most likely possibility for contributions from Western Europe was Italy. Italy had a strong tradition of supporting physics in general and HEP in particular; it also had a heavy involvement in the Collider Detector at Fermilab (CDF). It had already contributed to foreign accelerator construction by providing superconducting magnets for HERA.

The situation in Japan was somewhat different. The Japanese were in the process of constructing an e^+e^- collider called TRISTAN (with an energy up to about 35 GeV in each beam), due to be finished in 1986. Thus, from the funding point of view, they were a much more likely partner in any new large particle physics facility. They had also established a tradition of working at US accelerators, the most significant being their large involvement in the CDF at the Tevatron. Keyworth realized this and as early as the summer of 1984, on his trip to Japan, broached the subject of Japanese participation in the SSC [104].

Keyworth's intervention had both positive and negative sides to it. On the plus side, the Japanese government circles were happy to be approached at early stages of the project, before full design was settled on. On the other hand, the Japanese HEP community felt bypassed. They wanted the initial contacts to be made on the physicist-to-physicist level, so that they would have had the opportunity to decide whether this was a project they want to be involved in. When I made my initial trip to Japan in the summer of 1985 and discussed the SSC with the Japanese HEP community members, that sentiment was emphasized to me quite strongly, especially by M. Koshiba.

The Japanese community was split regarding their future. The accelerator physicists wanted to pursue an e^+e^- linear collider as the next initiative; that presented a greater challenge, since the SSC was considered a relatively uninteresting machine from the accelerator physics point of view. The HEP experimental community, on the other hand, wanted to take advantage of nearer term opportunities that would be provided by the SSC. The latter argument appeared to prevail, to a large extent thanks to Panofsky, who made strong arguments about very different time scales for these two efforts.

There were several other possibilities. Canadian physicists expressed strong interest in experiments at the SSC and were suggesting a cross-border site for the SSC, spanning northern New York State and southern Quebec. Should that materialize some contribution could be expected, financial or in-kind, as well as provision of cheap electric power during operations. The potential difficulty was that TRIUMF, the Canadian HEP laboratory in Vancouver, was proposing construction of a high intensity, medium energy proton synchrotron called KAON with a price tag of around US$400 million. Small contributions might have been forthcoming from China, Russia and India. Russia, however, was trying then to build its own facility, a 3 TeV superconducting proton synchrotron, UNK, at Serpukhov, to be completed no earlier than the mid-1990's. Initially a fixed target machine, there were vague plans to eventually convert it to a collider. Subsequently, a very ambitious proposal was put forward to build a 1 TeV electron–positron linear collider in Protvino [105].

The issue of international collaboration was discussed extensively by the SSC BOO, starting already in 1984. In his memo to the Board, Panofsky outlined four possible ways in which one could proceed with the international construction of a high energy hadron collider:

- Financial participation by other nations in SSC construction authorized by the US and located in the US.
- Contributions in kind by other nations to an SSC authorized by the US and located there (essentially a HERA model).
- International collaboration on exploitation of the SSC, i.e. in construction and operation of the detectors (the standard mode of collaboration to date).
- Abandon the idea of the SSC in the US and instead participate in construction and exploitation of a hadron collider in the LEP tunnel.

In the first draft Panofsky's personal preference was for the third option. The subsequent draft stated that construction of the SSC should be carried out within the national US program but one should seek participation from foreign countries at all levels of R&D, design, engineering and construction. It also advocated internationalization of SSC Laboratory during its operational phase. It was unlikely that the second option, if successful, would reduce significantly the US part of the SSC cost. According to Soergel's Congressional testimony, 20% (by cost) of the total components for HERA were contributed by countries other than Germany, mainly Italy, France and Canada. The actual cost offset was less than that, however, because of additional management complications, such a scenario required and because of loss of competitive bidding. On the other hand, success in that alternative might be important politically, providing an outside endorsement of the value of the SSC.

During the mid-1980's there was a considerable effort both to "spread the word" about the SSC outside of the US and to involve physicists from abroad in various SSC activities. Numerous visits to foreign universities and laboratories were undertaken on a relatively informal basis by the members of the CDG, of the BOO, and of the HEP community in general. One of my principal responsibilities as Deputy Director for External Relations was to oversee, nurture and participate in these activities. During the CDG days I probably made about ten trips to Japan, a large fraction of them as part of official US delegations once the SSC received official endorsement from the executive branch. Contacts were also being made with industrial concerns abroad; thus, for example, Tigner made a trip to Japan to visit Japanese companies with a potential interest in the SSC.

In parallel, we made efforts to include scientists from abroad in US conferences and workshops on the SSC, and in different committees and task forces dealing with SSC matters. Thus, for example, there were European and Japanese physicists on the Detector Cost Model Advisory Panel as well as on the Advisory Committee for the Generic Detector R&D. The latter had fewer political barriers, since the generic nature of the efforts discussed was such that these efforts would find application in many different projects. On the SSC Users' Executive Committee, 4 of the 11 members were scientists from Europe, Japan or Canada.

15. Presidential Approval — "Throw Deep"

The excellent review of the Conceptual Design Report convinced DOE at the level of the Office of Energy Research (i.e. Trivelpiece) that the SSC was very likely a viable and well-understood project. It was also clear that it would be difficult to obtain an increase in the level of SSC R&D funding without the go-ahead for construction. Thus, after completion of the CDR, the efforts were intensified to convince the DOE Secretary to endorse SSC construction. Many members of Congress were strongly in favor of the SSC, but the cost of the project was so high that to be built it needed a strong push from the Administration. Thus, for example, on April 11, 1986, 91 Members of Congress signed a letter to the President urging him to support the SSC [106]. Congress was unwilling to keep doling out the R&D money without some positive action from the Administration that they would go ahead with it [107]. The House Appropriations Committee in its FY87 appropriation language stated: "The Committee is concerned by the lack of commitment by the Administration with regard to the Superconducting Super Collider." It reduced the HEP appropriation by US$27 million from the President's budget request, as a way of putting pressure on the Administration to make a decision.

There was a concerted effort by the SSC proponents to convince Secretary Herrington to make the SSC a high priority within DOE. Herrington was very close to Reagan and his opinion would carry significant weight with the President. Vigorous activism was needed since the opposition to the project from scientists in other fields was intensifying. The Association of American Universities (AAU), an organization composed of some 50 presidents of the nation's leading universities, approved a letter to Herrington urging him to back the project [108]. A number of prominent scientists and industrialists[h] sent letters to the key people in government arguing for the SSC approval. On the whole, the mood in the country at that time was still relatively favorable toward the SSC. A *Los Angeles Times* editorial stated: "The Super Collider would provide the next breakthrough in our understanding of matter. That alone is a very good reason to build it... What the scientists are proposing should rank high on our list of national objectives [109]." An important meeting was a lunch Herrington had with several well-known scientists[i] to solicit their views about the SSC.

The first review of the SSC by the Domestic Policy Council took place in December 1986; another one in the same forum was held shortly before Christmas. Herrington and Trivelpiece presented the SSC case. The meetings were inconclusive and the decision was postponed till January 29, when Reagan presided over the meeting [110]. Trivelpiece gave the main presentation, about the history and goals of HEP and the connection between that field and technology. As expected, there were concerns about budget implications for other sciences. This was at least partly defused by proposing the first major expenditure of US$348 million only in FY89. Subsequently US$600–700 million would need to be spent annually for five years but that would be during the next Administration.

After the presentation and a question period, Reagan recalled an anecdote. The Oakland Raiders' well-known quarterback, Ken Stabler, was reputedly asked the meaning of a poem by Jack London that London considered as his credo:

> I would rather be ashes than dust
> I would rather that my spark
> Should burn out in a brilliant blaze
> Than it should be stifled in dry rot
>
> I would rather be a superb meteor
> Every atom of me in magnificent glow
> Than a sleepy and permanent planet

The credo meant "Throw deep," according to Stabler, and Reagan directed Herrington to follow that motto by going ahead with the SSC [111, 112]. The next morning, the White House formally announced its approval of the SSC.

16. Site Selection

Shortly after Reagan's decision to "throw deep," Secretary Herrington announced on February 10, 1987, the mechanics of site selection process. The initial

[h]Some of the industrialists who wrote were John Akers of IBM, Douglas Danforth of Westinghouse Electric, Edward Jefferson of DuPont and Roger Smith of General Motors [111].

[i]Some of the scientists attending were Solomon Bucksbaum, Roy Schwitters, H. Guyford Stever, Charles Townes and Steven Weinberg (Ref. 111 and Schwitters' notes in *Fermilab Archives*).

step was establishment of an SSC Site Task Force, reporting to the Director of DOE's Office of Energy Research, and chaired by Wilmot N. Hess, Associate Director of the Office of High Energy and Nuclear Physics, who replaced Jim Leiss in that position.[j] The Task Force issued on April 1, 1987, an "Invitation for Site Proposals for the Superconducting Super Collider (SSC)." This 76-page document [113] described the SSC, spelled out the requirements for site proposals, outlined the site selection process with the timelines for different steps, and described all the information that had to be provided. The technical aspects of the invitation were based very closely on the work done in the conventional facilities area by the CDG, and several CDG physicists participated in its preparation, but as individuals rather than members of the CDG. Tigner wanted to make sure that the CDG would not be involved in what might be a politically charged process. The underlying principle of the selection process, as stated in the invitation, was: "The goal in evaluating sites is to select a site that will permit the highest level of research productivity and overall effectiveness of the SSC facility at a reasonable cost of construction and operation and with minimum adverse impact on the environment."

The schedule for the process, outlined in the invitation, was very tight. The deadline for submission of proposals was August 3, 1987, 2:00 p.m. (later extended to September 2, 1987). The first filter of the proposals was to be conducted by DOE with the goal of eliminating proposals that did not meet the requirements in the invitation. The remaining proposals would then be handed over in September 1987, to a broadly based committee, selected by the National Academy of Science (NAS) and the National Academy of Engineering (NAE), which would evaluate them based solely on provided written documentation (i.e. no site visits). That committee was to produce a Best Qualified List (BQL) by December of that year; the list was to be unranked and it was up to the committee to decide on the number of sites making that list. Subsequently, a DOE-appointed group would conduct visits of the sites on the BQL and perform additional detailed evaluation of the BQL proposals. These evaluations would be based on both technical and cost information, the latter being the life cycle cost (LCC), i.e. construction cost plus operating cost for a 25-year period. Based on this information, the Secretary of Energy would identify the preferred site. When all the environmental requirements were satisfied, the site would become the final selected site.

The invitation was rather specific as to the format of the proposals and the information that needed to be provided. The six general criteria, in order of importance, were: Geology and Tunnelling, Regional Resources, Environment, Setting, Regional Conditions, and Utilities. It was estimated that the proposal length would be about 200 pages.

The proponents were required to donate the proposed site to the federal government and were encouraged to provide financial and other incentives. The invitation stated: "The site proposed must be entirely located in the United States of America. . . ." The transfer of all the required land (about 16,000 acres) was to be completed by April 1, 1990. Preference was expressed for locating the ring in a horizontal plane but a tilt up to 0.5° would be permissible. The water and utilities requirements were similar to those in the original "Site Requirement" document produced by the CDG in 1985.

A great deal of interest was generated in response to this invitation all over the country [114]. The funds spent by different states on proposals were reported as ranging from US$300,000 to about US$10 million. Several states published glossy brochures extolling the virtues of their sites.[k] However, there were concerns and complaints, expressed by a number of states, that the schedule was too tight, the four months allocated for preparation of the proposals being insufficient. In addition, objections were raised to the provision that the financial incentives in the offer would influence the decision. It was argued that this provision favored larger states. Sen. Pete Domenici from New Mexico introduced an amendment, enacted on July 11, 1987, which required DOE to use only qualities of the sites as the selection criteria. DOE amended the invitation to conform with this act and extended the proposal deadline by one month [115].

[j] Jim Leiss retired toward the end of 1985 and for about a year Bill Wallenmeyer held that position on an acting basis while continuing as Director of the Division of High Energy Physics.

[k] I have seen glossy brochures produced by Arizona, Colorado, Illinois, Ohio and Washington.

DOE received 43 proposals. Seven of them were judged as not meeting the basic qualification criteria set forth in the invitation. One of those was the trans-border proposal submitted by New York State jointly with the province of Quebec, it not being located entirely in the US. In addition, the Wallkill Valley, New York, proposal was withdrawn in October 1987. Thus 35 proposals were transmitted on September 27, 1987, to the NAS/NAE committee for evaluation.

The NAS/NAE committee had 21 members [116, 117] and was chaired by Edward A. Frieman from the Scripps Institution of Oceanography and the University of California, San Diego. The membership was very diverse, including high energy physicists (both experimental and theoretical), accelerator experts, high level university and national laboratory officials and representatives from industry, the Army Corps of Engineers and public institutions. The Academies provided staff support under the leadership of Raphael G. Kasper, who served as Project Director. In addition, the committee had the benefit of comments from a number of other colleagues at different subgroup meetings.

The committee established seven working groups, each one focusing on one of the six technical criteria and the costs. Each working group was composed of committee members with specific expertise in the area of focus of that working group. Besides the meetings of the working groups, the whole committee met as a whole on three occasions, June 30–July 1, October 8–10 and November 13–14, 1987. At the committee's final meeting there was a full discussion on the strengths and weaknesses of each of the 35 proposals. No ranking of chosen sites was made and no *a priori* decision was made as to the appropriate number of sites to be placed on the BQL. The final BQL included all the sites on which the committee consensus for inclusion was reached.

The report summarizing the committee's deliberations and conclusions was forwarded to DOE on December 24, 1987, and published later by the National Academy Press [118]. It listed the eight sites being put on the BQL and for each gave a short description of its strengths and weaknesses, as seen by the committee. These sites were, in alphabetical order: Arizona/Maricopa, Colorado, Illinois, Michigan/Stockbridge, New York/Rochester, North Carolina, Tennessee and Texas/Dallas–Fort Worth [119]. The DOE Energy System Acquisition Advisory Board accepted this list without any modifications on January 15, 1988. On the same day, the New York site was withdrawn by the proposers because of significant local opposition.

The Illinois proposal presented a special situation. The potential usage of the Fermilab accelerator complex and other laboratory infrastructure could offset partially the cost of the SSC construction. DOE had performed a study of the technical feasibility and potential cost savings of using the Tevatron complex as the injector for the SSC. The report concluded that it was reasonable to expect that the Tevatron complex could meet the SSC requirements. It was not clear, however, whether that complex could meet the SSC reliability criteria and what fraction of the existing components would have to be replaced.

The next step was a detailed evaluation of all the BQL sites by the DOE Task Force. The evaluation started with week-long visits to all the sites by the staff of a DOE contractor, RTK, with a DOE representative, to gather additional data required for the eventual EIS. Subsequently, between April and June, the Task Force visited each BQL site and then assigned a ranking on each technical subcriterion for each proposal. At this time the Task Force also updated the life cycle cost analyses based on additional data submitted by the proponents, the information obtained during the site visits, and the geotechnical investigations of the sites. The costs ranged from US$10.7 billion to US$11.5 billion for the six non-Illinois sites. The LCC for the Illinois site was estimated to be US$11.4 billion if no credit was given for the Fermilab facilities, and between US$10.4 billion and US$10.9 billion if credit was given, with the range being due to uncertainties in estimating the suitability and lifetime of the Tevatron complex.[1] A very different cost estimate was obtained by Illinois site advocates, who argued that siting the SSC at Fermilab would save US$3.28 billion over the lifetime of the project [120]. Finally, the Task Force determined that all the BQL sites met the

[1]In an April 27, 1987, letter to Wallenmeyer, responding to his inquiry, Tigner stated that he could not see any reasons why Tevatron would not be suitable as the SSC injector. Any additional modification costs were said to be small compared to the estimated cost of the injector systems of US$341.4 million (FY88 $).

equal opportunity requirements in the original invitation [121].

Because of the many complaints received subsequently about procedures for the process, the General Accounting Office (GAO) was asked by Congress to assess the fairness of the process by examining a number of different issues. GAO interviewed most of the key participants in the process and recommended in its report [122] dated January 1989 that "the Secretary of Energy ensure for any future site selection process similar to the SSC that potential site proposers be given the maximum information possible in the invitation about the relative importance of the selection criteria." DOE responded that the relative importance of different criteria was clearly defined in the invitation.

The report of the Task Force, providing the evaluations of the BQL sites and their LCC estimates, was forwarded by Hess to Robert Hunter, Director of DOE Office of Research, on November 7, 1988. Shortly afterward, Secretary Herrington announced the choice of the Texas/Dallas–Fort Worth site as the preferred site [123–125]. In December of that year, DOE issued the final EIS for the SSC, and on January 18, 1989, finalized the decision to site the SSC in Texas [126].

The fairness of the site selection process has been intensively debated in the subsequent years, with accusations that Texas was selected because of its political clout and its planned contribution to offset the cost of the SSC [127]. Claims were made that some negative aspects of the Texas site were ignored, such as fire ants [128]. The financial offer (US$1 billion) was not part of the official proposal but it was well known that Texas, if selected, would make a contribution of this magnitude. I was a member of the NAS/NAE panel and saw no evidence for the above allegations. The Texas site clearly belonged on the BQL and it ranked very high in the minds of many panel members. But it was clear that states tried to exercise as much political pressure as possible [129]. I have no inside knowledge as to how much impact this had on the final decision.

But there were flaws in the site selection procedure. The most important one was ignoring the sociological and practical implications of starting a new laboratory on a green site, much more important than the issue of costs. Such a site meant starting from scratch in all respects, the most important being recruitment of staff and creation of technical support facilities. Furthermore, a green site would provide no fiscal flexibility in case of construction stretch-out. All of the costs at the laboratory had to be charged against the construction project, since there were no other laboratory activities to which one could shift the idled personnel.

Another deficiency was the use of the life cycle costs. Even though cost differences in downstream years were discounted, this procedure underestimated the importance (if only political and emotional) of the initial construction cost. It was the initial construction cost that would determine how fast the SSC could be built and how vulnerable it would be to potential cancelation during its construction. An estimate of construction and operating costs separately, possibly with a guideline to the Task Force regarding the relative weight to be allotted to each, would have been much more appropriate.

Finally, the decision not to consider the transborder site in New York State had long range repercussions. Rep. Sherwood Boehlert (R-NY) stated formally his objection to this decision in a comment [130] appended to a House authorization bill: "Our pleas for international cooperation are going to sound awfully hollow if we turn down this opportunity to reduce the cost of this hugely expensive project." Boehlert later became one of the strongest and most influential opponents of the SSC in Congress. Undoubtedly the site decision influenced his stance but it has also been widely reported that Krumhansl (who was in Boehlert's district) played a large role in turning him against the SSC.

17. Post-CDR Technical Activities

The technical work at the CDG continued at an intensive pace after the CDR was finished. The main focus was on extending the studies made for the CDR and on the R&D for long lead time critical path hardware systems such as superconductor, magnets and cryogenic systems. Here I only mention some highlights.

After magnet type selection was made, further development of the baseline dipole design was a shared responsibility between BNL, Fermilab, LBL and CDG. LBL focused on cable development and short magnets, BNL on cold mass production for long magnets, and Fermilab on cryostats and magnet testing. The CDG provided direction and coordination of this effort. This was quite challenging in light of these geographically highly dispersed efforts. Nominally Vic Karpenko, as Magnet Division Head, had the principal responsibility in this area, but his experience in a somewhat different culture at Livermore generated some difficulties in his interactions with the labs. A scheme adopted by Tigner was to have a "Gang of Four" — Tigner, Goldwasser, Limon and Karpenko — assume joint management responsibility and visit periodically all the labs. Ned Goldwasser was the former Deputy Director at Fermilab who was spending two years at the CDG as Associate Director, starting in September 1986. This scheme was a temporary solution lasting about a year, until late in 1987, when John Peoples from Fermilab was recruited to take over as Magnet Division Head for a year (he was then replaced by Tom Kirk). Peoples took an experimental approach and continued and enlarged a program of producing a number of magnets, each with a somewhat different design. That strategy, coupled with extensive instrumentation on the magnets, gave much better understanding of potential problems. By the end of 1988 a dipole coil configuration was developed which was believed to provide optimum magnetic characteristics. It was anticipated that the R&D baseline magnet design would be obtained by the end of 1989 [131].

There was also extensive work in other areas, such as bore tube corrector development, design and fabrication of the required tooling, cryostat design and testing, and accelerated life tests of the magnets. There was also work on collider quadrupole design and on conceptual design of HEB and injector magnets. Another important activity pursued in parallel was the development and initiation of the magnet industrialization program. The cable R&D focused on optimization of the superconducting material, improving the critical current operating margin, and on technology transfer to the cable industry. A cabling machine was designed, produced in industry, and after successful tests shipped to a potential cable manufacturer — New England Electric Wire. In October 1988, the machine made cable successfully at its design speed. Cable sufficient for six dipoles was produced during December 1988 and January 1989.

The studies of desorption continued both at the Synchrotron Light Source at BNL and in a collaborative experiment at KEK. There was also work on neutron backgrounds in the tunnel and experimental halls, both via simulations and experimental measurements at the Tevatron. Understanding the aperture requirement continued, with theoretical work and with focused experiments at the Tevatron. [132] There was additional work on various lattice issues that were site-dependent. Similarly, more detailed work was commenced in the area of conventional facilities, specifically on the design of the experimental halls, on determination of the tunnel cross section, injector concepts and footprint studies. It was clear, based on the initial detector designs, that the experimental halls would need to be larger than was suggested in the CDR.

FY89 appeared to be the first year with a significant increase in SSC R&D funding — US$100 million in the President's request (US$95 million were eventually appropriated). About half of that sum would be allocated to the magnet program, with US$16 million of that slated for the industrialization activity. There were also plans to increase significantly generic detector R&D and initiate preliminary engineering and design (at a US$16 million level).

18. Starting the SSC — Getting Congressional Approval

Obtaining a site for the potential location of the SSC still left two major issues to be resolved before SSC Laboratory could become a reality. The first one was obtaining Congressional appropriation of the initial construction funds. The second one was creating a management organization for SSC Laboratory.

Reagan's endorsement of the SSC had an important symbolic value but, from the practical point of view, did not represent a major step toward making the SSC a reality. He was approaching the end of his term and his influence on subsequent appropriations was waning. As a matter of fact, Congress did not appropriate any construction funds for the SSC for either FY88 or FY89. Thus our challenge was to persuade Congress of the value of the SSC and to convince the future Administration (as well as the subsequent ones) to pursue the SSC [133].

The overall funding climate in 1987 was already significantly different from that in 1983. There was a deeper concern about the budget deficits and growing realization that the "credit card" financing of federal expenditures could not go on forever. Many scientists in other disciplines began to worry seriously that construction of the SSC would drain funds away from their own fields.[m] Thus substantial opposition began to develop in scientific circles against the SSC even though there was very little evidence that at this time the SSC had a negative impact on other basic research; the SSC might even have been pushing funding up in other areas [134] (for example, NSF was embarking on a budget-doubling five year program [135]). The expected foreign commitments to SSC construction were not materializing and there was a strong sentiment in the House to postpone construction approval until definite agreements were reached on cost sharing [136]. It was realized that some of the support for the SSC was rather soft and could disappear once the site was selected [137]. When the NAS committee reduced the number of possible SSC sites to seven, many previous backers (Rep. Boehlert is the best-known example) became either neutral or even anti-SSC. Furthermore, two new developments fueled the opposition.

The first of these was increased publicity for potential construction of the LHC in the LEP tunnel at CERN. In his April 1987 testimony before the US House Science, Space and Technology Committee, Schopper still advocated a 5–6 TeV collider in the LEP tunnel, claiming that it could be built for 1/3 to 1/4 of the SSC cost [138]. The situation in Europe, however, was far from rosy at the time and there were many pressures to reduce HEP funds. Great Britain's Kendrew report was recommending scaling back its high energy budget [139] by 25%; other countries were rumored to want to follow suit. The CERN Council set up a management committee, with Anatole Abragam as chair, to evaluate the CERN future under various funding scenarios. At the same time, a Long Range Planning Committee was appointed, with Carlo Rubbia as chair, to look at different options for a new post-LEP accelerator facility at CERN. Preliminary reports [140–142], presented to the Council by these two committees in June 1987, discussed the possibility of a hadron collider in the LEP tunnel, but it was far from a certainty and one certainly did not know at that time its parameters, its cost, or its potential time scale. It was very likely that if the US would proceed expeditiously with the SSC, CERN might focus on an electron–positron collider as its future facility. Nevertheless, in the US, the LHC was being put forward by some as an obvious alternative to the SSC at one-quarter of the cost.

The other event was the exciting new discovery in the field of superconductivity [143]. Late in 1986, Bednorz and Muller, working at IBM Laboratory in Zurich, discovered superconductivity in previously unexplored materials. Lanthanum–barium–copper oxide was shown to become superconducting up to temperatures over 30 K. Shortly afterward materials were found with even higher critical temperatures [144–146]. Chu and his collaborators at Houston, working with a group at the University of Alabama, observed stable superconductivity at temperatures as high as 95 K, significantly above the boiling temperature of nitrogen at 77 K. The potential significance of these new discoveries was not lost on either the proponents or the opponents of the SSC [147, 148].

In response to these developments Tigner convened a CDG Task Force to explore potential applications of this new technology to the SSC and to understand the challenges that had to be overcome before it could be utilized [150]. The Task Force identified a large number of problems that had to be solved before practical applications could be realized, and estimated that even under optimistic assumptions the use of these materials could reduce the overall SSC cost by only 3%.

There were significantly different claims made, however, by some of the SSC opponents. James Krumhansl, serving at that time as the President-elect of the APS, in his testimony [151] to Congress on August 7, 1987, said: "In regard to the SSC these superconducting materials offer the possibility of obtaining much higher magnetic fields than those now planned... it might be possible to achieve the projected design energy with a ring perhaps 10 miles rather than 50 miles in circumference... a pause in

[m] A good example is Krumhansl's letter to Herrington [152]. From the very beginning, however, the HEP community argued that the SSC would have to be built with "new" money and hopefully stimulate increased support for all basic research; a good example is an editorial by J. D. Bjorken, *Phys. Today* **41**, 136 (1987). This was also the DOE position; its Energy Research Advisory Board (ERAB) stated, speaking of the SSC: "It cannot be undertaken without a multibillion dollar incremental commitment to basic science over the next decade." On the whole, basic research fared reasonably well during the Reagan administration; see C. Norman, *Science* **231**, 785 (1986).

the commitment to SSC and its siting should be given serious consideration." In a letter of February 19 to Secretary Herrington he was even more optimistic [152]: "They unquestionably have the potential to save billions of dollars in construction and operation of particle accelerators like SSC. Because they are easily fabricated, I have little hesitation that they will be brought to technological usability in three to five years."

Philip Anderson included in his testimony [153] at the same hearing: "It is possible to anticipate that one can either omit Helium cooling or reduce the size of the ring or both within a matter of 2–3 years." But he was not completely self-consistent, stating also: "Any good engineer will tell you that the last thing in the world you want for your large engineering project to depend on is truly innovative technology.... The delays and mistakes inevitable in new technology are just too common."

To provide an objective assessment of the potential benefits of high temperature superconductors and the possible time scale, the NAS convened a panel with a charge to provide a Research Briefing on High-Temperature Superconductivity. This panel, chaired by John Hulm of Westinghouse, concluded in its 1987 report [154]: "The complexity of the materials technology and of many of these applications makes a long term view of research and development essential for success in commercialization. The infectious enthusiasm in the press and elsewhere may have contributed to premature public expectations of revolutionary technology on a very short time scale."

In parallel, on April 23, 1987, Alvin Trivelpiece requested the Basic Energy Sciences Advisory Committee (analog of HEPAP in materials science) to study the possible use of these materials in particle accelerators. The panel convened for this purpose [155] was chaired by Albert Narath of AT&T Bell Laboratories and reached conclusions similar to those of the NAS Committee: "On the basis of rather optimistic assumptions, it appears possible to develop the necessary technology and demonstrate acceptable filamentary conductors in about 4 years. However, the panel believes that under realistic conditions an additional 8 years would be needed before full-scale production of magnets equivalent to the SSC benchmark could commence." It became clear already in early 1989 that even these "realistic" panel reports were much too optimistic [156–158]. And today, some 22 years and many man-years of intensive scientific and engineering effort later, we are still far away from any significant large scale application of those materials.

Even though objective analyses showed that the new superconducting materials were not ready for applications to accelerator magnets, the seeds planted by the SSC opponents found a resonance in the popular press. Numerous articles and editorials appeared, with titles like "New Findings Could Make Supercollider Obsolete." *The New York Times*, in an editorial [159] titled "Super Hasty on the Supercollider," cited Philip Anderson's claim that "new materials could make magnets 10 times more powerful... permitting reduction of circumference from 50 miles to 5 miles." It is not surprising that such reports made an impact on the US Congress. Undoubtedly influenced by these developments and the associated publicity, the Senate Budget Committee requested the Congressional Budget Office (CBO) to prepare a report evaluating the risks and benefits of building the SSC. Their report [160], published in October 1988, formally expressed no recommendations but only options for Congressional actions. But it treated highly futuristic possibilities like an e^+e^- collider in the TeV range and a hadron collider in the LEP tunnel on the same footing as the SSC. Thus it gave credibility to the notion that there were real alternatives to the SSC in the same time frame. DOE made an effort [161] to rebut some of the CBO assessments but they did not appear to be 100% effective.

The advocacy for the SSC was weakened by the departure from the Administration of two key SSC supporters, Keyworth and Trivelpiece. Keyworth [162] left at the end of 1985 and his position was not filled for a year, until William Graham, former NASA Administrator, was confirmed by the Senate. Trivelpiece, so influential in getting the SSC to this point, left DOE in April 1987, to assume the position of Executive Director of AAAS [163]. He had established a good relationship with Congress and thus his departure made subsequent interactions with Congress more difficult. During the next five years the position of DOE OER Director was held a large fraction of the time, on an acting basis, by James Decker, a career DOE official, since invariably there would be a long time interval between the resignation of one director and the confirmation of his or her successor.

These concerns about the SSC design and a proposed steep rise in the future annual SSC budgets (US$363 million request for FY89) contributed to Congressional hesitancy in starting construction of the SSC. The House Committee on Science, Space, and Technology authorized [164] funds for SSC R&D and construction for FY88, FY89 and FY90 (US$878,000 total) on October 15, 1987, but only R&D funds were appropriated for FY88 and FY89 — US$33 million and US$95 million, respectively. The significant increase for FY89, the words associated with it, and the House authorization gave hope, however, that Congress might approve SSC construction in the following year. The Report of the Senate Energy and Water Development Appropriation Bill, 1988, stated [165]: "The Committee provides $35,000,000 for the SSC in FY88. This includes funds to continue the SSC R&D program, support site selection studies, and undertake other necessary activities in support of the project. This will keep the design team together and support other non-site activities for one more year while the administration explores means of funding this project."

19. Starting the SSC — Management Issue

The other very important issue was management of SSC Laboratory. Examining the precedents, one could see three distinct modes as possibilities:

- Management by a university or a university consortium. The prime examples at that time of this mode were SLAC, LBL and Fermilab.
- Management by an industrial concern, examples being Sandia and Oak Ridge.
- Formation of a new government entity like NASA or NIST.

There was a general concern in the government circles that the SSC construction was simply too big a task to entrust to academics. As a result there was some sentiment to decouple the construction and operation of the SSC: the SSC would be built by an industrial venture or a government entity like the Army Corps of Engineers and then its operation turned over to another organization charged with subsequent management of the SSC. Panofsky argued forcefully and persuasively against this choice on the grounds that an accelerator laboratory is a "living" entity, constantly being upgraded and modified as new needs arise or new technologies become available. Thus the experience gained in the construction phase is essential for subsequent operation.

The HEP community wanted the management contract to be given to URA, for both construction and operation. URA submitted an unsolicited bid for such a contract on March 2, 1987 (and another, somewhat modified version on February 22, 1988), prepared under the direction of Ned Goldwasser. That proposal was never acted on by the DOE [166]. To avoid concerns about a potential conflict of interest between Fermilab and SSCL, early in 1988 URA had reorganized: a Board of Trustees was to be responsible for corporate URA affairs and two separate Boards of Overseers, one for Fermilab and one for SSCL, would be responsible for laboratory oversight. On June 1, 1988, McDaniel, in his capacity as chair of the SSC BOO, wrote to Joseph Salgado, Undersecretary of DOE, urging that "DOE take immediate action to set up an SSC organization where management responsibility is centralized in some entity drawn from the scientific community." Salgado's response of July 22, 1988, stated: "We are now developing a plan of action for establishing a permanent management structure for the SSC" [167].

On August 3, 1988, DOE announced an open competition for management of SSC design, construction and operation [168, 169]. The actual Request for Proposals (RFP) was not issued till August 22, with a preproposal conference held on September 8. November 4, 1988, was the deadline for proposal submission; the selected contractor was to take over SSC management in FY89. There were two qualification criteria the proponents had to satisfy: they needed to have had recent significant involvement/association and experience with the US particle physics community and be willing to accept the contract regardless of the location chosen for the SSC. Ed Knapp, URA President at that time, hired Doug Pewitt, Keyworth's deputy in the early 1980's, and Francis Allhoff from EG&G, as the principal consultants to lead the URA effort of proposal preparation.

This process was organized and directed by URA and the BOO, with significant technical support from scientists in the CDG and the labs but as individuals. Formally, the responsibility rested with Ed Knapp as the URA President. URA had made a decision to team up with industrial concerns and submit a partnership proposal. It would be the prime contractor and one or more industrial concerns would be chosen as subcontractors to perform a substantial part of the work. This concept was pushed by Pewitt, with the argument that this would increase the political clout of the URA/SSC team, both in getting the contract and in subsequently getting the Congressional go-ahead.

In September 1988, URA invited several interested companies to make presentations in Washington.[n] EG&G and Sverdrup were eventually chosen as proposed subcontractors to be included in the proposal. EG&G had extensive experience as a DOE contractor based on their management role at the Nevada Test Site and Idaho Engineering Laboratory and their support operations for NASA's Kennedy Space Center. Their main asset for the SSC appeared to be the ability to provide professional support personnel on short notice. Sverdrup was a company with previous experience in construction of large, advanced technology facilities, such as the TRIDENT Support Facilities at King's Bay, Georgia, and the Space Shuttle Launch Complex #6 at Vanderburg AFB, California.

Time was short, so the proposal had to be written very hurriedly. The BOO established the SSC Oversight Committee, chaired by Martin Blume from BNL, to oversee writing of the proposal. The close-to-final draft was circulated to the full BOO in October 20, with a recommendation that the BOO approve the proposal. The time scale was so abbreviated that two of the members did not even have a chance to read the proposal when it was forwarded [170] in October 26 by McDaniel to John Marburger, President of the State University of New York, Stony Brook, and chair of the URA Board of Trustees, with a recommendation that the Board accept the proposal and submit it to DOE.

The proposal submitted to DOE [171] identified four individuals for the key personnel positions which were defined in the DOE-issued RFP:

- Roy Schwitters, Professor of Physics at Harvard, was designated as SSC Director. He held a leadership role in the very successful SPEAR experiment at SLAC and subsequently was one of the co-leaders of the CDF group at Fermilab.
- Helen Edwards was selected as the head of the Accelerator Systems Division. She was serving in that role at Fermilab and was one of the leaders in the Tevatron project.
- Bruce Chrisman was designated as the head of Laboratory Administrative Services. Chrisman, a high energy physicist by training, was Associate Director for Administration at Fermilab and previously Vice President for Administration at Yale University.
- Robert Robbins was designated as the head of the Conventional Construction Division of SSC Laboratory. He was then Vice President of the Sverdrup Corporation and Manager of Operations for Sverdrup's 850-member Central Group.

The RFP actually defined four other key positions: Deputy Director, Project Manager, Head of Magnet Systems, and Head of Physics Research. Maury Tigner, Director of the SSC CDG, was designated in the proposal as Deputy Laboratory Director but no detailed information (e.g. a CV) was included for him like for the other four positions. Tigner's specific duties were not defined in the proposal except to say that he would play a key role in the SSC/CDG-to-SSCL transition. There were no designees for the other three key positions.

The proposal was submitted to DOE on November 4, 1988. No one else made a bid. Long and painful negotiations followed regarding the division of responsibilities between URA and DOE. DOE wanted to have much more control over all the decisions than was customary [172]. The contract was finally signed on January 16, 1989, two days before the site was officially finalized. The Superconducting Super Collider Laboratory could now be officially launched once Congress appropriated construction funds.

These decisions were crucial to the future of the lab and, not surprisingly, were controversial. One question is whether teaming up with industrial partners was a wise or necessary decision. Marilyn Lloyd, a Representative from Tennessee and the chair of the House Science, Space and Technology Committee, wrote to Joe Salgado, Undersecretary of DOE, complaining that the terms of the RFP were biased in favor of URA [173]. So industrial partnership might

[n] Nine different companies made presentations at a meeting held in URA offices in Washington on Sep. 8 and 9, 1988.

have been needed to satisfy Congress. The fact that one of the consultants, Allhoff, was an employee of EG&G raised concerns about a potential conflict of interest. Both Sverdrup and EG&G could have been hired as contractors later on (if their services were necessary). This decision to go into partnership was the beginning of a drift away from the historical HEP culture in laboratory management. Maybe such a change was unavoidable given the scope of the SSC. But the ensuing clash between this new industrial/military culture and the traditional HEP culture contributed to many subsequent SSC difficulties. Later on, the SSCL was most faulted in the area of documentation and lack of adequate cost and schedule systems; it was unfortunate that the need for expertise in this area was not anticipated in choosing industrial partners.

The other obvious question had to do with the selection process and the choice of the top SSCL personnel. The search process for the Director is described in the URA M&O proposal submitted to DOE [174]. It states that the planning process began in May 1988, when a list of attributes desired in a Director was drawn up and a charge was formulated for the Search Committee. At the August 20, 1988, BOO meeting, each member of the Committee agreed to interview eight or more persons by phone and report on their views at the subsequent meeting. 115 persons comprising "prominent physicists, statesmen of science, past government administrators, laboratory directors, university presidents, accelerator builders, and industrialists" were interviewed and the results reported at the August 30 BOO meeting. 58 candidates were suggested; the ensuring discussions reduced the list to 11 and then to 5. A secret ballot was then taken, with the candidates being ranked. There was significant preference for three of them and they were acceptable to all present. Another ballot was taken, with these three ranked in preferred order; Roy Schwitters emerged as the top-ranked person, and his name and those of the other two top-ranking candidates were submitted to the Executive Committee of the Board of Trustees. There were no interviews with any of the top candidates. The Executive Committee approved this ranked list by acclamation at its September 1 teleconference meeting [175]. The full Board approved the nomination on September 8, 1988, and shortly afterward Schwitters accepted the position.

I would like to expand on, comment on and clarify the above description of the selection process. My comments are based on the BOO and Search Committee minutes, Boyce McDaniel's personal notes, correspondence and other relevant records, and discussions with several former BOO or Search Committee members. At the May 7–8, 1988, BOO meeting referred to above, Panofsky stressed the importance of having a search committee in place as soon as possible and Trilling presented a suggested procedure for selection of the SSC Director. It was similar to typical search processes, with advertisements, invitations for applications and nominations, and broad consultations. In spite of the expressed urgency of proceeding with the establishment of the search committee, the only relevant BOO action at that meeting was the appointment of the Search Procedures Committee, with George Trilling as chair [176]. The next meeting was scheduled for three-and-a-half months later, on August 19–20.

At that meeting Trilling distributed a sheet containing "Desirable Attributes of SSC Director" and "Proposed Charge to Director of Search Committee" and suggested several individuals for the Search Committee. This list of attributes and the charge were adopted by the BOO pretty much verbatim at that meeting (rather than at the May one). On the grounds that there was very little time available to prepare the proposal, the BOO established itself as the Search Committee and for that purpose excused Trilling and Schwitters (as potential candidates) and invited Jerome Friedman, Frank Sciulli and Martin Walt (from Lockheed Missiles and Space Co.) to serve on the Committee [177].

The only in-person meeting of the Search Committee took place eight days later, on August 28, 1988, at O'Hare Hilton. The minutes [178] are rather terse and just state that after reports from all members of the Committee on their outside consultations and a following discussion, "a ranked list of three candidates was developed and agreed to by a vote of the Committee." The other two top candidates, in that order, were Nick Samios and Leon Lederman.[o] The only other meeting of the Search Committee was via teleconference two days later, lasting only

[o]Neither the minutes nor the M&O proposal mentions these names or the ranking order. My information comes from McDaniel's notes and conversations with BOO members.

one hour and with 7 of the 12 Search Committee members participating [179]. At the beginning Jerry Friedman reported on his phone conversations with 11 individuals who had worked with Schwitters on the CDF. There was apparently no effort to contact other former Schwitters collaborators or make similar inquiries about the other two top candidates. Afterward, the motion was offered to recommend to the URA Board of Trustees the appointment of Roy Schwitters as the director designate. The motion was agreed to by acclamation.

On the whole, I would classify the search as rather hurried and narrow, and suffering from a lack of openness (the former justified as being due to the very short time frame available.)[p] I have not seen a full list of the 115 individuals reportedly consulted, but from what I have learned, I would conclude that it was not very representative of the HEP community. Such a high number appears somewhat inconsistent with what I was able to learn unless a large majority of the interviews were by Panofsky and/or Treiman. Furthermore, not all interviews occurred early enough to be useful for the selection discussion. One BOO member made his interview phone calls on September 7. Strangely, no current member of the CDG was contacted for suggestions or advice during the process.

Panofsky informed Tigner of the Board's decision on September 4, 1988. The members of the CDG staff were not formally informed until October 30, when the BOO held an Overseers' meeting at LBL. Marburger, McDaniel and Knapp met at that time with the senior staff of the CDG to inform them of the decision. It was not a pleasant event and a lot of acrimony was exchanged, mainly having to do with the search process.

There is no doubt that Schwitters was an outstanding physicist with a lot of experience in detector construction and operation. He was also politically astute and was viewed as having a personality that would probably resonate with Congress and senior government officials. But his contributions were always within the framework of an existing laboratory and on projects considerably smaller; starting from scratch a new, large-scale, multi-billion-dollar accelerator laboratory on a green site might well call for quite different talents and expertise.

One of the seven points in the adopted list of attributes desired in a Director [180] was: "Strong management abilities, including understanding of engineering and construction, excellent judgement and the ability to attract outstanding individuals to the SSC Laboratory." A large number of people felt that Tigner was a more appropriate choice based on that criterion, especially in light of his excellent performance as leader of the CDG. Another criterion in the attributes list was "Ability to promote and defend the SSC in all the appropriate forums, and to work effectively with the DOE." There was a feeling in some circles (BOO, URA and DOE) that Tigner would not meet this criterion well because his primary interest was in accelerators rather than in experimental particle physics and he did not have sufficient breadth of support in the HEP community and in parts of DOE.

Several members of the BOO and of the URA management felt that Tigner's personality was such that he would not be an appropriate Director. Everyone recognized Tigner's technical excellence but there was a feeling that his rather dry general manner would not go over well with members of Congress and high level officials in the executive branch. I do not share that viewpoint. Tigner and I participated in a number of joint meetings with Government officials and reporters and members of editorial boards of newspapers and magazines. Even though Tigner did not come across as a casual, easygoing person, he conveyed a very authoritative, but not threatening or condescending, manner and left no doubt about his mastery of facts and issues. His testimony before Congressional committees, which I heard on several occasions, tended to be on the dry side but was very logical and clear and generally well received by the audience.[q]

I always believed that the duties of the SSC Laboratory Director (especially initially) could not be

[p]The most recent Director's search processes in HEP, first at Fermilab and later at SLAC, were quite formal, lasted several months, and had as committee members individuals not directly associated with the nominating body. I can speak from personal experience as a member of the most recent Fermilab Director's Search Committee. We held about 6–8 two-day meetings, interviewed or heard from a large number of individuals and had half-day-long in-person interviews with the top six candidates.

[q]Rep. W. Carney was quoted in Ref. 52: "Tigner is one of the most informed and imperturbable witnesses I have seen on Capitol Hill."

handled by only one person. One might have investigated splitting the responsibilities for building the accelerator and creating the lab (with associated political interactions). The two-equal-co-directors model was tried successfully by CERN with Adams and van Hove when the SPS was being constructed.[r] A Schwitters/Tigner team might have been the optimum solution for the SSC Laboratory construction. It would have made the CDG-to-SSCL transition significantly smoother and put a truly dedicated hands-on person in charge of the accelerator design and construction. I do not believe that the BOO ever considered such a coequal directorship. Their ideal was a Schwitters/Tigner team but with Schwitters as the Director and Tigner as the Deputy and/or Technical Director. The personalities and the views of the two people involved might not have made a codirectorship possible and it would have been an unorthodox arrangement; it is not clear whether DOE would accept it at that time.[s] Nevertheless, it was worth a try.

Acknowledgments

I would like to express my thanks to a number of individuals who have helped me in the preparation of this article. I am very grateful to Adrienne Kolb, Fermilab archivist, for facilitating my access to the Fermilab Archives, the current repository for a lot of original SSC materials. The SLAC Library personnel, especially Abraham Wheeler, have been extremely helpful in retrieving articles and government documents not readily available on the shelves. I thank Maury Tigner and David Corson for facilitating access to the materials in the Cornell Library Archives. Ezra Heitowit and URA staff were very helpful in allowings me to see several relevant URA documents from the SSC era.

I have profited greatly from a number of interviews, in person or by phone, with many of my current or former colleagues involved in this stage of the SSC project. I would like to acknowledge very informative conversations with Marty Blume, Bruce Chrisman, Jerry Friedman, "Gil" Gilchriese, Ezra Heitowit, J. David Jackson, Tom Kirk, Neal Lane, Chris Laughton, Bob Matyas, John Peoples, Chris Quigg, Jim Sanford, Frank Sciulli, Roy Schwitters, Jim Siegrist, Maury Tigner and George Trilling. I thank them all for their generosity with their time and for their willingness to share with me their recollections.

Finally, I would like to thank Gilchriese, Jackson, Siegrist, Tigner and Trilling for reading and commenting on an early draft of this article. I also thank Lillian Hoddesen, Adrienne Kolb and Michael Riordan, for doing the same on an almost-final draft. The responsibility for any factual errors or erroneous conclusions is, however, entirely mine.

This work was partially supported by the NSF grant PHY-0354945.

Appendix A. Glossary of Abbreviations

AAAS: American Association for Advancement of Science
A&E: architectural and engineering
AEC: Atomic Energy Commission
APS: American Physical Society
BNL: Brookhaven National Laboratory
BOO: Board of Overseers (for the SSC)
BQL: Best Qualified List
CBA: Colliding Beam Accelerator (originally ISABELLE)
CBO: Congressional Budget Office
CDF: Collider Detector at Fermilab
CDG: Central Design Group
CDR: Conceptual Design Report
CERN: European Organization for Nuclear Research (the acronym is for a former name in French: Conseil Europeen pour la Recherche Nucleaire)

[r] The situation at CERN in 1971 was not quite identical to that at the SSC even though there were many parallels. CERN-Meyrin and the "new" CERN were defined as two separate laboratories by the CERN Council in 1969 (*CERN Courier*, Jun. 1969, pp. 162–165) when other sites than the Geneva area were being contemplated for the new machine. Each laboratory was to have its own director-general. When the decision was made to build the SPS across the border in France, this structure was preserved (*CERN Courier*, Feb. 1981, pp. 36 and 37), even though now there was a very strong coupling between the two laboratories: one responsible for construction of a new facility (eventually to be linked with the old facilities), the other for operating a physics program at an ongoing laboratory.

[s] O'Leary's solution for "SSC management problems" from late 1993 — separate contractors, one for design and operations and another for construction and installation — had some features in common with this scheme. Another relevant observation is that the standard practice today for large experiments is to have two cospokespersons with equal overall authority and responsibility. In most cases, this system seems to work well.

DESY : Deutches Electronen-Synchrotron (high energy physics laboratory in Hamburg, Germany)
DG : Director-General
DOE : Department of Energy
DPF : Division of Particles and Fields
DRC : DOE Review Committee
EIS : Environmental Impact Statement
ERDA : Energy Research and Development Administration
FY : fiscal year
GAO : General Accounting Office
HEB : High Energy Booster
HEP : high energy physics
HEPAP : High Energy Physics Advisory Panel
HERA : Hadron Electron Ring Accelerator
IBM : International Business Machines
ICE : Independent Cost Estimators
ICFA : International Committee for Future Accelerators
IG : Inspector-General
ILC : International Linear Collider
ISABELLE : proposed pp collider at BNL; later renamed CBA
IUPAP : International Union of Pure and Applied Physics
KAON : Kaon–Antiproton–Other hadron–Neutrino Factory
KEK : National Laboratory for High Energy Physics, in Tsukuba, Japan (Japanese acronym)
LBL : Lawrence Berkeley Laboratory
LCC : life cycle cost
LEB : Low Energy Booster
LEP : Large Electron–Positron collider
LHC : Large Hadron Collider
MEB : Medium Energy Booster
MIT : Massachusetts Institute of Technology
M&O : management and operations
MOU : memorandum of understanding
MSAP : Magnet Selection Advisory Panel
NAE : National Academy of Engineering
NAS : National Academy of Science
NASA : National Aeronautics and Space Administration
NEPA : National Environmental Protection Agency
NIST : National Institute of Standards and Technology
NSF : National Science Foundation
OER : Office of Energy Research (in DOE)
OMB : Office of Management and Budget
OSTP : Office of Science and Technology Policy
PAC : Program Advisory Committee
PEP : Positron–Electron Project
PETRA : German acronym for a 30 GeV positron–electron collider at DESY
R&D : research and development
RFP : Request for Proposals
RTK : Raymond Kaiser Engineers, Tudor Engineering Company, Keller & Gannon-Knight joint venture
SDI : Strategic Defense Initiative
SLAC : Stanford Linear Accelerator Center
SLC : SLAC Linear Collider
SPC : Scientific Policy Committee
SPS : Super Proton Synchrotron
SSC : Superconducting Super Collider
SSCL : Superconducting Super Collider Laboratory
TAC : Texas Accelerator Center
TMRP : Technical Magnet Review Panel
TRISTAN : 60 GeV positron–electron collider in KEK Lab, Tsukuba, Japan
TRIUMF : TRIUniversity Meson Facility (Canada's National Laboratory for Particle and Nuclear Physics)
UOSSC : Users' Organization of the SSC
URA : Universities Research Association
UTAP : Underground Tunneling Advisory Panel
VBA : Very Big Accelerator

References

[1] The account of the SSC's Texas days will appear in the *Reviews of Accelerator Science and Technology*, Vol. 2 (2009).

[2] For an alternative description of these events, written from a very different vantage point, see: L. Hoddeson and A. Kolb, The Superconducting Super Collider's frontier outpost, 1983–1988, *Minerva* **38**, 271 (2000).

[3] J. Irvine *et al.*, *Phys. Today* **39**, 27 (1986).

[4] L. Hoddeson, The first large-scale application of superconductivity: the Fermilab Energy Doubler, 1972–1983, *Historical Studies in the Physical and Biological Sciences* **8/1**, 25 (1987).

[5] For a detailed history of ISABELLE see: R. P. Crease, Quenched! The ISABELLE saga, in

Physics in Perspective **7/3** (2005), part I, pp. 330–376; and part II, pp. 404–452.

[6] Report of the 1977 Subpanel on New Facilities of the High Energy Physics Advisory Panel, ERDA 77-71, Jun. 1977. The initial BNL proposal, considered and approved by the 1975 Subpanel, was for 200 GeV in each beam. It was subsequently modified in light of scientific and technological developments.

[7] *Phys. Today* **34**, 17 (1981).

[8] P. David, *Nature* **303**, 465 (1993).

[9] G. Keyworth, editorial in *Science* **219**, 801 (1983).

[10] Report of the Subpanel on Long Range Planning for the US High Energy Physics Program of the High Energy Physics Advisory Panel (DOE-RE-0128, Jan. 1982), App. A.

[11] Report of the Subpanel on Long Range Planning for the US High Energy Physics Program of the High Energy Physics Advisory Panel (DOE-RE-0128, Jan. 1982), App. A., pp. 48–55; *Phys. Today* **35**, 51 (1982).

[12] *Proc. Workshop on Possibilities and Limitations of Accelerators and Detectors* (Batavia, Illinois; Oct. 15–21, 1978), ed. D. Edwards.

[13] *Proc. 2nd ICFA Workshop on Possibilities and Limitations of Accelerators and Detectors* (Les Diablerets, Switzerland), CERN Report RD/450-1 (1979), ed. U. Amaldi.

[14] For a comprehensive discussion on the history of the VBA, see A. Kolb and L. Hoddeson, The mirage of the world accelerator for world peace and the origins of the SSC, *Historical Studies in the Physical and Biological Sciences* **24**, 101 (1993).

[15] *Proc. 1982 DPF Summer Study on Elementary Particle Physics and Future Facilities* (June 23–July 16, 1982, Snowmass, Colorado), eds. R. Donaldson, R. Gustafson and F. Paige; *Phys. Today* **36**, 19 (1983).

[16] M. Tigner, in *Proc. 1982 DPF Summer Study on Elementary Particle Physics and Future Facilities* (June 28–July 16, 1982, Snowmass, Colorado), eds. R. Donaldson, R. Gustafson and F. Paige, pp. 50–53.

[17] Report of the 1983 HEPAP Subpanel on New Facilities for the US High Energy Physics Program (DOE/ER-0169, July. 1983), App. A.

[18] Report of the 1983 HEPAP Subpanel on New Facilities for the US High Energy Physics Program (DOE/ER-0169, July. 1983), App. B

[19] M. M. Waldrop, *Science* **220**, 809 (1983).

[20] Memo from S. Wojcicki to members of 1983 HEPAP Subpanel on New Facilities after the Organizational Subpanel meeting in Feb. (undated)

[21] "Dear Colleague" letter from S. Wojcicki to DPF membership (Mar. 11, 1983).

[22] Proposal for a Dedicated Collider at Fermi National Accelerator Laboratory (May 1983).

[23] P. Reardon's letter to S. Wojcicki, May 17, 1983, on the occasion of the Subpanel's meeting at BNL, and the enclosure "Sandatron possibilities at BNL." The estimated additional cost for a 15 on 15 TeV collider was US$883 million ± 250 million.

[24] B. Richter, presentation to the 1983 Subpanel at its meeting at SLAC, May 20, 1983. The statement was made based on a very preliminary study (~10 people for a few weeks), that a 2 TeV e^+e^- collider could be built using existing technology (20 MeV/m gradient) for US$3.29 billion, with power consumption of 400 MW. This did not include R&D, detectors, contingency or escalation.

[25] Report of the 20 TeV Hadron Collider Technical Workshop (Cornell, Mar. 28–Apr. 2, 1983).

[26] *Phys. Today* **36**, 17 (1983).

[27] UA1 Collab. (G. Arnison *et al.*), *Phys. Lett.* **B 122** 103 (1983).

[28] UA2 Collab. (G. Banner *et al.*), *Phys. Lett.* **B 122**, 476 (1983).

[29] UA1 Collab. (G. Arnison *et al.*), *Phys. Lett.* **B 126**, 398 (1983).

[30] Memo from J. Adams to Subpanel members: Some remarks about new facilities for high-energy particle physics research in the USA (Apr. 22, 1983).

[31] Europe 3, US not even z-zero, *New York Times* editorial (Jun. 6, 1983).

[32] Report of the 1983 HEPAP Subpanel on New Facilities for the US High Energy Physics Program (DOE/ER-0169, July. 1983), pp. 5–14.

[33] Report of the 1983 HEPAP Subpanel on New Facilities for the US High Energy Physics Program (DOE/ER-0169, July. 1983), p. 67.

[34] W. Sullivan, Panel of experts deadlocked on next U.S. atom smashers, *New York Times* (Jul. 2, 1983).

[35] Cable from L. Lederman to chairs of physics departments in the US (Jul. 3, 1983).

[36] *Phys. Today* **36**, 17 (1983).

[37] Cover letter from J. Sandweiss to A. Trivelpiece, Report of the 1983 HEPAP Subpanel on New Facilities for the US High Energy Physics Program (DOE/ER-0169, July. 1983), App. A., pp. i–iii.

[38] M. M. Waldrop, *Science* **221**, 1038 (1983).

[39] Remarks by D. Pewitt to the Third US Summer School on High Energy Particle Accelerators at Brookhaven National Laboratory (Jul. 15, 1983).

[40] Letter from A. Trivelpiece to J. Sandweiss (Aug. 11, 1983).

[41] Letter from W. K. H. Panofsky to J. Sandweiss (Sep. 9, 1983).

[42] Transcript of hearing before the Subcommittee on Energy Development and Applications of the Committee on Science and Technology, US House of Representatives (Oct. 19, 1983).

[43] *Phys. Today* **36**, 41 (1983).

[44] Report of the Reference Designs Study Group on the Superconducting Super Collider, DOE/ER-0213, May 8, 1984; the DOE Independent Cost Estimating (ICE) staff obtained estimates that were higher by US$531 million, US$403 million and US$1060 million for the three designs; *Phys. Today* **37**, 17 (1984).

[45] *Proc. Ann Arbor Workshop on Accelerator Physics Issues for a Superconducting Super Collider*, UM HE84-1 (Ann Arbor, Michigan, Dec. 12–17, 1983), ed. M. Tigner.

[46] J. E. Pilcher and A. R. White (eds.), Pbar-p Options for the Supercollider (University of Chicago, 1984).

[47] *Summary Report of the PSSC Discussion Group Meeting*, eds. P. Hale and B. Winstein (FNAL, 1984).

[48] *Proc. 1984 Summer Study on the Design and Utilization of the Superconducting Super Collider* (June 23–July 13, 1984, Snowmass, Colorado), eds. R. Donaldson and J. Morfin.

[49] E. Eichten *et al. Rev. Mod. Phys.* **56**, 579 (1984); also an addendum in *Proc. 1984 Summer Study on the Design and Utilization of the Superconducting Super Collider* (June 23–July 13, 1984, Snowmass, Colorado), eds. R. Donaldson and J. Morfin, pp. 99–103.

[50] Letter from H. G. Stever, President of URA, to D. F. Hodel, Secretary of the US Department of Energy (Jul. 14, 1983).

[51] Minutes of the SSC R&D and Conceptual Design Board Meeting (Mar. 23, 1984; Washington, DC).

[52] I. Goodwin, *Phys. Today* **37**, 69 (1984).

[53] Memorandum of Understanding between LBL, the host, and URA with regard to location and support of the SSC Design Center (Sep. 1984).

[54] Report of the DOE Review Committee on the Reference Design Study for the SSC (DOE/ER Report; May 18, 1984).

[55] *Phys. Today* **37**, 21 (1984).

[56] Minutes of the Board of Overseers Meeting, at Brookhaven National Laboratory (Sep. 10–11, 1984).

[57] A more detailed listing of Phase 1 Milestones is given in the first issue of the *SSC Newslett.* (Nov. 12, 1984).

[58] R. Palmer and A. V. Tollestrup, *Annu. Rev. Nucl. Part. Sci.* **34**, 247 (1984).

[59] *Phys. Today* **37**, 17 (1984).

[60] R. Wilson, in *Proc. 1982 DPF Summer Study on Elementary Particle Physics and Future Facilities*, (June 28–July 16, 1982, Snowmass, Colorado), eds. R. Donaldson, R. Gustafson and F. Paige, pp. 330–334.

[61] *Phys. Today* **38**, 63 (1985).

[62] D. Larbalestier, G. Fisk, B. Montgomery and D. Hawksworth, *Phys. Today* **39**, 24 (1986).

[63] R. Talman, in *Proc. 1984 Summer Study on the Design and Utilization of the Superconducting Super Collider* (June 28–July 13, 1984, Snowmass, Colorado), eds. R. Donaldson and J. Morfin, pp. 349–352.

[64] SSC Aperture Estimates for Cost Comparisons (SSC-SR-1012, Aug. 1985).

[65] L. Jones, in *Proc. 1982 DPF Summer Study on Elementary Particle Physics and Future Facilities*, (June 28–July 16, 1982, Snowmass, Colorado, 1982), eds. R. Donaldson, R. Gustafson and F. Paige, pp. 345–346.

[66] Aperture Estimates. Presentation to the SSC MSAP by A. W. Chao on Aug. 26, 1985.

[67] Magnet Technical Review Panel Report (SSC-SR-1010, Jul. 1985).

[68] The selection process and the recommendation are discussed in detail in M. Tigner's memo to file, dated Sep. 13, 1986, App. C, in Report of the HEPAP Subpanel on Review of Recent Information on Superferric Magnets (DOE/ER-0272, May 1986).

[69] B. Schwarzschild, *Phys. Today* **38**, 58 (1985).

[70] Letter from F. R. Huson and P. M. McIntyre to W. Wallenmeyer dated Apr. 7, 1986, and attached as App. D in Report of the HEPAP Subpanel on Review of Recent Information on Superferric Magnets (DOE/ER-0272, May 1986).

[71] Report of the HEPAP Subpanel on Review of Recent Information on Superferric Magnets (DOE/ER-0272, May 1986).

[72] B. Goss-Levi and B. Schwarzschild, *Phys. Today* **41**, 17 (1988).

[73] *Phys. Today* **38**, 53 (1985).

[74] US National Committee on Tunneling Technology, *Contracting Practices for the Underground Construction of the Superconducting Super Collider* (National Academy Press, Washington, DC 1989).

[75] Conceptual Design of the Superconducting Super Collider by the SSC Central Design Group (SSC-SR-2020, Mar. 1986).

[76] An Assessment of the Antiproton–Proton Option for the SSC (SSC-SR-1022, May 1986).

[77] Report of the DOE Review Committee on the Conceptual Design of the Superconducting Super Collider (DOE/ER-0267, May 1986).

[78] Detector cost and off-line computing advisory panels created, *The Lattice* (Mar./Apr. 1986), p. 5.

[79] Cost Estimate of the Initial SSC Experimental Equipment (SSC-SR-1023, June 1986).

[80] Committee on Science, Engineering, and Public Policy, *Report of the Research Briefing Panel on Scientific Opportunities and the Super Collider* (National Academy Press, 1985).

[81] SSC users organization established, *The Lattice* (Aug./Sep. 1987), pp. 5–6.

[82] *Proc. 1986 Summer Study on the Physics of the Superconducting Supercollider* (Snowmass, Colorado, Jun. 23–Jul. 11, 1986), eds. R. Donaldson and J. Marx.

[83] *Proc. Workshop on Experiments, Detectors, and Experimental Areas for the Supercollider* (Berkeley, California, July 7–17, 1987), eds. R. Donaldson and M. G. D. Gilchriese.

[84] *Proc. Summer Study on High Energy Physics in the 1990's* (Jun./Jul. 1988), ed. Sharon Jensen (World Scientific).

[85] SSC establishes detector R&D coordinating office, *The Lattice* (Mar./Apr. 1987), p. 4.

[86] R. Huson, L. M. Lederman and R. Schwitters, in *Proc. 1982 DPF Summer Study on Elementary Particle Physics and Future Facilities*, (June 28–July 16, 1982, Snowmass, Colorado, 1982), eds. R. Donaldson, R. Gustafson and F. Paige, pp. 361–368.

[87] Detector research and development and computing. Presentation by M. G. D. Gilchriese at the DOE/SSC Annual Review (Jan. 30–Feb. 1, 1989).

[88] *Appraising the Ring: Statements in Support of the Superconducting Super Collider*. compiled and introduced by L. M. Lederman and C. Quigg (Universities Research Association, 1988).

[89] SSC videotape documentary available, *The Lattice* (Jun./Aug. 1988), p. 7.

[90] An article on the SSC even appeared on Jul. 23, 1985, in *American Way*, the in-flight magazine of the American Airlines.

[91] Colorado hosts SSC symposium, *The Lattice* (Jan./Feb. 1988), p. 4.

[92] "First SSC Industrial Symposium, Feb. 8–10, *The Lattice* (Sept.–Dec. 1988), p. 2.

[93] *Proc. Int. Industrial Symposium on the Supercollider* (Feb. 8–10, 1989), ed. M. McAshan (Plenum, 1989).

[94] M. Bianchi-Streit *et al.*, *CERN* **84-14** (Dec. 11, 1984).

[95] D. C. Mowery and W. E. Steinmueller, Economic payoffs from basic research: an examination of high energy physics (Feb. 5, 1986).

[96] P. A. David, D. C. Mowery and W. E. Steinmueller (1992), *Economics of Innovation and New Technology* **2**(1), 73 (1991).

[97] D. Dickson, *Science* **220**, 1252 (1983).

[98] D. Dickson, *Science* **224**, 1317 (1984).

[99] G. Flugge, *Particle Physics Facilities from the User's Point of View: The Role of ECFA and ICFA, Large Facilities in Physics, eds., M. Jacob and H. Schopper (World Scientific, 1995), pp. 79–98.*

[100] G. Brianti *et al.*, Summary Report of Workshop on the Feasibility of Hadron Colliders in the LEP Tunnel (CERN/LEP, Mar. 21–27, 1984).

[101] D. Dickson, *Science* **224**, 1216 (1984).

[102] H. Schopper's testimony at the hearing of the House Science, Space and Technology Committee on Apr. 7, 1987.

[103] V. Soergel's prepared testimony at the hearing of the House Science, Space and Technology Committee on Apr. 7, 1987.

[104] M. M. Waldrop, *Science* **225**, 490 (1984).

[105] M. Crawford, *Science* **238**, 16 (1987).

[106] M. Waldrop, *Nature* **232**, 571 (1986); this letter was organized by V. Fazio and R. Packard, representatives from California.

[107] I. Goodwin, *Phys. Today* **40**, 55 (1986).

[108] C. Norman, *Science* **232**, 705 (1986).

[109] *Los Angeles Times* editorial (Jan. 21, 1987).

[110] R. Pear, Plan offered for vast atom smasher, *The New York Times* (Jan. 19, 1987).

[111] I. Goodwin, *Phys. Today* **40**, 47 (1987).

[112] George Will's syndicated column "The Super Collider" (Feb. 15, 1988).

[113] Invitation for Site Proposal for the Superconducting Super Collider (SSC) (DOE/ER-0315, Apr. 1987).

[114] C. D. May, States politicking in race for atom smasher, *The New York Times* (Apr. 27, 1987).

[115] I. Goodwin, *Phys. Today* **41**, 47 (1987).

[116] The list of members is given in the Academy's report *Siting the Superconducting Super Collider* (National Academy Press, Washington, DC, 1988).

[117] I. Goodwin, *Phys. Today* **41**, 52 (1987).

[118] *Siting the Superconducting Super Collider* (National Academy Press, Washington, DC, 1988).

[119] I. Goodwin, *Phys. Today* **42**, 69 (1988).

[120] SSC at Fermilab means $3.28 billion saving. *SSC for Fermilab Newsletter*, Spring 1988.

[121] SSC Site Evaluations: A Report by the SSC Site Task Force (DOE/ER-0392, Nov. 1988).

[122] Determination of the Best Qualified Sites for DOE's Super Collider (GAO/RCED-89-18, Jan. 1989).

[123] Secretary Herrington's Preferred Site Selection statement dated Nov. 10, 1988.

[124] M. Crawford, *Science* **242**, 1004 (1988).

[125] IT'S HERE, *Waxahachie Daily Light* (Nov. 10, 1988).

[126] I. Goodwin, *Phys. Today* **43**, 95 (1989).

[127] M. D. Lemonick, A controversial prize for Texas, *Time* (Nov. 28, 1988).

[128] P. Weingarten, Collider, fire ants head for collision, *Chicago Tribune* (Dec. 18, 1988).

[129] The Texas efforts to land the SSC are thoroughly described in: P. T. Flawn, *The Story of the Texas National Research Laboratory Commission and the Superconducting Super Collider* (University of Texas at Austin, 2003).

[130] Additional Views of Hon. Sherwood Boehlert, appended to the SSC Authorization Act of 1987, HR3228.

[131] The superconducting magnet program during this time frame is discussed extensively in: B. Goss-Levi and B. Schwarzschild, *Phys. Today* **41**, 17 (1988).

[132] A. Chao *et al.*, *Phys. Rev. Lett.* **61**, 2752 (1988).

[133] D. Dickson, *Science* **236**, 246 (1987).

[134] M. Crawford, *Science* **237**, 22 (1987).
[135] J. Walsh, *Science* **235**, 1458 (1987).
[136] M. Crawford, *Science* **244**, 24 (1989).
[137] D. Thomsen, *Science News* **132**, 374 (1987).
[138] Schopper's testimony, presented at the hearing of the House Committee on Science, Space, and Technology House (Apr. 7, 1987).
[139] *Phys. Today* **38**, 67 (1985).
[140] D. Dickson, *Science* **235**, 1567 (1987).
[141] W. Sweet, *Phys. Today* **40**, 71 (1987).
[142] C. Sutton, *New Scientist* 30 (1987).
[143] J. G. Bednorz and K. A. Muller, *Z. Phys. B* **64**, 189 (1986).
[144] M. K. Wu *et al.*, *Phys. Rev. Lett.* **58**, 908 (1987).
[145] A. Khurana, *Phys. Today* **41**, 17 (1987).
[146] W. Sullivan, Team reports breakthrough in conductivity of electricity, *The New York Times* (Febr. 16, 1987).
[147] *Science* **236**, 247 (1987).
[148] I. Goodwin, *Phys. Today* **41**, 50 (1987).
[149] J. Gleick, Advances pose obstacles to atom smasher plan, *The New York Times* (Apr. 14, 1987).
[150] New superconductors examined for SSC, *The Lattice* (Jun./Jul. 1987), pp. 3–4.
[151] J. Krumhansl, prepared testimony for the hearing before the House Science, Space and Technology Committee on Apr. 7, 1987.
[152] J. Krumhansl, letter to Secretary John S. Herrington, dated Feb. 19, 1987; quoted in *Phys. Today* **41**, 50 (1987).
[153] P. Anderson, prepared testimony for the hearing before the House Science, Space and Technology Committee on Apr. 7, 1987.
[154] *Report of the Research Briefing Panel on High-Temperature Superconductivity*, National Academy Press, Washington, D. C. 1987
[155] Report of the Basic Energy Sciences Advisory Committee, Panel on High-T_c Superconducting Magnet Applications in Particle Physics (DOE/ER-0358, Dec. 1987).
[156] R. Pool, *Science* **243**, 162 (1989).
[157] R. Pool, *Science* **244**, 914 (1989).
[158] R. Pool, *Science* **245**, 1331 (1989).
[159] Super hasty on the supercollider, *The New York Times editorial*, Apr. 28, 1988.
[160] *Risks and Benefits of Building the Super Collider; A Special Study by the Congressional Budget Office* (Washington, DC, Oct. 1988).
[161] Department of Energy Response to the Congressional Budget Office Report on the Superconducting Super Collider, Febr. 1989; letter from Robert Hunter, Director, Office of Research, DOE, to James Blum, Acting Director, CBO (Nov. 9, 1988).
[162] E. Marshall, *Science* **230**, 1249 (1985).
[163] B. Culliton, *Science* **235**, 840, (1987).
[164] Superconducting Super Collider Authorization Act of 1987, HR3228.
[165] Energy and Water Development Appropriation Bill, 1988, (Sep. 16, 1987).
[166] Proposal to Serve as Contractor for the Construction and Operation of the Superconducting Super Collider Laboratory. Submitted by URA (Mar. 2, 1987).
[167] Jun. 1, 167, 1988, letter from Boyce McDaniel, for the SSC Board of Overseers, to Joseph Salgado, Undersecretary of Energy, DOE, and Salgado's response of Jul. 22, 1988.
[168] Request for Proposals DE-RP02-88ER40486 for the Selection of a Management and Operating Contractor for the Establishment, Management, and Initial Operation of the Superconducting Super Collider Laboratory, US DOE, Chicago Operations Office.
[169] I. Goodwin, *Phys. Today* **42**, 49 (1988).
[170] Letter from Boyce McDaniel, chair of BOO, to John Marburger, chair of the URA Board of Trustees, dated Oct. 26, 1988, and the enclosed attachments.
[171] Proposal for the Selection of a Management and Operating Contractor for the Establishment, Management, and Initial Operation of the Superconducting Super Collider Laboratory. Submitted by URA, Washington, DC (Nov. 4, 1988).
[172] For more detailed discussion of these negotiations, see : L. Hoddeson and A. Kolb, The Superconducting Super Colliders frontier outpost, 1983–1988, *Minerva* **38**, 271 (2000). and : L. Hoddeson and A. Kolb, The Superconducting Super Colliders frontier outpost, 1983–1988, *Minerva* **38**, 271 (2000).
[173] Aug. 11, 1988, letter from Marilyn Lloyd, chair of the House Subcommittee on Energy, Research and Development, to Joseph Salgado, Undersecretary, DOE; an article in the Aug. 15, 1988, issue of Inside Energy, titled "DOE announcement on M&O contract for collider seen favoring URA," discussed the concerns along those lines both in Congress and among some of the potential industrial bidders.
[174] Proposal for the Selection of a Management and Operating Contractor for the Establishment, Management, and Initial Operation of the Superconducting Super Collider Laboratory. Submitted by URA, Washington, DC (Nov. 4, 1988), Vol. 1, pp. 4.1–3.
[175] Minutes of the Executive Committee meeting of the URA Board of Trustees, Sep. 1, 1998.
[176] Minutes of the Meeting of the SSC Board of Overseers, May 7–8, 1988, O'Hare Hilton, Chicago, Illinois.
[177] Minutes of the Meeting of the SSC Board of Overseers, Aug. 19–20, 1988, O'Hare Hilton, Chicago, Illinois.
[178] Minutes of the Search Committee Meeting, Aug. 28, 1988.
[179] Minutes of the Search Committee Meeting, Aug. 30, 1988.

[180] Proposal for the Selection of a Management and Operating Contractor for the Establishment, Management, and Initial Operation of the Superconducting Super Collider Laboratory. Submitted by URA, Washington, DC (Nov. 4, 1988), Vol. 1, 4.1–3.

Stanley Wojcicki is a professor of physics at Stanford University. During the early SSC days he took a 4-year leave of absence to work at Berkeley in the Central Design Group. His current professional interests focus on study of neutrino oscillations. He served as chairman of the DOE High Energy Physics Advisory Panel in 1990–1996. His hobbies include hiking and travel to exotic places.

Reviews of Accelerator Science and Technology
Vol. 2 (2009) 265–301

World Scientific
www.worldscientific.com

The Supercollider: The Texas Days

A Personal Recollection of Its Short Life and Demise

Stanley Wojcicki
Department of Physics, Stanford University
Stanford, CA 94305, USA
sgwojcicki@gmail.com

This article is the second in a two-part account of the history of the Superconducting Super Collider (SSC). The narrative starts with the move of the SSC activities to its Waxahachie site in Texas and the subsequent organization of its administrative structure. The technical design changes incorporated into the site-specific design are described together with their impact on costs. The principal activities at the SSC — technical progress, conventional construction and planning of the experimental program — are described briefly. The article then discusses the efforts to obtain international collaboration, the growth of the opposition both among the public at large and in Congress, and the final events leading to termination of the SSC. It ends with some subjective views on what went wrong with the SSC and on the prospects for construction of large scientific facilities in the US in the future.

Keywords: SSC; Supercollider; accelerator; Texas; DOE; Congress.

1. Introduction

This is the second of two articles [1] describing the history of the Superconducting Super Collider (SSC), or Supercollider for short. It spans a little over four-and-a-half years, from early 1989, the start of the transfer of the Central Design Group (CDG) activities in Berkeley, California, to Waxahachie,Texas, to 1993, the year of the SSC termination.

The history recounted here has two closely interwoven threads. The first one is the effort to build, on the "green site" at Waxahachie, a frontier high energy laboratory with a first class physics program. The second one is the struggle to convince Congress to fund this multibillion-dollar project in the time of changing administrations, large budget deficits and significant opposition from various quarters. Thus the story is not only about science, technology and organizational matters but even more so about politics and public relations.

Whereas I was intimately involved with the SSC during its CDG phase (in Berkeley), during the Texas days most of my involvement was from outside, in my role as a member of the High Energy Physics Advisory Panel (HEPAP) and later as its chair. I did spend eight months at the SSC Laboratory (SSCL) on my sabbatical in 1993, just before SSC cancelation, and obtained additional insight from that vantage point.

This account is based very much on my own personal recollections but in writing it I also consulted extensively other sources: articles in popular scientific journals and newspapers, government reports, personal notes, correspondence between major players, minutes of meetings, technical notes and transcripts of Congressional hearings. I also profited from a number of conversations about these events with colleagues who were intimately involved with the SSC. Nevertheless, the article should be viewed as a personal account and no claim is made that it is a true scholarly historical work. While I try to relate the events as accurately as possible, I also occasionally express my own personal opinions, always attempting to identify them as such.

The story and the lessons of the SSC might be especially timely today, since the international high energy physics (HEP) community is proposing to build a new facility, the International Linear Collider (ILC), which would enable deeper probes into the ultimate nature of matter and forces that govern it. There are many parallels with the SSC — in cost, technological challenge, and difficulty of its realization. Study of what went wrong, but also what went right, with the SSC might be instructive as we grapple with how one might best achieve the HEP goals.

2. SSCL Beginnings

The first SSC construction funds, totaling US$126.6 million, were approved in the fall of 1989 [2–4] for FY90. An additional US$87.9 million was appropriated for R&D and auxiliary equipment. Construction of the SSCL could now be formally begun. The initial challenge was to effect a smooth transition from CDG to SSCL, capture all the knowledge and experience acquired during the CDG lifetime, and create a smoothly functioning team with well-defined goals accepted by all.

One of the first tasks was to find a temporary home for the SSCL staff and the initial SSCL activities. The final collider would eventually be located about 35 miles south of downtown Dallas and would encircle the city of Waxahachie, as shown in Fig. 1. For the initial period, before the appropriate land acquisition was finalized, the Laboratory rented a large warehouse on the north side of the future ring and converted that space into the required offices, laboratories, library, cafeteria, etc. Later on, as the staff grew and more technical activities were undertaken in Texas, a second warehouse, closer to Waxahachie, was rented for technical activities. The initial address of the SSCL was 2550 Beckleymeade Avenue, Dallas, TX 75237, USA.

The recruitment of the senior staff did not go as smoothly. The bypassing of CDG members in the process of identifying the Director in August 1988 (as discussed in Ref. 1) created a deeply felt hurt among many CDG members. I do not think that this omission was done maliciously. When I pointed it out to Boyce McDaniel, chair of the Board of Overseers (BOO) at the time, he was obviously

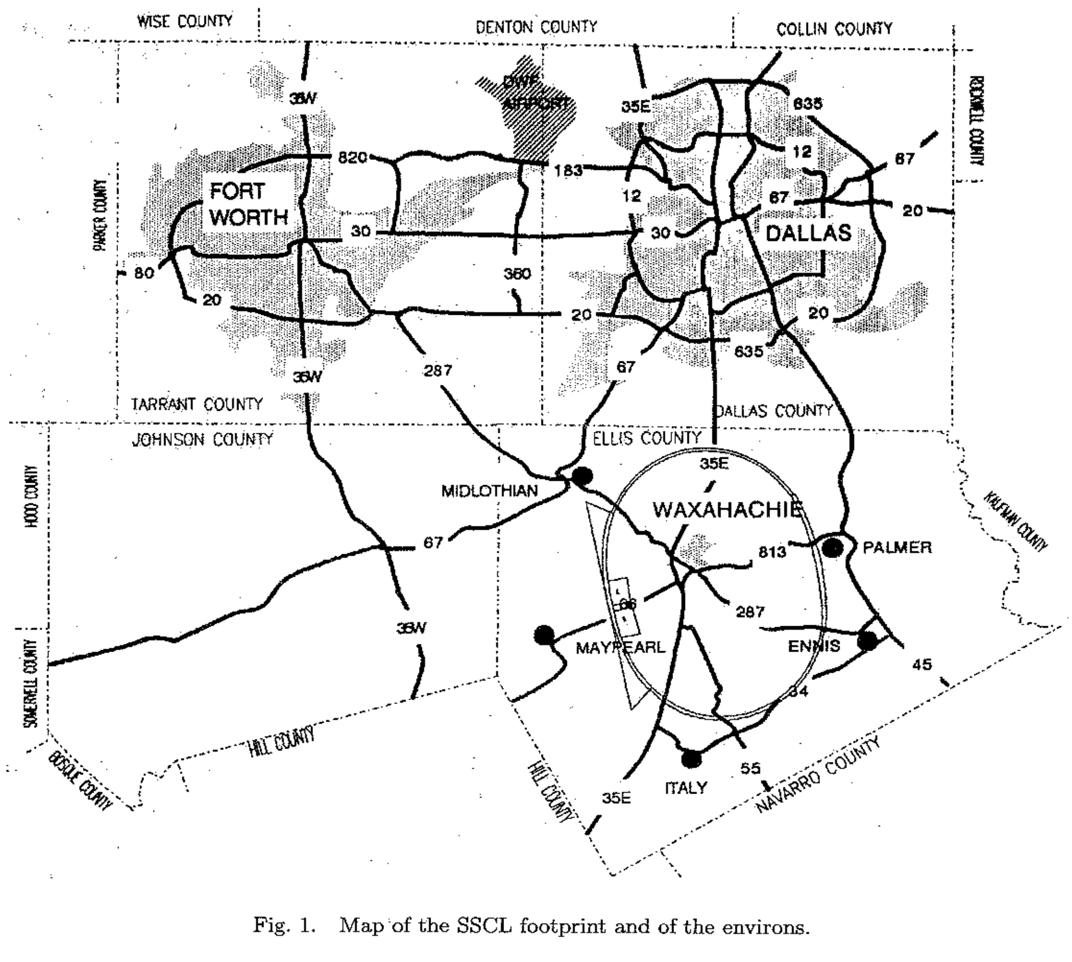

Fig. 1. Map of the SSCL footprint and of the environs.

embarrassed and I got the impression that the BOO did not even realize this shortcoming of the search process. McDaniel visited the CDG after the fact on September 27 and talked to the senior people there; the comments he received were pretty unanimous in expressing great unhappiness about the process and the outcome. There was also strong concern that what happened would make the transition from the CDG to the SSCL much more difficult [5].

Indeed, the nature of the search process and the subsequent handling of the transition from CDG to SSCL did have a strong negative impact. I think that Schwitters, as well as the BOO and the URA, underestimated the effort required to recruit truly competent and congenial staff. Many key people of the CDG did not move to the SSCL. Individual reasons for these decisions varied but invariably a contributing factor was that there was insufficient effort made to recruit them. This may have been partly due to lack of recognition of their unique abilities and experience but probably also partly because of the desire of the new Directorate to build their own team. Several of those who did join the SSCL went with some amount of disappointment and bitterness.

By far the most important loss to the project was Maury Tigner. Schwitters did offer him the positions of Technical Director/Project Manager and Deputy Director, with responsibility for the construction of the machine [6]. From talking to Tigner shortly afterward, I had the impression that a *modus vivendi* could be worked out. However, after several conversations with Schwitters, Tigner decided that he could not work within the framework that was being proposed (the main issue being whether he would be the ultimate authority in deciding on key personnel in the accelerator area). This decision must have been taken before November 4, 1988 (proposal submission date), since Helen Edwards was listed on the proposal as being in charge of the Accelerator Division. Tigner submitted his resignation to the URA on January 18, 1989, effective as of February 20, 1989. It was a real tragedy that there was not enough effort to recruit Tigner. Given his talents, experience, previous achievements and stature in the community, he would have been a great asset to the project. I feel confident that any disagreements between him and Schwitters regarding relative responsibilities and authority were not beyond resolution given sufficient goodwill on the part of both individuals. In retrospect, the BOO and the URA can be faulted for not being more forceful in trying to broker an arrangement satisfactory to both individuals.

The CDG became one of the divisions of the SSCL and its activities were going to be overseen by the SSCL. During the transition period lasting until 1990, Chris Quigg was to be in charge of those activities as SSCL Associate Director. These administrative changes and Tigner's resignation were communicated to the CDG staff, numbering 116 people, in January 1989, via a memo [7] from the URA President, Ed Knapp. Quigg would have been another valuable CDG member who could have helped in transferring some of the knowledge gained there to the SSCL but he chose to return to Fermilab, telling me that he was not offered anything of sufficient interest and challenge at the SSCL. Another key person who chose not to go to the SSCL was Peter Limon, who also decided to return to Fermilab, largely for personal reasons. I had made a decision some time earlier to return to Stanford once the site was chosen and the management team selected, and followed up on that plan when the CDG activities were being terminated. Schwitters did invite me later, in July 1989, to join the SSCL as an Associate Director at Large but for a number of different reasons I was unable to accept [8].

Schwitters had more difficulty than anticipated in recruiting top level staff. I think his efforts were made more difficult by what was seen as a rather perfunctory search process for the key people. He was apparently turned down by his top choice for the Research Director and eventually appointed M. K. Gilchriese, a physics professor at Cornell, to that position. Without detracting anything from Gilchriese, he was at that stage of his career where it would make more sense for him to lead one of the new SSC experiments than be mired in administrative work. He resigned after a short time in that position and Fred Gilman from SLAC accepted the job in January 1990. Bruce Chrisman from Fermilab was identified as the Associate Director for Business already in the initial M&O proposal but left the SSCL in July 1989 to go into the private sector.

The industrial partnership with Sverdrup did not work out satisfactorily [9]. Robert Robbins, after a short stay as head of Conventional Construction, was replaced by another Sverdrup employee, Lew Smith, and soon afterward by Jim Sanford on an acting basis. It is a mystery to me why Sverdrup was chosen as a partner in the M&O proposal. They had essentially no experience in underground tunneling. Their involvement had a negative result of shunting off experienced CDG members like Jim Sanford, Tim Toohig and Chris Laughton to secondary roles. The partnership with Sverdrup was terminated by the URA within a year.

Schwitters went outside the HEP community to appoint the Deputy Director and the Head of the Magnet Division. Tom Kirk, a former member of the CDG and then at Argonne National Laboratory, was considered for the latter position but Schwitters eventually decided to go with Tom Bush, a retired Navy captain who had extensive experience with missile design, development and production. Richard Briggs, originally Associate Director at Livermore National Laboratory in charge of fusion activities, became the Deputy Director of the SSCL.

Politically most sensitive and most important was the position of Project Manager. Initially it was filled by Doug Pewitt for a short time and then by Briggs, followed by Ted Kozman from LBNL on an acting basis. Such a position has been traditionally occupied by a member of the high energy community with significant accelerator project management experience. However, the new Secretary of the DOE in the Bush Administration, retired Navy admiral James D. Watkins, had strong and somewhat different views on this subject which were influenced by his experience with military contractors. The key ingredients was the belief that "academics" do not have competence and experience to manage a large scale construction project and that one needs strong management by federal officials. Schwitters and the URA proposed Paul Reardon as Project Manager. Reardon was an accelerator physicist with extensive management experience, both at BNL and at Fermilab, and was then Vice-President of Scientific Applications International Corporation (SAIC). Watkins, however, wanted the position given to Edward Siskin, a former vice-president of a large construction company, Stone and Webster, and someone he knew from his Navy days.

This impasse was finally resolved in October 1990, by creating the new position of General Manager for Siskin. Reardon was made Project Manager reporting to Siskin and responsible for construction of the accelerator. George Robertson, a retired major general in the US Army Corps of Engineers, became Reardon's Deputy. Siskin's position created another bureaucratic layer between the Director and Project Manager and Division Heads (except for the head of Physics Research, which officially reported directly to the Director and was connected to Siskin only by a dashed line). Other key personnel changes occurred in the same time frame: John Ives, a retired admiral, became head of Conventional Construction and Ted Kozman head of Accelerator Systems [10].

This new management scheme at the SSCL created more complex reporting lines between the accelerator designers — certainly one of the prime activities of the Laboratory at that time — and the Directorate. There was a position of Technical Director reporting to the Project Manager, which was held initially by Helen Edwards from Fermilab, who resigned in 1991, reportedly because of conflicts with the management [11]. Also under Reardon was an Accelerator Design and Operations Division, headed by Don Edwards, also from Fermilab. One of the four departments in that division was Accelerator Physics, responsible for theoretical calculations. Thus the input from that very important aspect of the accelerator design was separated from the Director — at least bureaucratically — by three or four layers. This full SSCL organization chart is shown in Fig. 2.

The SSCL tried to merge two cultures at the top management levels — industrial/military and academic/laboratory, with very different styles and different ways of doing things. This two-culture situation persisted and intensified with time and seriously handicapped the functioning of the Laboratory [12]. The free exchange of ideas at low levels, typical of HEP laboratories, was stifled and in some areas expressly forbidden. This distinction was probably most pronounced between the magnet and accelerator designers, and this was the area where the conflicts were most severe. The appointment of Siskin accentuated this situation. He had personal access to Watkins, which resulted in his having an extraordinary amount of authority and autonomy.

The overall situation was not helped by Schwitters' required heavy involvement in various SSC-related interactions outside the Laboratory. Most people I talked to felt that this seriously limited his

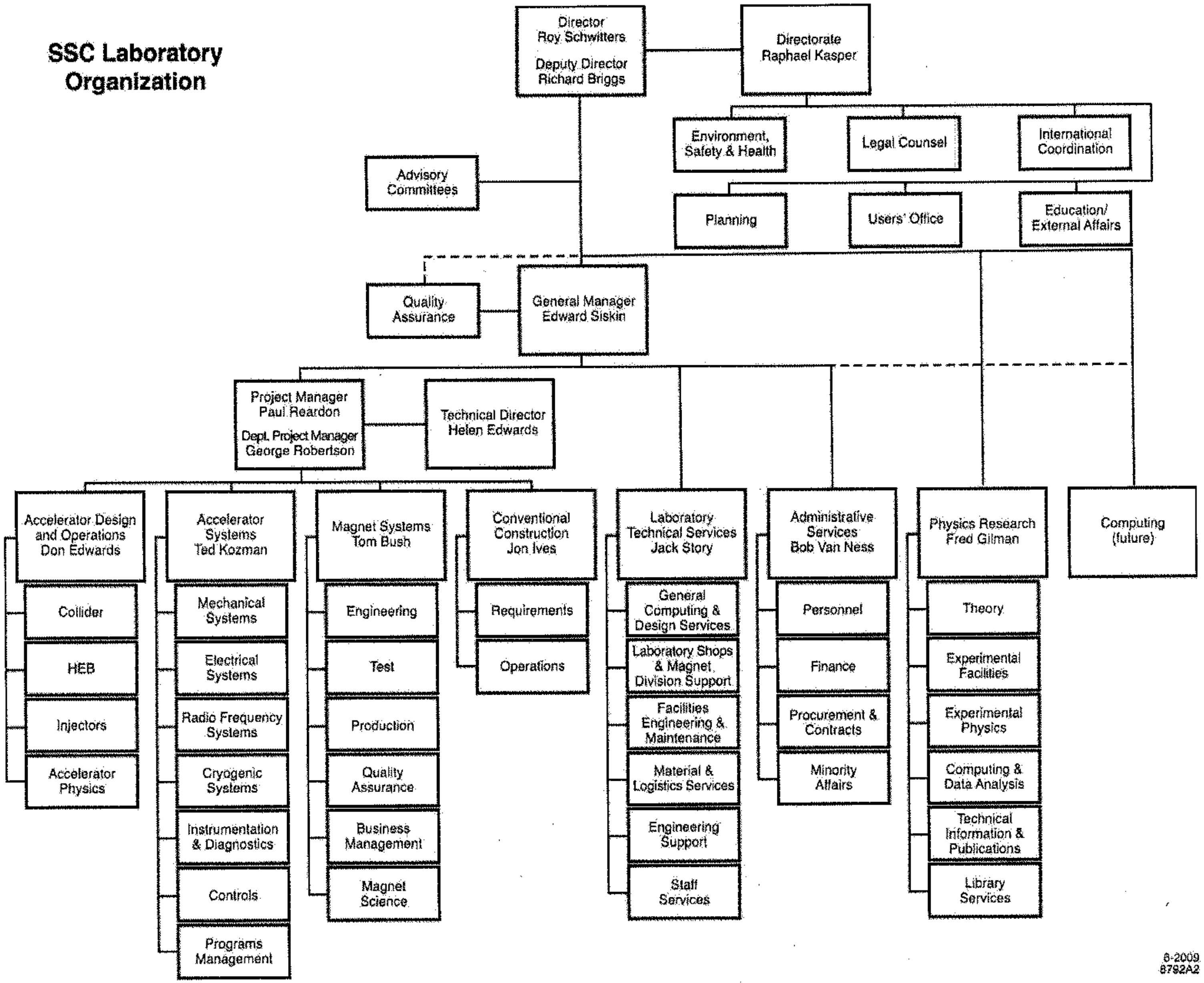

Fig. 2. Initial organization chart of the SSC Laboratory.

ability to be on top of the most important SSCL issues and carry out his intention of being a Director with an "open door policy." Furthermore, as time went on, the future physics program at the SSCL, an area that was of paramount interest to Schwitters, was attracting more and more of his attention at great cost to his primary responsibility, namely creating a well-functioning laboratory dedicated to building the collider as its principal goal.

3. Site-Specific CDR and Its Validation Process

An early SSCL activity was re-examination of the status of the magnet R&D program and basic machine parameters, taking into account site-specific information [13]. This was done mainly in light of what had been learned since the CDG CDR but there was also a tendency sometimes to look at some issues from scratch. Thus, for example, the possibility of bore tube corrections, which were being developed at the CDG and which appeared to indicate that one could live with 4 cm magnet apertures, was not pursued at the SSCL because of its perceived added complexity. The most relevant new information came from the operational experience of the Tevatron and HERA [14]. After ramping down from high energy to much lower energy for injection, there were residual circulating currents which distorted the magnetic field and were not reproducible from cycle to cycle. New programs were developed to investigate these issues, and the availability of more computing power enabled one to follow the proton trajectories over a longer period of time [15], providing new information regarding the required magnetic field quality.

Schwitters appointed an Aperture Task Force to study these effects. They reported their findings to a standing Machine Advisory Committee (MAC), which recommended several changes to the machine design [16–18]. It was argued that without these changes machine commissioning would be much more difficult and operation of the machine could be compromised. There was also the argument that future upgrades, for example to higher luminosities, might not be possible without these changes because a 4 cm aperture would preclude insertion of a liner that might be needed to intercept synchrotron radiation. A rough estimate suggested that these modifications and recalculation of labor, material and R&D costs, necessary for assuring reliable machine operation, would generate a 20–30% cost increase [19]. The three major recommended changes were:

- Increase the diameter of the dipole aperture from 4 to 5 cm. That would give a larger volume with a uniform field.
- Increase the energy of the HEB from 1 to 2 TeV and at the same time make a corresponding change in MEB (from 100 to 200 GeV) and in LEB (from 8 to 12 GeV).
- Provide stronger focusing in the main ring, necessitating an increase in the circumference of the main ring from 52 to 54 miles.

Strangely, the diameter of the quadrupole aperture remained in this iteration at 4 cm. Several people with whom I discussed this issue claimed that it was apparently due to a miscommunication between Schwitters and the MAC. Whatever the reason, it did become subsequently a source of significant debate and controversy [20]. It was still unresolved in January 1991, when at the HEPAP meeting Don Edwards gave a presentation enumerating potential options and their pros and cons in this area [21].

These proposed design changes were discussed within the Laboratory as well as with the SSC Scientific Policy Committee, the Users Executive Committee and an ad hoc Committee on SSC Physics [22]. They were also presented to the SSC BOO and URA Board of Trustees. The BOO Executive Committee was leaning initially [23] toward trying to keep the original price and finding a "solution focused on 17 TeV energy" rather than the original 20 TeV. The final BOO consensus, however, was that the changes proposed by the Laboratory should be adopted. The other groups also supported the Laboratory decision. The ultimate decision rested formally with the Director, Roy Schwitters, but he was clearly constrained by the opinions of both his advisory groups and his oversight bodies. The final decision was controversial then and remains so today. Even some members of the MAC had doubts about the wisdom of that recommendation. Clearly, the suggested changes made the design more conservative, facilitating magnet fabrication and making the machine easier to commission and easier to operate. But many are still convinced that the original design would have worked, especially if one allows for advances in instrumentation, computing and, most importantly, human ingenuity. On the whole, I found from talking to the people involved, that this is an issue on which there is a significant divergence of opinion.

The next question was whether one should descope the machine so as to recover the original cost figure of US$5.9 billion (see later section for cost discussion). The easiest way to achieve any significant savings was to reduce the energy by about 25%. To obtain guidance on this issue, the DOE asked HEPAP to appoint a blue ribbon Subpanel to address the question "How much physics potential would be lost by reducing the energy below 20 TeV?" The Subpanel was chaired by Sid Drell from SLAC and included 15 members, 5 of them Nobel laureates in physics. The Subpanel concluded that some of the physics objectives might not be attained if energy were reduced to 15 TeV per beam. Furthermore, they did not see any other reasonable alternative which would give a significant-enough cost saving [24, 25].

The report was presented to HEPAP by T. D. Lee (since Drell could not be there) at its January 12, 1990, meeting. The meeting was notable for the presence of Sen. Phil Gramm (R-TX), probably the only time that a sitting Senator participated in a HEPAP discussion. Gramm was supportive of the changes if they were really necessary. He cautioned the community against "bells and whistles" in the machine and hoped that the design was "a sturdy Chevrolet and not a Cadillac." In retrospect, I wonder if we tried to build a Buick. After some discussion, HEPAP unanimously endorsed the report [26].

As a result of these proposed changes [27], the DOE called for another HEPAP Subpanel to provide an independent assessment of the SSC cost estimate (US$7.8 billion) and the realism of the proposed schedules and funding profiles. The Subpanel had six members and was chaired by John Townsend, Jr., Director of the Goddard Space Flight Center. In report, delivered to HEPAP on July 19, 1990, the Subpanel stated its main conclusions [28]:

- The cost-estimating methodology was appropriate and was being properly applied.
- The estimates were complete and credible to the extent that they were determinable. But the low contingency used made the overall cost estimate not credible.
- The proposed schedule and assumed funding profile were not realistic. The schedule was found to have no float for events not under project control and the projected huge increase in Congressional appropriation from FY91 to FY92 seemed highly unlikely. The Subpanel recommended adding 6–12 months to the early part of the schedule.
- The DOE and the SSCL had failed to date to staff all key managerial and technical positions.
- The estimate of cost to completion should be raised from US$7.8 billion to US$8.6 billion. An additional US$300 million for detectors was recommended.

Within errors, these conclusions paralleled those reached by the 82-member DOE review panel [29] which met at the SSCL on June 25–30, 1990. Their estimate was US$8.37 billion. A third cost estimate was obtained by DOE independent cost estimators (ICEs), who arrived at US$9.33 billion. They also suggested, however, that US$2.492 billion in additional cost elements be considered for inclusion in the total project cost. Almost half of that (US$1.18 billion) was for an increased detector scope. Based on all this input the DOE decided to present to Congress a baseline TPC estimate of US$8.25 billion with a completion date at the end of FY1999 [30, 31].

This significant cost increase was a serious blow to the SSC project and gave powerful ammunition to the SSC opponents. Undoubtedly, it also had a strong impact on the future DOE–SSC relationship, discussed in the next section. It is tempting to examine, with the benefit of hindsight, whether the situation could have been handled better. One big shortcoming in the Drell Subpanel process was the narrowness of the charge, i.e. limiting it to pure physics considerations. Certainly, their recommendation made sense on pure physics grounds, especially considering that the SSC would be the main focus of US HEP for at least a quarter of a century. However, a very important question — what would be the risk of the SSC not happening at all if such a design change were to be implemented — was not part of the charge and, to my knowledge, was never considered in any significant depth by the SSCL Directorate, by the Subpanel, by HEPAP or by the DOE. A good yardstick by which one could have appraised the potential future difficulties was the fact that the required annual expenditures for the SSC construction would have to be significantly higher than the total annual HEP budget at that time. In an ideal world, that risk should have been weighed against the risk of longer commissioning time and more difficult operation. An alternative charge, which might have given a recommendation that would lead to a more successful course of future events, would have been to ask how much physics one could do within the constraints of the initial cost estimate.

The Drell Subpanel report argued that all possible cost-saving options were looked at and they were all unsatisfactory. But some of the Subpanel's assumptions, such as the statement "...present detector technology does not allow use of higher luminosities for a broad range of experiments," ignored the possibility of progress. Developments over the last decade showed that the capability to deal with order-of-magnitude higher luminosities can be achieved (the LHC design luminosity is above $10^{34}\,\mathrm{cm}^{-2}\,\mathrm{s}^{-1}$). To reduce initial costs, experimental areas and detectors could have been staged or the level of their sophistication could be coupled to foreign contributions. If necessary, energy could have been raised later by lowering the temperature and raising the field. Similar staged strategy was adopted by the CERN Directorate in 1993, with the missing magnet scheme, in getting the Council to approve the LHC within a certain budgetary cap. Small savings could probably have been achieved by cutting back or postponing some of the campus buildings or trying to get some of the buildings built with private funds.

One of the arguments against reducing the circumference, such as for a 17 TeV machine, was that "a smaller ring would require a new footprint and environmental study, causing delays, which implies increased cost." Apparently, however, no comparison, was made of the delay caused by this versus the delay caused by the cost increase, and no allowance was made for the time gained due to the resulting shorter construction period. The process of acquiring land was still in very early stages at that time, so there would not be much wasted effort in that area. Furthermore, the Townsend report argued for the necessity of a 6–12-month delay in the first half of the schedule. And obviously the footprint already had to be modified somewhat for an increased circumference.

One of the functions of the oversight and management groups like the BOO and the URA was to provide advice on issues broader than Laboratory governance, like the potential repercussions of such a design change. I discussed this issue with Panofsky while the BOO deliberations were taking place and he told me that he was the only member of the BOO arguing until the end for energy reduction to 17 TeV [32, 33]. In hindsight, a "cocktail" approach of reducing the energy slightly, scaling back on detectors and experimental areas, economizing on the campus area, modifying staffing levels and not going all the way on MAC recommendations which aimed for a very conservative design, might have made it possible to limit the cost increase without prejudicing the physics goals. For example, it was argued that the energy ratio between successive accelerators could not be greater than 10 for adequate operation; HERA used bore tube correctors and managed to run efficiently with a 40 GeV injector and an 820 GeV proton ring energy — a factor of over 20. They were able to handle the persistent current problem by connecting two identical reference magnets into the main magnet circuit [34]. It is true, however, that HERA injection time and hence coasting time at low energy were much shorter and thus the stability problem was less severe. On the whole, I feel that there was not sufficient attention given to potential cost-saving solutions; the oversight system appears to have failed in this instance.

4. SSCL–DOE Relationship

Besides the internal SSCL organizational problems, there were major issues connected with the DOE–SSCL relationship and the DOE management structure for the SSCL. These issues were very closely interrelated. The initial DOE–SSC organization, adopted still in the CDG days, had the Office of SSC, headed by Robert Diebold, reporting to the OER Associate Director for the Office of Nuclear and High Energy Physics (part of the OER), a position held at that time by Wilmot Hess. This arrangement was modified by Robert Hunter, the OER Director appointed late in Reagan's second term to fill a position that was held for over a year after Al Trivelpiece's resignation on an acting basis by James Decker. Hunter wanted to strengthen the direct control by the DOE over the SSCL and for that purpose he adopted a system where the SSC Office at Headquarters (OSSC) reported directly to the OER Director. The OSSC, in turn, would direct the SSC site office at the SSCL. This system would bypass the Office of High Energy and Nuclear Physics and effectively exclude those in the DOE who were most familiar with the SSC project and the field of high energy physics.

DOE headquarters wanted the OSSC to be able to exert strong technical direction and management of the project rather than the more traditional oversight. This was strongly resisted by the SSCL and the URA as being contrary both to the signed contract and to the practice in HEP in the past [35]. The OER would have liked to manage the SSCL as a procurement where the overall integration responsibility for the project would rest with DOE headquarters. There would be two contractors — a scientific one, i.e. the URA, and a general industrial contractor (GIC), responsible for the construction of the Laboratory. This plan was presented to a group of senior reviewers on May 4–5, 1989, and their negative comments led to rejection of the scheme. On September 7, 1989, Watkins directed Hunter to establish the OSSC at the DOE headquarters mainly for oversight [36]. This was viewed as a victory by the BOO but turned out to be only the end of the first chapter in the DOE/SSCL management struggles. The issue as to what was to be the exact role of the OSSC was a contentious one throughout the rest of the SSCL's life.

Hunter managed to antagonize a number of key people in Congress, especially members of the Texas and New Jersey delegations: the former over his heavy-handed approach to SSC management and the latter over proposed reductions in the Princeton Plasma Lab funding. As a result, late in 1989 Hunter was forced to resign and Decker again assumed the position of an acting director of the OER. This new situation made it easier for Watkins to mold the formal organizational structure in such a way as to give him much closer personal control over the SSCL. Watkins had the DOE SSC (on site) Project Director, Joseph Cipriano (another Watkins colleague from his Navy days), report directly to him, short-circuiting the normal lines of authority in the DOE [37]. This allowed him to exercise much more direct control but marginalized even further the influence of the DOE people with the HEP experience. The OSSC [38], which was to be headed by an Associate Director of the OER, was directed for a long time on an acting basis by Garry Gibbs, a long term DOE official, until Cipriano assumed that role in June 1991, continuing also to serve as the on-site DOE Project Manager. The anticipated staffing level of SSCL offices at the headquarters and on site was 110 DOE personnel. All of these changes and innovations and the manner in which they were instituted caused a great deal of friction between the URA, the SSCL Directorate and the DOE, friction which persisted and grew throughout the life of the project.

Watkins was undoubtedly strongly influenced in making these personnel actions by the significant increase in the SSC projected cost, discussed in the previous section. The cost estimate was released by the SSCL in June 1990, but was known by Watkins already in the fall of 1989 [39]. Also influential was probably the fact that the management of the project by the DOE had been the subject of criticism at various levels from the very beginning. These criticisms were clearly expressed both in an early DOE IG report [40] and in a GAO report [41] commissioned by the Senate Budget Committee in March 1990. They were quite critical of the DOE for its slowness in building up an oversight organization that could respond promptly and effectively to SSCL-proposed actions requiring DOE approval. From his response [42] to the GAO report, it is clear that Watkins' intention was to reshape the management system so as to allow him to keep tight control over the SSCL. In his May 8, 1991 letter to Jamie Whitten, chair of the House Committee on Appropriations, Watkins wrote: "Deputy Secretary Henson Moore is involved in the program on a daily basis.... I personally look to Mr. Joseph Cipriano and Mr. Edward Siskin to manage this project on a daily basis and report to me...."

It was in that time frame that I was appointed by Watkins as the new HEPAP chair, to replace Francis Low from MIT, as of September 1, 1990. It was already clear then that the management issues at the SSCL were of paramount importance and potentially a real trouble spot. On October 4, 1990, I wrote a letter [43] to the DOE Deputy Secretary, Henson Moore, citing my charge as the HEPAP Chair to "provide the best possible advice on the High Energy program" and expressing my concerns about the formal SSCL–DOE relationship. I quote a few sentences from it, because they pretty much summarize the concerns at that time of many in the HEP community: "The SSC situation exemplifies an ever-increasing trend toward shifting more and more of the authority for key decisions away from the scientific and technical performers in the field and towards Washington. Such a course must lead to a disaster... adversarial relationship that follows naturally from this course, results in time, energy, and resources being spent fighting unnecessary battles rather than being directed towards the project itself."

Such concerns were expressed earlier by the SSC BOO. In a letter [44] to the Chairman of the URA Board of Trustees, the BOO stated: "... the approach of the DOE to the oversight of the URA contract for the establishment and management of the SSC Laboratory appears to be developing in the direction of detailed DOE management of the project rather than oversight of the performance and accountability of the contractor."

5. Planning the Physics Program

Planning of the SSC physics program was initiated with a letter [45] sent by the Director to the US HEP community early in 1989 outlining his views on the initiation of the program. The key components were the establishment of the Scientific Policy Committee (SPC), composed of 17 senior members of the international HEP community, to advise him on general policy issues and of the Program Advisory Committee (PAC) to provide advice on more specific details, including review of detector initiatives. The PAC was composed of 18 senior scientists from all over the world. Jerry Friedman from MIT was appointed as chair of the SPC and Jack Sandweiss from Yale as chair of the PAC.

The PAC met for the first time on February 9–10, 1990. Guided by their advice, the SSCL issued a call and guidance for expressions of interest (EOIs), to be due on May 25, 1990. By that time 15 EOIs were received, authored by 1946 collaborators from 303 institutions, demonstrating broad, worldwide interest. There was a strong emphasis in the EOIs on both high-P_T and B physics, but there were also ideas for smaller experiments. The EOIs were considered at the next two meetings of the PAC, in June and July 1990. Based on advice from the SPC and the PAC, it was decided that the initial program should have two large high-P_T detectors (plus smaller experiments) that would be complementary and overlapping [46]. One of them should be a general purpose detector with tracking capability in the magnetic field; the other should emphasize calorimetry and muon identification. The funding allocation (within the new US$8.25 billion total SSCL cost) for the initial SSC experimental program was US$842 million; based on that, the US part of the cost of each large detector was capped at US$250 million, to allow funds for other initiatives. With these guidelines, letters of intent (LOIs) for the large detectors were requested with a deadline of November 30, 1990.

A few explanatory words may be appropriate here, explaining the relationship between the large detector collaborations and the host laboratory. These collaborations become semiautonomous international organizations, with their own leaders and governance, and they are the ones that are responsible for the design and construction of the detectors and the subsequent analysis and publication of the data taken. A large fraction of the funds for the construction comes from the funding agencies supporting the collaborating institutions, whether in the US or abroad. The Laboratory's principal involvement, once a given collaboration is approved for their physics program, is in the financial area. The Laboratory has the responsibility for making sure that the approved design is consistent with the funds available and that the detector will be completed on time and on budget. This will often create some friction when the physics ambitions of the Collaboration exceed the funds available for their detector.

Three LOIs for such large high-P_T detectors were received [47] and considered by the PAC at their meeting on December 13–15, 1990. Based on discussions at that meeting and PAC recommendations, the following decisions were taken by the Director [48]:

- The Solenoidal Detector Collaboration (SDC), with George Trilling, professor at UC Berkeley, as spokesperson, was approved for support in its development of a formal proposal/design report.
- The L* (Lone Star) Collaboration, with Sam Ting, professor at MIT, as spokesperson, was not approved even though it was seen as potentially competitive and complementary to the SDC. Concerns were raised about the reliability of its cost estimate, the adequacy of US participation, and the composition and mode of governance of the

Collaboration. A definite recommendation would await the resolution of these issues.

- The EMPACT/TEXAS proposal, with Mike Marx, professor at New York State University at Stony Brook, as spokesperson, was not recommended to proceed to a technical proposal.
- The deadline for formal proposals for the two large detectors was set for April 1992.

The L* proposal never materialized [49]. There were a number of crucial issues where significant differences existed between the SSCL and the L* management teams and between Schwitters and Ting. The principal ones had to do with the Laboratory's insistence on greater transparency of what funds (and from where) were available for the construction of the detector, as well as the SSCL pressure for greater involvement of US groups in the L* management [50]. This latter requirement did not sit well with some of the non-US groups. This unhappiness was voiced in letters to Schwitters from V. G. Shevchenko, Vice-Director of ITEP in Moscow (February 18, 1991), and K. Lubelsmeyer, Professor and Institute Director at Aachen, Germany (March 4, 1991). In response to the requirement for strengthened US management, Barry Barish, professor at Caltech, was asked to join the experiment as a coleader with Ting. A tentative management plan was presented to Ting in a letter from Barish on March 5. Ting would be the spokesman and Barish the chair of the Management Board and US Group Leader. Six out of seven seats on the Management Board would be held by the Europeans. This scheme was accepted by the L* Collaboration at its meeting the following day. At its March 10–12 meeting, the SSCL PAC still expressed some reservations about L* but recommended [51] that "L* be supported to proceed toward the development of a technical stage report, subject to the prompt development of scientific and technical leadership that is acceptable to the SSCL Director." Before any serious attempt could be made to resolve the outstanding issues, both Hans Hofer, professor at ETH in Zurich, Switzerland (on March 13), and Lubelsmeyer (on March 28) announced the withdrawal of their institutes from the Collaboration. After his fruitless trip to CERN for a meeting with the L* principals to try to resolve the impasse, Schwitters announced [52] on May 3, 1991 that L* would not be supported.

Later that May the Laboratory invited the HEP community to propose a second detector to do high-P_T physics and complement the SDC [53]. Shortly afterward, Barish joined with Bill Willis, professor at Columbia, to form a new collaboration [54] with the same goals as L*, called Gammas, Electrons, Muons (GEM). Initially the Collaboration was composed mainly of the former members of L* and EMPACT/TEXAS. Both the SDC and GEM proceeded and eventually obtained a more formal go-ahead from the SSCL: the SDC Technical Design Report (TDR) was approved [55] by the Director on July 31, 1992; GEM, somewhat further behind, submitted its TDR [56] on April 30, 1993, which was to be reviewed by the PAC later that summer (a meeting that was later canceled in light of SSCL funding uncertainties). Based on these proposals, the Laboratory initiated work on the design of interaction regions, both the underground halls and the surface facilities. Some decisions on smaller experiments were anticipated for the 1993/94 time frame.

Both large detectors relied in their financial planning on significant contributions from abroad. Indeed, there were a large number of physicists from abroad (over a hundred from Japan in the SDC) who were collaborators on these experiments but there was no corresponding commitment of funds. Because it was more mature, the SDC's plans were more developed: of its US$589 million estimated cost, there was the expectation that US$231 million would come from abroad [57]. Thus the scope of the eventual SSC physics program was very much dependent on success in negotiations for foreign contributions [58].

The very successful generic detector R&D program started at the CDG was discontinued after FY90 (US$17 million were allocated to it in FY90). In its place, the SSC Detector Subsystem R&D Program was initiated, designed to address technical issues relevant to the approved and potential experimental program [59]. This was also proposal-driven; 30 proposals were considered by the Laboratory's R&D committee in October 1990. Twenty-four of them were recommended for support, with a sum total of US$18.4 million [60]. As part of the detector R&D effort, the SSCL organized [61] the Symposium on Detector Research and Development for the SSC at Fort Worth, Texas, held on October 15–18, 1990. About 500 scientists participated.

The SSCL also started a physics research program, in addition to work on building the collider. Initially the work was centered on detector R&D for the SSCL detectors, both the SDC and GEM. As part of the GEM R&D program, that collaboration had constructed at the Laboratory an instrumented cosmic ray muon spectrometer [62] (Texas Test Rig — TTR) in which one could test and compare various muon detector prototypes. There was also some involvement in the CDF experiment at Fermilab. Longer range plans involved submitting a proposal for support of a broader research program. In its explanatory document of August 1992, the DOE explicitly stated that the DOE HEP program would support efforts of SSCL physicists in their physics research activities on a competitive basis with other proposals submitted to the US HEP program [63].

The SSCL hosted the XXVI International Conference on High Energy Physics (ICHEP) in Dallas in July 1992. On the positive side, this conference helped to stimulate international interest in the SSC and many visitors from abroad were able to see for themselves the progress being made in building the Laboratory. There were 1286 delegates, from 47 countries. On the other hand, that time frame overlapped with the period of Congressional deliberations on the SSC's fate (see below) and thus undoubtedly reinforced the doubts in some people's minds whether the SSC would come to fruition.

I should mention one other important activity in the physics area. Some US$100 million of the US$1 billion pledged to the SSC project by the state of Texas via the Texas National Research Laboratory Commission (TNLRC) was devoted to supporting SSC-related research activities by young people, at the SSCL and at other institutions. The awards were given on a competitive basis — based on proposals submitted to a committee formed for that purpose by the TNRLC. It was an excellent program which filled a real need in the time of funding shortfalls and helped to nurture a pro-SSC climate in the whole country. The first 20 of these awards were made in 1990/91, with a total value of US$11 million; several of the awardees subsequently went on to distinguished careers in particle physics, The detectors at the SSCL would confront many technical challenges due to the high rates, high energies and high radiation environment. The TNLRC program provided significant help in funding the R&D required to address these challenges.

6. Technical Progress

Something that is frequently ignored and forgotten when the SSC's history is discussed is the rather impressive progress in building the collider and the Laboratory. Regardless of any possible management shortcomings and rough SSCL–DOE interactions, significant progress was being made [64]. This great progress in the early stages of the SSCL life was made possible by many collaborative efforts from other national laboratories (both in the US and abroad), which assumed major or total responsibility for many subsystems and R&D activities. Limited space does not allow me to do justice to the many accomplishments at the Laboratory during its short lifetime, but a brief enumeration of them is needed to at least give a flavor of that aspect of the SSC's history. My choice of items to mention here is undoubtedly quite subjective and for a much more complete story the reader is referred to a very comprehensive description of the SSCL achievements in the document "A Retrospective Summary," prepared within the framework of the SSCL closeout activities [65]. This 400-plus-page document, written by the technical leaders of the SSCL construction project, gives a very detailed account of the technical accomplishments during the SSCL's existence in Texas.

There was significant progress in the conventional construction area. Modifications were made in the details of the collider tunnel placement and the location of the experimental halls, to better conform to the site. Tradeoff studies of the number, size and configuration of the collider shafts resulted in an improved design and significant cost savings. There were claims by the DOE IG office that the SSC architectural design, engineering and construction management contractor, the Parsons Brinckerhoff/Morrison Knudsen joint venture (PB/MK), had significant cost overruns [66, 67] on early surface work. These claims were disputed by the SSCL, on the grounds that they were based on misinterpreted data. The underground work on the collider tunnel was proceeding very well and most of the contracts were bid at or below the estimated price. At the time of the termination, four tunnel boring machines (TBMs) were in operation and 14.7 miles (27%) of the total tunnel was excavated. Several new tunneling records were established: 420 feet in a day, 1294 feet in a week and 4269 feet in a month [68].

The 110,000-square-foot Magnet Development Laboratory, to be used for production of special purpose magnets, was completed in the fall of 1991. The somewhat smaller Magnet Test Laboratory, for testing industrially produced magnets, was completed a year later. At the time of termination, it was nearly ready for operation. The contract for the linac tunnel and linac buildings was awarded in May 1992. The tunnel work was completed and turned over to the Laboratory on June 21, 1993, and there was good progress on the buildings. Most of the components had also been ordered; the first three, H-ion source, low energy beam transport (LEBT) and the radio frequency quadrupole (RFQ), which accelerated the beam to 2.5 MeV, were installed and operating by April 1993; the first full energy 600 MeV linac beam was scheduled for April 1995.

An important achievement of the SSCL was significant progress on superconducting magnets, both dipoles and quadrupoles. Initial work was mainly at the national labs where the expertise resided. As facilities at the SSCL became available, more of these activities were moved there. In parallel, there were industrialization efforts, starting with the participation of industry people in the work at BNL, LBNL, Fermilab and SSCL. The magnet performance was good, both of the older 4-cm-diameter design and of the new 5-cm-diameter design; the first six 5-cm-diameter design short magnets achieved 110–118% of the required operating current; in tests at 1.8 K, dipoles reached 10,000 A, indicating excellent mechanical robustness. A milestone was reached on August 14, 1992, six weeks ahead of schedule [69], with the completion of the Accelerator Systems String Test (ASST). It involved operating a collider half-cell consisting of five dipoles, one quadrupole and two spool pieces at the design current of 6500 A. The magnets in the test were assembled at Fermilab by General Dynamics personnel. Subsequently, a full cell, with eight General Dynamics–produced dipoles and two Westinghouse dipoles, was assembled and cooled down. It was ready for electric and magnetic tests when the SSC was terminated. It was planned to have the ASST serve in the future as a test bed for further string tests, accelerator systems, development of operations procedures, and personnel training.

Based on the responses to the RFP [70], General Dynamics and Westinghouse were selected as the leader and follower contractors for the dipole magnets [71]. Contracts were signed with both General Dynamics and Westinghouse for US$166 million and US$101 million, respectively, for the initial complement of some 500 prototype, preproduction and low rate production magnets [72]. The adopted leader/follower arrangement was a departure from the standard HEP procedure with multiple vendors and was somewhat controversial. The leader contractor had the responsibility for the design of the magnets and the associated tooling. Both companies would produce a number of magnets, at which point a decision would be made about allocation of the rest of the order. At the time of termination, the first dipoles assembled at the new GD plant at Hammond, LA, were on the verge of completion. Babcock and Wilcox won the bid for the US$62 million contract for the final design and initial production of the quadrupole magnets for the collider [73].

There was also work on the magnets for the HEB, contracted to Westinghouse, and on magnets for the warm machines. Some of the contracts for their production were given to the labs or industries abroad, frequently resulting in significant savings. Thus, for example, the Budker Institute for Nuclear Physics (BINP) in Novosibirsk assumed responsibility for the design, engineering and fabrication of the LEB magnets. The Moscow Radiotechnical Institute was given the contract for quadrupoles in MEB [74].

An important issue was the quality of the collider ring vacuum, specifically the impact of the desorption induced by the synchrotron radiation photons striking the beam tube. This potential problem was known during the CDG days and several experiments were performed to get a better understanding of the phenomenon. The issue, however, turned out to be more complex than initially anticipated. Because this was a critical path item affecting magnet design a two-prong approach was taken. A number of experiments were initiated to understand better this effect and its possible remediation, and simultaneously an effort was started to design an optimum liner for the magnet bore tube.

The analytical and numerical accelerator physics studies continued with a focus on optimizing the collider lattice. The SSCL management decided eventually to increase the quadrupole aperture from a 4 cm to a 5 cm coil winding diameter. This simplified the lattice, and several beam components included in the 1989 design could be eliminated. A very important development came as a result of more extensive numerical tracking calculations. It was learned that the dynamic aperture available did not decrease as quickly with the lower injection energy as one thought in 1989 and thus there were plans to propose reducing the HEB energy to 1.5 TeV from the nominal 2 TeV [75].

7. International Collaboration Efforts

In the 1989–1993 time frame there continued to be major efforts to enlist other countries in SSC participation. But it was not the coherent effort that it needed to be, with all the major players — Department of Energy, State Department, both Houses of Congress and the US HEP community — following a well-defined strategy. From the beginning it was clear that it was not going to be an easy task, especially with Western Europe. Carlo Rubbia became CERN DG in 1989 and he harbored strong hopes of an early and quick LHC start even though LEP construction was not yet finished. In a letter [76] to Francis Low, current HEPAP chair, Rubbia wrote in May 1989: "... it seems a logical step to consider the construction of the LHC, *on the fastest possible timescale*, as a very effective precursor of the SSC." Even for some time afterward CERN management continued to project highly optimistic (and unrealistic) cost and time scale projections for the LHC without any specific design to back them up [77]. As late as 1993 the stated plan was still to locate the new superconducting magnets above the LEP magnets and to preserve the electron–proton (e–p) collider option.

There was a meeting on September 7, 1990, between the senior Laboratory staff, a few members of the SPC (including three from abroad), and DOE and State Department representatives, to lay out some general guidelines and discuss possibilities. I attended that meeting as HEPAP chair. It was recognized that because of the LHC, commitments from Europe were unlikely at that time, and that foreign participation in construction most likely would take the form of "in kind" contributions. It was agreed that ideally contribution to construction should not affect ability to participate in the scientific program. Mutual visits of principals were strongly encouraged. The issue of coupling participation to contributions was not trivial. The stated policy that there should be no connection between possible exploitation participation and construction contributions was the norm in the worldwide HEP, but costs were becoming so large that people were beginning to question it. In a second (July 31, 1989) letter [78] to Low, Rubbia wrote: "I doubt that the governments would be willing to pay large sums for the accelerator if they knew that the proposals from their scientists will be accepted or rejected with no correlation to whether or not they have contributed to the funding of the machine."

The initial efforts in the international area involved laboratory-to-laboratory agreements on collaboration to construct SSC accelerator components, and negotiations within the detector collaborations on how the construction costs of the detector were to be apportioned. Already early in his tenure as Director, Schwitters made several trips abroad to discuss possible collaborations [79]. Several protocols were signed, but no SSC funds (except for US$50 million pledged by India) materialized at that time as a result. Thus, for example, there was an agreement signed by Schwitters with Rubbia regarding interactions between CERN and the SSCL [80] and a similar agreement with Erich Vogt [81], Director of TRIUMF, and with several Russian institutes. Any serious discussion with CERN about collaboration was made difficult by Rubbia's very optimistic view of the LHC time scale. In the second letter to Low he mentioned 1996/7 as the tentative completion date for the LHC and 1999 as the date for the e–p option.

There were also initial efforts to conclude more formal government-to-government agreements. In June 1990 Deputy Secretary Hensen Moore visited Japan and Korea, where he delivered letters from President Bush to Prime Minister Kaifu (Japan) and President Roh (Korea), inviting them to participate in the SSC. There was positive response from the Koreans, and the Japanese said that it would be at least a year before they could decide. A relatively slow response to any new initiative was consistent with standard practice in Japan, where an effort was always made to first reach a consensus. In September 1991, Secretary Watkins and his Korean counterpart agreed to form a joint working group to discuss possible collaboration on the SSC. A similar working group was established with the Russians in July 1991, during Moore's visit to Moscow. It was followed on January 6, 1993, with an agreement (signed by Watkins) between the DOE and Russia's Ministry of Atomic Energy for a program of collaboration on the SSC.

Japan had always been viewed as the most likely significant contributor to the SSC, and thus the principal effort in enlisting foreign contributions was directed toward the Japanese. The Japanese high energy physicists expended significant effort toward participating in the SDC, making up about 20% of the collaboration. There was a segment of that community interested in building a domestic linear collider but most people realized that the time scale for it was significantly later than for the SSC [82]. Subsequent to Moore's trip already mentioned, there was a visit by H. Sugawara, DG of KEK, to the SSC late in 1990. He was not optimistic about Japanese contribution and emphasized that negotiations must be done at a very high level and involve the Finance Ministry [83].

A major effort toward Japan was made in 1991 with a series of trips, with higher level contact on each succeeding visit. It was generally accepted that for a large contribution at the level of US$1 billion, the request has to be made by the President himself to the Prime Minister. Thus the goal of the initial trips was to lay the groundwork for an eventual meeting between President Bush and the new Japanese Prime Minister, Miyazawa. The first visit took place in early October 1991, and was led by Will Happer, Director of Energy Research in the DOE at that time. I participated in that trip, together with Roy Schwitters, Steve Weinberg and Jerry Friedman. We met with middle/high level officials and also had a number of interactions with the Japanese HEP community, the general public and the press. We were given a good reception but the main point that was made to us was that the top priority of the Japanese was to refurbish the infrastructure in their universities. Thus, just like in the US, any SSC support had to be "on top" of funding for their basic research and education needs and therefore probably had to be approved at the highest levels [84]. At one of those meetings Will Happer urged the Japanese to press for such an ideal course of events, namely strengthening the university support and also becoming a partner in the SSC.

This first visit was followed up with a trip by D. Allan Bromley, President Bush's science advisor and Director of OSTP, on October 14–18 and by Watkins on December 3–5. Bromley offered the Japanese "true partnership" in the SSC and invited them to participate in the planning, construction and management of the SSC in return for investment of cash and provision of Japanese-built components. In his memoirs [85] he claims that he received enthusiastic support from the Science Council of Japan for his suggestion of a significant increase in the Japanese science budget, part of it to be allocated for upgrading the Japanese universities and part for participation in the SSC at the level of about US$1.5 billion. Subsequently, on his visit, Watkins met with Prime Minister Miyazawa and also made a strong pitch for Japanese participation in the SSC.

President Bush made a trip to Japan on January 7–10, 1992, and it was widely expected that a formal agreement would be reached with the new PM regarding the Japanese contribution to the SSC. However, the topic of the Japanese support for the SSC apparently never came up in the direct Bush–Miyazawa talks. Bromley claims that it was the White House Chief of Staff, Samuel Skinner, who was responsible for this agenda omission by changing its emphasis to the trade issues. The fact that Bush became sick and threw up during the formal state dinner was undoubtedly a damper on the negotiations. The only substantive outcome of the meetings that was relevant to the SSC was the establishment of a US–Japan working group on the SSCL. According to Japanese press reports, one of the goals of the working group would be to decide by the end of 1992 whether and how Japan should participate in the SSC [86, 87]. This group was led on the US side by Will Happer and Jim Decker, Deputy Director of the DOE OER, and it met several times during the Bush term, starting with a meeting on April 9–10, 1992. The initial discussions focused on resolving significant differences in estimated construction and component costs and establishing common costing methodology; some progress in that area was apparently achieved.

These studies were put on the back burner while awaiting news regarding the Democratic presidential nominee Bill Clinton's position on the SSC.

The negative House Congressional vote of 1992 (discussed below) undoubtedly reinforced this "go slow" attitude. After winning the November 1982 election Clinton supported the SSC with a US$640 million budgetary request for FY94 but also planned a three-year delay in its completion. He met with Miyazawa in Washington on April 16, 1993, but the SSC was not discussed at that meeting. The fact that the US was engaged at that time in rather difficult trade talks with the Japanese, which were high on Clinton's priority list, probably had a role in eliminating the SSC from the agenda. Clinton certainly had no comparable interest in the SSC that Bush did and undoubtedly did not want to use up his credit with the Japanese on a controversial project that was not among his priorities. The delays in reaching an agreement worked against the possibility of eventual success of the project; as time went on, more and more contracts were being awarded and hence there was less and less of an incentive for the Japanese to get involved in the construction.

The situation was made more complicated by the fact that there were influential voices against seeking foreign contributions in kind, which were the only ones that might materialize [88]. The Senate, led by Bennett Johnston from Louisiana, argued for a purely national effort. Initially there was also a sentiment against such contributions in the House [89], but with time, cost-cutting sentiments prevailed there. The GAO pointed out that "any such subcontracts with foreign suppliers will further reduce the multiplier effect of the investment on the US economy." A number of industry leaders expressed concern about the fact that foreign companies were allowed to compete with the US ones, resulting in sending many jobs overseas. Thus there was ambivalence about signing production contracts with foreign laboratories with significantly lower price, which could be viewed as an in-kind contribution to the SSC construction [90].

Had the SSCL survived, some international contributions would have most likely been forthcoming even though nowhere near the one-third of the total cost initially claimed by the DOE. Going over press reports on these negotiations during 1990–1993, I am struck by the naiveté on the American part about the workings of the Japanese decision-making process and the lack of understanding of how to read the Japanese "signals." Negotiations were much more complex, difficult, and time-consuming than anticipated. They required strong coherent high level effort from the Administration, Congress, the SSCL and the HEP community, an effort which at best was sporadic. I have no doubt that the Congressional vacillations regarding the SSC funding played an important role in discouraging foreign collaborations. Barry Barish, GEM co-leader, said that a lot of their foreign collaborators together with their contributions "evaporated" after the House action of 1992 (discussed below). For example, Taiwan withdrew their offer of a US$50 million contribution to the GEM detector. It was a vicious circle, a true catch-22 situation, well summarized by Rep. Brown [91]: "Major foreign participation has remained elusive because of uncertainty about the US commitment to the project, yet our own commitment has wavered in large part because of the absence of foreign funding."

8. Change in the Political and Public Opinion Landscape

By 1990 there was a significant deterioration of the public sentiment toward the SSC. There were many factors that contributed to this change, and they tended to reinforce one other. Certainly, the decision to site the SSCL in Texas cooled the ardor of other site-candidate states. But the most important factor was probably the growing budget deficit. The concerns about the deficit were beginning to be present already when Reagan announced his support for the SSC. On the political front they reflected themselves in the Gramm–Hollings–Rudman Act, officially the Balanced Budget and Emergency Deficit Control Act, which was enacted on December 12, 1985. The law was modified in 1987 so as to comply with the court ruling which found some of its provisions unconstitutional. The act provided for automatic spending cuts if the proposed budgets failed to reach established targets for reducing the deficit [92].

This act failed to curb the deficits and as a result was replaced by a Budget Enforcement Act in 1990. The first provision of the act was the establishment of a limit on the level of discretionary spending, divided into defense, international and domestic sectors. The second one established the pay-as-you-go procedure requiring that increases in direct spending or reductions in revenues due to adopted legislation in a given sector be offset by other legislative actions so that there is no net increase in the deficit. As far as the SSC was concerned, the implication of the law was that any increase in spending would have to be either offset by cuts in other discretionary spending or funded by new revenue. This clearly provided additional fuel to many who opposed the SSC on the grounds that it would eat into financial support for other sciences.

The first significant impact of this new law occurred in 1991, when the resulting decrease in the DOE OER budget forced its Director, Will Happer, to convene a special Panel chaired by Charles Townes, Nobel Prize winner from UC Berkeley, to establish priorities across all the fields in the OER's portfolio [93]. Projected budget levels were such that drastic cuts were necessary; in HEP, the recommendations of the Panel were to delay the start of both the Main Injector at Fermilab and the B-Factory at SLAC. The SSC was placed outside the Panel purview as a presidential initiative, which caused understandable resentment among many physicists. This decision was probably a strategic mistake; for the SSC to be viable in this climate it had to be judged on its scientific merits *vis-a-vis* other potential science projects and come out significantly better [94]. One of the outcomes of the Panel's recommendations was the convening of another HEPAP Subpanel, chaired by Michael Witherell, a professor at the University of California in Santa Barbara. In view of the very pessimistic budgetary guidelines given to the Subpanel, it was not surprising that the resulting recommendations advocated rather severe curtailment of the HEP program, even though it did urge proceeding with the SSC on a fast track schedule [95, 96].

The concern about funding was especially prevalent among the non-HEP scientists, even those who supported the SSC on intellectual grounds. It was clearly stated already in Congressional testimony in April 1987 by many prominent scientists: Sheila Widnall, president of the American Association for Advancement of Science, wrote in her testimony [97]: "We believe that the nation should undertake the construction and operations of SSC, but that it must not be done at the expense of investment in other areas of basic research." Dan Kleppner, an atomic physicist from MIT and generally quite sympathetic to HEP goals, testified [98] at the same hearing: "I believe that the nation can and should build the SSC. However, if we are to have a future in science, it is essential to provide our universities with the resources to maintain their roles as world leaders in education and research." Senator Bennett Johnston, one of the strongest supporters of the SSC in the Senate, was quoted as saying: "I don't want to cannibalize everything in science for this one project." The Administration tried to alleviate some of these concerns by proposing rather robust budgets for civilian science R&D [99]. For example, the proposed increase for the NSF basic research funds for FY93 was 21%.

Given this emphasis on fiscal austerity and the concerns about deficits, aggravated by the first indications of a recession, the escalating cost of the SSC became a major issue. Another contributing factor was the lack of any significant commitment from abroad to contribute to the SSCL construction and thus reduce US expenditures. Together, these two factors drove the required federal allocations to such high levels that they were deemed by many to be unacceptable.

The situation was aggravated by a previously mentioned, rather critical 1991 GAO report on the SSCL and the DOE, prepared at the request of Sen. Jim Sasser (D-Tenn), chair of the Senate Budget Committee [100]. The report noted that the DOE Office of the SSC "lacked stable leadership since it was created in January 1989" and identified a number of technical areas where they felt that there was a considerable performance and cost risk. The DOE tried to vigorously rebut the report point by point in a letter [101] by Secretary Watkins to Jamie Whitten, Chairman of the House Committee on Appropriations, but undoubtedly the report did influence the opinion of many in Congress and hence subsequent deliberations there.

The conclusion of the site competition significantly decreased the ranks of SSC proponents — one notable example was Rep. Boehlert from New York State. A way to describe this shift in sentiment could be: "What might have been a potential US$4.4 billion exciting science project in our backyard turned out to be a US$8.3 billion pork barrel boondoggle in Texas." The SSC contractor, the URA, was ill-suited to lead a strong campaign for the SSC, being composed of some 80 universities, none of which had a strong stake in the SSC. Many of them had strong local opposition against it within its own non-HEP faculty.

The SSC was also hurt by being lumped in public discussion with the International Space Station [102]. Articles and editorials were written arguing that we cannot afford two huge science projects, even though there never was a good case made that the Space Station was motivated on scientific grounds. The Space Station also had numerous opponents in Congress and one year its appropriation survived in the House by only one vote. However, in a direct competition, the SSC had to lose out for a number of reasons. It was much easier for a layman to understand the Space Station, the stars being visible, unlike the inner structure of a nucleus. Paradoxically, its much higher cost had some advantages, as it provided opportunity for large and widely dispersed industrial involvement in it and hence a larger army of potential lobbyists arguing its case. NASA had a strong tradition of involving industrial concerns in the development and construction of its projects and learned how to use this connection. It certainly was much more experienced and more successful in lobbying Congress than the DOE Office of Research. Finally, not being associated with any single state, it was harder to characterize the Space Station as a local pork barrel project, as opposed to the SSC, for which most of the new jobs were going to be in either Texas or Louisiana.

False information about the SSC played a major role in turning the opinion of many against it. I have already mentioned the unrealistic hopes about the high temperature superconductors (in the first article [1]) and the lure of the LHC at CERN as a cheap early alternative. A new potential option that was gaining more prominence was a possible e^+e^- linear collider [103] in the 1 TeV energy range. There was vigorous worldwide R&D in this area, especially in the US and Japan, and SLAC was beginning to observe collisions with its prototype SLAC Linear Collider (SLC). However, it was apparent to everyone involved in this work that construction of a high luminosity TeV collider was still more than a decade away [104]. Nevertheless, these three potential alternatives were frequently cited as arguments for delaying or canceling the SSC.

The general change in the mood of the country is well illustrated by an editorial [105] in *The New York Times* in 1990 titled "The Behemoth and the Boson." This presents a remarkable contrast to the one published less than seven years earlier [106]. A few quotes illustrate this new, somewhat naive and misinformed anti-SSC point of view: "By buying into the Europeans' Large Hadron Collider, which would cost only US$1 billion to build, American physicists would be assured of a ringside seat... its magnets might use the newly discovered materials that become superconducting at high temperatures... why not invest in linear colliders instead of a machine at the dead end of evolution?... nothing of scientific interest may lie in the energy range the supercollider opens up.... These risks would be worth running in better times, but not in the straitened circumstances that now prevail."

Even the once-unequivocal support from the HEP community for the SSC began to weaken. The 1983 panel, after extensive discussion, decided that an intermediate time scale machine was not essential for the health of the US HEP. That decision was based on an estimated time scale for the SSCL of about 12 years from then. In 1990, the projected SSC completion time was September 1999, 4 years later than the original estimate, and even that projection was based on unrealistically high funding levels during the peak years. Thus many in the community began turning their attention to other areas. There were great pressures not to sacrifice any elements of the existing program (which might be required by the newly enacted budgetary caps) and even to expand it by construction of the Main Injector at Fermilab and the B-Factory at SLAC [107]. Such attitudes reinforced among other scientists the image of greedy HEP physicists. In addition, the uncertainty in the completion date and/or doubt whether it would ever be completed made recruitment of staff to the SSCL quite difficult.

Important changes also occurred in the national political landscape shortly after the establishment of the SSCL in Texas. The first one was the resignation of House Speaker James Wright on June 6, 1989, induced by charges of ethics violations. Wright represented the 12th Texas Congressional District, which included Fort Worth, not very far by the Texas standards from the SSCL location. He had served as Speaker since 1987, and previously as majority leader since 1976. He was replaced as Speaker by Tom Foley from the State of Washington (which also submitted a bid for the SSC). Wright was a strong supporter of the SSC and his resignation from the House created a climate that was much more favorable to anti-SSC sentiments. In the early 1990's the activities of the Texas delegation probably had a negative impact on the SSC. While they argued for the SSC funding, a majority of them also pushed for budgetary cuts in social services and supported the constitutional amendment requiring a balanced budget.

The other important change arose from the results of the November 1992 elections. Bush lost his bid for the second term to Bill Clinton, and thus the SSCL lost another powerful advocate. Bush was strongly committed to the SSC and clearly wanted to carry on the Reagan-initiated project; Texas was his home state; he appears to have been genuinely intrigued and interested by the scientific issues the SSC was going to address. Soon after the SSC concept was initiated, during his Vice-Presidency, he was given a briefing on the SSC by the TAC people. Before the crucial SSC votes in Congress in 1992, Bush actively lobbied Senators and House members who were on the fence regarding the SSC. He visited the Laboratory in July 20, 1992. Bush's science advisor, D. Allan Bromley, was a nuclear physicist who actively worked to make the SSC a reality.

In contrast, Clinton was probably initially neutral on the SSC and was very much focused on balancing the budget. Already in the initial FY94 budget proposal the SSC completion date was delayed by three years [108]. Vice-President Gore had voted against the SSC as a Senator in 1991. The new Energy Secretary, Hazel O'Leary, stated that she was not "passionate" about the SSC [109]. The new Director of Office of Management and Budget, Leon Panetta, voted against the SSC when he was a Congressman from California and argued initially for canceling the SSC. Clinton chose Lloyd Bentsen, Senator from Texas, as his new Treasury Secretary, thus removing from the Senate an ardent and highly influential supporter of the SSC. Finally, 114 new members were elected to the House in 1992, many of them on a budget-balancing platform. Thus their natural instinct might be to vote against the SSC. Approval of a major project in Congress requires strong commitment from one or more key people in the Government who are willing to do horse-trading and arm-twisting to get the required votes for it. There was no one in the new Administration or new Congress who would be doing that for the SSC.

The final relevant event here was the end of the Cold War. With the fall of the Berlin Wall in 1989, there also went away the argument dating back to the Manhattan Project that physicists might be useful in case of a national emergency. A corollary of this change was that now support for science would have to be justified in terms of clear immediate payoffs. In addition, nationalistic arguments, professing that being on the technological and scientific frontier is essential for a superpower, which were so important for getting the SSC initiated in the Reagan era, were no longer persuasive to most people, especially for pure science projects without any obvious immediate benefits.

9. Cost History Analysis

Much has been said about the cost escalation of the SSC and it was the major issue in the Congressional SSC debates. I summarize here the relevant history [110] but caution the reader that the numbers do not tell the whole story and could be misleading. The cost numbers quoted at different times have been heavily influenced by the methodology used for the different estimates, as well as by the assumed length of the construction period and the construction start date.

Very rough cost estimates were made during the Snowmass '82 meeting, but the first serious cost estimate was made at the Cornell workshop in 1983, with a range of US$2.7–3.05 billion in 1983 dollars for the collider only. The first "official" cost estimate for the collider was US$3.01 billion in the CDR. At that time it was becoming standard, at the urgings of the GAO [111], to quote the so-called total project cost (TPC), which included, besides accelerator construction, supporting R&D, detectors and computers, and operating costs incurred prior to project completion. Using this criterion, the SSC cost range, as estimated by the CDG, was US$3935–4247 million in *1986 dollars*. The CDG estimated that the construction time was $6\frac{1}{2}$ years. Based on their own and ICE reviews, the DOE increased the total cost somewhat and extended the schedule by two years to 1996. Using the new schedule and minor cost changes, the DOE developed the first TPC estimate in *then year dollars* as US$5.32 billion. It was based on the assumption that the construction would start in 1988 and follow the stipulated funding profile. This was the budget submitted to Congress soon after Reagan's endorsement of the SSC. For the sake of comparison with future estimates, that is the number which should be used as the initial official cost estimate.

Congress did not appropriate any construction funds until FY90; this made it impossible to maintain the FY96 completion date. The date was moved by two years to FY98. This delay and stretch-out of the schedule resulted in a cost increase of about 10%; the TPC figure submitted to Congress as part of the FY90 budget request was US$5.89 billion [112].

The next DOE SSCL cost estimate of US$8.25 billion was generated in 1990 after the site specific CDR and the associated cost estimate were prepared by the SSCL and their design and cost estimates extensively reviewed by several committees. It took into account significant design modifications and also a one-year delay in the SSC completion till FY99. Very roughly, of the total cost increase of about 55% over the base design (8.25 vs. 5.32), a little over one-third, could be attributed to inflation and schedule stretch-out, and the rest to changes in design. The state of Texas agreed to contribute US$1 billion to the construction of the SSC [113], so the federal cost would be US$7.25 billion minus any foreign contributions.

In Fig. 3 I reproduce the cost history as given in Elioff's summary [110], where he reduces all the numbers to FY93 dollars. This gives a fairer measure of the actual cost increase due to technical changes and/or extension of the schedule by ignoring the part of the increase due to inflation. The construction cost of the accelerator itself, the so-called total estimated cost (TEC), is indicated as the solid line. The TPC, which in addition takes into account associated R&D, equipment, detectors and preoperations, is shown as the dashed line. At the beginning of the SSC's history it was standard to use just the accelerator construction cost (TEC); at the end the standard was the total cost (TPC). As the figure shows, the only significant cost increase due to technical modifications was in 1990 and was associated with the previously discussed site-specific redesign. The figure does not consider potential subsequent cost increases (discussed below) which might have occurred had the SSC been allowed to continue.

There was no subsequent formal SSCL cost estimate. In the first three years of construction (FY91–93), the appropriated funds were US$233 miillion less than in the projected profile. That did not bode well for completion of the project within the baseline cost. The new Clinton administration was under great pressure to reduce the expenditures across the board, including the SSC. To reduce annual expenditures, they proposed a stretch-out of the SSCL schedule by three years [114], with completion in FY2002, and requested the Laboratory to provide another baseline estimate taking into account the new funding profile. The DOE estimate [115] of this change was that it would result in a US$2 billion increase in the as-spent project cost, this estimate being accurate to 20%. In parallel, the DOE appointed a 75-member committee to review the status of the SSC (DOE Review Committee on the Baseline Validation of the SSC) and provide a written report in August 1993. Their charge was to compare their findings with the projections in the SSCL monthly report from May 1993 — the Cost Performance Report (CPR). The committee spent over two weeks at the SSCL looking at the SSCL records and interviewing SSCL officials.

In general, the committee found that most of the technical systems were 4–12 months behind schedule and identified a number of critical path items that had to be delayed because of funding shortages. They flagged the collider magnet construction as a major item with potentially significantly higher eventual cost than was then stated. The only possible identified technical unknown was the issue of synchrotron radiation in the beam pipe which was under study at that time. They estimated a potential cost increase of up to a total of US$9.94 billion if changes in management organization and some new procedures were not implemented. In addition, they identified US$1.21 billion in costs that were related to the SSC but not included in the TPC (spares, preoperating costs, foreign contribution to the detectors). They also suggested adding US$219 million to the contingency.

The report of this Review Committee formed the basis of the claim that the SSCL costs went up to over US$11 billion. But it must be noted that the Review Committee did not do an independent cost re-evaluation but merely compared their cost estimates of "to go" items (i.e. parts of the project yet to be completed) with the May 1993 SSC Cost Performance Review (essentially the DOE monthly review of the SSCL). No optimization of schedules, tradeoffs, procedures, staffing levels, construction models or design modifications were part of this review. The initial SSCL study of possible ways to mitigate

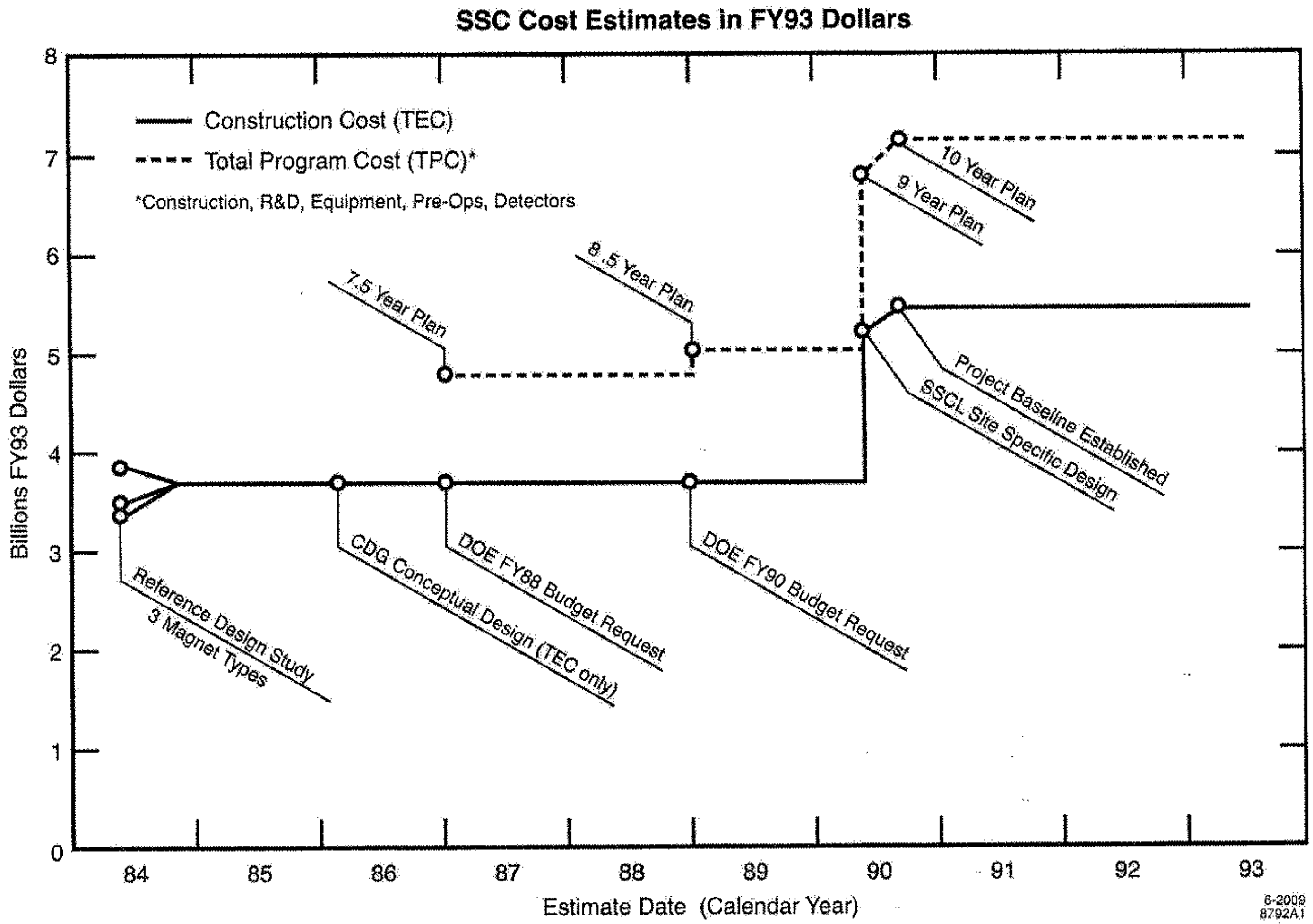

Fig. 3. Estimated SSC costs in FY93 dollars shown as a function of time. Both TEC (accelerator construction cost) and TPC (total project cost) are shown.

potential cost risks indicated that significant reduction of these risks could be achieved [116]. This value engineering exercise indicated that the US$1.354 billion total cost risk estimated by the Review Committee could be reduced by US$852 million. These studies were never completed or reviewed because of the SSC cancelation in October 1993.

What the cost of the SSCL would have been will never be known, since a full-blown rebaseline effort was never completed and the Laboratory was not allowed to continue. Quite likely, the biggest unknown would still have been the funding profile that the Administration and Congress would decide on [117]. SSC opponents postulated even higher costs (Sen. Bumpers put forward US$39 billion), based on including interest on the extra debt incurred by the federal government by deficit funding of the SSC and/or including operations for 25 years, with interest on the cost of those operations. The cost issue was murky and easily amenable to different interpretations and emotional excesses.

Besides the cost of construction, the issue of SSCL operating cost was also frequently distorted. There were a number of cost estimates for SSC operation and they changed very little with time. The cost calculated by CDG [118] was US$224 million (FY85 dollars) without capital equipment, Accelerator Improvement Projects or General Plant Projects. The latest estimates from the SSCL ranged from US$317 million (FY2001) to US$369 million (FY2005) [119]. These estimates were quite consistent with what one might obtain by applying simple scaling from the operating costs of the existing laboratories. However, some of the SSC opponents inflated them significantly without providing any rationale for it. For example, Jim Krumhansl, in his letter to Secretary Herrington [120] wrote: "An additional operating budget of $1–2 billion a year at least will be required."

One final point needs to be emphasized. A project on a green site is much more vulnerable to schedule stretch-out than one in an ongoing laboratory. On a green site, every dollar spent is counted against the TPC. Expenses that would be part of operating costs in an existing laboratory (most of laboratory administration, library, upkeep of infrastructure, general R&D, etc.) are charged to the project. If a project is stretched out, there is some flexibility within an existing lab to shift costs to other efforts and thus reduce the impact of a stretch-out. No such flexibility exists on a green site.

10. Congressional Roller Coaster

The country's new mood was also reflected in the Congressional attitudes. Whereas the Senate was generally favorable to the SSC, strong vocal opposition arose in the House shortly after selection of the Texas site. The four Members most prominent in this opposition were Sherwood Boehlert (R-NY), James Slattery (D-KS), Dennis Eckart (D-OH) and Howard Wolpe (D-MI). Whereas the last three had no scientific background or significant involvement in science issues, Boehlert had been a supporter of science and basic research during his Congressional career. He supported the SSC initially, but turned against it in 1988, after the New York state sites were no longer in competition (the transborder site was disqualified by the DOE, the Newburgh site was withdrawn early, and the Rochester site was withdrawn upon the BQL announcement). He was also persuaded by Krumhansl about the negative impact of the SSC on support of other sciences. None of the other three Members remained in the House after 1994; Slattery and Wolpe left (in 1995 and 1993, respectively) to run for Governor in their respective states (both unsuccessfully) and having fiscally conservative credentials was clearly important for those races. Eckart chose not to run for the 93/94 term and left political life altogether.

Initially there was a strong majority in the House in favor of the SSC. The first substantive vote took place in June 1989 on the proposed FY90 US$190 million appropriation that included the first construction funds for the SSC. Rep. Eckart's amendment to remove the US$100 million in construction funds failed by a vote of 330 to 93 [121]. A year later, a similar motion to stop SSC construction was also defeated [122] by a very lopsided vote of 309 to 109 even though the proposed SSC allocation rose to US$340 million.

A clear sign of changing mood occurred in 1991; Eckart again proposed an amendment to strike the SSC funds (US$483 million) from the budget, which was again defeated, but by a significantly narrower margin of 251 to 163. A similar amendment in the Senate introduced by Sen. Bumpers from Arkansas lost by a 62–37 margin [123]. These were the first votes after the design change and announcement of an US$8.25 billion price tag for the SSC.

The SSC opposition grew substantially in the House during the following year. 1992 was an election year and in times of deficits showing fiscal austerity was a definite plus. Furthermore, the negative publicity about the SSC (technical alternatives, cost escalation, mismanagement accusations, lack of foreign contributions) began to have an impact.

The President's budget request for FY93 was US$650 million. The House Appropriations Subcommittee on Energy and Water, chaired by Tom Bevill (D-AL), voted to allocate US$484 million to the SSC but the Eckart amendment [124] to strike all but US$33.7 million from that proposal passed the House by a surprisingly large majority of 232 to 181. The House vote occurred just a few days after the vote on a constitutional amendment to require balanced federal budgets. Many Representatives who voted against that amendment wanted to restore their fiscal credentials by voting against the SSC.

This House action galvanized the SSC proponents, many of them abroad, into sending letters to key individuals in the government urging them to try to reverse this vote [125]. The result was positive and the Senate voted to allocate US$550 million to the SSC, helped to a large extent by the personal intervention of both Bush [126] and Bromley. As is the standard procedure, a conference committee was appointed to arrive at a compromise. The House and Senate leaders pick the committee, and only SSC proponents were chosen as House conferees. Thus it was not surprising that the recommendation was to approve the SSC funding, with a figure of US$514.5 million (about US$135 million below the President's request). The House Appropriations Committee, in allocating this lower sum, recognized in the associated language [127] that such action "will adversely impact both final cost and schedule." Subsequently, both Houses approved the bill coming out of the conference.

The SSC was saved for another year, but it was severely wounded, and short of drastic action it would be even more vulnerable the following year. For some time now there had been simmering resentment in Congress against the Appropriators, who tended to use their positions to obtain funding for "pork barrel" projects in their districts. Going completely against the will of the House in the conference undoubtedly amplified this feeling. Generally members of Congress are reluctant to vote against energy and water bills because of the "goodies" that they bring to their districts or their states; this undoubtedly explains the positive House vote on the final bill. But one could expect that the SSC opponents would try to come up with a more effective strategy for the following year.

11. The Death Blow

As the time approached for the FY94 Congressional appropriations activities, it was clear that the SSC question — to fund or to kill — would come up again [128]. Furthermore, several other important events happened during the intervening year — essentially all of them boding poorly for the SSC's fate. Maybe the most important one was the re-examination by the Clinton Administration, as part of its budget planning process, of the SSC schedule. Its outcome, stretch-out of the construction through 2002 and the resultant rise of the projected total SSC cost to about US$11 billion, fueled the arguments of the SSC opponents that its costs were out of control.

There was intensified criticism of the DOE oversight and SSCL management. A report by the GAO published in February 1993 was highly critical of the DOE, URA and SSCL in those areas. It argued that the SSC was over budget and behind schedule. A major complaint had to do with the absence of standard management tools used to track progress and expenditures, e.g. the Cost and Schedule Control System. The report accused the SSCL staff of impeding the GAO investigation by being less than forthright in providing requested information. In the same time frame the office of the Inspector General (IG) of the DOE performed their own investigation of the SSCL and came up with similar conclusions.

On February 25, *ABC Prime Time Live* devoted a segment to the SSC, which focused entirely on the negative news in these reports. The other investigation ongoing at the Laboratory early in 1993 was by the staff of the House Subcommittee on Oversight and Investigation, chaired by Rep. John Dingell (D-MI), in preparation for subsequent hearings. Looking back on these investigations, it is clear that the focus was on bureaucratic issues rather than on the level of performance. The criticism was that the proper procedures were not followed, that formal control systems were not established, that various actions were not documented adequately. There was very little attention paid to how well the goals were being accomplished.

These investigations impacted the morale in the Laboratory and were a significant distraction for everyday work; things were also aggravated by the ever-increasing antagonism between the DOE SSC officials and the SSCL managers and also among the SSC staff. There were serious efforts by several DOE officials to remove some of the high level URA SSCL managers but such action was vetoed by Watkins. In a leaked memo from his computer, Cipriano was reported to argue to higher level DOE officials that Schwitters had to be replaced to save the SSC [129]. As a result of all of these events a number of SSCL employees started to leave. There was a net decrease in the headcount at the SSC in September of that year. The level of frustration is well illustrated in the following quote from Schwitters [130]: "Every one of us here spends most of his or her time answering questions from Washington or trying to comply with the latest twists in accounting rules dreamed up by people who want to kill the SSC. We should be devoting ourselves to completing this machine as rapidly and cheaply as possible and getting on with real science. Instead our time and energy are being sapped by bureaucrats and politicians. The SSC is becoming a victim of the revenge of the C students."

The IG report was dated May 19, 1993, and its content leaked shortly before the House vote on the appropriation bill for energy and water. That vote was 280 to 150 to terminate the SSC; the 114 House freshmen voted almost 3 to 1 in favor of termination [131].

The Dingell Subcommittee hearing was held on June 30 and it highlighted the issues that both the GAO and the IG found amiss [132]. The tenor of Dingell's introductory remarks was very scathing of the DOE and SSCL management: "SSC ranks among the worst projects that we have seen in terms of plain contract mismanagement and failed government oversight... The inability of the Department and the inability of the prime contractor to take control of this program is a problem that has persisted for far too long, and, like cancer, it has grown." There were certainly some management issues that were a fair target for criticism. But many other complaints and accusations showed either naivete, pettiness or deliberate intention to cast the SSC in the worst possible light. It was a perfect example of "guilty until proven innocent." Happer's paraphrase [133] of an ancient Greek proverb, "He whom the gods would destroy, they first make to appear foolish," seems especially on the mark here.

The Laboratory was criticized for improper subcontracting without bids for contracts valued at US$216 million; US$156 million of that sum was for work by BNL and Fermilab on prototype magnets, for which no industrial concern had the required expertise. The discussion of budgetary issues ignored the contingency held by the DOE, of which only 3.5% has been spent by the summer of 1993. The Laboratory was accused [134] of "lavish spending of taxpayers' money on luxuries and entertainment"; one of the highlighted items was the US$21,000 spent for annual maintenance of plants used to brighten up the warehouses modified for use as office space. The use of warehouses, rather than conventional office space, was said by the SSCL to save the lab US$500,000 annually [135]. Incidentally, the idea that one would spend some money for esthetic enhancement of buildings is not radical. Several states require that a certain fraction of construction funds for state-funded buildings be spent on artworks. According to the DOE IG, John C. Layton, in his testimony before the Dingell Subcommittee, the DOE requested an audit "to determine the allowability of the $250 million in incurred costs. The audit identified about $233,000 in questioned costs." This represented 93 cents out of each $1000 spent.

Clinton's Secretary of Energy, Hazel O'Leary, testified at that hearing. She did not defend the SSC very strongly in her testimony and agreed with a lot of the criticism, especially on management issues [136]. The general tenor of her remarks was that most of the current difficulties at the SSCL were due to the lax oversight by the previous administration and rather unusual reporting procedures set up by them. She promised to study all the problems and said that she was prepared to revamp the management drastically.

On August 3, 1993, Sen. Pryor from Arkansas urged another investigation of the SSC [137]. In a letter to the DOE IG, he expressed concern that some individuals at the SSCL were engaged in improper and/or illegal lobbying activities, via various letters, meetings or phone calls. The SSC was terminated before this investigation could be initiated.

SSC was confronted with additional problems on September 1, when the DOE released a report from its review committee [138] (DOE Review Committee on the Baseline Validation of the SSC) which outlined the potential cost risks discussed previously. But the report also said many positive things. To quote O'Leary [139]: "The committee confirmed that the project is 20 percent complete, which is where we should be on the project... also found that the 73 major subcontracts awarded to date, in aggregate, have come in at approximately seven percent under budget." The report mentioned the difficulty of obtaining solid cost estimates and reliable schedules due to unsatisfactory records and bookkeeping even though they stated that "SSCL was most responsive to the committee." On the whole the report was rather objective but quite detailed and lengthy; as such it lent itself very well to 30-second sound bites by the SSCL opponents.

The URA and the SSCL made rather detailed written and oral rebuttals [140] about the charges made in the IG and GAO reports and during the Dingell Subcommittee hearing. Rep. Bryant from Texas, a member of the Dingell committee, also tried to refute some of the charges during the hearing. But the charges were given much more attention by the press than the rather complex explanations.

Secretary O'Leary tried to save the SSC by initiating several steps designed to contain the SSC costs. Five planned actions were enumerated in the DOE news release [141] of September 1, 1993, one of them being reduction of the cost through downscoping. The plan was to appoint a task force, to be chaired by the DOE's Wilmot Hess, which would identify by November 30, 1993, items in the current design that could be eliminated. As HEPAP chair, I was asked [142] on September 20, 1993, to appoint a HEPAP Subpanel which would "rank them in order of potential impact on the scientific objectives of the SSC. These project changes will be implemented, in the order established by HEPAP, to the extent that they are necessary to avoid cost overruns of the project." The subsequent events, however, made these plans irrelevant.

The Secretary tried to deflect the management criticisms by a drastic action: replacing the URA in most of its duties by an industrial contractor experienced in construction of large projects. The URA would retain the responsibility for design and operation, but the new contractor would oversee conventional construction, magnet production and installation [143, 144].

During that spring and summer a significant effort was made by the advocates of the SSC to influence Congress to vote in favor of continuation of the SSC. There were numerous letters and visits by physicists and other SSC supporters to their Congressional representatives. The TNLRC made an organized effort to shift the public opinion in favor of the SSC. This was done by sponsoring trips by teams of 3–5 members (typically a TNLRC member, a member of the SSCL, a physicist from the area who was involved in the SSC, and a member of a local industrial company with an SSCL contract) for meetings with editorial boards of local newspapers. There was certainly a need for some activity along those lines; it seemed that a favorite sport of many editorial boards at that time was to write negative opinions about the SSC with cute titles. Some examples from California (where I participated in one of those trips) were: "Science in a Pork Barrel" and later "Atomize this Boondoggle" (*Santa Ana Orange County Register*), "Superconducting Superporc" (*Sacramento Bee*) and "Quashing the Atom-Smasher" (*La Habra Daily Star Progress*). But these activities were "too little and too late." By the time they were initiated the opposition was well organized [145] and general opinions were pretty much set; these efforts had very little noticeable influence.

There was essentially no significant lobbying for the SSC by the upper levels of the Administration. Clinton's support was limited to a letter written to the House leadership in which he expressed his continuing support for the project [146]. The overall White House support was lukewarm at best. Speaking of the SSC and the Space Station, the press secretary Dee Dee Myers stated [147] that they remain in the President's budget "at this point" but added that the two projects are not among essential "principles."

Thus by the summer of 1993 the situation looked very bleak for the SSC but Sen. Bennett Johnston (D-LA), chief strategist on the Senate side and the SSC's staunchest supporter, thought that the situation could still be saved. He wanted to have a strong pro-SSC vote in the Senate with the hope that a stacked conference committee would again be willing to go along with the Senate recommendation [148, 149]. Johnston organized a joint hearing on the SSC of the Senate Committee on Energy and Natural Resources and of the Senate Appropriations Subcommittee on Energy and Water Development, with the idea that distinguished scientists would testify to the benefit of the SSC and sway the Senate to approve the full funding.

The initial steps went as hoped for. A number of prominent scientists, among them Jerry Friedman, Leon Lederman and Steve Weinberg, testified in favor of the SSC. Roy Schwitters, in his initial remarks, tried to rebut the allegations made against the SSC during the previous months [150]. There was some opposition led by Sen. Bumpers, who was supported in his downplaying of possible SSC spinoffs by N. Bloembergen, Nobel Prize–winning physics professor from Harvard. How influential the hearing was is not clear; what was important was that the Senate voted 57 to 42 for full US$640 million funding for the SSC. The next step was also successful. The conference committee went along with the Senate and reported out US$640 million appropriation for the SSC.

But after this, the whole plan unraveled. When the conference committee report was brought back to the full House on October 19 for the vote, Slattery introduced an amendment to send the report back to the Conference with a specific instruction to eliminate SSC funding. There was a heated debate on the floor, with both opponents and proponents repeating the arguments that had been made previously. The escalating costs, the need to balance the budget, the poor management, the lack of foreign support, and the esoteric nature of the science were all put forward in the arguments against the SSC. I do not think that the debate influenced many people to change their vote. The perceived need to show fiscal austerity overwhelmed all other considerations. The SSC was doomed to lose this one even before the debate began. The final vote was 242 to 183 in favor of Slattery's amendment; 81 of 113 freshmen voted with the majority [151].

When the conferees met on October 21 it was clear that the opposition had won. This was acknowledged by the leadership when a rather unusual step was taken of inviting both Slattery and Boehlert (and also SSC proponents from Texas) to the conference committee meeting. The opposition to the SSC in the House was so strong that there was no chance that termination could be avoided. The debate was reduced to a discussion on what fraction of the US$640 million should be used for the closeout and how the SSC employees should be treated. There was also a discussion on how to make the best use of what had been learned and developed at the SSCL. Both Johnston and the Texas delegation argued passionately for fair treatment of all the staff, especially in light of the suddenness of the termination. In the end, the full $640M was allocated for termination costs. The conference report was formally approved by both Houses and signed into law by President Clinton a few days later. The SSC was dead [152].

I mention only briefly some of the subsequent SSC-related events [153]. Roy Schwitters resigned on November 5, 1993, as the Director and took a faculty position at the University of Texas in Austin. He was replaced by John Peoples, Director of Fermilab, who was given the painful job of orderly termination of the SSC. Of the 1943 employees at the end, many were able to return to their previous places of employment, laboratories or universities. A number found new positions in their line of work. But a number of others suffered serious professional and financial setbacks [154]. There was also a pretty universal feeling in the US HEP community that the field had suffered a devastating blow [155]. The closeout activities lasted two years, coming to an end only near the end of 1995 [156]. The total cost of the SSC termination was US$736 million.

The first HEPAP meeting after the SSC termination took place on November 8–9. Secretary O'Leary wrote to me as HEPAP chair: "... we would like recommendations on how to make the best use of the investment that has been made in the SSC," and also "DOE would like HEPAP to turn its attention immediately to the task of defining a long term program to pursue the most important high energy physics goals... we request that a dedicated subpanel be constituted to address these issues." At that HEPAP meeting, the CERN Director of Research, Lorenzo Foa, extended an invitation to the US HEP community to participate in exploitation of the LHC. An effort was made to dispose of SSCL tangible assets but they brought only a fraction of the original investment.

The most significant subsequent event was the convening of the Drell Subpanel in the spring of 1994, which recommended US participation and contribution to the LHC [157]. That led to initiation of lengthy but eventually successful negotiations about US involvement. The US contribution enabled immediate construction of the full LHC rather than the staged approach initially approved by the CERN Council [158]. The US financial contributions to the detectors enabled the currently planned LHC experimental program. Many ideas developed within the framework of the SSC detector R&D program influenced significantly the design of both LHC detectors: large scale silicon strip trackers and silicon pixel detectors deserve special mention. Nevertheless, even with the large scale US involvement, the LHC construction was completed only in the summer of 2008, some 24 years after Schopper's discussion of it at the AAAS meeting [159]. The first collisions are currently (summer 2009) planned for late fall of 2009. Clearly, the long time scales for construction of major science projects are a fact of life, a fact which needs to be kept in mind when planning such future initiatives.

12. Why Did It Fail?

My primary goal for this article was to describe the facts to the best of my knowledge in as objective a way as possible. I end by giving some subjective conclusions. The SSC demise generated a number of postmortem papers with explanations as to what went wrong with the SSC and what caused its demise [160–162]. I will give my views on this question but first I will address an equally important one, one that is seldom asked but should be: What went right with the SSC? We sometimes forget that the SSC almost succeeded; considering the challenge it presented and the long odds it faced at its beginnings in 1983, many things must have obviously been done right. Al Trivelpiece told me some time around 1986: "There are hundreds of different ways that we can fail with the SSC but probably only one way to make it happen. We need to find that way."

Several things allowed the SSC to progress as far as it did. First, in the CDG there was a group of highly dedicated and highly competent people, under inspired centralized leadership, who devoted several years of their professional careers to making the SSC happen. Their ability to work together toward a common goal only increased with time. The milestones were met in spite of limited budgets. Secondly, in the CDG days there was still a relatively collegial relationship with the DOE; there were innumerable reports, reviews and conflicts, but the performers on the whole were allowed to perform without daily micromanagement. There was no strong feeling of "us vs. them," but rather of two groups with different obligations working toward a common goal. Similarly, in these early times there was no adversarial relationship with Congress; the key people in Congress who took interest in the SSC saw it as an important scientific tool and wanted it to succeed. Undoubtedly many others were driven by more parochial interests, i.e. hopes of landing the SSC in their state; but there were also others, like Vic Fazio and Ron Packard from California (with whom I had many interactions), who remained steady SSC supporters until the end, well after California was eliminated from the site competition. Finally, and this is somewhat controversial, the SSC got as far and as fast as it did because it was initially conceived as a national enterprise and "sold" to the public and the government as such. Key decisions required for initiation of the project could be taken without having to engage in international negotiations. But there was a price to pay for this fast start later on: future negotiations for international participation and material support would be much more difficult.

So why did it eventually fail? There was a spectrum of reasons. Almost every player in the SSC "game" bears a share of the blame and I will address that shortly. I feel that there were three key branch points at which decisions went the wrong way; alternative actions might well have saved the SSC. This is a very personal, hindsight view; a number of my colleagues, whose views I deeply respect, see these events differently.

The first one involved the choice of the site. In retrospect, choosing a green site in Texas was a mistake. There were many hidden costs and additional complexities generated by not using an existing national laboratory site. One of the most important ones was the need to build up scientific staff from scratch. One of the shortcomings of the site selection process was that the ability to recruit first class technical staff was not given sufficient weight. Whether the Tevatron energy was or was not high enough was almost irrelevant; one could have always replaced the old magnets with new, higher field ones and have a 1.5 TeV injector. Funding shortfalls would have been easier to absorb. Even though it might have been harder to get political support for the SSC initially, in the long term the political battles in Congress would have been minimized. The SSC at Fermilab would have been much more difficult to criticize as a pork barrel project. And, very importantly, one would have avoided a very serious and potentially divisive future problem for the field: What does one do with Fermilab when the SSCL is fully operating? Finally, a cancelation of the SSC at Fermilab by one Congress could conceivably have been reversed by another a few years later.

The second key branch point involved the decisions regarding the SSCL management team and the transition from CDG to SSCL. There were a number of missteps, some of which I alluded to in the previous article. First of all, there was the question of partnership with Sverdrup and EG&G. This changed the culture of the management and of the Laboratory from what was traditional in HEP and it was not clear that it contributed much in either technical expertise or political support; it also was a major factor in generating a large "standing army" from the very beginning, a deadly situation when the project had to be stretched out. Secondly, as described in the first article, there was a somewhat behind-the-scenes selection process of the Directorate which created a lot of antagonism in the community. In addition, the process was too hurried and there never was enough thought given to the qualifications that the Director must have to be able to adequately face the challenges associated with the job. Thirdly, there was a lack of appreciation of the collective wealth of technical information residing in the CDG. An effort should have been made to build the new organization with the CDG people as its basis. Such a smooth transition would have made the site-specific planning more efficient. Even if there were difficulties in creating a working arrangement between Schwitters and Tigner that was satisfactory to both of them, the BOO and the URA should have worked much harder to find a satisfactory solution.

The final ill-fated decision point had to do with the design modifications. As mentioned earlier, the possibility that a resulting cost increase could doom the project was apparently never considered seriously. There were many negative repercussions of this cost escalation. This escalation stretched out the time scale of the project, resulting in additional cost increase due to the "standing army" effect. At termination, SSCL staff numbered 1943 people (not including contractors' people on site), projected to rise to about 2500, requiring an annual expenditure for salaries and benefits probably over US$150 million. The needed annual appropriation was now so close to the maximum feasible that significant stretch-outs (and hence additional cost increases) were unavoidable. Equally important was that it fueled the argument that the "egghead academics" could not manage a big project without cost overruns. Stretch-out of the project influenced the research plans of the HEP community, resulting in increased demand for other facilities and in growing difficulties in hiring people for the SSCL. It also decreased the chances of getting foreign support.

How could the different parties have done better? The URA and the BOO could have done a better job in deciding on industrial partners, in choosing the Directorate and in negotiating details of the M&O contract. The URA should have used the influence of the universities to keep the record straight regarding the SSCL and its science and provide stronger advocacy for the SSC at all levels. The SSCL management should have been more aggressive in keeping a lid on the costs; specifically, it can be faulted for not anticipating better the potential repercussions due to the cost escalation caused by the design changes. It could have done a much better public relations job, especially in the area of refuting the ill-informed anti-SSC attacks. It can also be faulted for staffing too quickly before the funding pattern was known. Greater reliance on subcontracts with the other national laboratories would have obviated the necessity of an immediately large staff and would have increased political support. The HEP community collectively should have been more steadfast in their support for the SSC, more involved in their advocacy and more prepared to sacrifice short term physics opportunities. They were also too careless in discussing the HEP and SSC benefits to the society as a whole, allowing others to blow them out of proportion. SSC opponents then seized on these exaggerated claims to discredit the arguments for the SSC.

The DOE should have maintained the tradition of partnership and mutual respect between overseers and performers, rather than introducing a micromanagement system that short-circuited traditional oversight mechanisms. Congress can be faulted for not making a greater effort to understand the cost history and repercussions of annual funding shortfalls (due to Congressional actions), the status and goals of the SSC, and for not taking a long term view of what is best for the country rather than focusing on short term political expediency. The non-HEP science community as a whole were rather shortsighted in their view as to how the SSC would affect long term science support in the US. They should have been more responsible and professional in their discussion of the pros and cons of the SSC. The press failed in their responsibility to objectively report on controversial issues and do its own investigation of the allegations, rather than succumbing to the temptation of "sensational" journalism.

I turn next to the issue of international collaboration. Lack thereof is cited by almost all the postmortem papers as a principal cause of the SSC failure [163]; based on that, the conclusion is then drawn that all future large scale science projects must be international. These are two distinct arguments and not necessarily related. Here I deal with the first one, which refers to the past history, i.e. the SSC. I will address the second one, the future situation, in the next section. After all, things do evolve and the future situations will not necessarily be identical to the SSC situation.

The SSC was able to get off the ground quickly because it was a national project, seen as being of benefit to the US through stimulating science and technology as well as the interest in these fields among the young, generating high-tech jobs and increasing the technical capabilities of US industry. As a US project, the SSC could be constructed relatively expeditiously because no complex international negotiations were required, either for site selection, for defining management and oversight organization, for creating laboratory directorate structure, or for apportioning the budgetary responsibilities. Resolving all of these would have taken years and the process might never have converged. A lot of the SSC support was due to the fact that it was a US national project. Already there were complaints from several US industrial concerns that foreign companies received a significant fraction of high-tech contracts. A HERA type model could have worked and it almost did with the Japanese providing the principal contribution from abroad. But the cost offset, especially after allowing for increased management complexities, would have been minimal — 10–15% at most — and thus would not be significant enough to help counter the anti-SSC arguments being made on fiscal austerity grounds. In my opinion, lack of international participation was an excuse, not a reason, for the votes against the SSC.

When we talk about the reasons for the SSC's demise, we must not ignore the bad political luck which befell it. I have already discussed how the most important events in that arena worked against the SSC. The SSC might well have materialized if a few of them had turned out the other way. Where would the SSC be today had Bush I won the 1992 election?

I will enumerate some other important factors. The renewed emphasis on fiscal austerity and budget deficits forced many Members of Congress to give at least an appearance of being concerned about this issue. Even though the proposed FY94 SSC appropriation was about 0.3% of the projected budget deficit for that year, being against it made for a good sound bite. To quote Sen. Bumpers: "It would be nice to know the origin of matter. It would be even nicer to have a balanced budget." The SSC was a natural scapegoat — big enough to be visible but not big enough (like the Space Station) to have attracted strong industrial involvement and hence strong lobbying support. There clearly was cost escalation and it was a big factor in turning opinion against the SSC. But a lot of this escalation was due to shortfalls in the funding profile and the cost increase did not even come close to that of the Space Station, which also had more serious management problems [164–166]. The 1993 House SSC vote came a day after the House voted to continue the Space Station by a 1-vote margin — 216 to 215. Many Representatives did not want to be seen as having voted for two multibillion-dollar "boondoggles."

Finally, but very importantly, we have an inadequacy of our funding system which played a major role in the SSC cost overruns and hence its termination. I make this point by quoting from one of Watkins' last letters [167] as Secretary of Energy (written on January 14, 1993), to Rep. George Brown, Chair of the House Committee on Science, Space, and Technology: "A new way of doing business should be pursued that will support the long term planning necessary to execute a large construction program. Annual Congressional debate and uncertainty over the proper funding level can be inordinately expensive. ... Experience in military has application here. Once a shipbuilding project is mature enough to move out of R&D, it is *fully funded* for construction of the entire ship at one time. The project director is given the flexibility needed to make real time trades to meet objectives. I strongly believe that we should pursue in FY 1994 a similar 'full funding' strategy for the SSC."

13. Lessons for the Future?

What can we learn from the SSC's history? What are the chances of succeeding with another big science project in the future? More specifically, what does the SSC's history tell us about the prospects of the ILC? I will end by giving my views on these issues.

Most importantly, several general conditions must be satisfied before a big new basic research project, in which the US plays a major part, can have a reasonable chance of becoming a reality:

- The US as a whole has to recognize the value of basic research. This appreciation has to be deep and not dependent on how many jobs are created directly by the project or by how many new or improved widgets the project will provide for us tomorrow.
- The deficit has to be brought under control. The SSC's history illustrates that a large deficit is like a suspended guillotine, ready to fall if properly exploited by the enemies of the project.
- A funding strategy has to be found which gives reasonable certainty that a long term project, once started, can be completed close to the original schedule. As Admiral Watkins argued, a big science project cannot be subjected to the vagaries of the annual congressional appropriations.

None of these conditions was satisfied at the time of the SSC, and this failure played a major part in its eventual demise.

I turn now to specifics. Starting a lab on a green site generated more difficulties than originally anticipated. Thus working within the framework of an existing laboratory is highly desirable. Excessive optimism, either about the total cost, schedule, technical readiness or benefits to society can be very damaging. One should have a conservative cost estimate at the beginning, with costing ground rules clearly spelled out and complying with the current procedures. One must expect attacks by the project's opponents, many of them far from factual, and be prepared to respond to them as soon as possible.

It is accepted today that an international collaboration is a must in planning and executing such a project. I agree that any future large HEP project will be so large and expensive that it will need to be planned in the context of a growing internationalization of all big science. But the execution in such a framework will lead to serious complications and introduce a host of new challenges. It is imperative to be aware of them in the planning process. There are several possible models which might work if the three prerequisites cited above are satisfied. None of these options will be easy to execute and all of them will take a significant amount of time to come to fruition:

- A modified HERA model with the US providing the site and playing a leading role is one possibility. The project should be planned internationally, but the US would make a decision to go ahead with only an informal commitment from other countries to contribute. This is basically a modification of the SSC situation with more upfront collaborative efforts and negotiations. The disadvantage is that the US would have to put up most of the money, probably exceeding the current threshold of what is possible; many of the present stringent immigration, visa, employment, etc. rules would have to be modified. But getting the project started would be much simpler and hence it might proceed faster. Internationalization of the operations once the construction is complete would be a desirable option.
- Starting a new international laboratory, either on a green site or within the framework of an existing one, either in the US or elsewhere [168]. All the arrangements for its construction and operation would have to be settled upfront; this probably would take at least a decade. This is the model followed by ITER. I have some skepticism that one could pull this off easily, in the present international climate. Due to the current distrust of the US in many countries which might be potential partners, especially in regard to the US' long term

reliability, I foresee a great reluctance to join in an enterprise sited in the US [169]. Similarly, the US Congress today is unlikely to approve major expenditures for a project on foreign soil.

- The US joining an international laboratory abroad as a full-fledged (or associate if one is willing to play a secondary role) member, with that laboratory committing itself to such broader membership and to the construction of the project. Today the only possible such candidate laboratory is CERN. This could be the mode that the US participation in the LHC would evolve to eventually but it is certainly not there today, the US having essentially no role in the management of the laboratory. This scheme gives up on having a frontier energy facility in this country and would probably result in a decline in the HEP activity in the US. It might be difficult to get Congressional and Administration approvals for such a scheme in the present climate. Approval might require a complementary action, involving construction in the US of a facility in some other field, with the other countries participating in its funding and management. Such a scheme is an attractive and sensible option, but rather difficult to carry out because of the need for a great deal of coordination [170].

The current strategy for building the ILC adopts essentially the second model. This is a model that probably requires the most negotiations, and thus I have always been skeptical that it could be accomplished on a time scale even close to what its proponents are projecting (if at all). I also see very little chance that the three conditions I listed above as prerequisites for success will be satisfied in the near future. Certainly, the budgetary action in 2008 — zeroing out the ITER contribution, stopping the ILC R&D program, and stopping the investments in the future of the US HEP — has indicated that the value given to the basic research in this country is rather low, that the current budgetary deficits put a big damper on any new significant initiatives, and that the US funding system has difficulty supporting any long term project. Thus none of my three conditions are satisfied today and they do not appear likely to be in the near future.

I end with a personal anecdote illustrating some of the difficulties that the US will encounter in the future when trying to build a large scale project in HEP. At the 1992 ICHEP conference in Dallas, during a tour of the SSCL, I sat on a bus next to Chris Llewellyn-Smith, a British particle theorist, who was to become CERN DG in 1994. CERN was trying hard at that time to get its Council's approval for the LHC construction. I asked Chris what was the justification for CERN building the LHC in light of the SSC. His answer was that there were two strong arguments for it: it was needed for CERN self-preservation and there was a possibility that the US would never complete the SSC. This answer clearly points to two of the issues that have to be confronted and resolved if we are to have any chance of future success.

Acknowledgments

In writing this history I tried to check my own recollections by reference to contemporary sources and to other people's recollections. I would like to express my gratitude to a number of individuals who have helped me in this area.

I am very grateful to Adrienne Kolb, Fermilab archivist, for facilitating my access to Fermilab Archives, the current repository for a lot of the original SSC materials. The SLAC Library personnel, especially Abraham Wheeler, have been extremely helpful in retrieving articles and government documents not readily available on the shelves. I thank Maury Tigner and David Corson for facilitating access to the materials in the Cornell Library Archives. Ezra Heitowit and URA staff helped by providing copies of several relevant URA documents from the SSC era.

I have profited greatly from a number of interviews, in person or by phone, with many of my colleagues involved in the SSC during its Texas days. I would like to acknowledge the very informative conversations with Barry Barish, Alex Chao, Roger Coombes, Helen Edwards, "Gil" Gilchriese, Tom Kirk, Chris Laughton, Vera Luth, Robert Matyas, Chris Quigg, John Rees, Jim Sanford, Jim Siegrist, Roy Schwitters, Jenny Thomas and George Trilling. I thank them all for their generosity with their time and for their willingness to share with me their recollections of those times.

Finally, I would like to thank Gil Gilchriese, J. David Jackson, Adrienne Kolb, Jim Sanford, Jim Siegrist and George Trilling for reading an early draft of this article and giving me their critical comments.

I am very grateful to Michael Riordan for reading a more recent draft and offering many useful comments and suggestions. The responsibility for any factual errors or erroneous conclusions is, however, entirely mine.

This work was partially supported by the National Science Foundation grant PHY-0354945.

List of Abbreviations

AAAS — American Association for the Advancement of Science
ASST — Accelerator Systems String Test
BINP — Budker Institute for Nuclear Physics
BNL — Brookhaven National Laboratory
BOO — Board of Overseers (for the SSC)
BQL — Best-Qualified List
CDG — Central Design Group
CDR — Conceptual Design Report
CERN — European Organization for Nuclear Research (this acronym is for a former name in French: Conseil Européen pour la Recherche Nucléaire)
CPR — Cost Performance Review
DESY — Deutches Electronen-Synchrotron (high energy physics laboratory in Hamburg, Germany)
DG — Director General
DOE — Department of Energy
EOI — expression of interest
FY — fiscal year
GEM — Gammas, Electrons, Muons (proposed SSC detector)
GAO — General Accounting Office
GIC — general industrial contractor
HEB — high energy booster
HEP — high energy physics
HEPAP — High Energy Physics Advisory Panel
HERA — Hadron Electron Ring Accelerator
ICE — independent cost estimator
ICHEP — International Conference on High Energy Physics
IG — Inspector General
ILC — International Linear Collider
ITEP — Institute of Theoretical and Experimental Physics (in Moscow)
ITER — International Thermonuclear Experimental Reactor
KAON — Kaon–Antiproton–Other-hadron–Neutrino Factory
KEK — National Laboratory for High Energy Physics, in Tsukuba, Japan (Japanese acronym)
LBL — Lawrence Berkeley Laboratory
LBNL — Lawrence Berkeley National Laboratory (new name for the former LBL)
LEB — low energy booster
LEBT — low energy beam transport (element in LEB)
LEP — Large Electron–Positron collider
LHC — Large Hadron Collider
LOI — letter of interest
MAC — Machine Advisory Committee
MEB — medium energy booster
MIT — Massachusetts Institute of Technology
M&O — Management and Operations
NSF — National Science Foundation
OER — Office of Energy Research (in the DOE)
OSSC — Office of SSC (at DOE headquarters)
OSTP — Office of Science and Technology Policy
PAC — Program Advisory Committee
PB/MK — Parsons Brinckerhoff/Morrison Knudsen joint venture
R&D — research and development
RFP — request for proposals
RFQ — radio frequency quadrupole
SAIC — Scientific Applications International Corporation
SDC — Solenoidal Detector Collaboration
SLAC — Stanford Linear Accelerator Center
SLC — SLAC Linear Collider
SPC — Scientific Policy Committee
SSC — Superconducting Super Collider
SSCL — Superconducting Super Collider Laboratory
TAC — Texas Accelerator Center
TBM — tunnel-boring machine
TDR — Technical Design Report

TNLRC — Texas National Research Laboratory Commission
TPC — total project cost
TRIUMF — Triuniversity Meson Facility (Canada's National Laboratory for Particle and Nuclear Physics)
URA — Universities Research Association

References and Notes

[1] The pre-Waxachachie SSC-related events are described in: S. Wojcicki, "The Supercollider — the pre-Texas days, *Reviews of Accelerator Science and Technology* (World Scientific, 2008), Vol. 1, pp. 259–302.
[2] M. Crawford, *Science* **245**, 25 (1989).
[3] I. Goodwin, *Phys. Today* **42**, 51 (1989).
[4] I. Goodwin, *Phys. Today* **43**, 45 (1990).
[5] McDaniel's handwritten notes from the September 27,1988, interviews at the CDG (from the Cornell archives) and comments made to the author by some of the interviewees.
[6] Letter from R. Schwitters to M. Tigner, dated September 19, 1988.
[7] Memorandum from Edward A. Knapp, URA President, to the staff of the URA/SSC Central Design Group, dated January 25, 1989.
[8] Letter from R. Schwitters to S. Wojcicki dated August 23, 1989, and Wojcicki's response of August 31.
[9] Everyone I talked to who was at the SSC during its early Texas days agreed that the Sverdrup partnership was a disaster. A generous way to summarize their opinions about Sverdrup staff at the SSC would be to say that they were not very competent and rather uninterested.
[10] It was this organization that was presented to HEPAP at HEPAP's first meeting on the SSC site, on January 4 and 5, 1991.
[11] *Science* **251**, 1551 (1991).
[12] For a detailed discussion on the two-culture syndrome at the SSCL, see M. Riordan, *Historical Studies in the Physical and Biological Sciences* **32**, Part 1, 125 (2001).
[13] Report of the SSC Collider Dipole Review Panel, eds. G. Voss and T. Kirk (June 1989, SSC-SR-1040).
[14] S. Dickman and G. C. Anderson, *Nature* **343**, 343 (1990).
[15] T. Garavaglia, S. K. Kaufmann, R. Stiening and D. M. Ritson, SSCL-268 (Apr. 1990).
[16] SSC Laboratory report SSC-SR-1040.
[17] M. Crawford, *Science* **245**, 809 (1989).
[18] B. Schwarzschild, *Phys. Today* 47 (1990).
[19] M. Crawford, *Science* **247**, 153 (1990).
[20] D. H. Hamilton, *Science* **249**, 731 (1990).
[21] Accelerator design status. Presentation by Don Edwards at the HEPAP Meeting, Jan. 4, 1991.
[22] Report of the Ad Hoc Committee on SSC Physics, SSC-250/Rev., Dec. 1989 (revised: Apr. 1990). This report did not make a very strong case for the need to go to 40 TeV. It stated that "total energy anywhere in the range from 30 to 40 TeV would provide an enormous increase in physics capability." The loss in the rate for production of heavy particles was estimated at 25% for each 5 TeV decrease.
[23] Minutes of the BOO's Executive Committee meeting on November 21, 1989.
[24] Report of the 1990 HEPAP Subpanel on SSC Physics (DOE-ER-0434, Jan. 1990).
[25] R. J. Smith, Full-scale super collider urged despite rise in cost, *The Washington Post* Jan. 11 (1990).
[26] I. Goodwin, *Phys. Today* **43**, 67 (1990).
[27] SSCL Site-Specific Conceptual Design (SSCL-SR-1056, July 1990).
[28] Report of the HEPAP Subpanel on SSC cost estimate oversight (DOE/ER-0464P, July 1990).
[29] Report of the DOE Office of Energy Research Review Committee on the Site-Specific Conceptual Design of the Superconducting Super Collider (DOE/ER-0463P, Sept. 1990).
[30] DOE Report on the SSC cost and schedule baseline (DOE/ER-0468P, Jan. 1991).
[31] D. P. Hamilton, *Science* **251**, 741 (1991).
[32] Personal conversation with Panofsky and minutes of the Joint Informational Meeting of the URA Board of Trustees and the SSC Board of Overseers (Arlington, Virginia, USA; Dec. 19, 1989).
[33] W. K. H. Panofsky, *Panofsky on Physics, Politics and Peace* (Springer, 2007), p. 144.
[34] *CERN Courier* (Jan./Feb. 1992), p. 12.
[35] This account of these issues and events is based on minutes of the BOO's meetings in 1989, Panofsky's memos to the BOO file, and Panofsky–McDaniel correspondence (in the Cornell archives).
[36] Minutes of the Meeting of the SSC Board of Overseers (Oct. 4–5, 1989).
[37] Interview with Joe Cipriano, *Super Collider News* (May 1991), pp. 3–7.
[38] High energy physics research and the Superconducting Super Collider: DOE policies and practices. DOE document, Aug. 1992.
[39] I. Goodwin, *Phys. Today* **43**, 45 (1990).
[40] Office of Inspector General, Special Report on the Department of Energy's Superconducting Super Collider Program (DOE-IG-0291, Nov. 16, 1990).
[41] Status of DOE's Superconducting Super Collider, (GAO Report GAO/RCED-91-116, Apr. 1991).
[42] Letter from Secretary Watkins to Jamie L. Whitten, Chairman, House Committee on Appropriations, dated May 8, 1991.

[43] Letter from S. Wojcicki, HEPAP chair, to Hensen Moore, Deputy Secretary of DOE, dated Oct. 4, 1990.

[44] Minutes of the Meeting of the SSC Board of Overseers, BNL (July 28, 1989).

[45] Letter from R. Schwitters to the US HEP community, dated Apr. 10, 1989.

[46] Summary of the Meeting and Recommendations of the Superconducting Super Collider Program Advisory Committee (Snowmass, Colorado, USA; July 1990). The need to have two new general purpose detectors at the start was not held universally. The SPC recommended at least one general purpose detector. Leon Lederman argued for "recycling" one of the Fermilab Tevatron detectors.

[47] R. Crease, *Science* **250**, 1648 (1990).

[48] R. Schwitters, Decision Memorandum — on aspects of the initial scientific program for the SSC (Jan. 4, 1991).

[49] D. P. Hamilton, *Science* **252**, 908 (1991).

[50] The history of the L* Collaboration is detailed in "Report to the L* Collaboration," by Hans Hofer, Deputy Spokesman, and Samuel C. C. Ting, Spokesman, dated May 15, 1991. The perspective is one from the L* point of view but a number of relevant documents, discussed here, are included there.

[51] Report of the Mar. 10–12, 1991, meeting of the SSCL PAC.

[52] R. F. Schwitters, Report to the PAC (May 3, 1991).

[53] Letter from R. F. Schwitters to the US HEP community, dated May 10, 1991.

[54] D. P. Hamilton, *Science* **252**, 1610 (1991).

[55] Solenoidal Detector Collaboration Technical Design Report (SSCL-SR-1215, Apr. 1, 1992).

[56] GEM Technical Design Report (SSCL-SR-1219, Apr. 30, 1993).

[57] J. Mervis, D. Dickson and D. Swinbanks, *Nature* **362**, 385 (1993).

[58] House vote cooled international ardor for GEM, *Super Collider News* (Oct. 1992), p. 3.

[59] F. Gilman, presentation to HEPAP on the SSC Experimental Program on June 4, 1991.

[60] Presentation by M. Gilchriese at the January 4–5, 1991, HEPAP meeting at the SSCL.

[61] *Proc. Symp. Detector Research and Development for the Superconducting Super Collider* (Oct. 15–18, 1990; Fort Worth, Texas, USA) (World Scientific), eds. T. Dombeck, V. Kelly and G. Yost.

[62] Texas Test Rig Project, Presentation by G. Mitselmakher at the Sept. 11, 1992, HEPAP meeting.

[63] High energy physics research and the Superconducting Super Collider: DOE policies and practices (DOE/ER, Aug. 1992), distributed at the Sept. 11–12, 1992, HEPAP meeting.

[64] For a summary of the SSC's technical status in Aug. 1992, see: R. F. Schwitters, *Proc. XXVI Int. Conf. High Energy Physics* (Aug. 6–12, 1992; Dallas, Texas, USA), ed. J. Sanford, pp. 306–320.

[65] *Superconducting Super Collider: A Retrospective Summary 1989–1993* (Apr. 1994), eds. G. F. Dugan and J. R. Sanford.

[66] 'Tail wagging dog' overrun picture drawn by IG, *Super Collider News* (Aug. 1992), p. 3.

[67] URA, PB/MK contractors severely criticized, *Super Collider News* (May 1992), p. 3.

[68] Tunneling records broken, *SSC News* (June 1993), p. 4.

[69] I. Goodwin, *Phys. Today* **45**, 54 (1992).

[70] SSC magnet (RFP SSC-90-A-01701).

[71] Magnets: GD, Westinghouse beat out Grumman, *Super Collider News* (Nov. 1990), p. 1.

[72] SSCL news release (July 19, 1991).

[73] SSCL news release (July 31, 1991).

[74] Russians to supply LEB accelerator magnets, *Super Collider News* (Mar. 1992), p. 1.

[75] R. Meinke *et al.*, Chap. 8. of Ref. 65.

[76] Letter from C. Rubbia to F. Low, dated May 18, 1989.

[77] F. Flam, *Science* **256**, 466 (1992).

[78] Letter from C. Rubbia to F. Low, dated July 31, 1989.

[79] For example: R. F. Schwitters, Foreign travel report: Soviet Union and Germany (Nov. 1990).

[80] C. Rubbia and R. Schwitters, Memorandum of Understanding between CERN as Scientific Institution and the SSC Laboratory, signed April 8, 1991.

[81] Memorandum of Understanding between TRIUMF and the SSC Laboratory, signed by Erich Vogt and Roy Schwitters, August 22, 1991.

[82] D. Swinbank, *Nature* **344**, 8 (1990).

[83] R. Schwitters, Conclusions from visit of H. Sugawara to SSCL. Memo to file, Dec. 12, 1990.

[84] D. Hamilton, *Science* **255**, 279 (1992).

[85] D. A. Bromley, *The President's Scientists: Reminiscences of a White House Adviser* (Yale University Press, 1994), pp. 211–214; J. Mervis, *Science* **265**, 1357 (1994). I am grateful to M. Riordan for bringing Bromley's memoirs to my attention.

[86] US/Japan SSC working group being formed, *Super Collider News* (Feb. 1992), p. 1.

[87] Japan's interest is strong but path is not clear, *Super Collider News* (May 1992), p. 1.

[88] I. Goodwin, *Phys. Today* **44**, 52 (1991).

[89] M. Crawford, *Science* **246**, 557 (1989).

[90] M. Crawford, *Science* **252**, 25 (1991).

[91] G. Taubes, *Science* **259**, 756 (1993).

[92] I. Goodwin, *Phys. Today* **42**, 49 (1989).

[93] I. Goodwin, *Phys. Today* **44**, 53 (1991).

[94] Potentially dire consequences of this action for the SSC were emphasized by Rep. George Brown, chair of the House Committee on Space, Science and Technology: *Phys. Today* **45**, 45 (1992).

[95] Report of the 1992 HEPAP Subpanel on the US Program of High Energy Physics Research (Apr. 1992, DOE/ER - 0542P).
[96] I. Goodwin, *Phys. Today* **45**, 54 (1992).
[97] Prepared statement of Sheila Widnall for the April 7, 1987, hearing of the House Committee on Science, Space and Technology.
[98] Testimony of Daniel Kleppner at the April 7, 1987, hearing of the House Committee on Science, Space and Technology.
[99] C. Norman, *Science* **255**, 672 (1992).
[100] Status of DOE's Superconducting Super Collider (GAO Report, Apr. 1991, GAO/RCED-91-116).
[101] Letter from Secretary Watkins to Jamie Whitten, dated May 8, 1991.
[102] Yes, big science. But which projects? *The New York Times* (May 20, 1988).
[103] F. Dyson, *Phys. Today* **41**, 77 (1988).
[104] A. M. Sessler, *Phys. Today* **41**, 26 (1988).
[105] *The New York Times* editorial (Mar. 21, 1990).
[106] *The New York Times* editorial titled "Europe 3, U.S. not even zero" (June 8, 1983).
[107] It should be emphasized that the leadership of the HEP, in its formal statements and reports, did maintain a united front regarding the proposed new facilities. See for example the joint statement by the Directors John Peoples (Fermilab) and Roy Schwitters (SSCL) dated May 29, 1990.
[108] I. Goodwin, *Phys. Today* **46**, 43 (1993).
[109] A. Reifenberg, Clinton may trim '94 collider funding, *The Dallas Morning News* (Feb. 4, 1993).
[110] The DOE summary of the early cost history is discussed in "Report on the Superconducting Super Collider cost and schedule baseline" (DOE/ER-0468P); for a detailed breakdown of the costs at different stages of the project see: T. Elioff, A Chronicle of costs (SSCL-SR-1242, Apr. 1994).
[111] Nuclear science: information on DOE accelerators should be better disclosed in the budget (GAO/RCED-86-79, Apr. 9, 1986).
[112] BOO claimed that the appropriate figure should have been US$6.243 billion, the difference being due to the DOE's failure to apply proper escalation to the R&D component of the project and to take into account the extra cost in management expenses due to the stretch-out (minutes of the BOO Executive Committee meeting, Nov. 21, 1989).
[113] Memorandum of Understanding between the DOE and the TNRLC, signed on Nov. 9, 1990, by Henson Moore and Morton Meyerson, Chairman of the TNRLC.
[114] The out year guidance given to the DOE for Federal Budget Authority for FY94–FY98 was US$640 million, US$551 million, US$570 million, US$591 million and US$812 million (Robert Diebold's presentation at the Apr. 7, 1993, HEPAP meeting). It is not clear whether the project could converge at that rate (see quote in Ref. 117).
[115] H. R. O'Leary, United States Department of Energy budget highlights (DOE/CR-0014, Apr. 1993).
[116] T. Elioff, in Ref. 110.
[117] In his testimony at the Dingell Committee hearing on June 30,1993, Victor Rezendez of the GAO stated: "The project could not be completed at a US$550 million federal funding level because overhead costs and reductions in buying power would consume most of the available funds after fiscal year 2000."
[118] Report of the Task Force on SSC Commissioning and Operations (SSC-SR-1005, Apr. 1985).
[119] Reference 94 and SSCL-SR-1216; Garry Gibbs, in his testimony on May 9, 1991, to the Subcommittee on Investigations and Oversight of the House Committee on Science, Space, and Technology, quoted an estimate of the annual operating cost of US$380 million in FY92 dollars.
[120] J. Krumhansl, letter to Secretary John S. Harrington, dated February 19, 1987; quoted in I. Goodwin, *Phys. Today* **40**, 50 (1987).
[121] M. Crawford, *Science* **245**, 25 (1989).
[122] D. H. Hamilton, *Science* **249**, 731 (1990).
[123] I. Goodwin, *Phys. Today* **44**, 52 (1991).
[124] D. Hamilton, *Science* **256**, 1752 (1992).
[125] For example, 40 prominent physicists signed strong pro-SSC letters sent on July 15 to both President Bush and Senator Johnston; subsequently 2032 other scientists added their signatures — I. Goodwin, *Phys. Today* (Aug. 1992) 59.
[126] A. McKenzie, President to rally support for collider with Thursday visit, *Dallas Morning News* (July 28, 1992); during his visit to the Waxahachie site Bush described the SSC in rather glowing terms: "And when you talk basic research, this is the Louvre, the Pyramids, Niagara Falls all rolled into one," quoted in: I. Goodwin, *Phys. Today* (1992), 55.
[127] I. Goodwin, *Phys. Today* **45**, 53 (1992).
[128] C. Anderson, *Science* **260**, 1421 (1993).
[129] S. LaFraniere, Energy dept official urges firing super collider chief, *The Washington Post* (Aug. 2, 1993).
[130] M. Browne, Scientist at work: Roy F. Schwitters; building a behemoth against great odds, *The New York Times* (Mar. 23, 1993, C11).
[131] C. Anderson, *Science* **288**, 288 (1993).
[132] Hearing before the House Committee on Energy and Commerce, Subcommittee on Oversight and Investigations, held on June 30, 1993.
[133] T. Beardsley and R. Ruthen, *Scientific American* (Sept. 1993), pp. 20–24.
[134] C. Frampton, "Audit questions supercollider costs as project faces crucial House vote, *The Wall Street Journal* (23 June, 1993).

[135] C. MacIlwain, *Nature* **364**, 92 (1993).
[136] V. Kiernan, *New Scientist* (July 10, 1993), p. 5.
[137] Letter from Sen. David Pryor, member of the Senate Committee on Government Affairs, to John Layton, Inspector General of the US Department of Energy, dated August 3, 1993.
[138] Report of the DOE Review Committee on the baseline validation of the Superconducting Super Collider (Sept. 1, 1993).
[139] DOE press release: Secretary O'Leary announces plan for containing costs of Superconducting Super Collider (Sep. 1, 1993).
[140] For example Roy F. Schwitters [150]; memo from W. K. H. Panofsky to SSC staff dated Juky 27, 1993 and R. Schwitters' All hands talk on July 21, 1993; "Some comments on the SSC costs and the GAO report," SSC publication (Mar. 11, 1993); "Comments on the draft Inspector General report on SSC Laboratory subcontractor expenditures (SSC press release, June 23, 1993).
[141] Reference 139.
[142] Letter from James Decker, Acting Director of Office of Energy Research, to S. Wojcicki, HEPAP chair, dated Sept. 20, 1993.
[143] DOE press release: DOE to strengthen super collider management (Aug. 4, 1993).
[144] I. Goodwin, *Phys. Today* **46**, 52 (Sept. 1993).
[145] One such anti-SSC coalition was Organizations Opposing the Super Collider (OOPS), aiming to stop SSC construction. Some of the participating organizations were the Council for Citizens Against Government Waste, Friends of the Earth, National Taxpayers Union, and Citizens for a Sound Economy.
[146] Letter from President Clinton to William Natcher, Chairman of the House Committee on Appropriations (June 16, 1993).
[147] E. Littlejohn, Foes try to kill supercollider funds, *Portland Oregonian* (June 9, 1993).
[148] J. Mervis and K. Fox, *Science* **261**, 288 (1993).
[149] I. Goodwin, *Phys. Today* **46**, 43 (1993).
[150] R. Schwitters' testimony before the Senate Committee on Energy and Natural Resources and the Subcommittee on Energy and Water Development of the Senate Committee on Apropriations, (Aug. 4, 1993).
[151] C. Krauss, Knocked out by the freshmen, *The New York Times* (Oct. 26, 1993).
[152] I. Goodwin, *Phys. Today* **46**, 77 (1993).
[153] I. Goodwin, *Phys. Today* **47**, 87 (1994).
[154] M. Browne, The Supercollider's demise disrupts many lives and rattles a profession, *The New York Times* (Nov. 14, 1993).
[155] F. Flam, *Science* **262**, 644 (1993).
[156] Report on the closure of the Superconducting Super Collider Laboratory (URA document, Sept. 30, 1995).
[157] Report of the High Energy Physics Advisory Panel's Subpanel on Vision for the Future of High Energy Physics (May 1994, DOE-ER-0614P).
[158] The CERN Council approved the LHC in a "1/3 missing magnets" configuration on Dec. 16, 1994 (*CERN Courier*, Jan./Feb. 1995, p. 1). The US participation was approved by Congress in 1995.
[159] D. Dickson, *Science* **224**, 1216 (1984).
[160] D. Ritson, *Nature* **366**, 607 (1993).
[161] W. K. H. Panofsky, *Phys. Today* **47**, 13 (1994).
[162] M. Riordan, *Phys. Perspective* **2**, 411 (2000).
[163] See for example *Nature* **365**, 771 (1993).
[164] M. Waldrop, *Science* **235**, 965 (1987).
[165] E. Marshall, *Science* **246**, 1110 (1989).
[166] M. Waldrop, *Science* **250**, 364 (1990).
[167] J. D. Watkins, letter to George E. Brown dated January 14, 1993.
[168] Sid Drell argued for full internationalization of the SSC, both for construction funding and for management, just before the SSC was killed. This would be in the spirit of my model (2): S. Drell, *Phys. Today* **46**, 73 (1993).
[169] This difficulty was pointed out by the President's Council of Advisors on Science and Technology Panel on Megaprojects, chaired by Harold Shapiro and John McTague (Dec. 1992). One of its findings was: "The United States has a history of failing to meet certain commitments to international partners for particular megaprojects that have long time horizons." A broad range of negative repercussions from a potential SSC cancelation was eloquently described by Will Happer at the time of his departure from OER — quoted in: I. Goodwin, *Phys. Today* **46**, 90 (1993).
[170] Such a "basket" approach, advocated by Burt Richter, was discussed at the 5th EPS International Conference on Large Facilities in Physics (Lausanne, Switzerland, Sept. 12–14, 1994), eds. M. Jacob and H. Schopper (World Scientific), pp. 457–461.

Stanley Wojcicki is a professor of physics at Stanford University. During the early SSC days he took a 4-year leave of absence to work at Berkeley in the Central Design Group. His current professional interests focus on study of neutrino oscillations. He served as chairman of the DOE High Energy Physics Advisory Panel in 1990–1996. His hobbies include hiking and travel to exotic places.