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THE SUPERCONDUCTING SUPER COLLIDER'S FRONTIER OUTPOST, 1983–1988

ABSTRACT. In 1993, after an optimistic beginning followed by a half-decade of conflict, the Superconducting Super Collider (SSC) project was abandoned. In an era of 'Big Science', a major scientific enterprise collapsed. Why? We employ the metaphor of the 'frontier outpost' to analyse a critical moment in the history of this vastly expensive project, when the physicists who designed the machine were forced to recognize that traditional post-war scientific values were no longer in harmony with government priorities.

INTRODUCTION

Between 1983 and 1993, a group of American physicists worked on building a huge new scientific instrument intended to lead them into unknown territory beyond the 'Standard Model' of particles and fields.[1] Had it been completed, the Superconducting Super Collider (SSC) would have been the largest, most costly scientific tool ever built. Its 53-mile 'circular iron-and-steel racetrack' would have been long enough to encircle Manhattan Island. The magnetic field created by the ten thousand superconducting magnets inside this 'colossus of colliders', was to have brought two beams of accelerated protons to collide at 20 trillion electron volts (TeV) to produce the highest energy ever created in a terrestrial laboratory.[2]

Unfortunately, the SSC did not fulfil its promise. In October 1993, after more than two billion dollars had been spent the US Congress voted to discontinue the project. What went wrong? Many have offered reasons – poor management, a lack of foreign funding, decreasing interest in high technology with the end of the Cold War, or a government administration committed to reducing the deficit. But the story is too rich and complex to allow a simple answer. The history of the SSC is an epic tale with multiple sub-plots involving a myriad of actors, including

[1] L. Hoddeson, L. M. Brown, M. Riordan, and M. Dresden (eds.), *The Rise of the Standard Model: Particle Physics in the 1960s and 1970s* (New York: Cambridge University Press, 1997).

[2] Natalie Angier and J. Madeleine Nash, 'The Colossus of Colliders', *Time* (11 November 1985), 81; M. Riordan, 'The Demise of the Superconducting Super Collider', *Physics in Perspective* (December, 2000).

physicists, engineers, contractors, officials in federal agencies, military-industrial managers, politicians, and journalists. Subtle processes were at work, changing the established relationships among physicists, politicians, directors of national laboratories, and the military-industrial complex.

This article focuses upon the physicists who designed the SSC in Berkeley, California, between 1983 and 1988, when the project moved to Texas. We portray the story as one of mismatched interests of 'frontier explorers' and Washington managers. When the SSC was transformed from a research project into a construction project, these interests collided. At this crossroads, in late 1988, the physicists recognized that they no longer held control of the project.

To help analyse this conflict, we draw on the powerful metaphor of the 'frontier', as used by physicists themselves to imagine their creative role in the landscape of high-energy physics. Describing their machine as a 'frontier' instrument, they adopted the image that Vannevar Bush evoked in his celebrated 1945 manifesto, *Science – The Endless Frontier*.[3] Following the Second World War, physicists, including proponents of the SSC, often used the metaphor in research proposals. For example, one SSC pamphlet explained to a popular readership:

> Today, physicists are exploring a new frontier. It is the realm of 'inner space', the submicroscopic world of the atom. Like any other new world, its landscape is largely unknown. To fully understand its mysteries will require one of the largest scientific instruments ever built – the Superconducting Super Collider. Like Columbus, physicists have some ideas about what they might find at the end of their journey. But they are also prepared to be surprised.[4]

Perhaps physicists hoped that captivating frontier imagery would appeal to Washington patrons and the American public. For despite recent persuasive revisions of Frederick Jackson Turner's thesis,[5] popular histories, Western movies, and other cultural representations (such as

3 Vannevar Bush, *Science – The Endless Frontier. A Report to the President on a Program for Postwar Scientific Research* (Washington, DC, July 1945), reprinted by the National Science Foundation (Washington, DC, July 1960). See also N. Reingold, 'Vannevar Bush's New Deal for Research: Or the Triumph of the Old Order', *Historical Studies in the Physical and Biological Sciences*, 17 (1987), 300–344; and for a different perspective, David M. Hart, *Forged Consensus: Science, Technology and Economic Policy in the United States, 1921–1953* (Princeton: Princeton University Press, 1998).

4 Fermilab History Collection (Batavia, Illinois), SSC Papers, afterwards FHC: 'The Superconducting Super Collider', pamphlet of the Central Design Group, published c.1988 by Universities Research Association, Washington, DC.

5 E.g. Patricia Limerick, *The Legacy of Conquest: The Unbroken Past of the American West* (New York: W. W. Norton, 1987); Richard White, *The Middle Ground* (New York: Cambridge University Press, 1991); *It's Your Misfortune and None of My Own: A New History of the American West* (Norman: University of Oklahoma, 1991).

'frontier towns') have rooted the metaphor in the American imagination. Throughout the 1980s, the President, Ronald Reagan, was frequently shown on horseback, reminding America of the frontier.[6] However, despite the frequent use of the frontier model,[7] historians of science have rarely exploited its familiar framework as an analytic tool.[8]

Here, we employ the 'frontier' as an aid to conceptualizing the tension between physicists and their Washington patrons in the late 1980s. This balance of interests, we argue, is similar to the tension in frontier stories between those who explore and those who pay. Within the framework of the frontier, we draw heavily on the 'frontier outpost', a fortress at the border of unknown territory. The staging ground from which explorers launch their ventures,[9] the outpost is intrinsically temporary, and always in danger of collapse. Explorers may lose their authority suddenly as power changes hands and investments shift.

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[6] This association derived in part from Reagan's career as host for the television program, 'Death Valley Days', and his appearance in numerous Western movies. One photograph on horseback appears on the cover of Bob Schieffer and Gary Paul Gates, *The Acting President* (New York: E. P. Dutton, 1989). See also *Reagan* (Public Broadcasting System documentary, 1998).

[7] There is an extensive bibliography on the frontier in American history. For a selected list, see Geoffrey C. Ward, *The West: An Illustrated History* (Boston: Little Brown, 1996), 435. A few notable works are Ray Allen Billington, *America's Frontier Heritage* (New York: Holt, Rinehart and Winston, 1966); William Cronon, 'Revisiting the Vanishing Frontier: The Legacy of Frederick Jackson Turner', in *The Western Historical Quarterly*, 18 (April 1987), 157–176; Richard White, 'Frederick Jackson Turner', in John R. Wunder (ed.), *Historians of the American Frontier* (New York: Greenwood, 1988); Richard Slotkin, *Gunfighter Nation: The Myth of the Frontier in Twentieth-Century America* (New York: Harper Collins, 1992); Frederick Jackson Turner, 'The Significance of the Frontier in American History', speech of 1893, reprinted in George Rogers Taylor (ed.), *The Turner Thesis: Concerning the Role of the Frontier in American History* (Boston: D. C. Heath and Co., 1956); Richard White and Patricia Nelson Limerick, 'The Frontier in American Culture', in James R. Grossman (ed.), *The Frontier in American Culture: An Exhibition at the Newberry Library*, 26 August 1994–1997 January 1995 (Berkeley: University of California Press).

[8] The few exceptions include William Coleman, 'Science and Symbol in the Turner Frontier Hypothesis', *American Historical Review*, 72 (1) (1966), 22–49, A. Hunter Dupree, *Science in the Federal Government* (Baltimore: Johns Hopkins Press, 1986), and Merrit Roe Smith, 'Frontiers of Change', *STS News* (September 1993; MIT Program in Science, Technology and Society), 1–2. This point is discussed in Adrienne Kolb and Lillian Hoddeson, 'A New Frontier in the Chicago Suburbs: Settling Fermilab, 1963–1972', *Illinois Historical Journal*, 88 (1), (Spring 1995), 2–18.

[9] Sources on the 'frontier outpost' include Stephen Ambrose, *Undaunted Courage: Meriwether Lewis, Thomas Jefferson, and the Opening of the American West* (New York: Simon and Schuster, 1996); Mike Davis, *City of Quartz: Excavating the Future in Los Angeles* (New York: Vintage, 1992); Limerick, *op. cit.* note 5; and White, *op. cit.* note 5.

The SSC's 'outpost years' in Berkeley encompassed 'Phase 0' plus 'Phase 1' – the Reference Designs Study (RDS) in 1983–1984 and the Central Design Group (CDG) in 1984–1988. They were 'exciting and very hectic' years, according to Alex Chao, Head of the Accelerator Physics Division. At their outpost, the SSC's pioneers gathered to store intellectual provisions (in the form of scientific knowledge and designs), and to organize scientific, technological, and managerial forays into the new territory. The atmosphere was 'fantastically exhilarating', according to Chris Quigg, Deputy Director for Operations for CDG from July 1987 to January 1989.[10] Ultimately, the exhilaration of pioneering research at the Berkeley outpost was overcome by the interests of staff at the SSC's permanent base in Texas, who responded to the disparate views of their patrons rather than those of the physicists who worked on the SSC's design. Many signs along the way had warned of this disjunction. In August 1988, the physicists saw it written in the bureaucratic terms of the Department of Energy's official 'Request for Proposals' (RFP), the formal description of the project that the DOE was prepared to fund.

The metaphor of 'seeing the elephant', employed by frontier writers, captures the sentiment of CDG leaders at this critical juncture. The expression stems from the 19th-century language of American farmers who spoke with awe about their visit to the elephant in a travelling circus. It meant 'seeing the world.' It was also used to convey reaching the breaking point in a pioneering venture, the point when one has seen enough, or all that one can endure. Civil War soldiers, voyagers to the Western Prairies, and those who rushed to California to find gold used the metaphor to denote the moment when they first sensed the monumental reality of their ordeal, or when they judged it time to turn back, typically because their sufferings had grown unbearable.[11]

CDG leaders 'saw the elephant' in this last sense in the fall of 1988, when they learned that the SSC's management consortium would comply with the bureaucratic terms of the DOE's RFP, issued in August. The ultimate rebuke of an earlier unsolicited proposal, in which CDG

[10] Chris Quigg interview by Lillian Hoddeson and Adrienne Kolb, 5 May 1993. All interviews cited here are on deposit and available to scholars in the FHC.

[11] J. E. Lighter (ed.), *Random House Historical Dictionary of American Slang* (New York: Random House, 1994), vol. 1, 702; Eric Partridge, *A Dictionary of Slang and Unconventional English from the Fifteenth Century to the Present Day* (New York: Macmillan, 1961), 256. For examples of the use of the metaphor, see, e.g., James Michener, 'The Wagon and the Elephant', *Centennial* (New York: Random House, 1974), ch. 6, 243–349, esp. 295–296, 329, and 332–333; and David G. Gutierrez, 'Seeing the Elephant: Myth and Myopia: Hispanic People and Western History', in Ward, *op. cit.* note 7, 118–171, esp. 125–126.

had described its idea for the SSC's management according to post-war academic practice, the RFP reflected a sea change in the relationship between the high-energy physicists and their government patrons.

LAUNCHING THE EXPEDITION

There are at least two possible accounts of the SSC at the edge of the frontier: that of the physicists, upon which we focus here, and that of politicians and managers in the United States government. The physicists' account begins during the summer of 1982, with a romantic vision of the new machine revealed at Snowmass, high in the Colorado Rocky Mountains.[12] Then, American particle physicists foresaw a new super-size American colliding beams machine operating at energies as high as 20 TeV on 20 TeV.[13] The machine was referred to as 'the Desertron', as it was thought to require a large expanse available only in the desert, and as it was to penetrate the bleak 'desert' of physical processes described by the theory of grand unification.[14] The new Reagan administration fully appreciated the implications. The project was to be a means of restoring competitiveness with Europe in high-energy physics, and of retaining world leadership in science and technology. George Keyworth, Reagan's Science Advisor, encouraged American physicists to be bold and adventurous in their designing of an accelerator of the highest possible energy.[15] In the spring of 1983, the 20 TeV Hadron Collider Workshop, held at Cornell University, put forth the technical groundwork for the frontier US accelerator.[16]

[12] The Washington account of the SSC began more than a year earlier. George Keyworth interview by L. Hoddeson, 12 March 2000, and Douglas Pewitt interview by Michael Riordan, 3 May 1998. The Snowmass workshops were organized by the American Physical Society's Division of Particles and Fields to give its members a voice in the definition of new facilities.

[13] L. M. Lederman, 'Fermilab and the Future of HEP', in R. Donaldson, R. Gustafson, and F. Paige (eds.), *Proceedings of the 1982 DPF Summer Study on Elementary Particle Physics and Future Facilities* (DPF/APS), 125–127, esp. 126.

[14] Hoddeson et al., *op. cit.* note 1.

[15] Adrienne Kolb and Lillian 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 (1), (1993), 101–124.

[16] FHC: 'Report of the 20 TeV Hadron Collider Technical Workshop', Cornell University, 1983. The recent achievement of the first beam in Fermilab's new superconducting accelerator boosted confidence in the feasibility of building high-energy superconducting-magnet accelerators.

The SSC was launched in July 1983, after Stanley Wojcicki' s subpanel of the High Energy Physics Advisory Panel (HEPAP) recommended that the DOE discontinue ISABELLE, Brookhaven National Laboratory's not yet completed 400 GeV collider project, whose energy was judged too low. Instead, the panel recommended funding the higher energy machine, which was to become known as the SSC.

By November, a cooperative arrangement was established to fund a study to map out the development of the SSC. The study was referred to as the Reference Designs Study (RDS), or Phase 0. Several prominent accelerator physicists wrote to the directors of the existing high-energy laboratories (Fermilab, Cornell, SLAC, and Brookhaven) with a request that they each contribute to the SSC by allowing a few members of their own staffs to do early ad hoc design work.[17] As no additional funds were required, the DOE endorsed this plan and the laboratory directors also signed on. The DOE secured $19.5 million for the RDS and spread it among the existing labs and the newly created Texas Accelerator Center (TAC).[18] In December 1983, the intensive five-month feasibility study, the 'SSC Reference Designs Study', began.[19] Laboratory directors agreed that their long-term contributions to the national project would not supercede the work of their own laboratories.[20] However, by setting their cooperative research priorities below those of their labs, the SSC was handicapped from the start.[21]

Maury Tigner of Cornell was the chosen leader of the Reference Designs Study. Among the strongest in the next generation of accelerator builders, this 46-year old experimental physicist came qualified with a long list of technical achievements. A graduate student of Fermilab's

[17] FHC: Herman Grunder, *et al.* to Lab Directors, 13 November 1983, reprinted in Appendix B, Report of the DOE Review Committee on the Reference Designs Study, 18 May 1984.

[18] SSC Newsletters (published by the APS/DPF), 15 February 1984 and 15 March 1984. The Texas Accelerator Center (TAC) was a recent entrepreneurial creation of Houston industrialist George Mitchell and high-energy physicist Peter McIntyre. Mitchell created the Houston Area Research Center and McIntyre began the TAC under its aegis. McIntyre gained the reputation among physicists of rogue inventor. Before Carlo Rubbia ultimately convinced CERN to build its proton-antiproton collider, he and McIntyre had proposed constructing such a collider at Fermilab, a proposal that Fermilab Director Robert Wilson rejected in 1976 in favour of completing the laboratory's Energy Doubler.

[19] FHC: *Reference Designs Study for U.S. Department of Energy: Superconducting Super Collider* (8 May 1984), draft II, p. iii.

[20] FHC: Memo from Laboratory Directors to Grunder, *et al.* (14 December 1983).

[21] Kolb and Hoddeson, 'Mirage', *op. cit.* note 15. A similar situation scuttled the design of an international accelerator proposed in 1975 by the International Committee on Future Accelerators (ICFA).

founding director, Robert R. Wilson, Tigner flourished in the inventive, pioneering, physicist-dominated accelerator building traditions established at Cornell by Wilson and his colleague Boyce McDaniel. At Cornell's Laboratory of Nuclear Studies, Tigner had worked on all the Cornell electron machines as well as on his own research programme on superconducting radio frequency (rf) cavities. Tigner was called upon regularly to serve on advisory panels. In 1982, he suggested how to configure the antiproton source of Fermilab's Tevatron.[22]

Tigner was also among the active participants of 'Physics at the SSC' (PSSC), the first continuing forum for designing the SSC. They met before and into the period of the Reference Designs Study to discuss physics questions affecting design issues, such as luminosity, whether to employ fixed or internal targets, whether collisions should be proton-proton or proton-antiproton, and whether the energy of beams should be 10 TeV or 20 TeV. Answers to these questions determined the choice of experimental detection apparatus as well as the feasibility of specific experiments. Led by Bruce Winstein of the University of Chicago and Fermilab, this PSSC group of some two hundred future SSC 'users', including the newly formed RDS, met on seven occasions between 30 September 1983 and 30 April 1984.[23] 'Often thirty to forty people of a working group would be jammed in a room discussing physics opportunities. ... There is clearly a great deal of interest in the community in the SSC', Winstein wrote.[24]

To plan the adventure, the RDS assembled in Berkeley from December 1983 through May 1984. Some 30 full-time staff were involved, with over 100 short-term visitors from major research laboratories and universities in cosponsored workshops at the Lawrence Berkeley Laboratory.[25] With leadership and provisions in place, the expedition was under way.

The design of the superconducting magnets that bend and focus the proton beams was the most crucial issue, for magnet strength plus beam energy determine the size of the accelerator ring. A stronger magnet allows a smaller radius. Three magnet designs were put forth: a Brookhaven and Berkeley design with superconducting wire windings and an iron yoke to

22 For example, in 1980 he chaired a DOE HEPAP Subpanel on Accelerator Research and Development, which analysed and recommended innovations in accelerators. *Physics Today*, 33 (2), (February 1980), 92.

23 They met at Fermilab through the fall of 1983, at Brookhaven and at a Texas site (The Woodlands) in the winter, and in the spring of 1984 at SLAC, concluding their work in April back at Fermilab.

24 FHC: PSSC records, Bruce Winstein Collection. *PSSC: Physics at the Superconducting Super Collider Summary Report* (Batavia: Fermilab, June, 1984), esp. V.

25 Starting in January 1984, the 'Task Coordinators Group' of the RDS dissected technical problems.

reach the high value of 6.5 tesla; a Fermilab design that was iron-free and would reach 5 tesla; and a design by the Texas Accelerator Center for a 3.5 tesla superferric magnet that used significantly more iron and would produce, in theory, a more uniform field but demanded a larger ring. The RDS judged all three magnets feasible. They planned to identify the best choice early in the subsequent Phase 1 effort.[26]

At the final PSSC meeting, held on April 30, the directors of the major laboratories reviewed the report of the Reference Designs Study. Tigner submitted a 441-page *Reference Designs Study* to DOE on 8 May 1984, meeting the first major milestone of the SSC project. Weighing the technical feasibility of the entire project, the RDS put forth a six-year timetable. Estimating the cost at $2.7–3 billion, the RDS was careful to point out that its budget excluded funding for 'research equipment, preconstruction R&D, and possible site acquisition.[27] The DOE review committee, in turn, approved the RDS – 'all three basic SSC designs ... appear to be technically feasible' and recommended an increase of the cost estimate by $200 million.[28] The Report then advanced on 4 June to Secretary of Energy Donald Hodel, who approved it in August. Commending those who had prepared the RDS, and affirming 'this project is totally in the spirit of this administration's commitment to the advancement of science and technology as an essential ingredient in the achievement of national goals',[29] Hodel released $20 million of DOE funds for further research on the SSC during the fiscal year 1985. As this money was transferred from an account that did not require Congressional endorsement, the SSC was able to maintain a relatively low profile.

The physicists who supported the SSC were triumphant. After almost two years of waiting, they saw approval of the Reference Designs Study as the first official sign that the brilliant vision revealed at Snowmass in 1982 would become real. In glowing rhetoric, physicist Paul Mantsch expressed the excitement of physics research at the frontier:

> The moment was right for the great vision to become reality. The mood of optimism was on the Land. The technical progress that enriches the lives of the People has at its foundation a vital and dynamic program of scientific research. The People know little of quarks and

[26] FHC: 'Executive Summary', *Superconducting Super Collider: Reference Designs Study for U.S. Department of Energy* (8 May 1984), p. iii. 'Phase 1 Program Milestones', Tigner Files, Central Design Group.

[27] FHC: 'Report of the DOE Review Committee on the Reference Designs Study' (18 May 1984), see 'Executive Summary'. The overall budget later increased when it became possible to estimate the cost of these essential aspects.

[28] *Ibid.*

[29] Gloria Lubkin, 'R&D Funding for the Super Collider', *Physics Today*, 37 (October 1984), 21.

leptons. But they know curiosity. They know asking questions and seeking answers. They share the spirit of pioneers at a new frontier. And they know it is good. For they were great People of a great Land where boldness and innovation is legendary. This, after all, is the Spirit of Snowmass.[30]

THE VIEW FROM THE TOP

Only a few in Washington DC followed the SSC design work in Berkeley. One was Alvin Trivelpiece, the DOE's political appointee overseeing the SSC; he was the Director of the DOE's Office of Energy Research and Scientific Advisor to Energy Secretary Hodel. A plasma physicist, Trivelpiece viewed the politicians, policy-makers, industrialists, journalists, and bureaucrats, as partners of the scientists. James Leiss, Associate Director for High-Energy and Nuclear Physics was Trivelpiece's deputy. As the highest ranking civil servant managing programmes in high-energy physics, Leiss directed funds for all existing high-energy machines,[31] but for the SSC, the endorsement of Secretary Hodel – two levels above Leiss – was required.[32]

From Washington's perspective, the SSC design work required a formal management structure. The DOE wanted to mediate between itself and the outpost. The natural choice was the Washington-based consortium known as the Universities Research Association, Inc. For over a decade and a half, the URA had successfully managed the construction of Fermilab, the world's highest energy accelerator laboratory. Moreover, as a consortium of more than fifty universities spread throughout the country, the URA had a broad base of support. Recognizing the organizational needs for the political process, Leiss wrote to the URA President H. Guyford Stever in March 1984, asking that the URA submit a management plan for the SSC's 'Phase 1'. He explained to Stever, a former NSF director and Science Advisor to President Gerald Ford, that even though the DOE was not yet committed to

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30 Paul Mantsch, 'Spirit of Snowmass Spreads Across Land', *Ferminews* (14 June 1984), 2–3. Mantsch recalls that after Lederman presented the idea for the SSC at Snowmass, 'Everyone forgot what else he was doing'. Personal communication: P. Mantsch to authors, 11 March 1998.

31 While members of the 'project', or 'operations' staff of DOE provided day-to-day administrative support, the 'program' staff provided long-range management and direction for scientific programs. Leiss oversaw programs in high-energy and nuclear physics.

32 Gloria B. Lubkin, 'SSC Design Goes to DOE: ICFA discusses CERN Hadron Collider', *Physics Today*, 37 (June 1984), 17–19; Irwin Goodwin, 'Tigner Named to Direct R&D Program for SSC', *Physics Today*, 37 (August 1984), 69.

supporting Phase 1, he wanted its management to be in place upon Hodel's approval.[33]

However, the URA's primary qualification, as the manager of Fermilab, was a potential conflict of interest. Although the initial Snowmass Desertron idea had suggested siting the supercollider in a wide-open space, presumably in the West, Trivelpiece decided that holding an open site competition might create further national support within the existing scientific, industrial, and political communities.[34] It was thus important to separate the URA's involvement with Fermilab from its interest in the SSC. To that end, in late summer of 1984, the URA established an independent Board of Overseers (BOO) to deal exclusively with the SSC project.[35]

In the background, steps were being taken toward international participation in the SSC, in part, to reduce US costs. The Japanese had intended to invest heavily in the SSC's ancestor, the international 'Very Big Accelerator' (VBA), put forth in 1975 by the International Committee on Future Accelerators (ICFA). But they were bitterly disappointed when, as they viewed it, the United States 'co-opted' the VBA in July 1983, by going forward with the American supercollider.[36] Ignoring this rift, in April 1984, Keyworth invited Japanese participation in the SSC.[37] At a May 1984 meeting in Japan, the ICFA mended its alliance with the US collider project and declared that its new role was to facilitate design and construction of new high-energy machines, rather than to suggest national or regional options.[38]

Both Hodel and Keyworth urged Trivelpiece to encourage foreign contributions for the SSC, continuing work via the Economic Summit Process, begun in June 1983 with the Williamsburg Summit and scheduled to meet again in France early in 1985.[39] Not yet acknowledged at

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33 FHC: URA Collection, James Leiss to Guy Stever, 1 March 1984.

34 Alvin V. Trivelpiece interview by Steve Weiss, 6 November 1996.

35 Prior to this point, Fermilab was overseen by the URA Board of Trustees. From this time on both labs had their own Board of Overseers (BOO) reporting to the URA President and Trustees.

36 Kolb and Hoddeson, 'Mirage', *op. cit.* note 15, see 107–121.

37 M. Mitchell Waldrop, 'The Supercollider, 1 Year Later', *Science*, 225 (4661), (3 August 1984), 490–491. The sensitive efforts of CDG physicist Murdock 'Gil' Gilchriese, on behalf of international collaboration, won back preliminary Japanese industrial and research interest. However these efforts were largely unrecognized in cabinet-level dialogues.

38 FHC: *Proceedings of the 1984 ICFA Seminar on 'Future Perspectives in High Energy Physics*, May 14–20, 1984, KEK, Report 84–14, September 1984.

39 FHC: Hodel to Trivelpiece, August 16, 1984; Gloria Lubkin, 'R&D Funding for the Supercollider', *Physics Today*, 37 (October 1984), 21; David Dickson, 'A New Push for European Science Cooperation', *Science*, 220 (4602), (10 June 1983), 1134–1136.

the highest levels of government were the underlying conflicts between securing international funding for the SSC and the goals of Reagan's current foreign and economic policies.[40]

FORMALIZING THE CENTRAL DESIGN GROUP

The Reference Designs Study contributed to many meetings. One of the larger gatherings was the second Snowmass workshop, where 250 physicists convened between 23 June to 13 July 1984 to consider the physics, detectors, magnet, and accelerator design questions of the SSC. Significant technological progress was reported since Snowmass '82. In particular, Fermilab had demonstrated that it was possible to employ superconducting magnets in large-scale high-energy accelerators. State-of-the-art computing and communications technology, provided by IBM and AT&T, was employed to test designs by simulation.[41] Interest in the SSC heightened when, on 3 July, the UA1 collaboration at CERN announced six candidate top quark events, a result that had a strong motivational impact, even though it was wrong.[42]

No one in the physics community was surprised when, on 20 June, shortly before Snowmass '84, the URA President Stever announced that its new SSC BOO had selected Tigner to lead Phase 1, the Central Design Group (CDG).[43] *Physics Today* reported widespread expectations that Tigner would 'maintain the head of steam ... that drove the Reference Designs Study to completion in three months'.[44] For his principal deputies, Tigner chose two who had sat with him on the Wojcicki subpanel.

[40] Reagan's unprecedented spending on national defence and provocative rhetoric did not win partners for the SSC. Because of the goal of reducing the lingering deficit, the administration in the 1980s could not make a consistent argument for offering jobs to foreign partners. Supporters in Congress expected to win contracts for jobs in their districts, not for work abroad, even if the work was supported by foreign contributions.

[41] The Doubler achieved its first 512 GeV beam in July 1983, then its first 800 GeV beam in February 1984. L. Hoddeson, 'The First Large-Scale Application of Superconductivity: The Fermilab Energy Doubler, 1972–1983', *Historical Studies in the Physical and Biological Sciences*, 18 (1), (1987), 25–54. Also, John Peoples, Jr., 'Introduction', in Rene Donaldson and Jorge G. Morfin (eds.), *Proceedings of the 1984 Summer Study on the Design and Utilization of the Superconducting Super Collider*, DPF, APS, DOE, NSF, URA, p. 5.

[42] *Physics Today*, 37 (August 1984), 17; see also John Krige (ed.), *History of CERN: Vol. III* (Amsterdam: Elsevier North Holland, 1996).

[43] FHC: Cornell Laboratory of Nuclear Studies personnel were notified in a memo from Boyce McDaniel, 22 June 1984. 'CDG information', 1984–1988.

[44] Irwin Goodwin, *op. cit.* note 32.

He appointed experimentalist Stanley Wojcicki, to be 'Deputy Director for External Relations' (i.e., to deal with the DOE, international partners, and the rest of the physics community); and theorist J. David Jackson, as 'Deputy Director for Operations' (i.e., to handle the CDG).

With the help of his staff (especially James Sanford and Tom Elioff) Tigner defined milestones for Phase 1, including the submission by April of 1985 of the Site Criteria Document; magnet selection in July 1985; preparation of the SSC's blueprint, the Conceptual Design Report, by March 1986; and selection of the site by December 1986. Construction was set to begin in October 1987.[45] In his talks to physicists, the DOE, and congressional oversight committees, Tigner reassured his audiences by projecting the milestones in overhead slides.

Optimism about Tigner and the SSC was widespread. *Physics Today* reported that DOE officials considered Tigner 'ideal as chief designer of the SSC.' Representative William Carney of New York stated that 'Tigner is one of the most informed and imperturbable witnesses I've seen on Capital Hill'.[46] In the background, however, were changes that would set in motion a downward spiral of support. In January 1985, the SSC lost a powerful Washington ally when Reagan transferred Energy Secretary Hodel to the Department of Interior. Tigner had to begin anew to win DOE support, working with California real estate developer, John Herrington, who succeeded Hodel in February 1985.

The CDG's management charter raised another warning. In their Memorandum of Understanding (MOU) of 1984, the directors of the existing high-energy laboratories agreed that the CDG would coordinate the design work of the participating laboratories.[47] But the DOE, rather than the CDG, would distribute funds directly to each laboratory.[48] Thus Tigner had few options for redirecting support as he saw fit. 'At the beginning of the fiscal year I negotiated with each of the Labs about the work plan for the coming year and the funding for it'. After the DOE dispersed funds, Tigner had none to use as incentives to 'harness their abilities and enthusiasm more efficiently'.[49]

[45] FHC: 'Phase 1 Program Milestones', Tigner Files, Central Design Group.

[46] Irwin Goodwin, 'Tigner named to direct R&D program for SSC', *Physics Today*, 37 (August 1984), 69.

[47] 'FHC: Memorandum of Understanding', Appendix E, revised unsolicited proposal of 22 February 1988 arguing for URA as contractor for the construction and operation of the Superconducting Super Collider Laboratory, draft of 9 November 1983.

[48] DOE frowned on labs holding funds in reserve for emergencies or incentives since leftover monies were in danger of being reclaimed by congressional appropriators.

[49] FHC: M. Tigner to L. Hoddeson, 10 February 1999.

A more formally structured CDG settled into its temporary offices northwest of the historic Bevatron at Lawrence Berkeley Laboratory.[50] With an infusion of younger recruits the core group grew to about 40 physicists. Workshops and panels of specialists brought in over 250 short-term visitors between June 1984 and March 1986. Tigner set a tone of excitement that would sustain his staff through long working hours and hold their work to a tight schedule. Rene Donaldson, Senior Writer for the CDG, recalled Tigner as a leader who inspired loyalty and devotion, one who worked hard himself and expected his staff to work almost as hard.[51]

A Blueprint for the SSC: 'The Conceptual Design'

To prepare a detailed design for the SSC, known as the SSC's 'Conceptual Design Report', the CDG needed to select one of the magnet designs. Brookhaven, Fermilab, and the Texas Accelerator Center built prototypes to evaluate specifications, performance and reliability. Because so many magnets were involved, the size of their aperture was a critical issue, for it greatly impacted the project's overall cost. Tigner assigned this urgent task to Alex Chao, a young accelerator physicist from the Stanford Linear Accelerator Center (SLAC). One wanted the smallest possible aperture, but decreasing it caused the magnet field quality to deteriorate. It was necessary to compromise. 'For accelerators before the SSC, one usually adopted a conservative approach, an easy way out', Chao reflected. 'This was no longer the case for the SSC'.[52] Trying 'to be inventive in our algorithms and evaluation criteria', they designed a 'correction system to compensate for the remaining effects of the magnet field errors'. One difficulty was predicting the so-called 'long-term dynamic aperture'. It was important to track particles for a long time.[53]

To settle the aperture question, Tigner formed a task force, which worked from October 1984 to July 1985 studying issues such as lattice design, correction schemes, extrapolation to what could be expected of technology in the near-term, and computer simulation of particles in the

50 Chris Quigg interview *op. cit.* note 10; J. David Jackson interview by A. Kolb and L. Hoddeson, 3 May 1996.

51 FHC: Notes from Rene Donaldson conversation with Michael Riordan, 24 August 1998.

52 FHC: A. Chao to L. Hoddeson, 29 December 1999.

53 FHC: A. Chao to L. Hoddeson, 29 December 1999, Alex Chao papers (Berkeley). Chao sites the reports SSC-3 (1984), SSC-TR-2002 (1984), and SSC-SR-2020 (1986) (the CDG Conceptual Design Report).

SSC.[54] As Head of the CDG's Accelerator Physics Division, Chao organized the Magnet Aperture Workshop in November 1984, for about sixty participants. Again, the practical organizational problem was that the task force 'was a loosely organized group of physicists from various labs who have their own work and priorities at their home institutions'. Fortunately, in this instance, 'everything finally came into place after countless numbers of skipped lunches, working into the evenings, and some sleepless nights.' The result was a preliminary aperture of 4 cm.[55]

To make the final decision on magnet design, the Technical Magnet Review Panel, chaired by Alvin Tollestrup of Fermilab, met during 1–2 July 1985 at the Texas Accelerator Center. Sensitive to the politics between DOE labs, the CDG had by then reduced its magnet choices to: a consensus candidate, the 'high-field cosine theta' (also called the 'conductor-dominated') magnet, developed at Berkeley and Brookhaven with Fermilab's help, and the superferric magnet proposed by the Texas Accelerator Center. In late August 1985, the CDG convened the Magnet Type Selection Advisory Committee, chaired by Frank Sciulli of Columbia University, which had less than a week to prepare its report due on 1 September. Cost, technical merit, and site politics were all considered in the choice of magnet.[56]

The two magnet designs projected rings of dramatically different size. In prototype testing of short sample magnets, the 6.5 tesla field of the high-field cosine theta magnet projected a ring about 52 miles in circumference. To achieve the same energy, the lower-field superferric magnet would require a ring about 100 miles in circumference, allowing few viable sites not in Texas. Perhaps cheaper to construct, the superferric magnets would increase the overall cost of tunneling and radio frequency systems. It was thus no surprise when the CDG announced on 18 September that it favoured the high-field cosine theta magnet. The Texas team grumbled, claiming that the decision reflected a bias for the established laboratories.

Now the three laboratories that had backed the high-field magnet divided tasks: Berkeley agreed to work on the superconducting cable and on construction of short magnets; Fermilab would focus on cryogenics

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[54] Chao personal papers (Berkeley): Tigner to Samios, 10 September 1984, and Tigner to Richter, 15 October 1984.

[55] Chao papers: SSC-TR-2001, November 1984. A. Chao to L. Hoddeson, 29 December 1999.

[56] M. Mitchell Waldrop, 'Magnets Chosen for Supercollider', *Science*, 230 (4 October 1985), 50; Bruce Chrisman interview by L. Hoddeson and A. Kolb, March 1993.

systems; and Brookhaven would build longer magnets.[57] Journalists' reports in the national media focused on the importance of the magnet choice for progress in basic research. Noting that scientists had compared the SSC to the Great Pyramids of Egypt and the Panama Canal, William Broad of *The New York Times* devoted a front-page article to explaining that the magnet choice marked a 'firm commitment to a particular design for the huge machine.'[58]

The Washington wheels continued to turn in preparation for the SSC as efforts to attract foreign interest proceeded and as the 'frontier' instrument became a visible political plum. In budget hearings on 29 October 1985 before the House Sub-Committee on Energy Development and Applications, Herwig Schopper and Carlo Rubbia of CERN endorsed the SSC as they requested support for CERN's Large Hadron Collider (LHC) as a 'stepping stone ... not replacement' for the American supercollider.[59]

Hoping to assure an open and fair site selection, the DOE called on the National Academies of Science and of Engineering. The site selection process had begun officially a year earlier when, in November 1984, Trivelpiece asked Robert M. White, President of the National Academy of Engineering, for his assistance.[60] The National Governors Association joined the effort in February 1985, passing a resolution '... urging DOE and Congress to proceed quickly in authorizing and building the SSC ... insuring U.S. preeminence in high energy physics.'[61] When in June 1985, Trivelpiece sent copies of the CDG's Siting Parameters Document to the governors of the 50 states, state officials lined up to learn more.[62] Soon the CDG physicists were heavily involved in explaining the project to proposal developers from more than twenty different states.[63]

Meanwhile, the URA President Stever announced his retirement. The consortium sought a new leader well connected in Washington DC. Edward Knapp, a member of the SSC Board of Overseers, was elected

[57] In July, Brookhaven had successfully tested a 4.5-meter demonstration magnet. 'Supercollider: Magnet Decision', *CERN Courier* (November 1985), 383–384; Waldrop, 'Magnets Chosen for Supcollider', *op. cit.* note 56; FHC: J. D. Jackson Notebook B4.

[58] William J. Broad, 'Supermagnet Design Chosen For a 60-Mile Atom Smasher', *The New York Times* (19 September 1985), 1, 17.

[59] M. Mitchell Waldrop, 'Congress Questions SSC Cost', *Science*, 230 (4727), (15 November 1985), 785.

[60] FHC: Trivelpiece to White, 30 November 1984.

[61] Kim McDonald, 'Gigantic Particle Accelerator Will Have No Modern Rival – If It's Built', *The Chronicle of Higher Education*, XXXI (7), (16 October 1985), 1, 10–11.

[62] 'DOE offers SSC site document, but sidesteps its endorsement', *Physics Today* 38 (September 1985), 53.

[63] Edward Knapp interview by Lillian Hoddeson, 14 July 1997.

sixth President of the URA on 1 August 1985.[64] A member of the nuclear physics staff at Los Alamos since 1958 and Director of the National Science Foundation from 1982 to 1985, Knapp's leadership style responded to Washington politics – indeed, so much that some members of the CDG staff complained that he rarely visited Berkeley. In October 1986, Knapp appointed as his Vice President and Secretary Ezra D. Heitowit, formerly a staff director in the Subcommittee on Science, Research and Technology of the House Science and Technology Committee.[65]

By the fall of 1985, the CDG was deeply immersed in writing its 4-volume Conceptual Design Report, which aimed to set out the footprint of the main collider and its associated systems on a generic site.[66] From September 1985 through March 1986, Jackson served as its editor-in-chief, coordinating the writing assignments and drafting most of the beginning chapters. The job was consuming. For Christmas, Jackson's wife gave him a T-shirt depicting a monkey on his back. In March, when the advance copy of the report was completed, another milestone had been met.[67]

The Conceptual Design Report was then subject to an extensive DOE management and administrative review, including an independent estimate of the cost, which remained $3 billion (i.e., decreased in real dollars). 'An army of 50–60 people invaded the CDG'; the review team was larger than the CDG. Edward Temple, head of Facilities Management in the Office of Energy Research, chaired the process. Referred to as a 'Temple Review', this had a reputation of being exacting and comprehensive, yet fair. Begun in the early 1980s, the Temple Reviews were a critical step toward formal DOE construction, causing laboratory managers to scrutinize their programmes and set specific goals.

A Temple Review subcommittee, chaired by Robert Siemann of Cornell, was concerned that the 4 cm aperture selected was 'too daring' and suggested they maintain the option of increasing the aperture to 5 cm as a contingency. The committee approved the 4 cm aperture but stated that until more was understood, 'one must hold open the possibility that the field qualities will have to be improved', for which, Chao recalled, the CDG had already planned.[68] Released in May 1986, the Temple Review's

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64 *Ferminews*, VIII (15), (8 August 1985), 1–3.

65 Knapp interview, *op. cit.* note 63.

66 SSC Conceptual Design Report, March 1986, SSC-SR-2020.

67 J. David Jackson interview, *op. cit.* note 50.

68 FHC: Chao to Hoddeson, and *Report of the DOE Review Committee on the Conceptual Design of the Superconducting Super Collider* (Washington, DC: US Department of Energy, May 1986), DOE/ER-0267.

Report deemed the SSC's cost and scope appropriate, emphasizing that strong management would be essential.[69]

Just as the DOE was reviewing the Conceptual Design Report, the outpost faced what it perceived as 'shooting from the left and the right.'[70] On 7 April 1986, Russ Huson and Peter MacIntyre from Texas A&M objected to the choice of the high-field cosine theta magnet in a well-publicized letter to Sciulli, who had headed the physicists' magnet selection panel. Claiming that the Texas superferric magnet would be more economical than the high-field choice, they asked the HEPAP to reconsider the CDG's earlier decision on the magnet design.[71] To investigate the claims, Trivelpiece appointed a subpanel chaired by Burton Richter, Director of SLAC. At its meeting on 15 May at LBL, the panel upheld the CDG's original recommendation.[72]

As the frontier mission gained visibility and thus scrutiny, and with each approval during 1986, the CDG saw little trouble on the horizon (other than the usual budget unease). Site selection was scheduled for the end of that year. Many more meetings through 1986 (e.g., Snowmass '86, and the international Rochester meeting at Berkeley in July, 1986) offered opportunities for expanding the SSC's support. After the submission of the Conceptual Design Report, several states jumped the official starting gun and began to prepare their site proposals.

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69 At this time the SSC's estimated cost was $3.01 billion in FY86 dollars, including $529 million in contingency. This figure excluded the additional later costs for essential R&D, site acquisition, commissioning, computing, and detectors. *Report of the DOE Review Committee on the Conceptual Design of the Superconducting Super Collider* (Washington, DC: US Department of Energy, May 1986), DOE/ER-0267, pp. i, ii, 7–8 and 9–2. The 1990 Temple Review of the SSC's cost and schedule later became widely known as 'the Temple Report on the SSC'.

70 Quigg Interview, *op. cit.* note 10; FHC: J. D. to A. Kolb and L. Hoddeson, 2 April 1999. Jackson Papers (Berkeley): J. D. Jackson notebook B6, pp. 82–9, 104–108.

71 Bertram Schwarzschild, 'Panel Reaffirms High-Field Magnet Choice for Supercollider', *Physics Today*, 39 (July 1986), 21–23.

72 FHC: 'Report of the HEPAP Subpanel to Review Recent Information on Superferric Magnets', DOE/ER-0272, May 1986. The grumbling from Texas continued through the first half of 1988. In May 1988 Huson and McIntyre sent a letter to *Physics Today* claiming that the SSC's 17-meter dipole magnets would not achieve design performance and would have little involvement by US industry. Tigner responded, 'a full-length SSC dipole has now operated at 7.6 tesla, 1 tesla above the nominal SSC operating point', *Physics Today*, 41 (August 1988), 13–15.

CHANGING WINDS

Even with the Temple Report's positive evaluation in May 1986, uncertainty hung over the project throughout that year.[73] When attention turned to reducing the deficit, recipients of federal funding worried that 'the automatic GRH (Gramm-Rudman-Hollings) ax' might fall on their work, and hopes for new money for the SSC began to fade.[74] Fermilab Director Leon Lederman captured this tension in March 1986, in a letter to the editor of *The New York Times*. As a leading statesman of science, Lederman expressed concern that the GRH process would 'reduce the deficit even if it destroys the country.' Once triggered, he explained, the result 'would represent a substantial rollback of basic scientific research in the nation, more precipitous than the retrenchment which began in 1967.' Expanding on the dangers of such a rollback, he explained, 'Retrenchment has a chilling and escalating drag on progress and an even greater negative influence on the essential process of recruiting and training. A new-year cycle of GRH-induced austerity will very quickly slow the process of research with even stronger effects on development. The response of the system is historically asymmetric – it is very easy to damage, exceedingly difficult to repair.'[75]

Correspondence between Tigner and Lederman reflects their financial worries. Both assumed the SSC would be funded with 'new' money added to the existing 'base program' for high-energy physics, but there was no sign that such money would come. Meanwhile, the SSC work was adding to the workloads of laboratories which were conducting research on the SSC magnets and accelerator architecture.[76] Lederman suggested building a less expensive SSC, which, by colliding protons against antiprotons, as in Fermilab's Tevatron, rather than against protons, would require only a single ring of magnets. However, a review of this suggestion in June 1986 concluded that such a proton-antiproton collider would be less reliable

[73] *The Scientist*, 1 (7), (23 February 1987), 1, 8; J. D. Papers: Notebooks B7: pp. 100, 07.29; p. 104.07.31; pp. 106, 112. 113, and B8: pp. 5, 08.19; pp. 10, 08.22; pp. 29, 09.11, p. 30.

[74] Congress and the President had imposed The Balanced Budget and Emergency Deficit Control Act, also known as Gramm-Rudman-Hollings (GRH), in December 1985, in an attempt to reduce the budget deficit by 1991. See Irwin Goodwin, 'R&D Budget for Fiscal 1987: Life at the Threshold of Pain', *Physics Today*, 39 (5), (May 1986), 55–60. Indeed, by the end of 1989 cuts from GRH were imposed on all federally funded programs, including the DOE Laboratories. David Ritson, 'Demise of the Texas Supercollider', *Nature*, 366 (16 December 1993), 607–610.

[75] FHC: Leon Lederman, unpublished letter to the editor of *The New York Times* (10 March 1986).

[76] FHC: 1986 Tigner correspondence with Lederman, Lederman Collection.

than a proton-proton collider and have inadequate luminosity for frontier research. Moreover, the estimated cost savings were not substantial (less than $200 million).[77]

The biggest problem was that Presidential approval needed for the multi-billion dollar budget was not assured. Budgets at the national laboratories remained tight.[78] It was unnerving when, after 91 Congressmen signed a petition urging Reagan to support the SSC, the DOE Secretary Herrington, after a year in office, stated in a 5 March 1986 budget hearing before the House Science and Technology Committee that 'he had not yet made a decision about its future.' He conceded, 'We certainly don't want to give the impression that we're opposed to the project', but 'we don't want to waste further funds at this time until we make a decision.' Summarizing the SSC deliberations on Capital Hill, Herrington admitted, 'The technology is there'. As for new construction funds, 'this is not the year'.[79]

The delay in construction was compounded by an administrative shuffle that removed all the SSC's godfathers, except for Trivelpiece, from their positions within eight months of the delivery of the Conceptual Design. Keyworth, who as Reagan's Science Advisor had encouraged the physicists to pursue the SSC, and who had served as their spokesman to Reagan, resigned from the Office of Science and Technology Policy (OSTP) at the end of 1985. His vacated position remained unfilled until November 1986.[80]

Moreover, at the end of 1985, another key SSC supporter, James Leiss, retired as DOE's Associate Director of Energy Research for High-Energy and Nuclear Physics. His position was filled in an acting capacity by William Wallenmeyer, who temporarily also continued as Head of the

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77 *Physics Today*, 39 (July 1986), 23. Also see FHC: Lederman Archives, Barish Comm.

78 Mark Crawford, 'Accelerator Labs Face Austere Year', *Science*, 234 (4781), (5 December 1986), 1195; Quigg interview, *op. cit.* note 10.

79 *Physics Today*, 39 (May 1986), 58–59. Although DOE was aware that the delay in construction would raise the overall cost of the project (due to inflation), DOE did not expect Congress to fund construction until a site had been chosen. Tigner had scheduled site selection for December 1986, but reviews and time-consuming political processes had stalled the schedule.

80 In June 1986, Reagan nominated William R. Graham as Keyworth's successor, but Graham was not confirmed until that October. Not well known in the scientific community, Graham rose to this position from acting Administrator of NASA, where his principal achievement had been trouble-shooting in the wake of the Challenger disaster of January 1986. *Physics Today*, 39 (November 1986), 57–58; Alun Anderson and Joseph Palca, 'White House science advisor takes the reigns after slow start', *Nature*, 329 (6139), (8–14 October 1987), 476.

Division of High Energy Physics.[81] But Wilmot Hess, appointed on 1 August 1986 to fill Leiss' post, was far less active than Leiss in his support of the SSC. When Wallenmeyer retired from the DOE in late 1987 to become President of the Southeastern Universities Research Association, his position remained vacant until John O'Fallon accepted it in September 1988. With the loss of Keyworth, Leiss, and Wallenmeyer, the SSC's supporters in Washington were substantially weakened. From late 1985 through late 1986, Trivelpiece was effectively the sole SSC advocate in Washington.

It was Trivelpiece's job to translate the Conceptual Design Report into a form suitable for presentation to President Reagan. Twice at the end of 1986, he briefed the White House Domestic Policy Council on the scientific merits of the supercollider.[82] He argued that the SSC would restore the United States' superiority in high-energy physics. But the country's recession threatened prestige-building programmes (including the Defense Department's Strategic Defense Initiative, 'Star Wars', NASA's 'Orient Express' space plane, and NASA's Space Station), and deadlocked the SSC. There was no money for new projects. The expected increases in federal revenues had not materialized. Removing the deadlock would require direct presidential intervention.

At the request of Secretary Herrington, Trivelpiece briefed Reagan in the White House when he attended a meeting of the Domestic Policy Council on 29 January 1987.[83] Ever the actor, Reagan responded with high drama. Pulling an index card from his pocket, he read a Jack London verse:

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 by dry rot.

I would rather be a superb meteor
Every atom of me in magnificent glow
Than a sleepy and permanent planet.

The proper function of man
Is to live, not to exist
I shall not waste my days in trying to prolong them
I shall use my time.

81 Wallenmeyer was a strong proponent of the 'three-laboratory system' in which three labs, Brookhaven, Fermilab, and SLAC would share the maintenance of basic research at the cutting edge of the national programme.

82 J. D. Jackson papers (Berkeley): J. D. Jackson Notebook B9, pp. 26–28, 33.

83 The fact that in this month Fermilab reached 1.8 TeV in its Tevatron undoubtedly boosted Trivelpiece's confidence. *The Chronicle of Higher Education* (11 February 1987), 7, 9.

Asked to explain what he meant, Reagan invoked a reference to American football. He replied that Kenny Stabler, the Oakland Raiders' quarterback, had once said the poem meant, 'Throw deep!'[84] Stabler had a reputation for calling risky offensive plays. With this sporting endorsement, the SSC began its life as a federal construction project.

THE MISFORTUNE OF THE UNSOLICITED PROPOSAL: THE ROAD NOT TAKEN

Now three communities with different interests began to scramble for the SSC: high-energy physicists and the URA, both interested in the SSC as a tool for research; the DOE, concerned with the Department's role as custodian of the new facility and its Department of Defense (DOD) size budget; and the politicians who cared about policy implications, such as international competitiveness, business, money, and prestige.[85] That there was serious conflict between the expectations of the DOE and the physicists soon became apparent, prompted by an unsolicited management proposal prepared by the CDG for the URA and submitted to the DOE in March 1987.

In September 1986, Edwin L. Goldwasser travelled to Berkeley to work with the CDG. A highly respected physics statesman, he had served as Deputy Director of Fermilab for eleven years and later as Vice Chancellor of the University of Illinois. He joined the CDG as Associate Director for Development. Shortly after his arrival at the CDG, Goldwasser drafted a proposal to the DOE arguing that URA was the obvious organization to manage the SSC.[86] The proposal reflected Goldwasser's judgement that everyone would save time, money, and effort by letting a noncompetitive contract to URA for the management and operation of the SSC. He was proceeding in a manner that physicists had used successfully in negotiating government contracts.

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84 See *The New York Times* (2 February 1987), 1, 5; *The Wall Street Journal* (2 February 1987), 1; *The New York Times* (3 February 1987), 15–16; Mark Crawford, 'Reagan Okays the Supercollider', *Science*, 235 (4789), (6 February 1987), 625; Irwin Goodwin, 'Reagan Endorses the SSC, a Colossus Among Colliders', *Physics Today*, 40 (March 1987), 47–49. Reagan's popular association with football derived from his role as George Gipp in the 1940 film, 'Knute Rockne – All American'.

85 *Physics Today*, 40 (August 1987), 47–52.

86 To avoid a conflict of interest, he resigned from the URA Board of Trustees to become an employee half time for CDG and half time directly for URA. It was agreed that half of his time would be devoted to writing the proposal and the rest to 'odd jobs.' Edwin Goldwasser interview by A. Kolb and L. Hoddeson, 8 May 1993.

Signs from Washington supported Goldwasser's confidence. On 30 January 1987, Secretary Herrington announced Reagan's support for the SSC as a $4.5 billion project.[87] Site selection was underway. A schedule for the submission, evaluation, and selection of site proposals was described on 10 February at a DOE press conference. The official invitation for site proposals was extended on 1 April, with proposals due on 2 September 1987. A committee organized by the National Academy of Sciences and the National Academy of Engineering was being assembled to provide the initial evaluation of proposals, and return a list of the best-qualified sites by the end of that year.[88]

In this favourable context Goldwasser submitted a concise and elegantly written fourteen-page proposal that explained why the URA was 'uniquely qualified to provide the broad representation of the nationwide academic community together with the management experience that is essential to the success of an SSC laboratory and research program.'[89] On 2 March 1987, Knapp transmitted the proposal to Trivelpiece, who, after determining the legality of letting an unsolicited contract, shepherded the proposal on through the DOE.

At a critical meeting between Trivelpiece and top DOE administrators, the main issue was whether to accept the URA's proposal. Herrington's Under Secretary, Joseph Salgado, is said to have 'had pen in hand ready to sign the unsolicited proposal.'[90] But he resisted the URA's attempt to bypass competitive bidding for the contract and asked Trivelpiece whether resolving the matter just then was urgent. Trivelpiece could not say it was.[91] Had he done so, the course of the SSC might have followed the trail blazed by the CDG pioneers.[92]

Instead the road was blocked. For in the new Washington culture, the URA would have to bid competitively for its government contract.[93]

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87 FHC: DOE Press release, 30 January 1987.

88 'Siting the Superconducting Super Collider', National Academy Press, 1988, p. 3; *Science and Government Report*, XVII (15), (1 October 1987), 1–34; *Physics Today*, 40 (August 1987), 52. See Glenn Sandiford's critical account of the site selection process, 'Reason and Rationality', in M. Riordan, L. Hoddeson, A. Kolb, R. Jacobs, and S. Weiss, *Tunnel Visions: The Rise and Fall of the Superconducting Super Collidor* (in preparation).

89 FHC: 'Proposal to Serve as Contractor for the Construction and Operation of the "Superconducting Super Collider" Laboratory', submitted by Universities Research Association, Inc., 2 March 1987. URA Collection.

90 FHC: D. Pewitt, Red Team Briefing Document, 30 September 1988.

91 Goldwasser interview *op. cit.* note 86. Goldwasser recalled that Trivelpiece told him of this incident.

92 Goldwasser interview, *op. cit.* note 86, and Trivelpiece interview, *op. cit.* note 34.

93 Mark Crawford, 'Science Community Okays Supercollider', *Science*, 238 (4826), (23 October 1987), 477.

Building the SSC would come to look more 'like building an aircraft carrier than like building a particle accelerator.'[94] The rejection by tabling of the unsolicited proposal, was the first clear sign of that change.[95]

SHIFTING GROUND

Just as the DOE was considering URA's unsolicited proposal, the SSC lost its last powerful DOE proponent. Trivelpiece resigned in April 1987. He left on a high note, after securing Reagan's endorsement.[96] Robert Hunter eventually replaced Trivelpiece in August 1988, after confirmation delays of over a year. In the interim, James Decker, a career civil servant, filled in for Trivelpiece and held on to the unsolicited proposal. Hunter became linked with a new style of 'integrated' management, in conflict with the physics culture in which Goldwasser and other CDG physicists had worked.

The DOE promoted the idea that the SSC was a project of interest to communities outside high energy-physics.[97] For example, in a DOE-sponsored Pre-Proposal Conference in Washington, on 29 April 1987, the SSC was touted as good for business and the US economy, promising jobs and economic spin-offs. At a December meeting in Denver, physicists met with industry and government officials.[98] Keyworth, now a consultant and Chairman of the Board of the Council on Superconductivity

[94] Knapp interview, *op. cit.* note 63.

[95] While the bureaucratic mechanism for issuing RFPs and letting procurement contracts was by no means new in government funding, and was well known to DOD contractors, it had not yet affected the budgets and procedures of high-energy physicists. Knapp interview, *op. cit.* note 63.

[96] He became Executive Director of the American Association for the Advancement of Science on 27 April 1987. *Science*, 236 (4800), (24 April 1987), 377. There was speculation that Trivelpiece was a candidate for SSC director and that he left the DOE to avoid a conflict of interest. A memo from W. J. H. Panofsky spelled out the conflict of interest provision according to which former employees of the DOE were prevented from taking a job that involved DOE contracts for the first year and for the second year from taking a job where there would had been substantial policy involvement in the past. FHC: Panofsky to BOO Files, 'Information from Lloyd Sides' (SLAC legal counsel), 26 August 1988, copied from Panofsky files, SLAC Archives.

[97] Ritson, *op. cit.* note 74.

[98] It was sponsored by the Division of Particles and Fields of the American Physical Society and hosted by the state of Colorado and a group of Colorado universities and industries. The proceedings, 'SSC Status Report to the Nation', reprinted numerous talks by physicists, politicians, and administrators expressing their views of the wide-ranging benefits of building the SSC.

for American Competitiveness,[99] voiced some of the reasons why he still so strongly favoured the SSC. He urged 'committing ourselves to building the Superconducting Super Collider, as a statement of our resolve to retain preeminence in science ... '

Why was the SSC 'so dear'? Keyworth said,

> It has to do with economic growth, with national aspirations, and with the 'C' word that we'll be hearing so much of during the next twelve months, 'competitiveness.' How do we do that? By applying the kinds of national competitive advantages that others envy US for. I'm referring, of course, to our science and technology, as well as to the entrepreneurial environment that enables us to take quick advantage of new opportunities. That's what I would call the basis for a national competitiveness policy, and, ultimately that's the broad force behind the SSC.[100]

Keyworth clearly saw the SSC as the linch pin to sound long-term investment linking research with competitiveness. From this meeting in Colorado grew the 'International Industrial Symposium on the Super Collider' (IISSC), which between 1987 and 1993 annually brought together physicists and politicians with representatives of industry.

The high visibility of the SSC site competition had some negative consequences. By late December 1987, eight states had been designated as the best qualified to host the SSC – Arizona, Colorado, Illinois, Michigan, New York, North Carolina, Tennessee, and Texas. But New York withdrew two weeks later, in response to local opposition over environmental and quality of life concerns.[101] With fewer states left in the running, Congress began to criticize the increasing budget. In October 1988, the Congressional Budget Office issued a scathing report, 'Risks and Benefits of Building the Superconducting Supercollider', claiming that the supercollider's cost would likely exceed $6 billion, and heralded a new era of congressional scrutiny.[102] Moreover, larger economic factors threatened its support. The stock market crash in October 1987 startled the American

[99] Irwin Goodwin, 'After a Wait, Hunter Joins DOE, Keyworth, Bernthal to New Jobs', *Physics Today*, 41 (November 1988), 52.

[100] In the same speech, Keyworth recognized and encouraged the growing trend towards interdisciplinary research and partnerships between universities and industry. FHC: George W. Morgenthaler and Uriel Nauenberg (eds.), *Draft of Proceedings: SSC Status Report to the Nation: A National Symposium on the Superconducting Super Collider* (University of Colorado, 24 October 1988).

[101] The seven remaining sites would continue to be reviewed for geological and environmental conditions until the final choice, deferred until after the Presidential election in November. Sandiford, 'Reason and Rationality', in *Tunnel Visions*, *op. cit.* note 88.

[102] Mark Crawford, 'CBO Cautions Congress on SSC', *Science*, 242 (4876), (14 October 1988), 186; Crawford, 'SSC Report Attacked', *Science*, 242 (4882), (25 November 1988), 1243.

public, and when Reagan's improved economic forecast failed to materialize, the SSC was caught in a political quagmire. The Iran Contra trials in the summer of 1987 raised doubts about American intentions. It became more difficult to pursue so expensive a research tool without international support.

Because Reagan endorsed the SSC, Trivelpiece's attempts to secure international funding could in principle have been pursued. But the priority for international partners dropped off the DOE's radar after Trivelpiece left. Decker was bogged down by the complex details of site selection and budget politics, and the DOE failed to authorize CDG to pursue foreign contributions.[103] While the SSC remained dependent on foreign contributions, particularly from Japan, the momentum and opportunity to bring international participants into the SSC was lost.[104]

The unsolicited proposal sat in suspense at the DOE. In an attempt to stir action, URA submitted a slightly revised version on 22 February 1988.[105] But as an 'acting' director, rather than one appointed and confirmed, Decker lacked the ability to launch a management contract on such a large project.[106]

[103] *The Scientist* (18 May 1987), 6. Elliot Marshall, 'Big Versus Little Science in the Federal Budget', *Science*, 236 (4799), (17 April 1987), 249.

[104] When CERN director Schopper advised the House Science and Technology Committee in spring budget hearings in 1987, this time asking the US to contribute to CERN's Large Hadron Collider (LHC), rather than build the SSC, he claimed the SSC was unlikely to become an international collaboration. Lederman had been warning the US high-energy physics community about this possibility since he stated at Snowmass in 1982, 'by the early 1990s there will be a European capability to pave the LEP (Large Electron Positron) tunnel with superconducting magnets', unless the US acted immediately to do something more dramatic and exciting than ISABELLE, which was already obsolete. L. Lederman, 'Fermilab and the Future of HEP', *Proceedings of the 1982 DPF Summer Study on Elementary Particle Physics and Future Facilities* (Division of Particles and Fields, American Physics Society, 1982), 125–127; Therese Lloyd, 'Europe Balks At Support For Collider', *The Scientist*, 1 (12), (4 May 1987), 1–2; Irwin Goodwin, 'Race for the Ring: DOE Reacts to Congress's Anxieties on SSC', *Physics Today*, 40 (8), (August 1987), 47–50.

[105] The revision consisted essentially of establishing a separate Board of Overseers for Fermilab, an effort to place more distance between Fermilab and URA and to minimize any perception of favouritism for Illinois at a point when site selection was in full swing.

[106] Quigg interview, *op. cit.* note 10. Early in 1989, Secretary of Energy James Watkins under President George Bush further weakened SSC foundations when direction of the SSC left the Office of Energy Research and was placed under the Office of the SSC (OSSC) overseen by Deputy Secretary of Energy, W. Henson Moore, a former Louisiana Congressman. FHC: Timothy Toohig manuscript 'SSC: the Anatomy of a Failure: a Case Study of Institutional Amnesia', 3 November 1996.

Magnet Deals and Ordeals

While the SSC lost political ground in Washington DC, the CDG's physicists, invigorated by Reagan's endorsement, continued to work with optimism. Chris Quigg, whom Tigner appointed as his new Deputy Director at the beginning of 1987, was impressed by 'the adventurousness of the design',[107] which even CERN's Carlo Rubbia had described as 'absolutely fantastic.'[108] Wolfgang Panofsky of SLAC concluded, 'the CDG has done an absolutely first-rate job in the face of unusual, and at times difficult, circumstances'.[109]

In fact, the CDG's newly designed magnets were not functioning as well as expected. One problem was that their coils shifted when the magnets were cooled down to superconducting temperatures or when activated with current, because the different materials used in constructing the magnets had different expansion coefficients. They needed a robust design for collars to hold the conductors in place. The challenge was addressed in the spirit of research by a team that unfortunately felt itself 'all in a fish bowl' and observed by 'nervous and often unfriendly bureaucrats.'[110] Tigner explained, 'Since the time of Lawrence and Wilson, physicists have appreciated the frontier nature of the instrumentation they needed ... not expecting that the first design of anything would work as it ultimately can be made to. When one is pushing the limits one must expect the unexpected.'[111]

That industry would be essential to the magnet programme was understood from the start, but the nature of the control that came with that involvement was unclear. and a 'source of continuing difficulty'. 'There

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107 Quigg interview, *op. cit.* note 10.

108 Broad, *op. cit.* note 58.

109 FHC: W.K.H. Panofsky to William Wallenmeyer, 22 January 1987, from Panofsky Papers, SLAC.

110 FHC: M. Tigner to L. Hoddeson, 9 February 1999; Also Goldwasser interview *op. cit.* note 86; and *The New York Times* (13 December 1987), Section Y, *Science Times*, 23, 28.

111 FHC: Tigner to Hoddeson, 9 Feburary 1999. This method had proved successful in the building of Fermilab's Main Ring as well as its superconducting accelerator, the Energy Doubler. C. Westfall and L. Hoddeson, 'Thinking Small in Big Science', *Technology and Culture*, 37 (3), (July 1996), 457–492. Hoddeson, 'The First Large-Scale Application of Superconductivity', *op. cit.* note 41. Under Tigner, CDG pursued the same approach to make available 'a significantly improved technological capability as cost effective as possible'. The results of CDG's programme increased the capability of the superconducting cable available industrially by 50% over what had existed five years earlier when the Doubler was being built.

is a great deal of misunderstanding concerning the term "industrial involvement" in the SSC, wrote Panofsky in January 1987.[112]

Another problem concerned how to organize the magnet programme, which extended to several laboratories. To minimize competition and motivate cooperation among the existing laboratories, the DOE decided that Brookhaven, Fermilab, and Berkeley should all be involved in the SSC magnet development. Berkeley was made responsible for wire and cable development, Brookhaven for fabrication, and Fermilab for testing.[113] The plan backfired. As Fermilab's earlier work on superconducting magnets had shown, it was not effective to separate designing from testing of magnets.[114] Moreover, Brookhaven and Fermilab had difficulty cooperating because of Brookhaven's lingering resentment over the death of ISABELLE, attributed to Fermilab's Leon Lederman. Also, Fermilab was committed to developing its Tevatron programme and could not easily accept more responsibility. And as Tigner did not control the purse strings, the CDG was unable to demand that the three laboratories cooperate.[115]

In a brute force attempt to tackle this collaboration problem, Tigner appointed a four-member 'Magnet Management Group', consisting of himself, Goldwasser, Peter Limon, and Victor Karpenko. Limon, from Fermilab, was serving as the CDG's Head of Accelerator Systems. Karpenko, a former Livermore engineer, was leading the CDG's magnet group. This 'gang of four', as they called themselves, would visit Brookhaven for two days, then Fermilab for two days every four weeks.[116] Such an arrangement might have worked in a wartime project where all groups shared a common defence goal, as in the Manhattan Project.[117] The SSC's magnet goals were not sufficiently unifying.

'It was a tricky business', Goldwasser explained. 'We would just be there hovering over what they were doing, meeting at their meetings, and impressing them with the fact that their responsibility was to do what we

[112] FHC: W. K. H. Panofsky to William Wallenmeyer, 22 January 1987, from Panofsky Papers, SLAC.

[113] There was enormous pressure to solve these problems in time to meet goals set for the industrial manufacturers who agreed to begin magnet production once the site was ready.

[114] Hoddeson, 'The First Large-Scale Application of Superconductivity', *op. cit.* note 41.

[115] Goldwasser interview, *op. cit.* note 86; FHC: Victor Karpenko to M. Tigner, 7 December 1987, and *SuperCollider Newsletter*, put out by APS/DPF, 15 March 1984, 15 February 1984. Quigg interview.

[116] Goldwasser interview, *op. cit.* note 86; Peter Limon interview by L. Hoddeson and A. Kolb, 5 May 1993.

[117] L. Hoddeson, P. Henriksen, R. Meade and C. Westfall, *Critical Assembly: A History of Los Alamos during the Oppenheimer Years, 1943–1945* (New York: Cambridge University Press, 1993).

asked. But that had to be done tactfully enough so that very bright and talented people would still feel that they had sufficient autonomy that their own ideas could be tested and given a fair hearing.' The business was 'even trickier because there was a different personality there every time. Sometimes Tigner, sometimes Peter Limon, sometimes Karpenko, and sometimes Goldwasser', he explained.[118] Limon's forthright manner of expressing his criticism produced so much resentment that Tigner relieved him of his responsibility, leaving the remaining three to make the inspection visits. The problems intrinsic to this unwieldy coordination attempt presaged more serious difficulties in the years ahead.[119]

When Karpenko announced his retirement, Goldwasser agreed to serve as temporary head of the CDG's magnet group for six months. But he felt that John Peoples, then Deputy Director of Fermilab's Accelerator Division, would be better at the job. With the help of several magnet experts (Tigner, Alvin Tollestrup, Richard Lundy, and Helen Edwards) Goldwasser managed to persuade Peoples to accept responsibility for the CDG's Magnet Division for one year.

When Peoples arrived at the CDG at the end of December 1987, 'I was a little bit like the plumber who'd been called in to fix the leaks and the toilets that are overflowing during a dinner party'.[120] He recalled, 'I wasn't exactly invited to dinner.' He began by establishing an empirical programme to research the magnet movement problems through extensive magnet testing. Goldwasser explained, 'We didn't know whether it was linear expansion of the magnets or radial expansion that had to be restrained.' Drawing on Fermilab's Energy Doubler experience, Peoples demanded that many magnets, each with slightly different parameters, be designed, built, and tested, so that one could separately try out the various possibilities. He methodically laid out a one-year programme, 'with every magnet identified, and what the changes in design would be, and what tests would be made, and what each would prove'.[121]

From his somewhat disengaged position at the the CDG, Peoples came to view the magnet problem as 'philosophical.' He recognized a misalignment between the management practices of national weapons laboratories (like Karpenko's Livermore)[122] and national research laboratories (like

[118] Goldwasser interview, *op. cit.* note 86.

[119] Ritson, 'Demise of the Texas Supercollider', *op. cit.* note 74.

[120] John Peoples interview by L. Hoddeson and A. Kolb, 23 October 1993.

[121] Goldwasser interview, *op. cit.* note 86. The magnet development programme was also the context in which the vast new capacity of computer modeling first made substantial impact on the SSC. Peoples interview, *op. cit.* note 120.

[122] The Livermore engineer had earlier been project manager of the Magnetic Fusion Test Facility (part of the nuclear underground testing project).

Fermilab or the SSC). As Tigner noted, while physicists can usually work with members of the standard industrial culture, they often clash with members of the military-industrial culture when 'military officers call the shots.'[123] Karpenko was 'used to one kind of management and really not used to trying to cajole people, seduce them into doing things. That of course is how you have to do it in this [physics] business', Peoples added.[124]

There was a mismatch in the very notion of engineering work – between those who understand engineering as having 'all relevant parameters well within the range of current practice', and others, including physicists, who work to 'achieve some novel purpose or achieve some advance in performance or specific cost. It is the latter that "frontiers" are all about', Tigner explained.[125]

SEEING THE ELEPHANT

The year 1988 began auspiciously, with Reagan's second strong endorsement of the SSC. His budget in March requested $363 million for SSC construction. At a ceremony in the Rose Garden on 30 March, Reagan stressed adventurous themes of research at the frontier: 'The Superconducting Super Collider is the doorway to that new world of quantum change, of quantum progress for science and for our economy.'[126] Quigg recalled, 'They had kids from every state in town.... We gave each kid an SSC T-shirt, and arranged for two of them to present one to the President.' Quigg remembers being 'very moved by the notion that our equations and ideas had, for fifteen minutes, gotten the attention of the most powerful leader on Earth ... and caused him to say things that were noble and true.'[127]

Other leaders of American politics, science, and industry spoke in favour of the SSC during the first half of 1988. Lederman and Quigg gathered fifty-eight of these statements in a compendium, *Appraising the*

123 FHC: Tigner to Hoddeson, 9 February 1999.

124 Peoples interview, *op. cit.* note 120. Peoples noted that an accident at Fermilab worked in his favour. Within a week of starting his new CDG assignment, a superconducting magnet had failed in a spectacular fashion: a segment of the coil evaporated, a design glitch capable of destroying the entire magnet. The ensuing investigation led to a much deeper understanding of the magnet's physics.

125 FHC: Tigner to Hoddeson, 9 February 1999.

126 FHC: 'Remarks by the President in Meeting with Supporters of the Superconducting Super Collider Program', 30 March 1988, The Rose Garden.

127 Quigg interview, *op. cit.* note 10.

Ring: Statements in Support of the Superconducting Super Collider, which URA published in July 1988. But the idealism expressed in the book was hard to maintain in the face of the budgetary crises that weighed on the physicists' dreams by the middle of 1988. Quigg remembers frequent calls to the CDG about the funding uncertainty, especially one from DOE on one particular 'black day', on Friday, 13 May 1988. Hess explained that the CDG's budget had been 'zeroed out' in the House Appropriations Committee, and told Quigg he should make a list of CDG people to be laid off. The problem was eventually resolved and the CDG spared, but the incident illustrates the tension of the working climate in that period.[128]

Early in August 1988, 'the elephant began to swish his tail and to threaten with his trunk',[129] as the DOE issued its official 'Request for Proposals' (RFP) for managing the SSC. Responses were due on 4 November 1988.[130] The formal document sent the URA the message that, compared to its earlier unsolicited proposals, in this new regime, 'DOE would not accept a bid without some strong construction management infrastructure' on equal footing with the physicists. Naming of top staff and industrial partners would be required. And so many bureaucratic requirements were imposed that it looked like 'your standard industrial buy-a-widget sort of contract', reflected Bruce Chrisman.[131]

Knapp, who had learned several weeks earlier of the DOE's decision to compete bids for the Management and Operation (M&O) contract, discussed how to respond to the RFP with Panofsky, who proposed that 'the responsibility for preparing the proposals should be that of the President of URA'. Knapp agreed.[132] He elected to respond to the RFP with a Department of Defense-style proposal, typical of the multi-billion-dollar budget-range proposals routinely written by military and industrial contractors. This decision was shaped in part by a rumour that Martin-Marietta, a huge military-industrial contractor, was also preparing a response.[133] Martin-Marietta never did present a proposal but the rumour

[128] *Ibid.*

[129] Michener, *op. cit.* note 11, pp. 329. 332–333, ch. 6.

[130] The announcement of the RFP was made in *Commerce Business Daily* (3 August 1988). The RFP itself was issued on 19 August 1988.

[131] Bruce Chrisman interview by Kolb, Hoddeson, and Weiss, 14 January 1998. Other physicists felt, as Richard Lundy, that to comply with the requirements of the RFP would be a Faustian bargain, and that 'we're getting in bed with the devil'. Attributed to Lundy in Chrisman interview.

[132] FHC: Panofsky to BOO Files, 1 August 1988, from Panofsky Papers, SLAC.

[133] Knapp interview, *op. cit.* note 63. Universities Research Association, Inc. submitted the sole response to the RFP on 4 November 1988, and the final contract between DOE and URA was signed on 16 January 1989. See also Appendix E, 'Key Personnel', *Request*

made a powerful and lasting difference for the SSC.[134] Knapp also devised a plan to identify the SSC's director and upper management.

Knapp asked Hugh Loweth, a long-time member of the Office of Management and Budget (OMB) to recommend someone who had the proper experience to write a military-industrial style proposal.[135] Loweth recommended N. Douglas Pewitt, a PhD physicist who had served as an analyst in the OMB and had been Keyworth's deputy in the Office of Science and Technology Policy (OSTP) in 1982 at the time Keyworth had encouraged the physicists to plan the SSC. Pewitt was now working in San Diego, at Science Applications International Corporation (SAIC), a firm of 'beltway bandits', the Washington name for consultants who serve as 'hired guns' to provide technical, managerial or lobbying support to government contractors. He knew all the political and budgetary requirements of a successful proposal of this kind.

Pewitt assembled a URA proposal writing team comprised of physicists, industrial representatives, and more beltway bandits. Responding to 'vibrations' from the DOE and Congress, he proceeded to establish industrial partnerships for the physicists, a move intended to deal with his and the DOE's new view that physicists were unqualified to manage multibillion dollar projects.[136] Over the next weeks, the URA contacted candidate companies and eventually selected as the partners, EG&G Inc., a manager of DOE and DOD facilities as well as a manufacturer of electronics and scientific instruments, and Sverdrup, Inc., a construction management firm and Air Force contractor.[137] 'Teaming agreements' were drafted and signed. The bureaucratic terms of the cooperation (including licensing

for Proposals. Number 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 (Chicago Operations Office, U.S. Department of Energy), Closing Date: 4 November 1988. The CDG did not expect to build the SSC without external industrial support. In fact, in the March 1986 Conceptual Design Report, the DOE-Chicago Operations Office had awarded a contract to RTK, a joint venture of three engineering companies to handle the architecture and engineering designs, for the period 24 June 1985 through 30 September 1987. However, such earlier subcontracting with industry, which was standard practice in the building of accelerators, did not elevate the subcontractors to equal partners, which was how Knapp and Pewitt came to interpret DOE's requirement in its RFP.

134 Knapp interview, *op. cit.* note 63.

135 *Ibid.*

136 *Ibid.* FHC: The argument for teaming with industrial partners is made in a document written on 7 October 1988 by D. Pewitt and R. Schwitters, 'Rationale for Teaming', FHC. Many physicists remember Pewitt as difficult to work with as he criticized their ability to manage big projects. Knapp interview, *op. cit.* note 63; Bruce Chrisman interview by Adrienne Kolb, December 1997.

137 FHC: Ezra Heitowit to David J. Norton, 8 September 1988.

procedures, news releases, raiding of personnel, handling of proprietary information, etc.) were clearly laid out.[138]

In the standard manner in which 'beltway bandits' write government proposals for building aircraft carriers – a manner very different from the way physicists prepare research proposals – Pewitt arranged that different teams would take part: a 'red team' would write the first draft; then a 'pink team', and finally a 'gold team' would critique what had been written, identifying what was missing, finding errors, and generally fixing whatever the 'red team' had not done to best advantage. A memorandum from Pewitt to Knapp on 24 August, 1988 laid out the procedure. A strict schedule was imposed: when drafts would be ready, when they would be reviewed by the Red Team and the Pink Teams, when printing would take place. Three volumes were planned: I, the Technical Proposal; II, the Business Management Proposal; and III, the Cost Proposal. Budgets would be coordinated by the URA's Washington office. Overall responsibility for coordinating and producing the proposal was assigned to EG&G in Falls Church, Virginia.[139]

The work of the Pewitt group differed in several ways from standard academic practice. The competitive nature of responding implied secrecy. The solicited response to the RFP was therefore organized behind closed doors. The URA judged it essential to exclude the CDG as it was working in the public sphere. The exclusion harboured lasting resentment. Also, the Red Team needed to decide whether the URA would be forced by the DOE to 'bring on as a "team member" a systems integrator', a familiar concept in systems engineering but as yet foreign to the high energy physicists.[140]

The URA participants began their proposal writing with a planning meeting in Berkeley on 14 September 1988.[141] After an unsuccessful

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[138] The agreement with Sverdrup was signed by its Vice President James C. Uselton on 22 September 1988. Knapp recalls that many felt resistant to the change, even DOE's Ed Temple, who felt the old way worked well, Knapp interview, *op. cit.* note 63. As of 26 August the organizations that informally had expressed interest in teaming at the SSC were: EG&G, IIT, Martin Marietta, Parson Brinkerhoff, The Ralph K. Parsons Company, SAIC, Stone & Webster Engineering, Corp., and Sverdrup Corporation, document in Panofsky papers (26 August 1988).

[139] FHC: The memo of 24 August 1988 was circulated to the URA in Washington, DC, to Wojcicki at CDG, to Jim Finks at Fermilab, and to the members of the SSC BOO Proposal Oversight Subcommittee (M. Blume, R. Frosch, R. Schwitters, W. Panofsky, and B. McDaniel, *ex officio*), calling for 'input from everyone by COB [close of business] Wednesday, 31 August'. so they could 'begin soliciting Red Team reviewers'.

[140] FHC: D. Pewitt to E. Knapp, 24 August 1988, on 'Significant Policy-Sensitive Matters on SSC O&M Proposal', sent to James Finks at Fermilab from Ken Shirley at URA. SSC Historical Collection, FHC.

[141] FHC: Meeting notice of 14 September 1988, Berkeley.

attempt by Pewitt to arrange that the proposal writing occur in Berkeley,[142] the effort was established in an industrial park in St. Charles, Illinois. Over the following six weeks, the group of about 20 writers worked seven days a week, eight to ten hours a day, in the five thousand square feet of rented office space.[143] Occasionally a member of the CDG was called to St. Charles to provide information,[144] but the CDG was otherwise excluded from the writing to avoid a conflict of interest. Members of the outpost could not help feeling ignored.[145]

The URA's resulting volumes six-inch thick submitted on 4 November 1988, differed notably from Goldwasser's fourteen-page, unsolicited proposal. The response encompassed a myriad of technical and practical details, including every aspect of daily site operations, staffing, interface with other national labs, construction, magnet industrialization, corporate interface, wages and salaries, fringe benefits, detailed costing procedures, and property management. 'Bureaucratically it was gorgeous', Knapp reflected.[146]

The sharpest blow to the CDG was the URA's choice of Lab Director: Roy Schwitters rather than Tigner.[147] An experimental physicist and Harvard professor, Schwitters had had experience in the management of high-energy physics, for he had served as a co-spokesman for the Collider Detector at Fermilab experiment in the late 1980s after a successful experimental research career at SLAC. But he had only been peripherally involved in the five-year design effort at the outpost, as a member of the URA's BOO for the SSC.

The CDG physicists expected that Tigner, their well-proven leader, would be named SSC director. While everyone admitted Tigner had been an outstanding director for the CDG, some outside the CDG, felt this

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142 According to Pewitt, CDG did not want to cooperate. Pewitt interview, *op. cit.* note 12. However, according to Tigner, Pewitt never discussed the matter with him. Tigner to Hoddeson, 9 February 1999.

143 Chrisman interviews, *op. cit.* notes 131 and 136.

144 Among those from CDG who came for short advisory visits were: Tim Toohig, advising on conventional construction, Tom Kirk on magnets, and Tom Elioff on cost and schedules. Chrisman interviews, *op. cit.* notes 131 and 136.

145 The SSC Board of Overseers reviewed the proposal, but the urgent deadline and the complexity of the proposal gave Pewitt and Allhoff significant power.

146 Knapp interview, *op. cit.* note 63. Jackson Papers (Berkeley): J. D. Jackson notebook B9, pp. 92–93.

147 Knapp interview, *op. cit.* note 63; Panofsky and McDaniel agreed on 2 September that Panofsky should inform Maury of that decision. He did so on Sunday, 4 September, the day before Labor Day. FHC: W. K. H. Panofsky to BOO Files, 6 September 1988, from SLAC Archives.

'cowboy in the Wilson tradition',[148] might not be 'the guy to go out and charm Congress.'[149] As a leader he was described as 'excellent technically but gruff and taciturn, which was not good in dealing with nonphysicist types in the various government bureaucracies'.[150] Knapp reflected that not many in the younger generation of physicists had the management skill needed for directing a project as large as the SSC – especially if it was going to be built in the way the DOE contracts office wanted, like 'an aircraft carrier.'[151]

Schwitters had been chosen in a secret process based on 'buddy system' discussions. That secrecy was necessary had seemed obvious to the URA and its advisers once the RFP appeared. Panofsky noted, 'formally identifying the director publicly before submittal of the proposal might be interpreted as violating the privileged nature of proposals.'[152] He admitted, 'Identifying a Director is tricky business. We must assume that competitive proponents will also attempt to identify a candidate, or may already have a candidate. For this reason and in general because of the competitive nature of the procurement, there cannot be a public search. If there were such a public search, then this would automatically establish a connection among the different proponents and again this would be illegal.'[153]

The SSC BOO elected to 'establish itself as the Search Committee for the SSC Director.' Schwitters and George Trilling were excused, as they were possible candidates. Jerome Friedman, Frank Sciulli, and Martin Walt of Lockheed were added.[154] Panofsky agreed to telephone around in the community to gather information. The many candidates discussed included Lederman, James Beggs (former Head of NASA), Tigner, Richter, Nick Samios (Brookhaven director), Sam Ting, Steven Weinberg, Charles Baltay, Wojcicki, Bjorn Wiik, Trivelpiece, Trilling, and, 'the leading candidate of the younger generation', Roy Schwitters.[155] The recommendations varied a great deal. Fred Gilman, President of the Division of Particles and Fields, suggested selecting a 'tycoon' from the

[148] Personal communication, Tom Kirk to Michael Riordan, 17 June 1999.

[149] Robert Diebold interview by Michael Riordan, 20 April 1995.

[150] FHC: M. Riordan notes of a conversation with Panofsky, 19 June 1998.

[151] Knapp interview, *op. cit.* note 63.

[152] FHC: Panofsky to BOO Files, 1 August 1988, from Panofsky Papers, SLAC.

[153] FHC: Panofsky, 'Talking Paper. Discussions with Trilling and McDaniel', 8 August 1988, from Panofsky Papers, SLAC.

[154] FHC: 'Minutes of the Meeting of the SSC Board of Overseers', 19–20 August 1988.

[155] Lloyd Sides, Staff Counsel to SLAC, was asked to investigate whether Trivelpiece might have a conflict of interest in serving as Director of the SSC, even though he left the office of Director of Energy Research more than a year earlier.

industrial/administrative ranks that could serve as 'a symbol of unity among small and big science', while giving Tigner free reign as technical director. On Sunday, 28 August, at a committee meeting held at the O'Hare Hilton in Chicago, the URA search committee selected Schwitters.[156]

The URA members who travelled to Berkeley to attend their mid-September meeting had the uncomfortable job of explaining their choice of Schwitters to members of CDG. John H. Marburger, President of the URA Board of Trustees, accompanied URA President Knapp and SSC BOO Chairman Boyce McDaniel on the trip. Hoping to mend fences, Knapp and MacDaniel awkwardly went back and forth at the podium in Berkeley, postponing their announcement of the bad news, which the CDG already knew.[157] It was too late. The CDG knew Schwitters had been chosen and that they had been excluded from the selection process while others in the community had been included.[158]

The URA listed Tigner in the proposal as Deputy Director.[159] Could he stay and work under Schwitters? They had too many differences. Those who have spoken with us recalled Tigner's opposition to the level of industrial teaming that Schwitters felt was essential.[160] Some noted Tigner's wish for his CDG people to have important positions at the SSC while Schwitters was circumspect with those loyal to Tigner. Others speculated that Tigner could not tolerate being subordinate to Schwitters. Tigner expected, but was not granted, full control over machine decisions. Tigner tried to work with Schwitters for a brief period, but 'the discussions with Roy did not go at all well.'[161]

The URA had created an 'uneasy alliance' in which Schwitters and Tigner would have had to negotiate their respective roles.[162] Schwitters recalls many visits and negotiations with Tigner between September 1988 and the end of January 1989.

> We discussed project manager, we discussed deputy, we discussed all kinds of things. I thought Maury was very difficult to talk to. Clearly, he appeared to be very disappointed after not being chosen ... And it never really converged on something that Maury obviously felt gave him what he wanted ... I had the uncomfortable feeling though that if we couldn't

[156] FHC: Panofsky to BOO Files, 'Search for Directorial Nominee by URA', 26 August 1998, from Panofsky Papers, SLAC.

[157] Quigg interview, *op. cit.* note 10.

[158] Roy Schwitters interview by Michael Riordan, 22 March 1997.

[159] Helen Edwards was listed as Director of Accelerator Systems; R. Robbins as Head of Conventional Construction; and Chrisman as Head of Laboratory Administrative Services.

[160] Schwitters interview *op. cit.* note 158.

[161] S. J. Wojcicki interview by A. Kolb and G. Sandiford.

[162] FHC: J.D. Jackson to Kolb and Hoddeson, 2 April 1999, p. 6.

have a heart to heart conversation that it could cause some uncomfortable feeling down the line.[163]

Schwitters aptly called their exchange, a 'classic conflict of styles.' Tigner represented the independent, confident, self-taught, tinkerer in the pioneering style of Bob Wilson, while Schwitters wanted to respond to what he and the URA construed as the new regime.[164]

Tigner, Quigg recalled, had a habit of whistling hymns when things got bad. When they were 'really bad, he would walk about absent-mindedly whistling the tune to "Once to every man and nation comes the time to decide" '.[165] On 20 February 1989, Tigner resigned from the SSC, a project to which he had devoted the previous five years of his life.[166]

On 10 November 1988, two days after George Bush was elected to succeed President Reagan, the national site competition announced Waxahachie, Texas as the SSC's future home. Now many non-Texan physicists and politicians sought ways to withdraw from the SSC project. An already weakened federal front-line defence of the SSC eventually gave way under the more tenuous support and micromanagement by the Bush administration's Department of Energy.[167] Few members of the CDG were asked and fewer elected to follow Schwitters to Texas.[168] 'Continuity of the SSC hard-learned corporate knowledge was basically lost.'[169]

Pewitt and the writing team crafted their response to the RFP around Schwitters.[170] As the DOE received only the URA's response to its RFP, a contract was negotiated based on Pewitt's proposal. Knapp remembers the contract negotiation as 'one of the more painful experiences I have ever had'. For the DOE 'wanted everything ... control over personnel policies, control over raise policies, control over vacation policies, control over rules and regulations'. The results of the first round of negotiation with the Chicago Operations Office made the URA but 'a contractor in name ... [with] all of the discretion taken away'. The URA did use to advantage

163 Schwitters interview, *op. cit.* note 158.

164 Schwitters interview, *op. cit.* note 158.

165 Quigg interview *op. cit.* note 10. The hymn comes from an abolitionist poem by James Russell Lowell.

166 Personal Communication: S. Wojcicki to M. Riordan, 1 September 1988; and Rene Donaldson to M. Riordon, 24 August 1998.

167 The loss of Texas Democrats, Jim Wright as Speaker of the House and Senator Lloyd Bentsen, weakened SSC support in Congress.

168 Those who did included Alex Chao, Tom Elioff, Jim Sanford, Tim Toohig, Tom Kirk, and Roger Coombs. Wojcicki and Jackson served later on the SSC's Program Advisory Committee.

169 FHC: Chao to Hoddeson, December 1999.

170 Pewitt interview, *op. cit.* note 12.

the fact that John Herrington had submitted his resignation just before Inauguration Day and was eager to finalize the SSC's contract before he left Washington. Knapp submitted his own resignation immediately after the contract was signed on January 16, 1989.[171]

> ...and out of the shadows rose the elephant. It was gigantic, thirty or forty feet tall, with wild, curving tusks and beady eyes that glowed.... It was overwhelming – the menace of that towering creature, and Levi knew that he was destined to turn back ... [172]

CONCLUSION

The compelling features of 'Big Science' have long been of concern to scholars of science. There is an abiding interest in the destinies of large cooperative, yet competitive, multi-university research teams; in the negotiation of enormous budgets; in the creation of powerful ties between science and government; and the implications of decade-long timescales of research and development. Many have studied relationships between theory and experiment, between science and engineering, and the political and institutional relationships.[173]

We have focused here upon the conflict of two of the communities involved in a particular 'Big Science' project. It was a conflict of ideas about how the SSC should be organized. As an analytical tool we have employed the metaphor of the 'frontier', which historian Frederick Jackson Turner devised and applied to America a century ago. The 'frontier outpost' usefully models the tension between the CDG physicists and their Washington managers between 1983 and 1988, and illuminates the conflict between explorers and those who support and control their missions. This conflict eventually fragmented the SSC community and contributed to the project's ultimate demise in 1993. That the outpost was destined to collapse was never in question. The physicists at the outpost in Berkeley 'understood from the very beginning the fate of "outposts" ', Tigner said, 'and discussed this many times among ourselves. Nevertheless we persevered.'[174]

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171 Knapp interview, *op. cit.* note 63.

172 James Michener, *op. cit.* note 11, esp. 332–333.

173 Most of the relevant references on big science are in: Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997); Peter Galison and Bruce Hevly, *Big Science: The Growth of Large-Scale Research* (Stanford: Stanford University Press, 1992); John Krige (ed.), *History of CERN: Vol. III* (Amsterdam: Elsevier North Holland, 1996; Armin Herman, John Krige, Ulrike Mersits, and Dominique Pestre, *History of CERN: Vols. I and II* (New York: North Holland, 1987 and 1990).

174 FHC: Maury Tigner to Lillian Hoddeson, 9 February 1999.

From the physicists' point of view, the SSC was a logical step in the evolution of experimental high-energy physics, a story that began in the 1930s in Berkeley, around the pioneering accelerators of Ernest Lawrence's laboratory.[175] The field had grown rapidly during the Second World War, and mushroomed with the establishment of new laboratories, including Brookhaven National Laboratory, and later, Stanford Linear Accelerator Center and Fermilab. High-energy physicists had seen their budgets grow from thousands to millions of dollars during the 1950s, and to hundreds of millions by the 1970s. In proposing a multibillion-dollar machine in the early 1980s, physicists thought they were simply advancing by another order of magnitude. But as the SSC's budget moved into the billion-dollar range, the project crossed an invisible 'bottom line' in Washington. When that line was crossed, the DOE shocked the physicists with their demand of formal management plans, industrial partnerships, environmental impact statements, and quality assurance plans. They were startled to recognize that the DOE did not believe physicists could manage such huge projects.[176] Until 1987 the SSC had largely been controlled by physicists; by late 1988 it was clear that their perception of how to manage their large projects was at odds with a new generation of Washington managers. The URA, caught between the physicists and the DOE, judged that compliance with Washington offered the only chance to secure the SSC.

At one crossroads in the spring of 1987, Trivelpiece believed it possible to proceed in the earlier manner. Had the unsolicited proposal assumed top priority at that moment, perhaps the SSC would have survived. Because of the postponement, we cannot know whether, in the changed DOE climate, conventional practice would have succeeded. Only top priorities survive in the world of very Big Science. In post-war times, through the mid-1970s, collegial relationships featured prominently. But from 1975, when the Atomic Energy Commission was replaced by the Energy Research and Development Administration, and later by the DOE, as the major patron of high-energy physics, physicists became 'just another group of constituents.'[177] One seasoned observer, Norman Ramsey, later stated that they didn't work hard enough to make new friends in Washington.[178]

175 J. L. Heilbron and Robert Seidel, *Lawrence and his Laboratory: Nuclear Science at Berkeley, 1931–1961* (Berkeley: University of California Press, 1989).

176 Chrisman interviews, *op. cit.* notes 131 and 136; FHC: Manuscript by Tim Toohig, 'SSC: The Anatomy of a Failure: A Case Study of Institutional Amnesia', Dallas Morning News interview with Moore on 30 July 1993.

177 Ritson, 'Demise of the Texas Supercollider', *op. cit.* note 74.

178 Remarks at international conference on the History of the Standard Model held at SLAC, June 1992.

This rift between explorers and their patrons rippled into the larger SSC community, dividing and weakening it.

As the Cold War wound down, Capitol Hill could hardly remember the physicists' glorious contributions to the Second World War.[179] Washington no longer saw the relevance of expensive science to the American people. The idea that basic research could serve national defence had lost the political valency it enjoyed when Vannevar Bush wrote *The Endless Frontier*. From the perspective of Washington, 'the Bob Wilson way of running things ... was *very*, very different from how everything else in government was. It was history and had been history for a long time'.[180] Frederick Jackson Turner's frontier thesis discusses waves of migration into a new territory.[181]

In our story, these waves, consisting of different populations, began with the Reference Design Study scouts, followed later by the CDG explorers and pioneers who populated the outpost and planned the expedition. Next came the military-industrial managers, who as developers saw lucrative gain. Each wave brought new motivations that changed the landscape. The CDG physicists had hoped to continue their large-scale studies of the endless frontier, just as they had throughout the Cold War years. But when the outpost lost its relevance at the end of 1988, they realized that their brass ring of opportunity, which earlier shone so brilliantly, had tarnished.

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179 Kim McDonald, 'Gigantic Particle Accelerator Will Have No Modern Rival – If It's Built', *The Chronicle of Higher Education*, 13 (7), (16 October 1985), 1, 10–11.

180 Doug Pewitt interview *op. cit.* note 12.

181 Frederick Jackson Turner, 'The Significance of the Frontier in American History', speech delivered in 1983, copyright 1920, reprinted in George Rogers Taylor (ed.), *The Turner Thesis: Concerning the Role of the Frontier in American History* (Boston: D.C. Health and Co., 1956), 9–10.

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