Jefferson Lab's 1985 Switch to Superconducting Accelerator Technology

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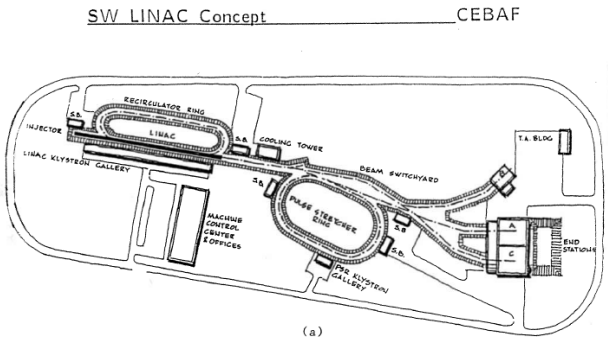

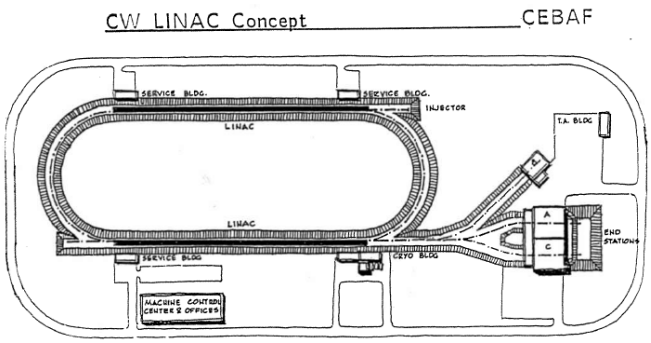

***On the cover**:* Contrasting accelerator concepts as depicted in the November 1985 *Scientific and Technological Assessment Report (STAR) on the Superconducting CW Linac Design for CEBAF*. The STAR report proposed that the Continuous Electron Beam Accelerator Facility's machine be built as a connected, antiparallel pair of continuous wave linear accelerators—the CW linac concept—based on superconducting radio-frequency (SRF) technology, even though approval was already in hand for a standing wave conventional linac with a pulse stretcher ring. In the racetrack-shaped SRF machine ultimately built at what is now called Jefferson Lab, electrons pass clockwise up to five times through two linacs.

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Catherine Westfall

Introduction

One of the most surprising turns in the history of accelerators came in 1985. After a six-year battle to gain approval to build a linear accelerator with a pulse stretcher ring (PSR), researchers at CEBAF, the Continuous Electron Beam Accelerator Facility (renamed Thomas Jefferson National Accelerator Facility in 1996) announced that they wanted instead to build an accelerator based on a risky new technology—superconducting radio-frequency (SRF) technology. Perhaps even more surprising, the Department of Energy (DOE) decided to approve the change. This paper explains how Hermann A. Grunder, CEBAF's first permanent director, made the decision to build an SRF accelerator, how he persuaded physicists and DOE officials to approve the change, and how he mobilized his staff to build this state-of-the-art machine.

Enter Grunder

In many ways, Grunder was an unlikely champion for superconducting RF. Although he had an established reputation as a builder of accelerators for nuclear physics, Grunder had not been involved in the international SRF research and development effort. Moreover, he had little close involvement with those designing or using electronuclear machines, aside from time spent on the 1983 panel that was convened with D. Allan Bromley of Yale as chair to help choose the next large such accelerator. In the early 1980s, in fact, much of Grunder's attention had been consumed by other issues. From mid-1982 to mid-1983, he learned the ways of Washington, working as a special assistant at DOE's Division of High Energy and Nuclear Physics under William Wallenmeyer and James Leiss. Shortly after returning to Lawrence Berkeley Laboratory (LBL), he was appointed deputy director and became deeply involved in developing plans for various non-physics projects, including an electron light source for use by chemists.[2] Ironically, the path to Grunder's advocacy for SRF began with an assessment of the problematic political environment destined to complicate CEBAF's switch to the new technology.

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[1]The author would like to thank Joseph Bisognano, Anthony Chargin, James Coleman, Teresa Danforth, Beverly Hartline, David Hendrie, Robert Johnson, Christoph Leemann, Thomas Moore, Estelle Seeley, Todd Smith, and Ronald Sundelin for many helpful comments and suggestions. This paper was written from documents available at Jefferson Lab. Although an interesting complement would be the tale of the switch to SRF from the perspective of decision-making and politics in Washington, this paper does not try to tell that tale, since sources describing the Washington perspective were not available. All interviews and conversations were conducted by the author and all documents and interview records are located in the Jefferson Lab Archives, unless otherwise noted.

[2]Hermann Grunder, curriculum vitae, January 1985; interview with Hermann Grunder, March 3, 1995. Although the Berkeley laboratory has since been renamed Ernest Orlando Lawrence Berkeley National Laboratory, the name in use in 1985 appears in this paper: Lawrence Berkeley Laboratory (LBL).

Grunder would later tell *Physics Today* that he was urged to accept the CEBAF directorship by two important Washington figures: George Keyworth, President Ronald Reagan's science advisor, and Alvin Trivelpiece, director of the DOE Office of Energy Research.[3] When Grunder visited Washington in early February 1985, he found out why CEBAF needed his help: at a time when construction funding hung in the balance, support for the project seemed to be eroding. His meeting notes reveal that although Leiss and Wallenmeyer joined Trivelpiece and Keyworth in advocating CEBAF, some worried that Virginia's Republican Senator John Warner had lost interest in the project and DOE Secretary Donald Hodel reportedly opposed it. In addition, rumor had it that DOE undersecretary John Deutsch had "whispered opposition into Senator Bennett Johnston's ears," at just the time when the powerful Democratic Louisiana senator felt "let down by the [physics] community," which had failed to continue support for Isabelle, a particle physics accelerator project "he carried the water for." One of Johnston's aides confirmed that he was "leaning toward opposition." Despite previous advisory panel support for CEBAF, Johnston, among others, worried that the physics community was "not backing the project." Could Grunder get "at least 60% of the physics community together" to support the project? Lack of support also complicated recruitment. Grunder admitted that he would "have to say no" to the directorship if he was "unable to get [a] first rate team" to build the SURA accelerator. In light of the many difficulties, one advisor said: "Don't take the job"; you "can't sell the machine." In the end, "Keyworth would take that response," that you just "can't sell it."[4]

Instead of being discouraged, Grunder was stimulated by the long-shot prospect of making a success of the project Keyworth hoped would be "a gem" for American science. As a first step toward building this gem, Grunder huddled with Keyworth, making a long list of physicists to contact to rally support for the SURA machine. Since a number of influential electronuclear physicists were professors at MIT, Grunder made a special trip to Cambridge on February 26, 1985. Arthur Kerman, Ernest Moniz, and Stanley Kowalski had a clear message for the prospective new director: "Barnes [was] wrong . . Quark probing needs to be at 8–12 GeV."[5] Grunder would subsequently hear repeated echoes of this argument, which had been raised two years earlier by Kowalski and others during the competition that decided who would build the accelerator.[6] In Grunder's words: "There was the recurring accusation that [CEBAF's] energy

[3]Irwin Goodwin, "CEBAF Wins Praise for Design, but Its Future Is Uncertain," *Physics Today* ***39*** (1986), p. 52.
[4]Quotes from Hermann Grunder, "Washington Visits with WAW (William A. Wallenmeyer), Jim Leiss, Judy Bostock, T. Palmieri, Proctor Jones," February 1, 1985, and "Meeting with Martha Krebs," February 5, 1985, Hermann Grunder log.
[5]Quotes from "Meeting at MIT with Kerman, Moniz and Kowalski," February 26, 1985, Hermann Grunder, "Meeting with JK," February 21, 1985, and Hermann Grunder, February 22, 1985, Hermann Grunder log.
[6]For more information on the events leading to the decision to build the accelerator proposed by SURA, see Catherine Westfall, "The Founding of CEBAF, 1979 to 1987," CEBAF, 1994.

range had to be above 10 GeV to make a difference." Such assertions left Grunder with a problem: the SURA design was not a likely candidate for upgrading to produce energies over 6 GeV.[7]

In March 1985 CEBAF's prospects began to improve, aided by Grunder's ambitious efforts to recruit staff members and rally support for the facility from physicists and from congressional and executive branch officials. On March 21 Grunder came to a planning meeting for the next heavy ion accelerator, the Relativistic Heavy Ion Collider (RHIC), armed with plausible figures and schedules showing that both CEBAF and RHIC could be accommodated in the budget. On the same day, Grunder met with Bennett Johnston to argue CEBAF's scientific merits. He characterized the meeting as "friendly, substantive," and reported that Johnston commented: "You and Panofsky explain these things so well." At a later meeting, Johnston was "hopeful" that CEBAF would succeed. On April 1, the day of Grunder's second meeting with Johnston, T. Palmieri, a top Office of Management and Budget (OMB) official, expressed confidence that Grunder would become the "focal point for [a] center of excellence and for keeping [the physics] community together."[8]

In late April, Grunder made his decision official, telling LBL director David Shirley that although it "was not easy ... to decide to leave and accept the directorship" of CEBAF, his "sense of duty to science in general and to nuclear physics in particular made it impossible ... to refuse." Although this announcement did not come in time for the annual funding hearings before the congressional appropriation committee at the first of the month, at that time SURA president Harry Holmgren emphasized the importance of Grunder's imminent appointment, noting that SURA was "now in the closing weeks of our negotiations with a well-known accelerator builder," who would "start work on the project by May 1" and "bring with him a core group of senior physicists to get this project into the next phase of construction." At the same time, Holmgren stressed CEBAF's other main advantage in words that would soon haunt the new director. "I would like to point out," he noted, "that we have one of the most thoroughly studied and reviewed design concepts ever brought before this committee."[9]

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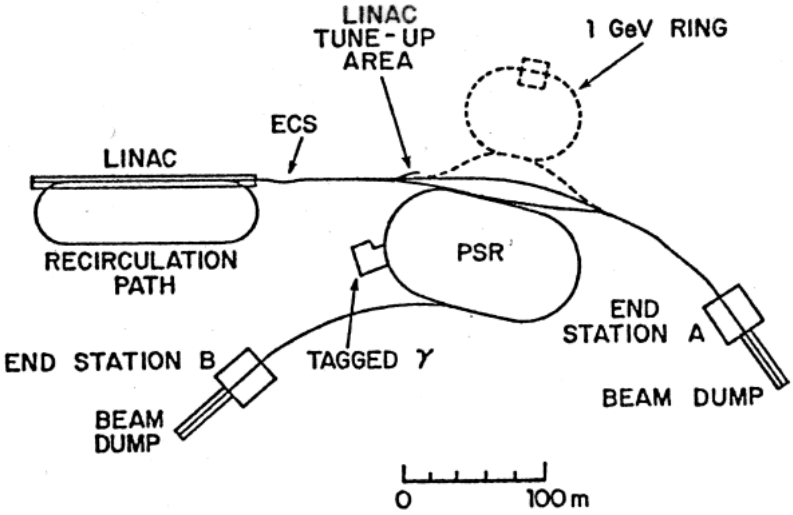
[7]Interview with Hermann Grunder, March 22, 1994.

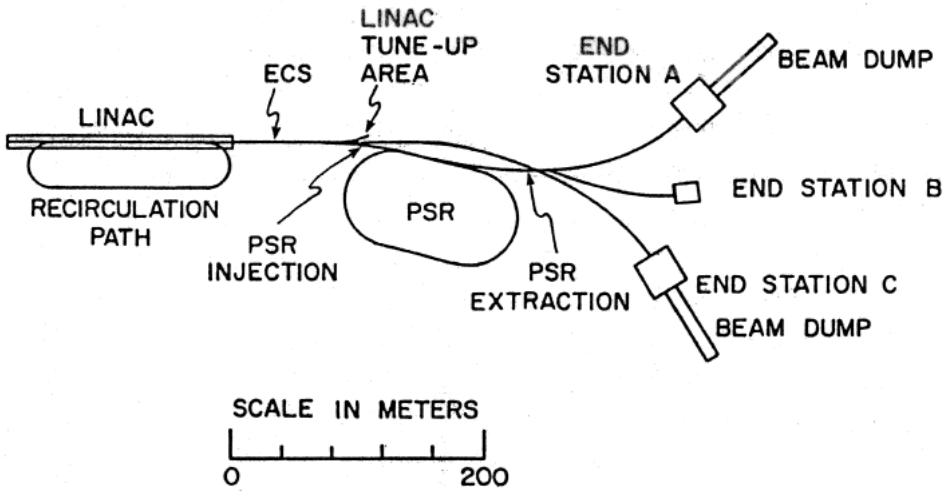
[8]Hermann Grunder's LBL assistant, Robert Johnson, recently judged that Grunder's maneuvers with leaders in the physics community were crucial: "Had [Grunder] not been the superb planner that he is, or if he had not taken the time and effort to sell CEBAF to the laboratory directors ... [leaders at] the other laboratories could have seized the opportunity" when questions were raised about changing the technology "to undermine CEBAF." Robert Johnson to Catherine Westfall, August 30, 1995. Text quotes from "Meeting with Johnston, P. Jungs, and B. Cooper," March 21, 1985, Hermann Grunder log, Robert Johnson notes on discussion with Hermann Grunder, April 1, 1985, and "Meeting with T. Palmieri," April 1, 1985, Hermann Grunder log. Also: "Memo from Robert Johnson," March 6, 1985, "Meeting with Proctor Jones," March 12, 1985, "Meetings with L. Rosen and G. Garvey," March 19, 1985, "Meeting with Judy Bostock," April 1, 1985, Hermann Grunder log.

[9]Quotes from Hermann Grunder to David Shirley, April 23, 1985, and Subcommittee of the Committee on Appropriations, House of Representatives, *Fiscal Year 1986 Department of Energy Authorization*, 99th Congress, First Session (Washington, D.C.: GPO, 1985) p. 2809. For the story of the SURA accelerator and its development, see Catherine Westfall, "The Founding of CEBAF, 1979 to 1987," CEBAF, 1994.

Grunder had his work cut out for him. In the still-uncertain funding environment of 1985, he needed to drum up unified support for CEBAF from the electronuclear physics community. On the one hand, SURA's design for a linac and pulse stretcher ring had already been officially designated as the community's top choice by a number of advisors, including former Bromley panel member Grunder. On the other hand, at this juncture the linac/PSR did not seem well suited for meeting the experimental needs—and thereby gaining the support—of the community it was meant to serve.



The linac/PSR as depicted in 1982. (Source: *Proposal for a National Electron Accelerator Laboratory, 1982*, Southeastern Universities Research Association.)



The linac/PSR as depicted in 1984. (Source: *Conceptual Design Report, Continuous Electron Beam Accelerator Facility*, Southeastern Universities Research Association, Charlottesville, Virginia.)

Reassessing the Design

As CEBAF's director, Grunder's first task was to recruit staff. By June he had hired three staff members who had previously worked with him at LBL. Christoph Leemann, who had worked on a number of LBL accelerator projects, came to head CEBAF's Accelerator Division; Beverly Hartline, who had excelled in LBL scientific planning, came to be Grunder's scientific assistant; James Coleman, who had been a top manager in LBL's Administration Division, was recruited from a vice-presidency at a New Mexico corporation to head CEBAF's Administration Division. Later that summer, Grunder recruited Anthony Chargin, a division leader from Lawrence Livermore Laboratory's Engineering Department, to head the Engineering and Project Management Division. In the first year of the project, these associate directors, or "AD's," plus Hartline (who later became an AD) would help Grunder lead CEBAF as his Director's Council. Coleman later characterized the distinctive management style that "developed very early on ... . Coming to consensus was crucial; so was communication. People were encouraged to think, to come up with new ideas. But there was no such thing as a four-to-one vote ... . Everybody had to agree or we had to talk about it more and more. But once having agreed on the principles and the directions, then the details were left up to the individuals."[10]

Expert advice was also crucial to the evolving decision-making style at CEBAF. Just as Grunder had sought information and advice from Washington insiders when considering the directorship, he and his AD's now sought information and support from physicists and engineers when building the laboratory. As a start, they recruited a number of different types of experts to join the staff. For example, accelerator theorists Joseph Bisognano and David Douglas came from LBL, project engineers Thomas Moore and Rolf Bork came from Livermore, cryogenics expert Claus Rode and mechanical engineer George Biallas came from Fermi National Accelerator Laboratory, radiation safety expert Geoffrey Stapleton came from the Rutherford Appleton Laboratory in England, vacuum expert Paul Brindza came from Vacuum Industries, Inc., systems engineer Jock Fugitt came from LBL, and project engineers Otto Matherny and William Alexander came from the DOE Oak Ridge Operations Office and private consulting, respectively. To give advice on experimental issues, CEBAF managers also recruited research physicists, including Volker Burkert and Bernhard Mecking, who both had substantial experience with electron physics. New staff members joined others already working on the project, including Franz Gross, Richard York, Blaine Norum, William Diamond, and a core support staff paid by the state of Virginia.[11]

To further the aim of obtaining the maximum amount of information and support from experts, Grunder scheduled a flurry of meetings and reviews, a tactic that fueled a fast working

[10]Quotes from Catherine Westfall and Curtis Brooks interview with James Coleman, September 29, 1993. Also: CEBAF, "CEBAF Key Staff: Biographical Sketches from the SURA Newsletter," August 1986.
[11]CEBAF, "CEBAF Key Staff: Biographical Sketches from the SURA Newsletter," August 1986.

pace for visitors as well as CEBAF's growing staff. In June, James McCarthy, Franz Gross, and others convened 76 researchers from all over the world to help plan the experimental program. The group first wrote a series of reports, based on ongoing discussions of possible CEBAF experiments. Forty-four researchers then met for a two-month summer study. By the end of August, this group had composed a preliminary plan for CEBAF experimental equipment.[12]

In addition to keeping in touch with evolving research plans, which would have an impact on overall project planning, the CEBAF staff also plunged into an intense review of the accelerator design. An early challenge was the need to respond to two DOE reviews, an Independent Cost Estimate (ICE) Review held in April and a Validation Review held in May. Both reviews reflected concerns about cost estimates. From June 1984 to May 1985, the cost estimate of the project had risen from $225 million to $236 million. Although much of this increase resulted from the later than expected start of the project, in light of higher costs, uncertainties loomed large. As the ICE Report noted, the vendors planning to produce modulators and klystrons, which were "big ticket" items representing "about 24 percent of the total construction cost for the technical components," had given "a wide spread in the production cost estimates indicating a high level of cost uncertainty." Part of the problem was that vendors were concerned about the risks and costs of producing the modulators and klystrons, which were scaled-up versions of Stanford Linear Accelerator Center (SLAC) components that pushed the technology to its limit. Reviewers, who noted discrepancies in plans for civil construction and other aspects of the design, were disconcerted because the uncertainty in the pricing of modulators and klystrons seemed to reflect "the present design status of the project," which was "still fluid," despite the fact that "the project" was "past the preconceptual phase." Although the design concepts for the SURA machine had been thoroughly reviewed, DOE still did not have a believable budget and a clear plan for building the new laboratory.[13]

Even if DOE had not been concerned, the design would have been reviewed at this juncture. Accelerator builders usually reassess a design before beginning construction of a major accelerator project. This preconstruction review allows them to consider new materials or technologies that have become available since the design was drafted and to propose more detailed information about budgets, deadlines, and procedures that are tailored to the time and place of construction and the accelerator building team. Thus, to no one's surprise, Grunder convened a technology review, which lasted from June 27 to August 30, 1985.[14]

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[12]H. Crannel and F. Gross, eds. "Introduction," in *Proceedings CEBAF/SURA 1985 Summer Workshop*," Newport News, Va., June 3–7, 1985.

[13]Quotes from "Independent Cost Estimate Report," July 1985, pp. 35–36. Also: Hermann Grunder to Dan Lehman and John Everhart, "CEBAF Cost Estimate Changes, June 1984 to May 1985 Validation Review," June 20, 1985; interview with Roger Miller, March 2, 1994.

[14]Hermann Grunder, "CEBAF Briefing for the Director, Office of Energy Research," January 9, 1986.

The technology review was not without its surprises, however. Leemann remembers being a little puzzled from the beginning. When Grunder asked him to convene the technology review, the new director "used very specific terminology." Leemann was asked to "assess the technology base" for building the machine. This "sounded grandiose ... because it seemed clear what we were going to build." From these instructions, however, Leemann concluded that Grunder felt "that if there were important other options" for machine design "one would pursue them." Grunder later admitted that he had "had an inkling," based on discussions with Alfred Maschke of TRW, Helmut Piel, Herbert Lengeler and other experts, that SRF technology might have matured sufficiently to be useful. His predominant concern at this point, however, was finding a way to build a machine that could eventually be upgraded to accelerate electrons beyond 4 GeV.[15]

As Leemann explained to DOE representatives on July 11, 1985, by that time the technology review was "underway to identify the most appropriate and cost-effective technology choices for CEBAF, and to develop the basis for the project work breakdown structure," a well-defined, coordinated plan for building the laboratory. To accomplish these goals, Leemann persuaded experts to come to CEBAF for informal conversations as well as for formal workshops. He also made arrangements for CEBAF staff members to travel elsewhere to gather information.[16]

Leemann remembers that at this stage he was thinking of ways of optimizing the linac/PSR design by replacing its traveling wave structures with standing wave structures, a plan that promised to save operations costs and reduce the risks and cost of producing high-power klystrons. In one of the many ongoing conferences, however, Grunder gave him an additional assignment: he was to investigate the potential of superconductivity.[17] Since Leemann was still skeptical, like many physicists, about the potential of SRF, he thought "that's going to be an easy one because everybody knows ... superconductivity doesn't work." Leemann subsequently persuaded Roger Miller, a SLAC accelerator physicist, to investigate the possibilities of using standing wave structures, and made preparations to travel with York to Cornell University, where Ronald M. Sundelin's group had been developing SRF technology with substantial success.[18]

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[15]Quotes from interviews with Christoph Leemann, September 29, 1993, and Hermann Grunder, March 22, 1994. Also: Irwin Goodwin, "CEBAF Wins Praise for Design, but Its Future is Uncertain," *Physics Today* **39** (1986), p. 52.

[16]Quotes from "CEBAF Monthly Progress Meeting," August 22, 1985. Also: Catherine Westfall and Curtis Brooks interview with Beverly Hartline, September 2, 1994.

[17]George Neil, who was at TRW at the time, and Joseph Bisognano, who was at CEBAF at the time, both remember feeling that the impetus for Grunder's interest was a visit with Maschke at TRW. Neil remembers that during the meeting, he gave Grunder a briefing on the subject and "soon after that ... he came back [to CEBAF] and sent ... the gang up to Cornell to get information." Quotes from interview with George Neil, June 22, 1994. Also: Interview with Joseph Bisognano, December 7, 1993.

[18]Quotes interview with Christoph Leemann, September 29, 1993. Also: interview with Roger Miller, March 2, 1994, and Catherine Westfall and Curtis Brooks interview with Ronald Sundelin, March 24, 1994.

Sundelin remembers that when the CEBAF scientists arrived he and other group members showed them around the lab and discussed test results with them, including beam test results from 1984. In addition, the Cornell group had prepared calculations for transferring the SRF technology to CEBAF. In Leemann's words, at this point he "got a rather thorough education in a hurry." Although he couldn't yet "pass judgment whether" they "could really make" superconducting RF cavities work at CEBAF, he now knew he "had enormously underestimated" the promise of the technology.[19]

When he returned to CEBAF, Leemann immediately recruited a number of SRF experts, including Sundelin, Piel, Lengeler, and Todd Smith to join the technical review. The laboratory then became the scene of an intense discussion of the relative advantages of three types of accelerators: the original linac/PSR design with traveling wave structures, a linac/PSR using standing wave structures, and a radically different design employing SRF technology. Planning proceeded in line with CEBAF's open-ended, consensual decision-making style. Roger Miller remembers that the SRF option quickly began to win more votes from reviewers than its competitors once the SRF experts arrived. For one thing, their excitement was contagious. In Miller's mind, the fact that Piel was there "all bubbly and enthusiastic" for SRF helped promote the technology, but even more convincing was the testimony of Sundelin "who just seemed so ... low key, but ... sure about it."[20] In Leemann's opinion, confidence in SRF continued to rise as the days wore on because more "facts began to emerge" about just how much progress had been made in developing the technology. The beam tests were persuasive, and for Leemann, a strong selling point for the technology was that "highly reputable ... international laboratories" were involved in the testing and willing, on the basis of results, to make further investments in it.[21] As Lengeler noted in 1995, he and Piel brought the news from CERN: "we had at this moment very promising results on 350 MHz [SRF] cavities ... [and] the firm intention to use SC cavities for LEP and the encouraging results of the CERN [SRF] particle separator." Leemann adds that at this point the CEBAF staff also learned "that DESY planned to incorporate" SRF cavities in PETRA, and that SRF technology was being used "at KEK in Japan."[22]

CEBAF managers changed their minds about the technology with dizzying swiftness. On July 23, Grunder mentioned SRF as only one of several options when listing possible designs for a CW electron accelerator at the SLAC Particle Accelerator Summer School. In fact, Hartline, who gathered the material for his talk, remembers that "Hermann gave no evidence that he was convinced that [SRF] was the technology we were going to be choosing ... . He didn't even seem

[19]Quotes from interviews with Ronald Sundelin, March 25, 1994, and Christoph Leemann, September 27, 1993.
[20]Quotes from interview with Roger Miller, March 2, 1994.
[21]Quotes from interview with Christoph Leemann, September 27, 1993, and Herbert Lengeler to Catherine Westfall, October 30, 1995.
[22]Interview with Christoph Leemann, September 27, 1993.

very enthusiastic" about it. By August 8, however, Grunder prepared a preliminary summary of the ongoing technology review which concluded that due to advances in SRF technology, the SRF linac merited "very serious consideration."[23]

Proposing the Switch to SRF

The idea of building an SRF accelerator prompted both excitement and concern at CEBAF. Such an accelerator would produce better-quality beams at lower operating cost, with greater potential for later upgrade to deliver higher energy. Moreover, since CEBAF would be developing a cutting-edge technology, the prospect of building an SRF machine was bound to please those concerned with the development of accelerator technology and make recruitment of accelerator experts easier. However, Grunder knew he was flirting with disaster in Washington, even by broaching the subject. Part of the problem was the skepticism about SRF. In addition, the linac/PSR design had been touted just months before in Congress for being unproblematic and for having the stamp of approval from advisory panels. How could former Bromley panelist Grunder come to DOE officials with a completely different, risky design? Grunder remembers: "I was afraid I would appear to be criticizing myself." In his opinion, raising the issue of a switch to SRF technology would place him in "an absolute quagmire" at a time when DOE and CEBAF leaders needed to work together to shore up the project's uncertain funding prospects.[24]

Despite such worries, by the time of the August 8 summary, Grunder and his staff had decided to inform those outside the laboratory of the possibility that the design would be changed. Hartline remembers that at the Director's Council meeting held on August 6, "we all went around the table and we all voted to" inform DOE immediately. According to meeting notes, the council concluded that it "would be unprofessional not to" let DOE know that CEBAF might decide to change the design. In the next few days, Grunder also informed key NSAC members, asking for their advice.[25]

On August 8, Grunder and other CEBAF staff members met with DOE officials and presented the preliminary summary of the technology review. DOE representatives clearly reflected ongoing skepticism about SRF. Although Wallenmeyer, who had replaced Leiss as the associate director of High Energy and Nuclear Physics, did not have a strong initial reaction, Hartline's notes from the meeting reveal that David Hendrie, newly appointed director of DOE's Division of Nuclear Physics, and coworker Enloe Ritter worried that SRF was not yet mature

[23]Quotes from Catherine Westfall and Curtis Brooks interview with Beverly Hartline, September 2, 1994, and Hermann Grunder, "Preliminary Briefing on Superconducting Linac Option for the Continuous Electron Beam Accelerator Facility," August 8, 1985. Also: Hermann Grunder, "Accelerators for Nuclear Physics," presented to the SLAC Particle Accelerator Summer School, Stanford, Ca., July 23, 1985.
[24]Quote from interview with Hermann Grunder, March 22, 1994.
[25]Quotes from Catherine Westfall and Curtis Brooks interview with Beverly Hartline, September 2, 1994. Also: "Director's Council," August 9, 1985.

enough for implementation. In particular, they wondered whether the technology had been thoroughly tested "at full scale" with "coupled cells." In addition, they expressed alarm that an SRF design was not "the proposal the Bromley panel approved." Indeed, by presenting such an option CEBAF "destroy[ed] the statements" that the laboratory had "a conservative design." The switch also created administrative headaches for DOE by derailing standard procedures, which called for having an approved conceptual design report (CDR) by this stage in the funding process. Ominously, Hendrie and Ritter warned that if CEBAF switched designs there was a "non-negligible probability of killing the project," due to "possible adverse reactions from Trivelpiece, OMB" and officials elsewhere in the executive branch.[26]

Higher-level officials also worried about adverse effects. Trivelpiece, who was immediately informed of the possible switch by his subordinates, later told *Physics Today* that he feared that "altering the design at this stage ... could give rise to untimely political questions." Wallenmeyer remembers worrying that the proposed switch would be judged illegal and the project killed. Hugh Loweth, head of the Energy and Science Division of the OMB recalls feeling "nervous about it ... . If [Grunder] couldn't sell it, there could be a backlash" in Congress.[27]

Grunder was aware of his tenuous position. As he explained in a 1994 interview: "Put yourself in my shoes ... . There was the DOE, Hendrie and Ritter ... . It was dangerous to go there, because you can't talk off the record. Then there was Trivelpiece ... . Then there was OMB ... and there was John McTague and others in the President's Science Advisor's Office. I was in urgent need of counsel. But where could I go?" Grunder admitted that by this point, he wanted to build an SRF accelerator. Again, the prospect of a long-shot challenge stimulated rather than deterred him. "The counsel I wanted was how to do it."[28]

In Grunder's opinion, the meeting he had on August 9 with OMB staffer Judy Bostock was "very instrumental" because "she gave the correct counsel, saying if you are convinced that you are dealing with an outdated technology, we must not [pursue it]." In retrospect, Grunder judges that she probably simply meant to be supportive and that he read more into her words than she intended. At the time, however, Grunder left the meeting with the impression that Bostock meant: "we absolutely have to [switch to SRF]."[29]

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[26]Quotes from Beverly Hartline, "Visit to DOE," August 8, 1985, Hartline papers. Also: Hermann Grunder, "Meetings in Washington," August 8, 1985, Hartline papers.

[27]Quotes from Irwin Goodwin, "CEBAF Wins Praise for Design, but Its Future Is Uncertain," *Physics Today* **39** (1986) p. 53 and interview with Hugh Loweth, July 26,1993. Also: interview with William Wallenmeyer, October 18, 1991.

[28]Interview with Hermann Grunder, March 22, 1994.

[29]Notes from the time report that Bostock was "supportive of the [superconducting] option," substantiating Grunder's judgment that Bostock was merely positive about SRF. Beverly Hartline, "Visit to DOE," August 8, 1985, Hartline papers. Text quotes from interview with Hermann Grunder, March 22, 1994, and Hermann Grunder, "Meetings in Washington," August 9, 1985.

In any event, Bostock's support was atypical. On August 13, Hendrie called Grunder to say that Trivelpiece was "disturbed" that Grunder had gone, in violation of the chain of command, to see Bostock; Trivelpiece now forbade the CEBAF staff from talking to Congress. Although at this point he was "not prepared to tell" the CEBAF staff to "stop considering" superconductivity, Trivelpiece wanted them "to know he [was] seriously considering not going forward with the project." In the next month, Trivelpiece's mood did not improve. Grunder received word on September 10 that in a meeting with Hendrie, Wallenmeyer, and others, Trivelpiece noted that he never favored the SRF option and insisted that CEBAF stick with plans for a room-temperature machine.[30] "But I was not to be talked out of [the switch]," Grunder explained in 1994. For Grunder, two arguments prevailed: the technology was superior, and by applying it, CEBAF could meet the needs, and thereby circumvent the criticism, of the electronuclear physics community. "This double argument gave me the moral courage" to persist.[31]

Thus, despite the official DOE bias against the superconducting option, Grunder forged ahead with plans for a superconducting machine. The task was daunting. Staff members had to quickly produce from scratch a conceptual design employing a complicated technology that was still regarded with great skepticism by many experts. At the same time, they had to develop a believable budget and solid technical and administrative plans. In light of DOE's unwillingness to approve the switch in designs, they had to do this work alongside continuing plans for a room-temperature machine.[32] Moreover, they had to perform their dual task on a tight budget, since the funding agency steadfastly refused Grunder's pleas for extra funds: to augment the roughly $5 million provided by DOE in fiscal year 1986, Grunder obtained $150,000 from SURA in fall 1985 and $1 million from Virginia through the fiscal year.[33] All the while staff members faced the realization that the entire project would be in jeopardy unless they found a way to convince disgruntled DOE officials that the switch to an SRF design was justified.

To accomplish this task, Grunder pursued a three-pronged approach: he sought to gather as much information as possible to build a strong case for SRF, used this information to obtain support for the technology, and then publicized this support—and gave his own arguments for the technology—to those in Washington. Grunder had taken decisive steps to implement this approach by mid-September. Aggressive attempts at estimating the cost of the SRF design began

[30]In retrospect David Hendrie insisted that he had become a convert to the notion of switching to an SRF design by September 1985. At this point he was simply transmitting Trivelpiece's misgivings about the switch. Conversation with David Hendrie, February 29, 1996. Text quotes from "Hermann's Long Phone Conversation with Dave Hendrie," August 13, 1985, Hartline papers. Also: Hartline to Grunder, September 10, 1985, Hartline papers.
[31]Interview with Hermann Grunder, March 22, 1994.
[32]"Minutes DOE/CEBAF Monthly Meeting," September 24, 1985.
[33]Committee on Science and Technology, U.S. House of Representatives, *Fiscal Year 1987 Department of Energy Authorization*, 99th Congress, 2nd Session (Washington, D.C.: GPO, 1986) p. 162; "Minutes of SURA Executive Committee Meeting," September 6, 1985.

shortly after the meetings in early August. Later that month, Grunder, Chargin, and Holmgren began a series of trips, visiting Darmstadt, CERN, and Cornell to see operating SRF cavities. While in Europe the CEBAF leaders made stops at Interatom and Dornier to assess their cavity production facilities and discuss their willingness to produce cavities for CEBAF. With the completion of these visits, the CEBAF staff had canvassed all the major companies interested in building SRF cavities: Grunder had already visited TRW in the U.S. As reported in early September, "Interatom, Wuppertal, CERN, and Dornier [were] all very interested in assisting CEBAF" with an SRF accelerator. Chargin remembers that the visits to Interatom, Dornier, and TRW were particularly crucial because they demonstrated industrial readiness to build cavities. "After that I felt I could make a strong argument because I always felt that if you could buy SRF cavities in industry, you could build an SRF accelerator."[34]

As a result of these efforts and continuing design work at CEBAF, the CEBAF staff had a good handle on the performance of SRF cavities, industrial readiness for SRF cavity production, and the cost of the SRF design in time for the September 13–14 meeting of the laboratory's National Advisory Board (NAB), a group of prominent experimental, theoretical, and accelerator physicists convened to assess CEBAF's plans. On September 11, the day after hearing of Trivelpiece's continuing negativity towards SRF, Grunder arranged to meet a few days after the NAB meeting with McTague from the Science Advisor's Office and Trivelpiece.[35]

By the time of the NAB review, Grunder also had written up the official conclusions of the technology review, articulating in the process his position on the SRF design.[36] The paper began with the admission that the Bromley panel had endorsed the linac/PSR design, which "until recently ... was the only reliable technology available to produce high-duty-factor, multi-GeV beams of electrons." The paper then boldly noted that "although reliable and adequate," this technology was "not ideal from three standpoints: power consumption, beam quality, and future energy increases." While mentioning that the technology review had produced a standing wave linac design as one improvement to the original design, the paper went on to stress the alternative preferred by reviewers. "The intrinsic advantages of a superconducting linac have

[34]Quotes from Anthony Chargin to Catherine Westfall, September 29, 1995. Also: "Director's Meeting," August 14, 1985; "Director's Meeting," August 25, 1985; "Director's Meeting," August 20, 1985; "Director's Meeting," September 3, 1985.

[35]The National Advisory Board included: E. Henley, R. Kropschot, L. Bollinger, R. Briggs, L. Cardman, H. Jackson, S. Koonin, S. Kowalski, J. Leiss, H. Lengeler, J. Lightbody, B. McDaniel, R. Miller, R. Minehart, E. Moniz, H. Piel, I. Sick, R. Sundelin, and P. Vander Arend. CEBAF, *Scientific and Technological Assessment Report (STAR) on the Superconducting CW Linac Design for CEBAF*, November 1985, p. A-2. Also: "Director's Meeting," September 17, 1985.

[36]The basic outlines of Hermann Grunder's argument in favor of SRF—that CERN's interest in the technology showed its feasibility, that Cornell had already developed a cavity design that could be employed, that industry was willing and able to produce cavities, and that CEBAF stood ready to host a thorough assessment of cost estimates and technological readiness—surfaced in Grunder's August 8 meeting with Wallenmeyer, Hendrie, and Ritter. The September review paper represents a polished, more completely articulated version of this argument. Hermann Grunder, "Meetings in Washington," August 8, 1985, Hartline papers.

been clear for some time: low cavity losses allow power savings of 75%, and CW operation with its low instantaneous beam current results in a ten-fold improvement of emittance and momentum spread. Furthermore, projecting the technology into the 5–10 year future, a three- to four-fold increase in energy should be possible for a small incremental cost." The paper next explained that the CEBAF staff was already "generating some cost estimates for a superconducting design" and "ascertaining industrial capability for fabrication." Although the paper contained no cost estimate, it later concluded that there was "a strong industrial base for superconducting RF technology in Europe, and growing capability in the United States," and discussed the capabilities of TRW, Interatom, and Dornier. Grunder also outlined the next steps for pursuing the SRF design: in addition to reviewing ongoing plans with the NAB, CEBAF planned to write a Science and Technology Assessment Report (STAR) to more fully evaluate SRF as a preliminary step to preparing a conceptual design report for an SRF accelerator.[37]

The bulk of the paper consisted of a summary of the "technological progress in RF superconductivity," which emphasized advances made in the two-year period since the Bromley panel, as well as an assessment of "implications for CEBAF" of this progress. The section began with the point that helped convince Leemann—that large laboratories, and in particular CERN, had already "invested significant effort and funds in developing RF superconductivity" for electron storage ring projects.[38]

The centerpiece of the article was a chart prepared by Piel, which squarely addressed Hendrie and Ritter's concerns about whether SRF was a mature technology that had been thoroughly tested. The chart (reproduced at the top of the next page) was introduced with the comment that "since the HEPL experience, it has been recognized that SC linac technology would have arrived when accelerating gradients of 5 MeV/m or higher can be achieved routinely." Piel's chart neatly provided an updated list of tests showing routine attainment of such gradients, including those at the Darmstadt/Wuppertal recyclotron as well as beam tests at CERN on LEP, at KEK on TRISTAN, at DESY on PETRA/HERA and at Cornell on CESR. Grunder then reported the conclusion Piel drew from this information: "It would be conservative to design a superconducting accelerator based on niobium cavities with a gradient of 5 MV/m and a Q of a few times 10^9."[39]

Anticipating another objection—that the gradient or Q of SRF cavities would degrade over time—the paper next emphasized an "impressive test," at Cornell, where a cavity was deliberately exposed to a degraded vacuum to simulate 20 years of operation. Even though the cavity was also accidentally exposed to titanium dust, washing and rinsing the cavity allowed it to regain its original Q and gradient. After a listing of reasons for improved performance, which

[37]Hermann Grunder, "The CW Linac: A Review Paper," September 1985.
[38]Hermann Grunder, "The CW Linac: A Review Paper," September 1985.
[39]Hermann Grunder, "The CW Linac: A Review Paper," September 1985.

LABORATORY	CERN			KEK	DESY	CORNELL	DARMSTADT/WUPPERTAL	
ACCELERATOR	LEP			TRISTAN	PETRA/HERA	CESR	130 MEV RECYCLOTRON	
MATERIAL	NB	NB	NB ON CU	NB	NB	NB	NB	NB_3SN
FREQUENCY IN MHZ	350	500	500	500	1000	1500	3000	3000
OPERATING TEMPERATURE	4.2 K	4.2 K	4.2 K	4.2 K	4.2 K	1.8 K	1.8 K	4.2 K
SINGLE CELL CAVITIES								
E_A (MV/M) **	10.8	13.0*	10.8	7.6*	5.5	22.8*	18.7*	7.2
Q AT E_A 1985	$1.8 \cdot 10^9$	$0.7 \cdot 10^9$	$0.4 \cdot 10^9$	$0.6 \cdot 10^9$	$5 \cdot 10^8$	$2.5 \cdot 10^9$	$3 \cdot 10^9$	$1.1 \cdot 10^9$
E_A (MV/M) **	4.7	7.6	-	6.5	5.5	8.5	10.0	-
Q AT E_A 1983	$5 \cdot 10^9$	$3.6 \cdot 10^9$	-	$4.1 \cdot 10^9$	$5 \cdot 10^8$	$7 \cdot 10^9$	$4 \cdot 10^9$	-
MULTICELL RESULTS	4-CELLS	5-CELLS	-	3-CELLS	9-CELLS	5-CELLS	5/20 CELLS	5-CELLS
E_A (MV/M) **	6.0*	5.0	-	5.8	5.5	15.3*	5.7/4.4	4
Q AT E_A 1985	$1.4 \cdot 10^9$	$0.7 \cdot 10^9$	-	$0.6 \cdot 10^9$	$0.5 \cdot 10^9$	$2.2 \cdot 10^9$	$4/3 \cdot 10^9$	$4.5 \cdot 10^9$
E_A (MV/M) **	-	2.8	-		2.5	5.4	3.7	-
Q AT E_A 1983	-	10^9	-		$5 \cdot 10^8$	$4.5 \cdot 10^9$	$1 \cdot 10^9$	-

*) CAVITIES FABRICATED FROM HIGH THERMAL CONDUCTIVITY NIOBIUM **) UNDER CONTINUOUS WAVE OPERATION

(from H. Piel, Wuppertal)

Piel's chart as it appeared in Grunder's paper.

emphasized the new information and capabilities that allowed researchers to avoid the higher-order modes that led to the regenerative beam breakup that plagued earlier efforts to implement SRF, Grunder listed the issues that CEBAF intended to address to assuage any lingering doubt about the feasibility of building an SRF accelerator: cavity tuning and RF coupling, RF frequency, beam breakup and the damping of higher-order modes, and cryogenics and refrigeration. Significantly, the Cornell experience was stressed in connection with two of these issues, RF frequency and the touchiest subject, higher-order modes. Grunder went so far as to note that a 1500 MHz frequency had been chosen in part because of "the Cornell group's experience and willingness to cooperate" in building a machine at CEBAF. Although the following general description of the proposed CEBAF machine—which employed five-cell niobium cavities to produce 4 GeV, 200 μA CW beams—did not mention the use of the Cornell cavity design, the emphasis on the Cornell achievements and experience was no accident: by this time he was already actively trying to recruit the group.[40]

Even before the end of the technology review, the decision had been made to exploit the work done at Cornell to the fullest: with the exception of a minor design change to the

[40]Quotes from Hermann Grunder, "The CW Linac: A Review Paper," September 1985; "Director's Meeting," August 20, 1985.

waveguide, the Cornell cavity could be used without modification at CEBAF. Todd Smith, who came to the review in part to give CEBAF researchers the benefit of HEPL's experience with SRF, later explained that "because of the political realities involved in even thinking about the switch" to the SRF design, "it was decided, and I concurred, that using a completely existing off-the shelf design piece ... which did not require one iota of development ... was the only way to fly."[41] CEBAF stood a better chance of converting SRF critics in the physics community and in Washington by sticking to a proven, tested cavity design and employing the specialists expert in implementing the problematic technology.

Momentum for the acceptance of SRF began to build with the NAB meeting. Chargin remembers that one key incident helped turn the tide. At the meeting, "representatives from a number of companies ... came to CEBAF to speak about industrial capability to produce SRF cavities. During one presentation, Hermann stood up and asked a question: 'Will you give us a fixed-price bid on this job?' None of us expected this question—it wasn't part of the plan, it came cold, and it was a gamble. I also felt sorry for the guy. What's he going to say? If he says no, he could kill his prospects. If he says yes, he's made a golden promise. After a pause that seemed like an eternity, but was probably only two seconds, he said yes." In Chargin's opinion, Grunder's successful gamble was crucial because the willingness to contract at a fixed price "signaled considerable industrial confidence—a company would normally want to specify that all costs would be covered plus a profit margin in case the task was problematic." At the meeting and afterwards, whenever someone would object that SRF was not mature enough to implement, the news of this willingness "stopped the argument."[42]

Although the NAB members held back unqualified endorsement of the plan to build an SRF accelerator, they did endorse continued pursuit of the SRF design, thanks to industrial readiness to produce SRF cavities and the results of the technology review. Moreover, they supported the step crucial to advancing the campaign for an SRF accelerator: further review. They accordingly suggested that the CEBAF staff compile further data on industrial capability, cost, schedules, and staffing so that plans for the design could be thoroughly assessed in time for DOE approval in 1986.[43]

With this good news in hand, the CEBAF staff worked frantically to prepare for Grunder's crucial meeting with Trivelpiece, which was scheduled just three days after the NAB met. Hartline, who spearheaded the preparation of briefing materials, remembers the excitement and pressure of this time. Due to scores of last-minute changes, on the evening before the meeting she and her support staff "hadn't managed to get the viewgraphs together the way Hermann needed them before he had to leave for his plane." As a result, she frantically

[41]Interview with Todd Smith, November 22, 1994.
[42]Quote from interview with Anthony Chargin, Thomas Moore, Gary Curnow, and Randy Pico, November 22, 1994.
[43]"CEBAF Briefing to the Director, Office of Energy Research," September 17, 1985, Hartline papers.

completed the task herself, using unfamiliar "computers because we didn't have any secretaries left at that hour." Hartline was not the only one who put in overtime. James Coleman recalls that the resulting "briefing paper" was not completed "until 8:30 or 9:00 P.M. that evening." This presented a problem because "the last flight had already left" Newport News for Washington. As a result, Coleman decided "to drive to Washington D.C. to deliver the papers." Although he figured he "could probably make the four-hour drive to Washington," his "fear was the four-hour drive back," so he took fellow administrator Paul Page to accompany him. Coleman then made the drive north, arriving in Washington shortly after 1:00 A.M. He then awakened Grunder, and the two carefully "went over the briefing and all the notes." Then Page safely drove back to Newport News.[44]

Despite the late-night conference with Coleman, Grunder remembers getting up at 7 A.M. When he met with Trivelpiece, Grunder "was polite" and "very apologetic" about the difficulties involved in adopting the SRF design. Nonetheless, the CEBAF director insisted that he "couldn't live with" the notion of returning to the linac/PSR design. He further argued that both CEBAF and DOE would look foolish if the machine were built with the old technology. Although Trivelpiece initially remained skeptical, he agreed to hear Grunder's case.[45] Using the hand-delivered briefing materials, Grunder presented the results of the technology review, summarized the characteristics and scientific potential of an SRF accelerator, and noted that the physics community supported the plan to switch designs and that the NAB was clearly positive about it. To ease concern that the abrupt switch would lead to ill-considered or poorly documented decisions, he emphasized that the CEBAF staff was taking great care to assess and document plans, including cost, for building the accelerator. The briefing ended with a summary of the current status of the capabilities of SRF—including a version of Piel's chart—along the lines of the September review paper.[46]

Grunder remembers that Trivelpiece first toyed with various alternatives for dealing with the situation, but eventually agreed that Grunder's plans had merit. As Grunder was leaving, Trivelpiece told him that he would make some calls and get back to him by 2 P.M. Grunder was "stunned" that the matter would be addressed so quickly. Indeed, by 2 P.M., Trivelpiece told Grunder: "Go ahead, make a proposal and we'll review it." By giving Grunder formal permission to proceed with the STAR evaluation of SRF, Trivelpiece demonstrated that he no longer opposed CEBAF's plans for an SRF accelerator. Hendrie later remembered that Grunder's meeting with Trivelpiece "turned the tide. Until then, Trivelpiece had been against the move. Hermann just managed to convince him." Although the successful bid for Trivelpiece's

[44]Quotes from Catherine Westfall and Curtis Brooks interview with Beverly Hartline, September 2, 1994, and Catherine Westfall and Curtis Brooks interview with James Coleman, September 29, 1993.
[45]Interview with Hermann Grunder, March 22, 1994.
[46]Hermann Grunder, "CEBAF Briefing to the Director, Office of Energy Research," September 17, 1985.

acceptance was a turning point in the campaign to switch designs, Grunder still had to make the case for an SRF accelerator, and he had to accomplish the task quickly. As Trivelpiece noted, due to the progress of other expensive projects, "if you don't make the [fiscal year] 1987 budget, your window [of opportunity] is closed."[47]

A New Conceptual Design

The already rapid pace at CEBAF accelerated after Grunder's meeting with Trivelpiece. By October 8 Grunder and L. Edward Temple, director of DOE's Construction, Environment and Safety Division, had ironed out the details of the DOE review process aimed at producing a new conceptual design. The three main events of this process were the STAR review, a second review following submission of the *Preconceptual Design Report* (PCDR review), and a third one following the *Conceptual Design Report* (CDR review).[48] As explained at the time, the STAR review on November 20–22 would "address the question of appropriateness and timeliness of superconducting RF cavity technology" for use at CEBAF. With this issue resolved, DOE representatives would return on December 17–20 for the PCDR review, which would be "both a review of construction readiness, cost and schedule, and validation of the CEBAF project." With these issues settled in principle, the laboratory would host the CDR review on February 11–14, 1986, so that DOE and CEBAF could reach a mutual agreement "on the project technical/cost/schedule baselines," and DOE could begin another Independent Cost Estimate (ICE) to assess the CEBAF budget for an SRF accelerator. To complement these DOE reviews, SURA planned an assessment of the project on December 2 by eminent physicists, including NSAC chairman John Schiffer, Bromley, and directors of other accelerator laboratories. Further evaluation of CEBAF plans would come on January 10–11, 1986, when the National Advisory Board would return for another visit to the laboratory.[49] These major reviews were interspersed with numerous other meetings: a list prepared in early 1986, for example, shows ten reviews in the five-month period between the NAB meeting in mid-September 1985 and the CDR review in February 1986.[50]

The workload was considerable. As Joseph Bisognano noted, since "it takes two weeks to prepare viewgraphs for a review," those involved "were constantly working on reviews." Anne Stewart, who worked as the administrative assistant for the Administration Division, remembers: "We worked every single weekend ... . And we had people at work until 1 o'clock in the morning, almost every night, including some with very large titles, who were doing very

[47]Quotes from conversation with David Hendrie, February 29, 1996, and interview with Hermann Grunder, March 22, 1994.
[48]L. Edward Temple to H. Grunder, email, October 8, 1985, Hartline papers.
[49]Quotes from David Hendrie and L. Edward Temple to Alvin Trivelpiece, November 26, 1985, "Hermann Grunder to All Staff," December 11, 1985, and L. Edward Temple to Grunder, January 24, 1986, Hartline papers.
[50]Hermann Grunder, "CEBAF Briefing for the Director, Office of Energy Research," January 9, 1986.

menial work, like xeroxing and stapling." Coleman added that spouses and children often pitched in, working for free.[51]

At the same time, during late 1985 and early 1986 the CEBAF staff had to perform a heavy load of other work so that reviewers could present a convincing case for CEBAF plans. As Hartline remembers, "in the midst of preparing for reviews, we were resolving technical issues," in particular regenerative beam breakup, and "developing the design." To demonstrate that CEBAF was poised to build a workable, cost-effective accelerator, staff members had to demonstrate firm engineering and administrative plans, including believable cost estimates. In Hartline's words, "the days were full of designing, calculating, describing." At the same time, CEBAF managers had to find the time to recruit new staff members to help build the accelerator.[52]

Creating a conceptual design for an SRF accelerator required calculations to guide the construction of beam transport and extraction systems. As this work proceeded, CEBAF staff members were particularly concerned about providing an airtight technical demonstration to convince SRF critics. In the words of accelerator theorist David Douglas, accomplishing this task hinged on their ability to address the problem of regenerative beam breakup from higher-order modes "no if ands or buts, because that's what clobbered HEPL." As a result, accelerator theorists focused considerable attention on showing that such modes could be anticipated so that the CEBAF accelerator would have a stable beam.[53]

Due to the intrinsic scientific interest of beam breakup, Joseph Bisognano began work on the subject in mid-summer 1985, even before the decision was made to pursue plans for an SRF accelerator. As he recently explained, he took "two basic approaches" to finding the threshold for beam breakup. For the first his point of departure was a computer simulation of cumulative breakup developed by Richard York which was, in turn, based on a computer code written by Richard Helms as part of extensive computer modeling of single-pass cumulative beam breakup of the SLAC machine. Bisognano intended to "modify the codes ... to start modeling multipass beam breakup." The required bunch-by-bunch simulation required discovering "what were the right questions to ask of the computer code," which was challenging since he "didn't really know what [beam breakup] would look like." The other approach, pursued simultaneously, was "to produce an analytical matrix model with a computer calculation of resulting expressions for stability thresholds to both test the code and to ... strengthen ... understanding" of beam breakup. Bisognano's strategy was to use the analytical model to benchmark the computer simulation,

[51]Quotes from Catherine Westfall and Curtis Brooks with Joseph Bisognano, December 7, 1993, interview with Anne Stewart, September 29, 1993, and Catherine Westfall and Curtis Brooks interview with James Coleman, July 29, 1993.
[52]Catherine Westfall and Curtis Brooks interview with Beverly Hartline, September 2, 1994.
[53]Quotes from Catherine Westfall and Curtis Brooks interview with David Douglas, September 28, 1993. Also: "CEBAF Narrative Progress Report," November 1985.

which he could then extend with some confidence to more general circumstances to obtain a picture of beam breakup that was as thorough as possible. His results would be further checked by comparison with findings at other laboratories, including HEPL.[54]

Bisognano showed his crude results to Arthur Vetter and Roy Rand, who had firsthand experience with multipass beam breakup at HEPL; Bisognano was happy to learn that he was on the right track. For help with the analytic model he began a collaboration with Robert Gluckstern. By this time beam breakup analysis was a crucial part of the laboratory-wide effort to demonstrate the feasibility of an SRF machine. Driven by the need to produce results for reviews, "in a period of a couple of months, all of a sudden," Bisognano and Gluckstern had results. As announced in the STAR report, they had produced "a computer model of multipass regenerative beam breakup that includes mode frequency spread and lattice variation ... . For a single, localized mode, this simulation [was] found to be in agreement with analytic estimates which have been successful in describing beam breakup in existing recirculating linacs." Crucial to calming concerns about the SRF machine was the finding that the 10 mA "threshold current for beam breakup" in the CEBAF SRF design was "more than an order of magnitude" higher than the 200 μA current planned for the machine.[55] In the next year and a half, Bisognano and other CEBAF physicists, such as Geoffrey Krafft, created better, more general simulations using a high-speed CRAY code that was much more powerful than the original VAX code. At the same time the analytical model was also improved so that it provided a more refined, general theory of multipass bream breakup. However, the early, cruder work was what made the difference in convincing reviewers that an SRF accelerator would work. In Bisognano's words, the result from the dual-pronged beam breakup analysis performed in a few months in the fall of 1985 provided a "very tight case" that beam breakup would be avoided and that the machine would have a stable beam.[56]

Another highly sensitive issue, alongside beam stability, was the cost estimate for the project. Records show that budget concerns dominated discussions with DOE representatives, who repeatedly urged that the costs be kept to a minimum and that estimates be reliable. While preparing for the reviews in fall 1985, Grunder carefully negotiated with DOE representatives

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[54]Quotes from Catherine Westfall and Curtis Brooks interview with Joseph Bisognano, December 7, 1993. Also: interview with Joseph Bisognano, September 11, 1995.

[55]Quotes from Catherine Westfall and Curtis Brooks interview with Joseph Bisognano, December 7, 1993, and CEBAF, *Scientific and Technological Assessment Report (STAR) on the Superconducting CW Linac Design for CEBAF*, November 1985, pp. 3-48–3-49. For an early account of the work on beam breakup, see Joseph Bisognano and Geoffrey Krafft, "Multipass Beam Breakup in the CEBAF Superconducting Linac" and R. L. Gluckstern, "Beam Breakup in a Multi-Section Recirculating Linac," in *1986 Linear Accelerator Conference Proceedings*, SLAC-303, September 1986, pp. 452–454 and 543–545, respectively. For a report on later results, see Joseph Bisognano, "Multipass Beam Breakup in Recirculating Linacs," and G. A. Krafft and J. Bisognano, "Two Dimensional Simulations of Multipass Beam Breakup," in *Proceedings of the 1987 IEEE Particle Accelerator Conference*, March 16–19, 1987, Vol. 2, pp. 1078–1079 and 1356–1358, respectively.

[56]Interview with Joseph Bisognano, September 11, 1995.

about budget ceilings, keeping overall costs within the $236 million originally requested for the linac/PSR machine. In addition, he sought cost information from vendors bidding on accelerator components and from Daniel, Mann, Johnson, and Mendenhall (DMJM), the architect-engineering firm in charge of civil construction.[57]

A primary responsibility for Chargin, after he became project manager in August, was to fulfill the obligation noted by Leemann the previous July—assembling cost information as part of an overall project plan. Drawing on his experience at Lawrence Livermore Laboratory with large projects, by late September Chargin had directed his staff to develop a "construction-oriented Work Breakdown Structure," or WBS, which organized work according to components, including both civil and accelerator construction, that needed to be assembled to build the laboratory. As CEBAF reported to DOE, on the basis of the WBS, staff members had also "implemented a project management system suitable for pre-construction planning and scheduling." CEBAF also explained to DOE that month that on the basis of the WBS, staff members had begun "assembling cost information on components ... into computerized databases." Chargin credits staff member Thomas Moore for coming up with a clever way to present the material so that the WBS for the standing wave option, which was pursued into fall 1985, matched that for the SRF option. "So if you only looked at the first page, you couldn't even tell it was a different machine ... . We did this to show that we could make the transition from one machine to the next seamless, at least from a planning point of view." Thanks to the work of Moore and others, Chargin was able to demonstrate at the reviews that CEBAF had, in his words, "the organizational principles to handle cost and accounting in a rational manner."[58]

In addition to demonstrating beam stability and providing solid cost estimates, the CEBAF staff had another crucial job: showing that an SRF accelerator could actually be built. In the fall of 1986, work proceeded quickly on the routine, but important, task of revising the conceptual designs of magnets, power supplies, klystrons, cryogenic components, and computers. Since industry had proven its capability to produce such components, this work prompted little concern. Demonstrating that SRF cavities could be produced was a more sensitive issue. Although CEBAF planned to use the Cornell cavities, the next step was to have such cavities produced by industry. Less than a week after Grunder's mid-September meeting with Trivelpiece, CEBAF told DOE that Interatom was already fabricating a cavity based on the Cornell specifications using funds from Virginia. In early October Grunder reported to SURA that "plans were underway to order four or five" such cavities "from several companies," with a

[57]"Hermann Grunder to Associate Directors," October 5, 1985; Hartline, "Notes on Budget Meeting with DOE," October 22, 1985, Hartline papers; "Director's Meeting," October 15, 1985; Hermann Grunder, "Meeting with HG, Chris Forsberg, Enloe Ritter, Bob Willard, IF, AC, JC, and CWL," November 7, 1985, Hartline papers.
[58]Quotes from "CEBAF Monthly Narrative Report," September 30, 1985, and interview with Anthony Chargin, Thomas Moore, Gary Curnow, and Randy Pico, November 22, 1994.

delivery date of March 1, 1986. Sundelin and his group stood ready to advise and help industrial producers.[59]

As technical and engineering work proceeded, Coleman labored to establish CEBAF's administrative framework. As a first step, in late 1985 he worked with DOE representatives to prepare and implement the full range of policies and procedures. In Coleman's opinion, as a result of CEBAF's consensus-style management, this administrative work was closely integrated, not only with DOE, but also with the rest of the work at the laboratory. "Any policy or procedure ... first had to get reviewed and approved by all the AD's and the director. If we didn't have a good idea, or if we couldn't sell it, it didn't happen." As a result, the Administration Division, in Coleman's opinion, "had to be user-friendly." At the same time, other managers were familiar with administrative as well as technical problems and decisions, which facilitated the necessary collaboration to simultaneously address administrative, engineering, and scientific issues.[60]

The importance of integrated technical and nontechnical decision-making was apparent in the completion of one of Coleman's early crucial tasks: developing the system for procuring cavity materials for industry. The job was a collaborative effort. CEBAF scientists and engineers specified the components in the conceptual design and the WBS, while DOE representatives related agency guidelines and regulations. Based on this information, the newly assembled procurement staff then had to actually create a workable procurement system. Mark Waite, who joined CEBAF as a senior procurement officer in September, remembers that an early challenge in implementing this task was "trying to decide how to find a way to select a cavity vendor. We were concerned not just with cost—we needed quality, too. We thought of various strategies, and discussed the issue at length with DOE." In the end, a plan emerged that demonstrated both technical and business savvy. To provide an objective guide for judging cost and reliability, technical experts, such as Sundelin, worked out a set of criteria for weighing a wide array of variables. In the meantime, the decision was made to actively encourage competition among bidders, both for cost and quality, to optimize the chance that a suitable cavity would be produced by the deadline.[61]

Employees at all levels in the organization remember this period as a singular, exciting time in their professional lives. Estelle Seeley, who worked in shipping and receiving, had been

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[59]Quote from "Minutes of the SURA Executive Committee Meeting," October 6, 1985. Also: Committee on Science and Technology, U.S. House of Representatives, *Fiscal Year 1987 Department of Energy Authorization*, 99th Congress, 2nd Session (Washington, D.C.: GPO, 1986), p. 183; "Minutes DOE/CEBAF Monthly Meeting," September 24, 1985; interview with Ronald Sundelin, March 24, 1994.

[60]Quotes from Catherine Westfall and Curtis Brooks interview with James Coleman, September 29, 1993. Also: "DOE/CEBAF Monthly Meeting," September 1985; "CEBAF Monthly Narrative Report," October 31, 1985; "CEBAF Narrative Progress Report," November, 1985; "CEBAF Monthly Narrative Report," December 1985.

[61]Quote from interview with Mark Waite, Estelle Seeley, and Teresa Danforth, March 6, 1995. Also: interview with Anthony Chargin, Thomas Moore, Gary Curnow, and Randy Pico, November 22, 1994; "CEBAF Monthly Narrative Report," September 30, 1985.

one of the original 17 state employees. "We watched new people come on board, one at a time, and then things changed. All of a sudden we were receiving construction material in the back door and getting it shipped out to get the cavities made. Everybody was concerned about whether the laboratory would fly. That was always in the back of your mind. But it was exciting to work in a world-class laboratory, to see ideas become a reality." Seeley and Teresa Danforth, who was the administrative assistant to Chargin, both remember that there was "a family feeling: we were all in this situation together." This feeling was reinforced by family parties hosted by Grunder and by the fact that family members sometimes lent a hand in preparing review materials. Waite remembers that it certainly was a different working environment from that of his previous employer, Lawrence Livermore Laboratory. He had less job security, and yet he and others had the authority "to do whatever it takes to get the job done." Similarly, Douglas remembers that when doing his accelerator physics calculations, he felt the "freedom to be creative, and people really listened. A fairly young technical guy could feel he was making a difference." Danforth summarizes that the atmosphere was "exhilarating and challenging." At least some CEBAF staff members were, like Grunder, stimulated by the challenge of uncertain prospects. As Coleman noted, "the bold step of demanding the switch to SRF energized the place. All our plans could go down in flames, but I kept thinking: how often in life do you have the opportunity to get that kind of excitement?"[62]

In the midst of the excitement, CEBAF managers continued the task of recruitment—the CEBAF staff would more than double from 1985 to 1986. Leemann remembers that the money from Virginia gave Grunder a significant recruitment advantage because it allowed him to offer top scientific and management staff members a "parachute" provision which would provide a tenured professorship at a state university if the project failed. In fall 1985, Grunder and his staff were particularly concerned about obtaining key specialists. In early October 1985, the AD's discussed the necessity of obtaining more "cryogenics expertise," a consideration that ultimately led to the recruitment of Claus Rode from Fermilab. Shortly afterward, Grunder successfully completed a particularly crucial negotiation—the recruitment of the members of Sundelin's group, who were already involved in the critical task of transferring SRF cavity technology to industry. As he told the AD's in late December, the group had signed a letter of understanding, formally agreeing to collaborate with CEBAF starting December 1, 1985. Sundelin remembers that by this time he was "logging in a lot of frequent flyer miles ... spending Thursday and

[62]Quotes from interview with Mark Waite, Estelle Seeley, and Teresa Danforth, March 6, 1995, Catherine Westfall and Curtis Brooks interview with David Douglas, September 28, 1993, and Catherine Westfall and Curtis Brooks interview with James Coleman, July 29, 1993.

Friday" in Newport News and "Monday through Wednesday" at home in Ithaca so that he could participate in reviews at CEBAF and still keep in touch with SRF cavity work at Cornell.[63]

The Response

When DOE gave its official response to the STAR review on November 26, four days after the meeting, Sundelin's contributions were not ignored. The committee, led by Hendrie and Temple, first noted the "impressive array of presentations" given by staff members and other collaborators, which included talks on higher-order mode suppression, news of industrial cavity development, and Chargin's cost estimates based on the WBS. Then it expressed the judgment that: "Especially important and impressive was the Cornell contingent led by Professor R. Sundelin." The format of the review also pleased them: "The preliminary agenda was revised throughout the review in order to focus as completely as possible on the critical issues identified by the committee and consultants in executive sessions." The strategy of fully addressing committee concerns proved effective. "The committee and consultants were unanimous in agreeing that superconducting RF cavities are the appropriate accelerating elements for the CEBAF project." As a result, they suggested a fiscal year 1987 construction start for such a machine. In an echo of the concern of Grunder and his AD's, the committee also stressed the importance of further recruitment of SRF and cryogenic expertise.[64]

After a few more weeks DOE representatives, including Hendrie and Temple, the chairman of the review, returned to CEBAF for the PCDR review. As Hartline noted in a memo to the AD's, this review "was Tony's show," focusing on how the newly defined conceptual design would actually be built, based on Chargin's cost estimates, schedules, and project management plans.[65] The design called for a linked pair of linear accelerators using SRF cavities, through which an electron beam would be cycled four times to produce a 4 GeV, 200 μA beam. Again, reviewers responded well to CEBAF plans: the committee found that "the CEBAF superconducting RF linac design [met] the high-current, high-energy, high-duty-factor accelerator performance goals of the nuclear physics community better than the earlier pulsed linac with a pulse stretcher ring." In addition, they endorsed the budget and schedule and noted

[63]Quotes from Hermann Grunder to AD's, "Summary of the Situation," October 5, 1985, Hartline papers, and interview with Ronald Sundelin, March 24, 1994. Also: "CEBAF: Year End Schedules," September 1992, p. 14; "Director's Meeting," October 22, 1985.

[64]Reviewers for STAR included, in addition to Hendrie and Temple: L. Bollinger, R. Louttit, S. Penner, S. Schriber, P. Vander Arend, D. R. Lehman, E. T. Ritter, R. Willard, J. R. Erskine, and W. T. Oosterhuis. Quotes from David Hendrie and L. Edward Temple to Alvin Trivelpiece, November 26, 1985. "STAR Review Participants," November 20–22, 1985. Text quotes from David Hendrie and L. Edward Temple to Alvin Trivelpiece, November 26, 1985. Also: "Revised Agenda, CEBAF Scientific and Technological Assessment (STAR) Review," Hartline papers.

[65]Quote from "Bev to Director's Council," December 4, 1985. Also: "Agenda, PCDR," December 17–19, 1985, Hartline papers. PCDR reviewers included, in addition to Temple: J. Everhart, D. Lehman, W. Oosterhuis, D. Hendrie, J. Erskine, E. Ritter, C. Williamson, K. Forsberg, R. Conaway, S. Sims, R. Willard, P. Mantsch, W. Nestander, P. Reardon, R. Siemann, B. Strauss, P. Vander Arend, and R. Wendt.

that "the management of the project is excellent." The strongly positive DOE review of the PCDR signaled DOE support for CEBAF plans for an SRF accelerator. DOE representatives would return in February 1986 after submission of the CDR, but that review would simply confirm the plan outlined in the PCDR so that CEBAF would have what every DOE project needed on the eve of construction—a formally approved conceptual design. Grunder and his team had overcome the obstacle of DOE skepticism.[66]

As CEBAF staff members gained DOE support, they also received the necessary stamp of approval demonstrating that they had overcome skepticism within the scientific community. While expressing concerns about possible "hidden costs" associated with the SRF accelerator and stressing the need for early scientific planning, the December 1985 SURA review committee led by Schiffer agreed that the superconducting design "appear[ed] to be a major improvement over the original design" and praised Grunder for "the quality and vitality" of his "leadership."[67]

Grunder was able to announce considerable progress to Congress in yearly funding authorization hearings in March. The negotiations with industry had been successful and Grunder's deadline for a workable cavity had been met. As he reported: four vendors had been contracted to fabricate seven prototypes and "two cavities from two vendors [had] been tested, and both exceed our specifications." The CEBAF staff also had a gratifying recruitment record, having hired "76 employees, including 47 professionals, who bring the appropriate mix of talents, experience, and knowledge to launch the project." Thanks to recruitment and planning efforts, "the staff and management structure to begin construction [was] in place." Harkening back to Holmgren's testimony the year before, Grunder then stressed CEBAF's biggest achievement—the new conceptual design. If the design presented in March 1985 was thoroughly studied and reviewed, the March 1986 design was: "developed, optimized, documented, and thoroughly reviewed." In conclusion, Grunder noted that the project was ready for construction funding.[68]

Grunder and his staff had won the battle against skepticism: they were now poised to build the accelerator they wanted. At the same time, the prospects for the future were far from certain. If Congress refused to grant construction funding later in 1986, the project would miss the window of opportunity noted by Trivelpiece. And even if funding was obtained, Grunder and his staff faced several challenging tasks: not only did they have to obtain suitable, cost-effective SRF cavities from industry, but they had to build and commission the promised SRF accelerator. Much had been done, but much remained to be accomplished.

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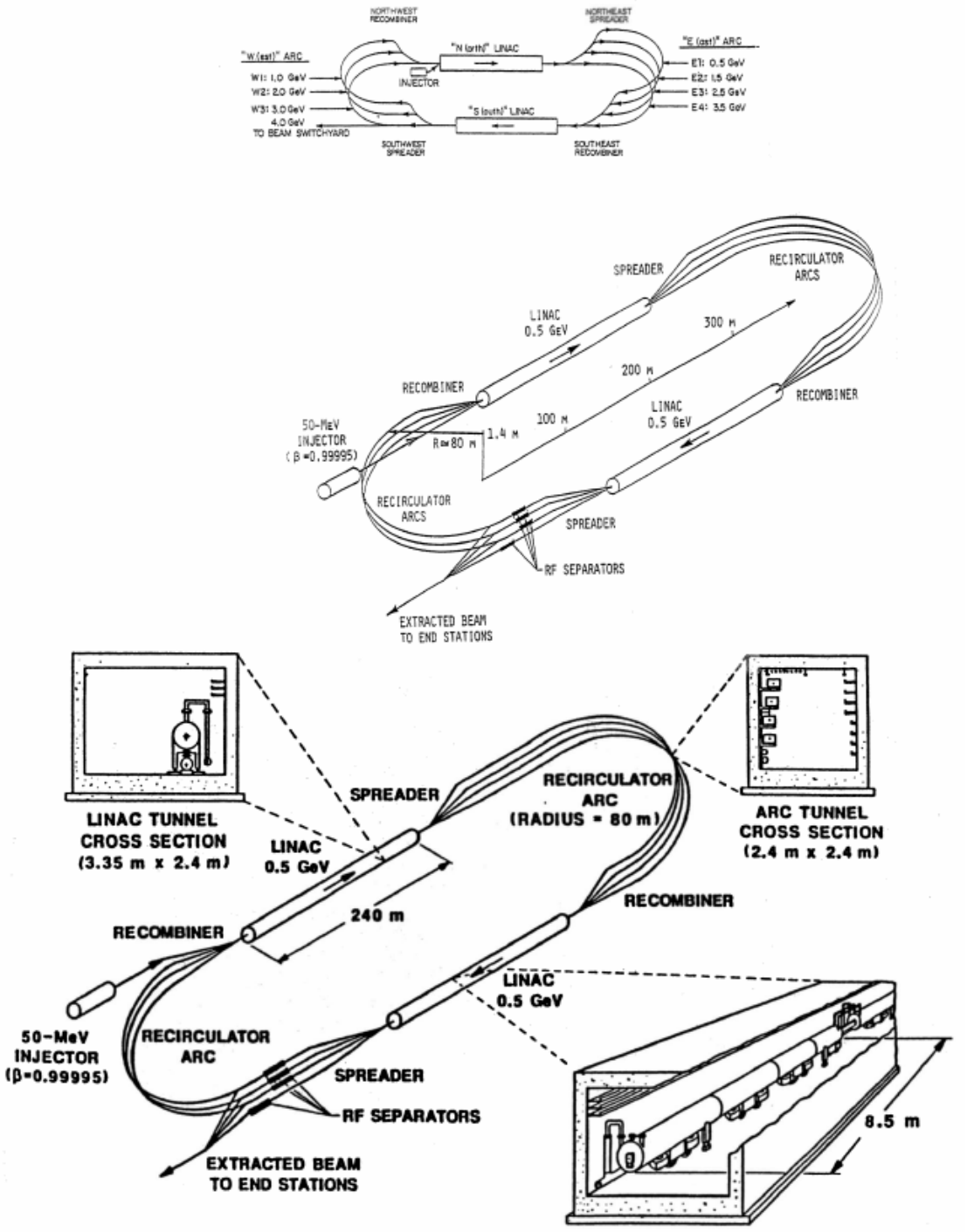
[66]Hermann Grunder to Beverly Hartline, "DOE Comments on PCDR Review," December 30, 1985.
[67]Quotations from "December 1985 Report, SURA Review Committee," Hartline papers, which lists D. A. Bromley, J. C. Browne, R. E. Hughes, E. A. Knapp, N. F. Ramsey, B. Richter, D. A. Shirley, and J. P. Schiffer as committee members present, with L. M. Lederman absent.
[68]Committee on Science and Technology, U.S. House of Representatives, *Fiscal Year 1987 Department of Energy Authorization*, 99th Congress, 2nd Session (Washington, D.C.: GPO, 1986) p. 195.



The CEBAF accelerator layout as depicted in late 1985 and early 1986. Top: from the November 1985 *Scientific and Technological Assessment Report (STAR) on the Superconducting CW Linac Design for CEBAF*, which showed both clockwise and counterclockwise beam paths in its illustrations. Middle: from CEBAF's December 1985 *Preconceptual Design Report*. Bottom: from CEBAF's February 1986 *Conceptual Design Report*. The accelerator layout schematic used at Jefferson Lab during the 1990s appears on the following page.

MACHINE CONFIGURATION

CEBAF