April 1997

The Prehistory of Jefferson Lab's SRF Accelerating Cavities, 1962 to 1985

Catherine Westfall

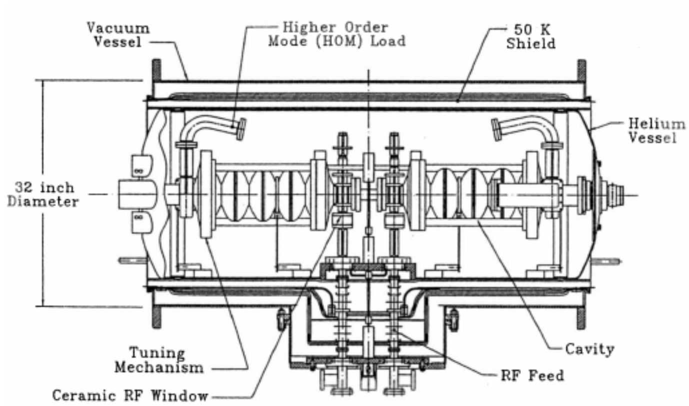
Michigan State University

and

Thomas Jefferson National Accelerator Facility

12000 Jefferson Avenue

Newport News, VA 23606



On the cover: A CEBAF-Cornell cavity pair illustration from H. Padamsee, K. W. Shepard, and R. Sundelin, "Physics and Accelerator Applications of RF Superconductivity," in *Annual Review of Nuclear and Particle Science* **43** (1993) 635-686.

The Prehistory of Jefferson Lab's SRF Accelerating Cavities, 1962 to 1985[1]

Catherine Westfall

Introduction

In April 1983, an advisory panel chaired by D. Allan Bromley of Yale University met to preside over a "shoot-out" competition among five organizations' proposals for high-duty-cycle electronuclear accelerators. The winning design was a multi-GeV linear accelerator (linac) with a pulse stretcher ring (PSR) proposed by the recently formed Southeastern Universities Research Association (SURA). One of the selling points of the SURA design was that it was based on well-known, reliable technology. In mid-1985 in Newport News, Virginia, detailed planning began for building SURA's Continuous Electron Beam Accelerator Facility (CEBAF, renamed in 1996 the Thomas Jefferson National Accelerator Facility). By early 1986, however, the decision had been made to abandon the linac-PSR design in favor of the one that was ultimately built: a recirculating machine with two superconducting radio-frequency (SRF) linacs. SRF technology had been dismissed as unworkable by all five of the Bromley panel competitors, despite earlier high hopes for its promise. Why did hopes for an SRF electronuclear accelerator rise in the 1960s, fall in the 1970s, then revive dramatically in 1985? To answer these questions, this paper focuses first on the struggles, the triumphs, and the failures experienced by Stanford University's High Energy Physics Laboratory, HEPL, where the intriguing but quirky technology was pioneered from the early 1960s through the early 1970s. After discussing how HEPL's problems diverted plans for an SRF electronuclear accelerator, the paper describes the development of workable cavities at Cornell from 1969 to 1985 against the backdrop of the painstakingly slow but ultimately successful international SRF research and development effort.

[1]This paper does not attempt to give a full account of the complex and interesting story of the development of SRF. Instead, as the title indicates, it aims simply to provide enough information on the development of SRF to place into perspective the subsequent implementation of the technology at CEBAF/Jefferson Lab. (See Catherine Westfall, "The Founding of CEBAF, 1979 to 1987," CEBAF/Jefferson Lab, 1994, and Westfall, "Jefferson Lab's 1985 Switch to Superconducting Technology," Jefferson Lab, 1996.) The author is grateful for the abundance of material and corrections provided by Alan Schwettman. The author would also like to thank Ronald Sundelin and Larry Cardman for generously providing information and advice from the outset. Thanks also go to Peter Kneisel, Yuzo Kojima, Herbert Lengeler, Gregory Loew, Wolfgang Panofsky, Wolfgang Weingarten, and Perry Wilson for

The Promise and Frustration of HEPL Work Before 1980[2]

The Early Experiments (to 1966)

The roots of SRF research reach back to 1934, when H. London pointed out that a two-fluid model of superconductivity predicts that contrary to the case for dc currents, resistive losses should be observed in superconductors at high frequencies. The argument was simple and compelling. The RF magnetic field that penetrates the surface of a superconductor produces, through Maxwell's equations, an RF electric field. In a two-fluid model this electric field drives the normal conducting electrons and thus produces resistive losses. The first SRF experiment was performed by London himself in 1940, followed by work at a number of laboratories, principally in England and the United States. A powerful new tool for understanding superconducting phenomena, the J. Bardeen, L. Cooper, J. R. Schrieffer (BCS) theory, emerged in 1957. This theory was applied to the RF properties of superconductors by D. C. Matttis and Bardeen, who provided detailed expressions for surface resistance and surface reactance.[3]

In the 1960s, the early SRF work gave rise to schemes for using SRF cavities to upgrade accelerators such as electron, proton, and ion linacs and microtrons (a type of recirculating accelerator).[4] The possibility of building an SRF electron linac at Stanford was the brainchild of physics professor William Fairbank, who had previously studied the superconducting properties of tin in his thesis research at Yale. Fairbank, whose enthusiasm would fuel the project's initial progress, received strong encouragement and help from Wolfgang "Pief" Panofsky, then director of HEPL, who had a keen interest in accelerator technology. Before coming to Stanford in 1951, Panofsky had worked at the University of California, Berkeley with two of the founding fathers of American accelerator science, Ernest Lawrence and Luis Alvarez. In 1959, Panofsky recruited a young Ph.D., Perry Wilson, to oversee operation and development of the HEPL Mark III

---

comments and to Curtis Brooks for providing references and other research assistance. The author conducted all interviews and received all private communications, unless otherwise specified.

[2]While information for this section was being compiled and the section itself was being written, Alan Schwettman made numerous contributions and suggested several text revisions through private communications. All quotations are from these private communications, unless otherwise noted.

[3]H. London, *Proceedings of the Royal Society,* A176 (1976), p. 522; J. Bardeen, L. Cooper, J. R. Schrieffer, *Phys. Rev.* **108** (1957), p. 1175; D. C. Mattis and J. Bardeen, "Theory of the Anomalous Skin Effect in Normal and Superconducting Metals," *Phys. Rev.* **111** (1958), p. 412.

[4]For one early example of such schemes, see A. P. Banford and G. H. Stafford, "The Feasibility of a Superconducting Proton Linear Accelerator," *Journal of Nuclear Energy Part C* **3** (1961), pp. 287–290.

accelerator, and in particular to install a new 80-foot extension so that the machine could reach 1 GeV.[5]

Wilson was particularly well suited for the job: after learning microwave technology as an electronics technician in the Navy, he earned his degree working with Edward Ginzton at Stanford's Microwave Laboratory. In the words of Wilson, "at the time of my return to Stanford in 1959, when Pief spelled out my job responsibilities, he also said I could spend some fraction of my time doing accelerator-related research ... . One area that might interest me, he said, would be to look into the possibility of superconducting accelerating structures in collaboration with Bill Fairbank." Due to their small losses, such structures would be ideal for accelerating the continuous wave electron beams needed for coincidence experiments, the desired next step in electronuclear research.[6]

By early 1961 Panofsky and Fairbank had set up the framework for the project and "the fun began." John Pierce, a beginning graduate student, was responsible for the cryogenic design and for electroplating the cavity, and Wilson was responsible for the RF design of the microwave cavity and for setting up the measurements system. Wilson, Pierce, and Fairbank together performed the early experiments. At this point Wilson made "one of the best technical decisions" of his life—choosing to make measurements with the TE_{011} cavity mode.[7]

A cavity working in this mode was large at the chosen operating frequency, 2856 MHz, compared to the resonator configurations previously used for SRF tests.[8] The frequency was chosen since it was the operating frequency of the HEPL Mark III and thus researchers could

---

[5]P. B. Wilson, "The Early Years of Superconductivity at HEPL: Personal Recollections," August 1995. For more information on plans for accelerators at Stanford, see Peter Galison, Bruce Hevly, and Rebecca Lowen, "Controlling the Monster: Stanford and the Growth of Physics Research, 1935–1962," in P. Galison and B. Hevly, eds., *Big Science: The Growth of Large-Scale Research* (Stanford: Stanford University Press, 1992), pp. 46–77, and Stuart Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993), pp. 160–187. For more information on accelerator building at Lawrence's Berkeley Laboratory and Panofsky's efforts there, see John Heilbron and Robert Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory* (Berkeley: University of California Press, 1989).

[6]P. B. Wilson, "The Early Years of Superconductivity at HEPL: Personal Recollections," August 1995.

[7]P. B. Wilson, "The Early Years of Superconductivity at HEPL: Personal Recollections," August 1995.

[8]Many early measurements from 1947 to 1952 had been made on so-called hairpin resonators. Such resonators are convenient for testing source superconducting materials, since researchers only needed a small sample in the form of a slender rod bent into a hairpin shape a quarter of a wavelength long. Fairbank and others had used TM_{011} cavities, but they are difficult to assemble without introducing additional loss at the joints. As in the case of a hairpin resonator, they also have both electric and magnetic RF fields at the cavity surface. See A. B. Pippard, "Metallic Conduction at High Frequencies and Low Temperatures" in *Advances in Electronics and Electron Physics*, Vol. 6 (1954), pp. 1–44.

make use of readily available RF instrumentation. Despite this advantage, the 2856 MHz TE_{011} mode cavity was greeted with surprise. "Fairbank seemed astonished that I wanted to use such brutish components, but I thought the advantages outweighed the disadvantages," Wilson explains. Disadvantages included the necessity of constructing a cryostat and an electroplating facility capable of handling such large cavity components—tasks that fell to Pierce. In addition to the benefit of available RF instrumentation, advantages included the fact that in this mode, the electric field at the cavity wall is zero, and the wall currents are azimuthal, thus permitting assembly of the cavity as a cylinder plus two end plates. The zero electric field made it possible to study the wall current loss, which is a specific superconducting state issue, without the complication of dielectric loss or electron loading phenomena. Furthermore, the possibility of assembling the cavity as a cylinder plus two separate end plates simplified surface preparations.[9] In their early experiments, the team introduced another innovation suggested by Wilson: a novel use of the decrement method to determine cavity performance.[10] As would often prove to be the case with SRF technology, meticulous work, innovation, and a little luck led to promising results. Using a lead-plated cavity at 1.8 K in June 1962, the team obtained a *Q* of 4×10^8, a value that was greater by an order of magnitude than any previously obtained in a full microwave cavity.[11]

Momentum began to build, fed not only by initial success, but also by the realization that much more needed to be done. Researchers knew that building an SRF accelerator required achieving *Q*-values of a few times 10^9, an order of magnitude higher than obtained in the first experiment. Even more critical was the need to obtain RF magnetic field levels of a few hundred oersted (Oe) at the cavity surface without significant *Q*-reduction. To help reach these

---

[9]Quotations from P. B. Wilson, "The Early Years of Superconductivity at HEPL: Personal Recollections," August 1995.

[10]A later description of this method explains: "The microwave power incident on the cavity is modulated in rectangular pulses; the width and spacing of the pulses are many cavity filling times so that steady state conditions are realized during" both pulse-on and pulse-off conditions. "The power reflected from the cavity is detected and presented on an oscilloscope. The loaded *Q* is determined by the decay constant of the power radiated from the cavity after the incident power has been cut off. The coupling coefficient ß is determined by the ratio of the initial radiated power to the incident power, which is equal to $[2ß/(1 + ß]^2$. From the expression $Q_0 = (1 + ß)\ Q_L$, we can then calculate the unloaded *Q*." H. A. Schwettman, P. B. Wilson, J. M. Pierce, and W. M. Fairbank *The Application of Superconductivity in Cryogenic Engineering* (New York: Plenum Press, 1965).

[11]P. B. Wilson, "The Early Years of Superconductivity at HEPL: Personal Recollections," August 1995; W. M. Fairbank, J. M. Pierce and P. B. Wilson, "High Power Superconducting Cavities for Accelerators," in *Proceedings of the Eighth International Conference on Low Temperature Physics* (Washington D.C.: Butterworth, 1963), pp. 324–325, and P. B. Wilson, "Investigations of the *Q* of a Superconducting Microwave Cavity," *Nucl. Instrum Methods* **20** (1963), pp. 336–340.

goals the SRF group recruited its first full-time participant, Alan Schwettman, who came to Stanford in September 1962 as a post-doctoral fellow to work with Fairbank.

One of Schwettman's tasks was to develop improvements in techniques for electroplating, rinsing, and drying the cavity—the sort of nitty-gritty details that make all the difference in SRF technology. In addition, he designed a new cryogenic system in which the cavity could be immersed in liquid helium to ensure good thermal cooling. In the course of devising the immersion cooling scheme, which was essential for achieving high RF magnetic fields, Schwettman developed another practical necessity: the indium seal. While this work proceeded, Wilson constructed a stable RF source which could deliver 1 kW of power. In 1964, with the complete immersion cryostat, the stable high-power RF source, and improved electroplating techniques, the group was able to achieve high *Q*-values at high magnetic fields. At the lowest temperatures they obtained a *Q*-value approaching 7×10^9, while at a temperature of 2 K they were able to maintain a *Q*-value of 2×10^9 at 200 Oe.[12] This was great news: a magnetic field level of 200 Oe is sufficient to support an accelerating gradient approaching 5 MV/m. Thus, at least for this gradient, acceleration of electrons in a superconducting electron linac appeared feasible.

Spurred by this result, the Stanford team pursued two mutually reinforcing projects through the mid-1960s: the exploration of SRF, a task that required advancing the fundamental understanding of the phenomenon as it related to accelerator building, and the development of a workable accelerating structure, a job that required very practical, hands-on problem-solving. Schwettman, by then a Stanford assistant professor, recognized that the very high *Q*-values achieved provided an opportunity to make a detailed comparison with the Mattis-Bardeen theory. With graduate student John Turneaure he made careful measurements of the surface resistance of superconducting tin and lead that revealed, for the first time, the exponential temperature dependence that follows from the BCS energy gap. At Schwettman's suggestion, Turneaure began detailed numerical calculations of the surface resistance based on the Mattis-Bardeen theory. The correspondence between theory and experiment demonstrated in these studies provided the basis for all future SRF work.[13]

[12]H. A. Schwettman, P. B. Wilson, J. M. Pierce, and W. M. Fairbank, *The Application of Superconductivity to Electron Linear Accelerators, International Advances in Cryogenic Engineering* (New York: Plenum Press, 1965), p. 88.

[13]J. P. Turneaure, Ph.D. Thesis, Stanford University, 1967.

Schwettman and Turneaure addressed another fundamental question of great relevance to building an SRF accelerator: will the superconducting state support RF magnetic fields approaching the thermodynamic critical field? This question was more easily answered for a superconducting tin surface, where the critical field is 305 Oe, and thus they made measurements on a superconducting lead-plated cavity in which one end plate was electroplated with tin. The temperature-dependent maximum RF magnetic field at the tin surface indeed followed the dc critical magnetic field.[14] Similar behavior for a superconducting lead surface where the dc critical field is 803 Oe would imply that a voltage gradient of 14 MV/m might be possible, at least in principle.

In parallel with these fundamental studies, Wilson embarked on the design of a workable accelerator structure and began assembly of a rudimentary 80 keV electron injector and an electron beam analysis system for the accelerator test. At the same time, Schwettman proceeded with the construction of a horizontal dewar system. Convinced that a superconducting structure should be designed as a standing wave device, Wilson and William Vorkoeper, a HEPL engineer, began constructing a $\pi/2$-mode standing wave structure of approximately ten cells. In an attempt to capture some of the advantages that the TE_{010} mode had offered, they split the structure along its length in two halves. The hope was that the split structure could be electroplated easily and that losses would be small, since the RF current flow was parallel to the juncture between halves.[15]

This time, hard work and hope led to disappointment and uncertainty: the problems of fabrication and assembly into a leak-tight cryogenic system proved to be substantial, and progress was painfully slow. Frustrated by the pace, Wilson and Schwettman, aided by G. Churilov, a visiting scientist from the Soviet Union, constructed a single-cell TE_{010}-mode cavity and a three-cell $2\pi/3$-mode structure. These cavities were plated with an electrode inserted through the beam hole and tested in a simple vertical dewar. In the first series of tests of these accelerator mode cavities the researchers obtained a maximum voltage gradient of 5.5 MV/m, limited by electron field emission. None of the early tests, however, yielded a measured *Q*-value that exceeded

[14] J. P. Turneaure and H. A. Schwettman, "The Surface Impedance of Superconducting Microwave Frequencies: A Comparison of Current Experiments with the BCS Theory," in *Proceedings of the X International Conference on Low Temperature Physics* (Moscow: 1967), p. 343.

[15] H. A. Schwettman, P. B. Wilson, and G. Y. Churilov, "Measurement of High Field Strengths on Superconducting Accelerator Cavities" in *Proceedings of the Fifth International Conference on High Energy Physics* (Rome: Camitato Nazionale per l'Energia Nucleare, 1966), pp. 690–692.

4×10^8. At this stage it was not clear whether the low *Q*-values could be attributed to electroplating difficulties or whether they indicated the appearance of dielectric losses. It was clear, however, that both the *Q*-value issue and the electron field emission issues required detailed investigation.[16]

By midsummer 1965, the team longed for a breakthrough in its attempt to accelerate electrons in a superconducting structure. At this point, in Schwettman's words, they "frantically constructed another three-cell $2\pi/3$-mode structure (this one with an off-axis RF power coupler) with the faint hope of accelerating electrons" in time for a conference to be held in Frascati, Italy, in September. Again, hope led first to disappointment. When the time came to leave for the conference, the new three-cell structure was just short of completion. While other team members went ahead to the conference, Schwettman stayed behind. After completing some last-minute tasks in the next few days, Schwettman managed to coax a 0.5 MeV electron beam from the S-band structure. He quickly flew to Italy, where the conference was still underway, to announce the news.

In Schwettman's words, "the demonstration of electron acceleration in a superconducting structure generated a surprising level of excitement at the Frascati Conference, and perhaps even greater excitement at Stanford." The new Stanford enthusiasts included Nobel laureate Robert Hofstadter, who was eager to facilitate the development of a continuous wave SRF accelerator, which would advance the nuclear structure research he had pioneered.[17] Hofstadter, Fairbank, and another Stanford colleague, Leonard Schiff, soon traveled to Washington to seek ONR funds for development of a superconducting linac.[18] By November 1965 Schwettman and other HEPL

[16]H. A. Schwettman, P. B. Wilson, and G. Y. Churilov, "Measurement of High Field Strengths on Superconducting Accelerator Cavities" in *Proceedings of the Fifth International Conference on High Energy Physics* (Rome: Camitato Nazionale per l'Energia Nucleare, 1966), pp. 690–692.

[17]Hofstadter won the Nobel Prize in physics in 1961 for his work on the internal structure of the nucleon or nuclei.

[18]Schwettman notes that plans for the SRF linac came on the heels of two other accelerator schemes. A presentation at the Frascati conference described preliminary study made by Wiik, Wilson, and Schwettman of a 200 MeV racetrack microtron; further work on such a machine was eclipsed by work on the SRF linac. See B. H. Wiik, H. A. Schwettman, and P. B. Wilson, "A 200 MeV Superconducting Racetrack Microtron" in *Proceedings of the Fifth International Conference on High Energy Acceleration* (Rome: Comitato Nazionale per l'Energia Nucleare, 1966), pp. 686–689. In 1963 Hofstadter had submitted a proposal, which the ONR rejected, for a conventional 4 GeV linac to replace Mark III. Galison, Hevly, and Lowen describe how this proposal arose from dissatisfaction with ongoing plans for the Stanford Linear Accelerator Center (SLAC)—in particular, from concern about the lack of research control for physicists accompanying the shift to larger-scale research—and suggest that differing views about the appropriate relationship between physicists and their sponsors created a rift between the Stanford faculty and the SLAC staff. Leslie Stuart extends the discussion of this era in the history of physics at Stanford by noting disagreements among Stanford faculty members about the advisability of sponsoring applied physics (which led to the formation of an Applied Physics Department) and about the potential dominance of high-energy physicists

colleagues presented a preliminary plan for the new machine to ONR. In the next five years, as the end station and a tunnel were being constructed, about $7 million was spent on the linac development program, which included the purchase of two novel devices—a 300 W superfluid helium refrigerator and a large ultrahigh vacuum furnace.[19]

The Linac Development Program (1967–1972)

The new money did not come without cost, however. Hofstadter's support for the project came with the understanding that the new accelerator would reach 2 GeV, the energy necessary to attain his scientific goals. This created an additional burden for the HEPL linac development group, since "establishing goals for development of a superconducting linac with the constraint that it should yield a 2 GeV accelerator on the HEPL site was at best a delicate matter," as Schwettman later explained. To achieve an energy of 2 GeV in the available space, a voltage gradient of 14 MV/m would be required. Furthermore, cw operation of such a machine would require approximately 7 kW of superfluid helium refrigeration, even for a *Q*-value of 4×10^9. Although the fundamental experiments of Schwettman and Turneaure indicated that the superconducting state would permit operation of a linac at this gradient and *Q*-value, the largest gradient actually produced at that time in the accelerator mode was 5.5 MV/m. Another problem was that for accelerating cavities, the *Q* was an order of magnitude lower than required. Help for these difficulties came from Todd Smith, a research associate new to the team. As a backup position for the linac development program, he introduced an interesting idea: recirculate the beam. With three passes through the linac, the demonstrated gradient of 5.5 MV/m was sufficient to achieve 2 GeV. Another advantage was that operation at the reduced gradient

(which led to difficulties in joint appointments between SLAC and the Stanford Physics Department). An interesting topic for future study would be an exploration of how the concurrent plans for the SRF linac fit into the picture and what the experience of HEPL, which was developed alongside the much larger SLAC project, demonstrates about the development of post–World War II American science. See Peter Galison, Bruce Hevly, and Rebecca Lowen, "Controlling the Monster: Stanford and the Growth of Physics Research, 1935–1962," in Peter Galison and Bruce Hevly, eds., *Big Science: The Growth of Large Scale Research* (Stanford: Stanford University Press, 1992), pp. 65–74, and Stuart Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993).

[19]Despite the increased funding, the laboratory actually had to cut corners to fulfill the agreed commitments. Schwettman remembers that 20% of the staff received layoff notices. Reference to the layoff can be found in M. D. O'Neill to Doran Padgett, January 3, 1969. Text references: High Energy Physics Laboratory, "Proposal to ONR," 1965; High Energy Physics Laboratory, "Proposal to ONR," 1966; High Energy Physics Laboratory, "Proposal to ONR," 1967, High Energy Physics Laboratory, "Proposal to ONR," 1969. The author would like to thank Alan Schwettman for these documents.

implied that the Q-value required for cw operation with 7 kW refrigeration power was nearly an order of magnitude smaller, 5×10^8, a value that had already been achieved in accelerator mode cavities.[20]

The team's task became more complicated, however, by the desire to exploit all potential advantages of the superconducting linac. Although Schwettman's background was in low-temperature physics rather than accelerator physics, he led the charge. He reasoned that the team could achieve an electron beam of exceptional quality and stability with careful injector design and with some form of stabilization. Continuous wave operation of the superconducting linac implied that the bunch charge was small and thus space charge effects in the injector could be ignored. Continuous wave operation also implied that the RF power required by wall losses and the electron beam was small compared to the stored RF energy, and thus the superconducting structure acted as a "flywheel." When Schwettman suggested that it might be possible to achieve one part in 10^4 resolution, a factor of 100 improvement over the Mark III linac at Stanford, Hofstadter responded enthusiastically, but immediately observed that if one were to do experiments at improved resolution, the average beam current would have to be increased. As Schwettman noted, the "resulting commitment to dramatically improve the beam quality and to increase the average beam current" proved to be one of his most important decisions. In the long run, this commitment would facilitate the subsequent free-electron laser development at Stanford and, later, CEBAF's physics program. In the short run, this commitment led to yet another ambitious performance goal.

Schwettman remembers that the linac development goals (Table 1), which were announced in late 1966, and the corresponding accelerator design plans prompted "widespread concern." Panofsky, who was then building SLAC, noted that "in a superconducting linac where most of the power was coupled to the beam, fluctuations in beam current would lead to significant fluctuations in energy." In addition, James Leiss, an eminent electronuclear physicist at the Bureau of Standards, predicted that team members were setting themselves up for "a monumental regenerative beam breakup problem," and Edwin McMillan, Nobel laureate and director of Lawrence Radiation Laboratory, cautioned that "it could take more than a decade to

[20] L. R. Suelzle, "Progress on RF Electron Superconducting Accelerators," in *IEEE Trans. Nucl. Sci.* **NS-20** (1973), p. 44.

get American industry to deliver" the "superfluid helium refrigerator" needed for the proposed accelerator.[21]

Table 1
Goals of the Superconducting Linac Development
(and Parameters of the Existing Mark III Linac)

	Mark III	SC Linac
Energy	1 GeV	2 GeV
Voltage gradient	13 MV/m	14 MV/m (one pass) 4.7 MV/m (three passes)
Duty factor	5×10^{-4}	CW
Q-value	1×10^{4}	4×10^{9} (one pass) 5×10^{8} (three passes)
Refrigeration (@ 1.8 K)	n/a	7 kW
Energy resolution (@ 16 GeV)	10^{-2}	10^{-4}
Average current	< 10 μA	100 μA

Source: Alan Schwettman.

In the next five years, these and other concerns would be addressed, as the linac development team labored to accomplish the sometimes rewarding but often vexing task of building the promised SRF accelerator. In 1966 Fairbank and Schwettman opened discussion of the technical issues surrounding superfluid helium refrigeration with potential manufacturers. Fairbank had already secured approval from the ONR for construction of a 300 W refrigerator. In fact, he hoped for approval of a 1 kW system, but the ONR felt that a 300 W unit would adequately support the development program. Favorable discussions with industry led to a request for proposals and a contract with Arthur D. Little. Among the proposals received in 1967 for construction of the Stanford refrigerator, the Arthur D. Little proposal was the most impressive, since Samuel Collins, inventor of the "Collins liquifier," had swiftly converted one of his liquifiers and demonstrated superfluid helium refrigeration.[22] The 300 W refrigerator was installed at Stanford in 1968, and in 1969 the device was subjected to acceptance tests under the

[21]McMillan won the Nobel Prize in chemistry, along with Glenn Seaborg, in 1951 for the discovery of plutonium.

[22]S. C. Collins, R. W. Stuart, and M. H. Streeter, *Rev. Sci. Instrum*. **38** (1967), p. 1654.

watchful eye of team member Mike McAshan. In Schwettman's words, "what McMillan had said could not be accomplished in ten years, was, in fact, accomplished in just 3 years."[23]

Although obtaining a suitable refrigerator had proved to be a straightforward proposition, addressing the other linac challenges required a multi-prong attack in the continued effort to understand both fundamental issues and to find solutions to numerous practical difficulties. To design the accelerator, for example, the team needed a quantitative understanding of viable options for superconducting standing wave structures. In the effort to gain such an understanding, Fairbank, Schwettman, and Todd Smith traveled to Los Alamos in 1966 to confer with Ed Knapp and Darragh Nagle, who had recently developed a standing wave side-coupled structure for LAMPF. According to Schwettman, they "returned from that visit convinced that a simple biperiodic π/2-mode structure (a simplified version of the LAMPF structure) was a viable option for a superconducting linac, but also intrigued by the possibility of using a π-mode structure of modest length." This was a novel idea for the time, since existing electron linacs were traveling wave devices in which use of the π-mode is not possible. Smith soon embarked on a detailed analysis of standing wave mode linac structures, addressing such issues as power flow and field profile sensitivity to errors. In the end he demonstrated that a π-mode structure of up to ten cells could be used in the Stanford superconducting linac.[24]

In 1969, John Turneaure with the help of Peter Bramham designed the final accelerator structure, which was a modification of the biperiodic π/2-mode with the excited cells of the π/2-mode replaced by seven cells excited in the π-mode. The complete structure consisted of seven substructures (a total of 55 cells) which could be individually fabricated and processed, and then joined together in an unexcited cell. The decision to couple π-mode structures in this way was driven by the cost of the RF system. The klystron and control electronics cost nearly as much for a single seven-cell π-mode structure as for the complete 55-cell structure. This benefit did not come without disadvantage, however. Schwettman would later conclude that although the decision "dramatically reduced RF costs, it guaranteed that structure performance would be determined by the worst of 55 cells and it made the unit of testing (a 55-cell structure) too large, thus slowing development."

---

[23] L. R. Suelzle, "Progress on RF Electron Superconducting Accelerators," *IEEE Trans. Nucl. Sci.* **NS-20** (1973), p. 44.

[24] T. I. Smith, HEPL Report #437, April 1966.

While the linac development team was facing—and solving—problems associated with the design of the accelerator structure and other related accelerator physics issues, they also struggled with one technological obstacle that proved particularly stubborn: the development of cavities capable of serving as the basic accelerating unit for the SRF accelerator. This task was as demanding and frustrating as it was important. In the two years following the Frascati Conference the team fabricated a number of lead-plated accelerator mode cavities and labored to improve electroplating in the axial electrode geometry. Despite the effort, however, they made little progress in improving either the 5.5 MV/m gradient or the 4×10^8 *Q*-value that had been achieved in 1965. "At this point," Schwettman recalls, "the *Q*-value problem loomed larger than the gradient problem. The cost of providing 7 kilowatts of refrigeration, as originally imagined, now seemed prohibitively high, and at a more realistic 700 watts, even with recirculation, the *Q*-value was an order of magnitude too small to achieve cw operation."[25]

The solution to the cavity problem came, in part, thanks to a bit of good fortune and the sort of perseverance demanded by SRF technology. In 1967 SLAC researcher Karl Brown introduced the team to Ira Weissman, who was working on the chemical vapor deposition (CVD) of niobium at Varian Associates. Schwettman assigned John Turneaure, who had recently completed his thesis research, to work with Weissman on the development of superconducting niobium cavities. The decision to focus on niobium and the introduction of Weissman's expertise were a step in what would turn out to be the right direction. However, as was often the case with SRF technology, the first results were nonetheless disappointing. Turneaure and Weissman first fabricated a TE_{011}-mode X-band cavity from niobium and proceeded with CVD of niobium on the surface, in the hope that the use of niobium as a substrate for the CVD layer would eliminate the substrate issue. Measurements on the first CVD cavity indicated that it was not superconducting at all! Although subsequent attempts showed superconductivity, the results were still disappointing. Turneaure and Weissman then made a crucial decision: hoping to shed some light on the difficulty, they made measurements on the niobium substrate itself. The measurements showed dramatic improvement in performance. During the next year, the team developed suitable buffered chemical polishing techniques and heat treatment techniques and achieved high fields and high *Q*-values in TE_{011}-mode cavities. From this point forward, at

[25]L. R. Suelzle, "Progress on RF Electron Superconducting Accelerators," *IEEE Trans. Nucl. Sci.* **NS-20** (1973), p. 44.

Stanford and elsewhere, niobium cavities became the cavity of choice for those building SRF accelerators.[26]

Unfortunately for the HEPL linac designers, other problems endured, despite the success with procuring a refrigerator, developing a workable accelerator structure, and finding a suitable cavity material. Although the niobium TE_{011}-mode cavity experiments were very encouraging, the central issue in development of the linac was performance in TM_{010}-mode cavities when the electric field on the wall could produce dielectric losses, electron multipacting, and electron field emission. One step in ensuring cavity performance was perfecting fabrication procedures. Late in 1968 the team established workable fabrication, processing, and assembly procedures that would set a standard for others constructing SRF cavities. With these procedures, they produced a series of TM_{010}-mode X-band cavities. The cavity parts, fabricated from reactor-grade niobium, were electron-beam-welded in vacuum and then fired at 1800˚ C in a commercial ultrahigh vacuum (UHV) furnace. After leaving the furnace, parts were chemically polished, and fired a second time. The cavity, kept in clean dry nitrogen until final assembly, was attached to a UHV system, baked at 100˚ C and then sealed by pinching the copper tubing. The typical values for peak magnetic and electric fields achieved in these X-band cavities were 750 Oe and 49 MV/m, respectively, and typical Q-values were 2×10^{10} at these fields.[27] This average performance exceeded the 14 MV/m gradient and the 4×10^{9} Q-value that were established as design goals for the 2 GeV superconducting linac. "It thus appeared that our proposed machine was, in principle, possible," in the words of Schwettman. More good news followed. In 1970, Turneaure and Viet Nguyen published the results of a landmark experiment: they produced peak magnetic and electric fields of 1080 Oe and 70 MV/m, respectively, in a TM_{010}-mode X-band cavity.[28]

In parallel with the low-temperature-technology efforts, the Stanford group addressed the accelerator physics issues that were critical to achieving one part in 10^4 energy resolution at 1 GeV. The first of these was the stability issue raised by Panofsky, who was concerned about the very heavy beam loading in a superconducting linac. The Stanford team recognized that the relatively small RF power required by the wall losses and the cw electron beam power, when

---

[26]J. P. Turneaure and I. Weissman, "Microwave Surface Resistance of Superconducting Niobium," *J. Appl. Phys.* **39** (1968), p. 4417.

[27]High Energy Physics Laboratory, "Proposal to the Office of Naval Research," 1969.

[28]J. P. Turneaure and N. T. Viet, "Superconducting Nb TM_{010} Mode Electron-Beam Welded Cavities," *Appl. Phys. Lett.* **16** (1970), pp. 333–335.

compared to the RF energy stored in the structure, made the superconducting machine an ideal candidate for feedback stabilization of the accelerating fields. After completing his thesis in high energy physics at Stanford, Larry Suelzle joined the linac development team in the summer of 1967 and attacked this problem. With his talent for electronics, Suelzle developed and demonstrated an RF amplitude and phase stabilization system by the summer of 1968 that held the amplitude to $\pm 7 \times 10^{-5}$ and the phase to $\pm 0.2^{\circ}$ in a test cavity.[29] The team successfully operated the stabilized system in a prototype injector with beam loading in 1969, thus laying to rest Panofsky's concern.[30]

After stabilizing the accelerating fields, the linac development team still faced more work: to achieve the desired high energy resolution they had to make careful beam dynamics calculations, particularly for the superconducting capture section, and carefully design the room-temperature injector systems. The important beam dynamics calculations were done by E. E. "Nip" Chambers and the injector systems were designed by Suelzle. The room-temperature injector incorporated an RF buncher and chopper system to define and shape the longitudinal phase space of the electron beam and a lens and aperture system to define the transverse phase space. In 1971 the injector systems including the superconducting capture and pre-accelerator sections were assembled and successfully tested.[31] These tests also provided an opportunity to evaluate the cryogenic system of the linac, designed by McAshan. The superfluid refrigerator was used to cool the injector cryomodule via a phase separator and two 100-meter-long cryogenic pipelines. The total heat leak into the cold parts of this system was about 6 W and the stability of the system was such that 120 W switched off and on in the injector cryomodule resulted in a vapor pressure change of less than 0.05 torr.[32]

When it became clear in 1969 that niobium was a superior superconducting material for the cavities of the new accelerator, work began on fabrication techniques that were suitable for a

[29]L. R. Suelzle, in Albert G. Prodell, ed., *Proceedings of the Summer Study on Superconducting Devices and Accelerators* (Washington, D.C.: Atomic Energy Commission, 1969).

[30]E. E. Jones, M. S. McAshan, and L.R. Suelzle, "Report on the Performance of the Superconducting Injector for the Stanford Linear Accelerator," *IEEE Trans. Nucl. Sci*. **NS-16** (1967), p. 1000.

[31]High Energy Physics Laboratory, "Proposal to ONR," September 1971, pp. 13–15; L. R. Suelzle, "Progress on RF Superconducting Accelerators," *IEEE Trans. Nucl. Sci*. **NS-20** (1973), p. 45.

[32]High Energy Physics Laboratory, "Proposal to ONR," September, 1971, pp. 2–6; L. R. Suelzle, "Progress on RF Superconducting Accelerators," *IEEE Trans. Nucl. Sci.* **NS-20** (1973), p. 45.

2 GeV electron linac operating at 1300 MHz.[33] The first task was construction of a suitable 1800˚ C UHV furnace, then a unique device because of its heat zone size and the temperature and pressure it was capable of producing. The linac development team fabricated and processed the first 1300 MHz niobium cavities in 1970. Yet again, the linac development team met disappointment: the single-cell 1300 MHz niobium cavity tests clearly indicated that thermal-magnetic breakdown problems, and particularly electron loading problems, were more severe at L-band than they had been at X-band. The single-cell L-band tests, in fact, set a practical limit for accelerating gradients at 3 to 6 MV/m, far less than the average 14 MV/m gradient obtained at X-band. Since they believed that the limitations encountered in the early 1300 MHz tests would ultimately be overcome, the team proceeded with construction of several prototype accelerator structures. These prototype structures, which were used to test Suelzle's injector system, also allowed the team to evaluate problems of structure tuning and beam breakup.[34]

The very high average beam current objective that was established at Stanford to make high momentum transfer experiments possible, along with the extraordinary *Q*-values required for a cw superconducting linac, raised the specter of regenerative beam breakup. The first of the superconducting cavities was fabricated in 1971, and with these, serious beam breakup studies began.[35] Schwettman remembers that "it was clear from the outset that some form of external loading of higher order modes (HOMs) would be required. In superconducting structures wall losses would only limit *Q*-values to 10^9 or 10^{10}, and thus even modes with small impedance could become involved if they were not properly loaded." The situation was quite different from that found in room-temperature structures where wall losses limited *Q*-values to 10^4 or 10^5. At this point Schwettman got help from two German colleagues, Heinz Schwarz who came from the Technische Hochschule at Darmstadt, and Karl Mittag, who had come to Stanford as a post-doctoral fellow from Kernforschungszentrum Karlsruhe (KfK) in Germany, where related issues were being investigated. The three measured the frequencies and profiles of all modes in the

[33]Schwettman notes that "the fabrication techniques developed at Stanford in 1969 are remarkably similar to those being used in 1996 at DESY for fabrication of the state-of-the-art TESLA structures."

[34]L. R. Suelzle, "Progress on RF Electron Superconducting Accelerators," *IEEE Trans. Nucl. Sci.* **NS-20** (1973), p. 44.

[35]L. R. Suelzle, "Progress on RF Electron Superconducting Accelerators," *IEEE Trans. Nucl. Sci.* **NS-20** (1973), p. 44.

lowest pass-bands of the Stanford structure. Since there were 55 cells in the Stanford structure, this task involved more than 150 modes.[36]

Once the mode structure was established, the Stanford group embarked on careful electron beam measurements to determine the threshold current for regenerative beam breakup, a decisive step in addressing Leiss's concern about the phenomenon. The novel technique used in these measurements, which was developed by Mittag while at Karlsruhe, permitted determination of the regenerative threshold even with a measuring beam current two orders of magnitude smaller than the threshold current. This capability was crucial for the superconducting linac. Armed with this data, the team developed an external loading scheme, which covered all modes of the lowest pass-bands, and designed suitable loading probes.[37]

In the five-year period from 1967 to 1971 the HEPL linac development team had accomplished a great deal. Team members had produced the first superfluid helium refrigerator and developed a helium distribution system and a suitable accelerator cryomodule. The team had also produced the first feedback system to stabilize accelerating fields, and thanks to the careful design of the injector systems, had demonstrated that it was possible to achieve one part in 10^4 energy resolution at 1 GeV. The group also produced the first superconducting niobium cavities, demonstrating that the superconducting state would support the high fields and high *Q*-values desired for an electron linac, and developed fabrication techniques that would serve as a model for others in the years to come. In addition, they developed a viable accelerator structure and provided external loading of HOMs, paving the way for solving the problem of regenerative beam breakup.

---

[36] K. Mittag, H. D. Schwarz, and H. A. Schwettman, "Beam Breakup in a Superconducting Electron Accelerator," *Proceedings of the 1972 Proton Linear Accelerator Conference* (Washington, D.C.: Atomic Energy Commission, 1972), p. 131.

[37] K. Mittag, H. D. Schwarz, and H. A. Schwettman, "Beam Breakup in a Superconducting Electron Accelerator," *Proceedings of the 1972 Proton Linear Accelerator Conference* (Washington, D.C.: Atomic Energy Commission, 1972), p. 131; K. Mittag, H. D. Schwarz, and H. A. Schwettman, "Beam Breakup in a 55-Cell Superconducting Accelerator Structure," *IEEE Trans. Nucl. Sci.* **NS-20** (1973), p. 86.

Why Hopes Fell: Problems and the Reaction[38]

The Battle with Voltage Gradient (1972–1976)

HEPL's accomplishments would continue to accumulate, along with increasingly grave difficulties. The first sections of the superconducting linear accelerator (SCA) were brought into operation at Stanford in 1971 and 1972 and tested. By the end of 1973 several full-length 55-cell superconducting structures were installed and tested. The initial operation of these structures produced energy gradients from 2.0 to 3.8 MV/m and values from 2 to 6×10^9 for Q.[39] Leiss was correct that the team faced a monumental task in suppressing beam breakup, but by careful work the ambitious goal of accelerating an average beam current of 100 µA was reached and exceeded. In fact, the loading probes were designed for an average beam current of 500 µA, a goal that was quickly achieved.[40] The exceptional beam quality and beam stability of the new superconducting linac demonstrated the feasibility of high-duty-factor beams for coincidence experiments, paved the way for a series of pioneering experiments that showing the feasibility of the free-electron laser (FEL), and held promise for facilitating future accelerator development.[41]

The lower-than-expected voltage gradients in 1300 MHz accelerating structures represented a critical problem, however, that would force HEPL researchers to launch new studies of superconducting cavities, painstaking work that would consume several more years. At this stage, the group expanded their studies to include work at an intermediate frequency, 2956 MHz (S-band), a move prompted by the dramatic difference in observed behavior of 8600 MHz X-band cavities and 1300 MHz L-band cavities. Initial cavity research focused on investigation of electron field emission loading, which appeared to be the most important limitation in achieving high fields. Accordingly, the group used a NaI (Tl) crystal scintillator to

---

[38] The portions of this section on HEPL are based on contributions and revisions supplied by Alan Schwettman in private communications. All quotations for Schwettman are from these private communications, unless otherwise indicated.

[39] J. P. Turneaure, H. A. Schwettman, H. D. Schwarz, and M. S. McAshan, "Performance of 6-m 1300-MHz Superconducting Niobium Accelerator Structures," *Appl. Phys. Lett.* **25** (1974), p. 247.

[40] M. S. McAshan, K. Mittag, H. A. Schwettman, L. R. Suelzle, and J. P. Turneaure, "Demonstration of the Superconducting Electron Accelerator as a High-Intensity High-Resolution Device," *Appl. Phys. Lett*. **22** (1973), p. 605.

[41] The pioneering experiments included the first measurement of FEL gain and the first demonstration of FEL oscillation. See Luis R. Elias, William M. Fairbank, John M. J. Madey, H. Alan Schwettman and Todd I. Smith, "Observation of Stimulated Emission of Radiation by Relativistic Electrons in a Spatially Periodic Transverse Magnetic Field," *Phys. Rev. Lett*. **36** (1976), p. 717, and D. A. G. Deacon, L. R. Elias, J. M. J. Madey, G. J. Ramian, H. A. Schwettman, and T. I. Smith, "First Operation of a Free-Electron Laser," *Phys. Rev. Lett*. **38** (1977), p. 892.

measure the X-radiation produced by field-emitted electrons. Group members measured radiation intensity as a function of field-emitted electrons, radiation intensity as a function of field level, and the X-radiation energy spectrum. Interpreting these observations was itself a formidable task. Accordingly, they developed a computer simulation program that took into account the field emission, the acceleration and the multiplication of electrons, and the production of X-radiation.[42] In addition, they developed a method for *in situ* sputter processing of cavities (at helium temperature) to reduce field emission.[43] As Schwettman later explained: "These studies contributed a good deal to our understanding of the field emission problem. We recognized that enhanced emission was in part due to surface contamination and in part due to bulk effects. We also realized that there was a scaling law for electron dynamics—for cavities of the same geometry, electrons follow the same trajectory with the same energy at each point, if the electric field divided by the cavity frequency is the same. Moreover, we recognized that the field emission originated from a rather small number of points per unit area and thus the larger surface area of the 1300 MHz cavities was statistically relevant."

Initial work gave rise to further lines of inquiry. The HEPL researchers inferred from the character of the field limitation observed in the full-length 55-cell superconducting structures that thermal-magnetic breakdown was the problem in the linac itself. In Schwettman's words: "Certainly, no electron field emission was observed at these gradients." This realization prompted three new projects: development of diagnostics for thermal mapping, development of a computer program to calculate the thermal stability (or instability) of defects, and physical characterization of the superconducting surface.

The HEPL group developed two quite different schemes for thermal mapping. For 55-cell structures the principal issue was identifying which of the seven substructures had broken down. Researchers developed a novel technique based on sound propagation in superfluid helium for this purpose. They used an array of 14 resistance thermometers distributed along the length of the structure to measure the time of arrival of the heat pulse initiated by breakdown. The point of origin could be established within ± 1 cell of the structure. For single-cell cavities and structures with a few cells, team members developed a different technique. They mounted

[42] I. Ben-Zvi, J. F. Crawford, and J. P. Turneaure, "Electron Multipaction in Cavities," *IEEE Trans. Nucl. Sci.* **NS-20** (1973), pp. 54–58.

[43] J. P. Turneaure, H. A. Schwettman, and R. F. Waites, "Evidence for Surface-State-Enhanced Field Emission in RF Superconducting Cavities," *J. Appl. Phys.* **45** (1974), p. 914.

resistance thermometers on a device which could be rotated azimuthally around the cavity axis so that they could determine the location, size, and shape of the hot spot. The team made an interesting observation from such measurements: in each of seven tests in S-band cavities, breakdown occurred on the bottom surface of the cavity, suggesting that particulate matter played an important role in thermal-magnetic breakdown. These thermal mapping efforts—and the insights arising from them—provided crucial information for the growing community of researchers determined to solve the problems preventing development of SRF cavities.[44]

In the meantime, developments elsewhere were stimulating additional HEPL work. Siemens researchers reported experiments which renewed concern about the thermal stability of a superconducting cavity that supports high RF magnetic fields.[45] As these experiments revealed, even a perfect, defect-free superconducting surface is vulnerable to thermal runaway at high field levels, and runaway can occur at a much lower magnetic field level if there is a defect on the surface. Stimulated by this result, Claude Lyneis, a graduate student of Schwettman's, began studies of the least stable defect geometry, calculating the breakdown field level as a function of defect line width, assuming the outer surface of the cavity was held at the bath temperature.[46] Unfortunately for the HEPL group, neither thermal mapping diagnostics, thermal stability calculations, nor physical characterization of the superconducting surface produced substantial improvement in the gradients achieved at 1300 MHz.

Even before this three-prong attack led to disappointment, HEPL researchers were pursuing other ideas for achieving a workable accelerator. In 1971, when it became clear that achieving high gradients in 1300 MHz cavities was going to be far more difficult than in X-band cavities, plans for recirculating the electron beam, which had been explored in the previous three years by Eifionydd Jones, a visitor from CERN, and by Hofstadter, gained momentum.[47] By mid-1972 Roy E. Rand outlined a detailed scheme for multiple recirculation of the SCA beam based on a novel multichannel magnet system. A prototype recirculation system was designed in

[44]C. M. Lyneis, M. S. McAshan, and Nguyen Tuong Viet, "Recent Measurements of S-band and L-Band Cavities at Stanford" in *Proceedings of the 1972 Proton Linear Accelerator Conference*, Los Alamos, New Mexico, 1972.

[45]H. Schnitzke, H. Martins, B. Vrillenbrand, and H. Diepers, "TE_{011} X-Band Cavity with Critical Magnetic Flux Density Higher than B_{c1}," *Phys. Lett.* **45A** (1973), p. 241.

[46]Claude Lyneis, "Fundamental Studies Related to Improved Performance of Superconducting RF Cavities," High Energy Physics Laboratory, "Proposal to the Office Of Naval Research," 1974.

[47]Eifionydd Jones, "Recirculating at the Stanford High Energy Physics Laboratory," HEPL-609, August 1969; Robert Hofstadter, "Recirculation of an Electron Beam in a Linear Accelerator," HEPL Report No. 664, December 1971.

1973 and subsequently built and tested.[48] This was the first demonstration of electron beam recirculation in a linac, and ultimately, three-pass operation of this prototype “recyclotron” was achieved.[49]

More disappointment lay ahead, however. From the beginning, HEPL researchers figured recirculation would complicate the beam breakup problem. The nature and extent of the difficulty, though, were only revealed in a series of experiments using the prototype “recyclotron.” In addition to quantitatively demonstrating the role of recirculation optics and the importance of 100% beam transmission, these experiments showed that the beam-cavity interaction in a recirculated-beam machine was qualitatively different than in a linac. Nature had played a cruel trick on the HEPL group: transverse cavity modes that play *no* role in linac breakup can, in fact, dominate breakup in a recirculating machine. HEPL researchers now realized that the very substantial engineering effort that had completely resolved the linac breakup problem would have to be repeated for the case of a recirculating machine.[50]

The continued inability to make progress in achieving high gradients was a crushing blow for electronuclear physicists wanting an electron linac in the 2 GeV range: the energy gradients of 2.0 to 3.8 MeV/m were far short of the 14 MV/m goal needed for such an accelerator. Although with recirculation an SRF accelerator would be practical if it could routinely achieve 5 MV/m, even this more humble goal seemed unattainable. As a result, as Schwettman noted in 1975, the “early hope that RF superconductivity would provide the means of constructing a high gradient linear accelerator has been dashed upon the rocks of physical reality. Investigations at numerous laboratories have clearly indicated that the technical problems of achieving very high gradients in full-scale superconducting structures are extremely difficult and that success in this venture will be slow, if indeed it can be achieved at all.”[51]

---

[48] R. E. Rand, “Multiple Recirculation of the SCA Beam,” HEPL TN-72-2, June, 1972; R. E. Rand, “A Multi-Orbit Recirculation System for a Superconducting Linear Accelerator,” *IEEE Trans. Nucl. Sci*. **NS-20** (1973), p. 938; J. R. Calarco, M. S. McAshan, H. A. Schwettman, T. I. Smith, J. P. Turneaure, and M. R. Yearian, “Initial Performance of the Stanford Superconducting Recyclotron,” *IEEE Trans. Nucl. Sci*. **NS-24** (1977), p. 3.

[49] T. I. Smith, C. M. Lyneis, M. C. McAshan, H. A. Schwettman, and J. P. Turneaure, “Unique Beam Properties of the Stanford 300 MeV Superconducting Recyclotron,” *IEEE Trans. Nucl. Sci*. **NS-28** (1981), p. 3445.

[50] M. S. Brittan, M. S. McAshan, H. A. Schwettman, T. I. Smith, and J. P. Turneaure, “Operating Characteristics and Beam Breakup Measurements in the Superconducting Recyclotron,” X International Conference on High Energy Accelerators, Serpukov, USSR, 1977; C. M. Lyneis, R. E. Rand, H. A. Schwettman, and A. M. Vetter, “Standing Wave Model of Regenerative Beam Breakup in Recirculating Electron Accelerators,” *Nucl. Instrum. Methods* **204** (1983), p. 269.

[51] H. Alan Schwettman, “Practical Considerations in the Design and Operation of Superconducting Structures,” *IEEE Trans. Nucl. Sci.* **NS-22** (1975), p. 1118.

In the 1975 paper, Schwettman also presented data that ultimately proved crucial in unraveling the voltage gradient problem encountered in the Stanford structures. The paper described detailed power absorption measurements in a seven-cell superconducting capture section and gave a comparison of the observed absorption levels with a simple multipacting interval rule. These measurements clearly indicated that the observed absorption levels were "the result of different order multipacting for one basic multipacting trajectory." The first clue that electron multipacting might be involved in limiting energy gradients came in the next year. Schwettman observed that breakdown in two structures occurred at the same field level and that this field level corresponded to one of the multipacting levels identified in the capture section power absorption experiment. Subsequent experiments showed that all but one of the Stanford structures broke down at a multipacting level.[52]

Why was it so difficult to learn that electron multipacting was the primary limitation to gradients in the Stanford structures? Schwettman later explained that in single-cell cavity tests electron multipacting always had a clear signature, evident in the reflected or transmitted RF power. This clear signature, however, was masked in the heavily overcoupled accelerator structures. With the heavy coupling there was a tendency to pass easily through low multipacting levels and then to encounter a level that instantly produced breakdown. Thus, the RF behavior mimicked thermal-magnetic breakdown.

Once they had identified multipacting as the primary culprit in their difficulties, the Stanford group moved quickly to understand the nature of this phenomenon. Schwettman suggested to Turneaure that he try to use thermal mapping techniques on a single-cell cavity to locate the multipacting trajectories. At that time Turneaure was working on an electron tracking program that included secondary electron production and backscattering at the cavity surface, and he preferred to attempt locating the trajectories by inputting random electrons into his simulation program. When this approach failed, Schwettman and Lyneis began a single-cell thermal mapping experiment. In Schwettman's words: "The results were shocking." They observed heat produced by the multipacting electron at the radius of the curvature between the outer cylindrical wall and the end wall of the cavity. Once the location of the multipacting trajectories was established, Turneaure's program identified the trajectory as one-point multipacting driven by the

[52] H. Alan Schwettman, "Practical Considerations in the Design and Operation of Superconducting Structures," *IEEE Trans. Nucl. Sci.* **NS-22** (1975), p. 1118.

small electric field at the radius of the curvature. Although problems with multipacting—and resulting low gradient—continued, researchers had taken a decisive step in their attempts to understand and overcome the problem.[53]

The Reaction

Despite such progress, in the late 1970s electronuclear physicists were gravely disappointed by the dismal prospects for the promised electronuclear accelerator. The multipacting problems, which still seemed insurmountable to some observers, eclipsed all other considerations. Although recirculation was an option, it was clear that proceeding along that path would still require extensive investigation of regenerative beam breakup.[54] Since the next generation of electronuclear accelerators could be built without resorting to the problematic new technology—both microtron and linac stretcher ring designs were viable alternatives—the apparent inability to achieve much over 2 MV/m led, in the words of Helmut Piel, "to the impression that high duty factor electron accelerators should be based on classical technology."[55] As a result of this impression, and the fact that key proponents of electronuclear machines had already made significant investments in conventional technology, many laboratories active in the field both in Europe and the U.S. began plans for new conventional high-duty-factor electron accelerators. These plans would lead in the U.S. to the five proposals brought before the Bromley Panel in 1983.[56]

As the designs for these new accelerators were drafted and judged, memories of HEPL's problems did not fade. It was widely believed that HEPL researchers had oversold their project. Wilson noted that many "thought it was unconscionable to promise to deliver" a high-duty-cycle

---

[53]C. M. Lyneis, H. A. Schwettman, and J. P. Turneaure, "Elimination of Electron Multipacting in Superconducting Structures for Electron Accelerators," *Appl. Phys. Lett.* **31** (1977), p. 541.

[54]Alan Schwettman to Catherine Westfall, April 2, 1996. The theory of regenerative beam breakup was comprehensively treated by Frank Sacherer in the mid-1970s. Frank Sacherer, in *Proceedings of the IXth Conference on High Energy Accelerators*, SLAC, May 2–7, 1974, pp. 347–351.

[55]Helmut Piel, "Superconducting Accelerating Structures for High Energy Accelerators," in *Proceedings of the 1984 Linear Accelerator Conference*, Seeheim, Germany, May 7–11, 1984, p. 260.

[56]The most efficient way for researchers at the Massachusetts Institute of Technology to proceed was to upgrade their existing conventional machine. The same was true for researchers at Saclay, who were building their machine in collaboration with MIT. Researchers at the National Bureau of Standards and at the University of Mainz had ongoing efforts aimed at improving room-temperature machines. Private Communication, Larry Cardman, July 24, 1995. For more information on how plans proceeded in the late 1970s and early 1980s for an electronuclear machine, see Catherine Westfall, "The Founding of CEBAF, 1979 to 1987," CEBAF/Jefferson Lab, 1994.

machine with a high gradient and actually obtain money for building “the tunnel and a giant experimental end station ... without thorough testing of some reasonably sized prototype.” Schwettman later placed the blame for HEPL’s problems both on “the premature appearance of a tunnel and end station” and on “the burden of 2 GeV milestones.”[57] In any event, in light of optimistic promises and disappointing results, those planning the next electronuclear machine faced the likelihood that funding would be impossible to obtain in the future for an SRF machine—even if the technical problems were solved, funding agencies might well refuse to believe the news. As time went on, many physicists remained deeply skeptical of progress in SRF, convinced that regenerative beam breakup would never actually be overcome. In the words of Andrew Sessler, a veteran accelerator builder not involved in SRF development, “HEPL poisoned people’s minds for the potential of superconducting RF.”[58]

Thus, by the late 1970s, three seemingly insurmountable obstacles lay in the path to an SRF electronuclear accelerator. In addition to technical difficulties, in particular regenerative beam breakup, progress was blocked by two consequences of the HEPL experience: the potential difficulty of obtaining funding for such a machine and the enduring conviction that intrinsic limitations would prevent the practical application of SRF technology for the near term, and perhaps for the future.

The Path to CEBAF’s SRF Cavity

SRF Cavities for Storage Rings

Despite the obstacles that can be traced to HEPL, in a circuitous manner the laboratory did facilitate the further progress of an SRF electronuclear accelerator. Due to their long experience and expertise, HEPL researchers continued to provide leadership in SRF and their research provided a point of departure for others. In fact, Ronald Sundelin, a member of the SRF group at Cornell, judges that over half of the progress in SRF was made at HEPL.[59] Perhaps most important, the excitement that began at HEPL led to an international research and

[57]Quotations, respectively, from interviews with Perry Wilson, March 2, 1994, and Alan Schwettman, “HEPL Memoirs,” December 1995.

[58]Quotations from interview with Andrew Sessler, March 3, 1994.

[59]Westfall and Brooks interview with Ronald Sundelin, March 24, 1994.

development effort extensive enough to survive the disappointment of HEPL's failure to produce the promised electronuclear machine.

Ironically, continued progress towards such a machine would be advanced by those interested in building other types of accelerators. Although the disappointments of HEPL deflected electronuclear physicists from building a full-scale SRF machine, the technology remained attractive for those who were willing to persevere in the hope that these limitations could be overcome in the long term. In 1980, at least 14 components employing superconducting technology operated in accelerators in the U.S., Japan, Israel, Germany, Italy, and Switzerland. HEPL researchers remained active in SRF research and development as did researchers at KfK, the other preeminent center in SRF research.[60] Those interested in building storage rings to investigate particle physics were a part of this vigorous international SRF effort. Unlike those designing electronuclear accelerators, storage ring builders—a category that included researchers at CERN, KEK, DESY, and Cornell—could not fall back on conventional technology: they continued development of SRF cavities because without them they had little hope for building cost-effective storage rings capable of accelerating electrons to the desired energy of hundreds of GeV in the center of mass.[61] The work at Cornell, nurtured within the environment of the international research and development effort, became the seed from which CEBAF grew. Thus, to explain why hopes for an SRF electronuclear accelerator revived in the mid-1980s requires a side trip to the Cornell effort. Motivated by hopes for a storage ring for particle physics research, Cornell researchers would successfully build and test SRF cavities. In the process they would prove that the technical obstacles preventing progress toward an SRF electronuclear accelerator had been overcome and pave the way for a convincing argument that such an accelerator could be built at CEBAF.[62]

---

[60]A. Citron, "Compilation of Experimental Results and Operating Experience" in M. Kuntze, ed., *Proceedings of the Workshop on RF Superconductivity,* Karlsruhe, Germany, July 2–4, 1980, p. 12.

[61]Maury Tigner, "Superconducting Cavities for High Energy Accelerators—Progress and Prospects," *Proceedings of the 1983 Particle Accelerator Conference, Accelerator Engineering and Technology*, Santa Fe, New Mexico, 21–23 March, 1983.

[62]Major technical reviews of SRF include: H. A. Grunder, J. J. Bisognano, W. I. Diamond, B. K. Hartline, C. W. Leemann, J. Mougey, R. M. Sundelin, R. C. York, "The Continuous Electron Beam Accelerator Facility" in *Proceedings of the 1987 IEEE Particle Accelerator Conference*, *IEEE,* **CH2387**, 1987, pp. 13–18; H. Lengeler, "Recent Developments in Superconducting Linac Structures," in *Proceedings of the 1986 Linear Accelerator Conference*, SLAC, June 2–6, 1986, pp. 188–193; Padamsee, Shepard, and Sundelin, "Physics and Accelerator Applications of RF Superconductivity," in *Ann. Rev. Nucl. Part. Sci*. **43** (1993), pp. 635–685; Helmut Piel, "Superconducting Accelerating Structures for High Energy Accelerators" in *Proceedings of the 1984 Linear Accelerator Conference*, Seeheim, Germany, May 7–11, 1984, pp. 260–264; Ronald Sundelin, "Superconducting

Early Work—1969 to 1979

Tracing the beginning of the path that would change the course of CEBAF history requires a bit of backtracking to the onset of SRF research at Cornell in the late 1960s. A complete account of the Cornell SRF effort is illuminating for several reasons. The Cornell project, when viewed against the backdrop of the international SRF research and development effort, provides an example of how the detailed problem-solving pioneered at HEPL continued and how this meticulous work and intense collaboration—and perseverance—ultimately led to SRF's maturity and thus the possibility of its application at CEBAF. Also, the innovations and experience gained at Cornell are a crucial part of the development of CEBAF because the Cornell cavity was used with only minor modifications at CEBAF and members of the Cornell group participated in the manufacturing, processing, and installation of CEBAF SRF cavities. As a result, the work of the Cornell group would be central to the technological development of the CEBAF SRF accelerator. Perhaps most importantly, CEBAF's access to the Cornell cavity and the expertise of Cornell workers would be crucial in the task of convincing the physics community and the Department of Energy that CEBAF could build an SRF accelerator.

Cornell SRF work began in 1969 when Maury Tigner became interested in the potential of this intriguing but problematic technology. Motivated by the desire to build SRF cavities for a very high-energy electron accelerator, he spearheaded a project at Cornell that became part of the international SRF research and development effort. Tigner sought to avoid synchrotron radiation problems by altering cavity shape. He hit upon the idea of designing a cavity in the shape of a muffin tin, with an upper and lower half, intending that synchrotron radiation would escape the cavity without hitting the surface in the empty midplane between the halves. For a year in 1974 and 1975, the Cornell group installed and then operated an 11-cell, 2856 MHz Nb muffin-tin cavity in the Cornell 12 GeV electron synchrotron, achieving a first—never before had a superconducting cavity been used in an electron synchrotron. The muffin-tin cavity still experienced multipacting and did not achieve the goal of 5 MeV/m. However, it did achieve a better gradient than the HEPL cylindrical cavity (4 MeV/m rather than 2 MeV/m) with *Q*-values

---

RF Activities at Cornell University" in *Proceedings of the Second Workshop on RF-Superconductivity*, Geneva, Switzerland, July 23–27, 1984, pp. 49–61; W. Weingarten, "Superconducting Cavities," *Proceedings of the 15th International Conference on High Energy Accelerators*, Vol. II (Singapore: World Scientific, 1993), pp. 678–685. The sources provided the core information for this section.

in the same order of magnitude.[63] In the next few years further progress would also be made in Europe. After six years of research, in 1977 a Karlsruhe-CERN collaboration installed two 3-meter deflectors operating at 3 GHz and 1.8 K in the CERN SPS. In Herbert Lengeler's words, this was the first instance of a superconducting device "used successfully in a truly routine way."[64]

CERN researcher Wolfgang Weingarten later placed these achievements into the perspective of overall SRF research and development. Work on the devices installed at Cornell and CERN, along with SRF cavity research at HEPL, provided a great deal of experience with "the metallurgy of Nb, on clean handling, electron beam welding, chemical and electrochemical cleaning methods and high temperature firing under ultra high vacuum."[65] The topics discussed at a 1980 SRF conference show the concerns of the time. In addition to individual talks on diagnostic methods and particular design and fabrication issues, which included discussion by Ronald Sundelin from Cornell on couplers for damping HOMs, the conference featured three talks on surface treatment. One of these was given by Peter Kneisel of KfK, who detailed "certain preparation techniques for niobium surfaces ... and certain rules for preparation," including directions for machining surfaces, electropolishing, chemical polishing, and heat treatment based on results from Cornell, HEPL, SLAC, Siemens, Wuppertal, as well as KfK, where researchers were testing a 500 MHz niobium cavity. Kneisel would later judge that surface treatment techniques developed by KfK and Siemens during this period were "instrumental in keeping SRF a viable technology for accelerators."[66]

---

[63]Interview with Ronald Sundelin, September 25, 1990; Ronald Sundelin, "Superconducting RF Activities at Cornell University," in *Proceedings of the Second Workshop on RF-Superconductivity*, Geneva, Switzerland, July 23–27, 1984, p. 49; W. Weingarten, "Superconducting Cavities," *Proceedings of the 15th International Conference on High Energy Accelerators*, Vol. II (Singapore: World Scientific, 1993), p. 679.

[64]Quote from Herbert Lengeler to Catherine Westfall, October 30, 1995. Also: CEBAF, *Scientific and Technological Assessment Report* (STAR) *on the Superconducting CW Linac Design for CEBAF*, November 1985, p. 4-9.

[65]W. Weingarten, "Superconducting Cavities" in *Proceedings of the 15th International Conference on High Energy Accelerators*, Vol. II (Singapore: World Scientific, 1993), p. 679.

[66]Quotations, respectively, from Peter Kneisel, "Surface Preparation of Niobium," in M. Kuntze, ed., *Proceedings of the Workshop on RF Superconductivity*, Karlsruhe, Germany, July 2–4, 1980, p. 27, and Peter Kneisel to Catherine Westfall, September 26, 1995.

Improving Cavity Shape, Manufacturing Procedures, and Niobium, Late 1970s to Early 1980s

To make further progress in the development of SRF cavities, researchers had to simultaneously mount three lines of inquiry: in addition to the continuing search for the right shape and coupling of cavities and for suitable manufacturing procedures, they sought ways to create high-quality superconductors. In the process of this work, SRF cavity testing began in earnest. As W. Bauer reported in 1981, the KfK 500 MHz niobium cavity had been installed into the DORIS storage ring at DESY and achieved 3.2 to 4 MV/m, providing "the first tests of [a] completely assembled system." By this time "similar cavities [had] been investigated at KfK, CERN and KEK" without ancillary equipment. In 1982, two five-cell, 1500 MHz Nb muffin-tin cavities were tested in the 8 GeV electron-positron storage ring at Cornell.[67]

At Cornell, work through the early 1980s centered on the muffin-tin cavities. Researchers found a way to reduce multipacting by using electro-discharge machining to groove the cup bottoms of their muffin-tin structure.[68] By this time, however, efforts to suppress or eliminate one-point multipacting had led to advances in cavity shape that clearly reveal the international collaboration that facilitated progress in the field. At Stanford, Turneaure and Lyneis designed and tested a cavity that more closely approached the right circular cylinder in geometry, motivated by the realization that one-point multipacting was driven by the small electric field that is caused by the radius of curvature between the cylindrical wall and the end wall of the cavity. In the meantime, T. Parodi and others at Genoa University serendipitously found that structures with continuously curving outer walls did not multipactor.[69] Drawing on HEPL work on one-point multipacting, Klein and Proch at Wuppertal University did a computer

---

[67]Quotations from W. Bauer, A. Brandelik, A. Citron, F. Graf, H. Halbritter, W. Herz, S. Noguchi, R. Lehm, W. Lehman, L. Szecsi, "Measurements on a Superconducting Accelerating Cavity for DORIS," *IEEE Trans. Nucl. Sci.* **NS-28**, No. 3, June 1981. Also: Ronald Sundelin, "Superconducting RF Activities at Cornell University," *Proceedings of the Second Workshop on RF-Superconductivity*, Geneva, Switzerland, July 23–27, 1984, p. 49; Yuzo Kojima, Takaaki Furuya, and Toshiharu Nakazato, "Recent Results on 500 MHz Superconducting Cavities at KEK," *Japanese Journal of Applied Physics* **21** (1982), pp. L86–L88.

[68]Ronald Sundelin, "Superconducting RF Activities at Cornell University" in *Proceedings of the Second Workshop on RF-Superconductivity,* Geneva, Switzerland, July 23–27, 1984, p. 49; interviews with Ronald Sundelin, September 25, 1990, and March 24, 1994.

[69]V. Lagomarsino et al., "Measurements on Niobium Superconducting C Band Cavities for Linear Accelerators Applications," *IEEE Trans. Magn.* **15** (1979), pp. 25–26. Also: H. A. Grunder, J. J. Bisognano, W. I. Diamond, B. K. Hartline, C. W. Leemann, J. Mougey, R. M. Sundelin, R. C. York, "The Continuous Electron Beam Accelerator Facility," *Proceedings of the 1987 IEEE Particle Accelerator Conference, IEEE,* **CH2387**, 1987, p. 14.

simulation that verified that spherical walls do not experience this effect. Kneisel subsequently refined the spherical cavity to an elliptical shape, adopted at Cornell and elsewhere, with lower peak surface fields and greater structural integrity.[70]

Although the elliptical shape suppressed multipacting, researchers were still faced with multipacting due to couplers. In the Cornell case, coupler multipacting was eliminated by equipping Cornell elliptical cavities with a revised version of the on-axis fundamental coupler used in the muffin-tin cavities, which avoided the problem of having a hole in the cell by putting both the input couplers and the couplers that extract the HOMs at the beam pipe rather than cutting through a cell wall.[71]

Alongside work on cavity shape, various groups worked to study and find ways to further minimize field limitations. As Padamsee of the Cornell group later summarized, "thermometry-based diagnostic systems played a key role in improving the understanding of field limitations." Thermometry at HEPL and elsewhere had already proved important in addressing the problems of thermal breakdown and multipacting.[72] In 1980, global temperature mapping came of age, when Piel described the first convenient temperature mapping scheme (Figure 1). The mapping system, which was developed at CERN, used a rotating arm of thermometers submerged in subcooled helium that would circle cavities that had been designed to be cylindrically symmetric so that one set of thermometers could circle the entire apparatus. In Piel's words, the system, which made "a 3 dimensional temperature map of [a] superconducting 500 MHz niobium cavity" consisted of "39 carbon thermometers ... gliding under spring tension on the cavity wall" which could "be turned all around the cavity. The resistor voltages and their angular position [were] read by a computer-controlled data acquisition system."[73] A measure of the advances made possible from this technique can be taken by the reflections of electronuclear experimentalist Larry Cardman, who had witnessed the trials of SRF firsthand at the University of Illinois. When

[70]U. Klein and D. Proch, "Multipacting in Superconducting RF Structures" in *Proceedings: Conference on Future Possibilities for Electron Accelerators*, Charlottesville, Virginia, 1979, pp. N-N-17; interview with Ronald Sundelin, March 24, 1994.

[71]Ronald Sundelin, "Superconducting RF Activities at Cornell University" *in Proceedings of the Second Workshop on RF-Superconductivity*, Geneva, Switzerland, July 23–27, 1984, p. 55; interview with Ronald Sundelin, March 24, 1994.

[72]Quotation from H. Padamsee, K. W. Shepard, and R. Sundelin, "Physics and Accelerator Applications of RF Superconductivity," in *Ann. Rev. Nucl. Part. Sci.* **43** (1993), p. 671. Also: C. Lyneis, in *Proceedings of the 1972 Proton Linear Accelerator Conference*, LASL, Los Alamos (1972), p. 98.

[73]Helmut Piel, "Diagnostic Methods of Superconducting Cavities and Identification of Phenomena" in M. Kuntze, ed., *Proceedings of the Workshop on RF Superconductivity*, Karlsruhe, July 2–4, 1980, pp. 105, 107.

Cardman visited Piel's laboratory in Wuppertal, where temperature mapping was used on a five-cell accelerating structure, he was "amazed that they could measure what was wrong, take a dentist's tool and grind off the impurities." In Cardman's opinion, the fruits of perseverance in the international SRF effort were now readily apparent, even to those who were not directly involved. "They were moving SRF from black magic to a science."[74] Fred Palmer and Sundelin at Cornell made an important contribution to this work by developing a room-temperature matrix multiplexing scheme which substantially reduced the large number of wires emerging from the cryostat.[75]

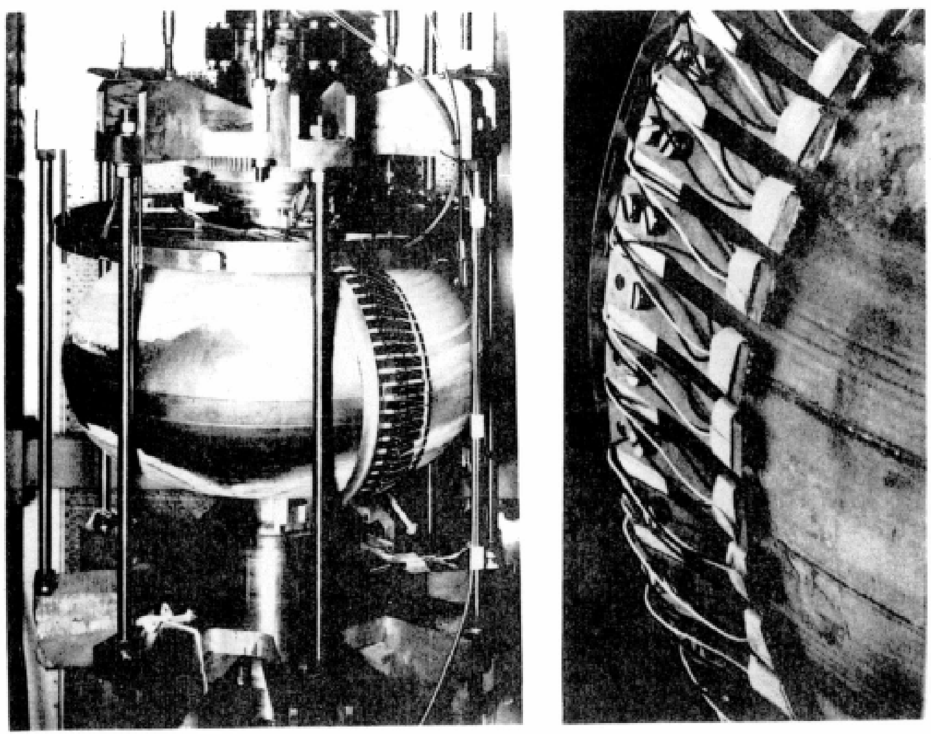


Figure 1. General configuration and close-up view of the carbon thermometer scanning system. (From H. Piel, "Diagnostic Methods of Superconducting Cavities and Identification of Phenomena" in *Proceedings of the Workshop on RF Superconductivity*, Karlsruhe, Federal Republic of Germany, July 2–4, 1980, p. 108.)

As hinted by early HEPL work, a key understanding to emerge from thermometry techniques, in Weingarten's words, was that field losses "were caused by two different mechanisms: pointlike normal conducting defects ... or impacting electrons from pointlike ...

[74]Larry Cardman, private communication, June 13, 1995.

[75]Ronald Sundelin, "Superconducting RF Activities at Cornell University" in *Proceedings of the Second Workshop on RF-Superconductivity*, Geneva, Switzerland, July 23–27, 1984, p. 51.

electron emitters" which "heat up the cavity wall (decrease its *Q*-value)" so that there can be "a sudden decrease of the stored energy in the cavity ('quench'), not admissible for reliable operation."[76] To avoid the first limit, researchers sought to develop niobium with higher thermal conductivity so that the superconducting surface would be stabilized against thermal runaway. Lengeler later credited Padamsee for making the crucial observation that "the threshold field for thermal instabilities could be increased if the thermal conductivity of the cavity wall is improved." Alan Schwettman remembered that this came as a surprise, since the previous assumption was that the metallic properties of niobium were causing the difficulty.[77] In the words of Piel, "interstitial impurities of O, N, C and H" in fact "dominate the poor conductivity of the material."[78] This realization led to the development of better-quality niobium at research centers as well as by industry.

As part of this effort, the Cornell group formed a team to experiment with niobium processing. As reported in 1985, the Cornell group worked with industry to reduce "the interstitial impurities by electron beam melting Nb more slowly and more times in a better vacuum than is customary, and by taking precautions to prevent recontamination of the Nb during rolling."[79] The group achieved further purification using a sublimated film of yttrium, employing a process patented by Padamsee, or titanium, using a method developed by Kneisel, who had joined the Cornell group.[80]

To combat field emission problems from impacting electrons from emitters, researchers at the University of Geneva and elsewhere—under the initiative of the CERN SC RF group—began dc field emission experiments. This work would lead by the mid-1980s to the conclusion

---

[76]W. Weingarten, "Superconducting Cavities" in *Proceedings of the 15th International Conference on High Energy Accelerators*, Vol. II (Singapore: World Scientific, 1993), p. 679.

[77]Quotations, respectively, from H. Lengeler, "Recent Developments in Superconducting Linac Structures" in *Proceedings of the 1986 Linear Accelerator Conference*, SLAC, June 2–6, 1986, p. 188, and private communication with Alan Schwettman, June 15, 1995.

[78]Helmut Piel, "Superconducting Accelerating Structures for High Energy Accelerators," in *Proceedings of the 1984 Linear Accelerator Conference*, Seeheim, Germany, May 7–11, 1984, p. 262.

[79]Ronald Sundelin, "High Gradient Superconducting Cavities for Storage Rings," 1985 Particle Accelerator Conference, *IEEE Trans. Nucl. Sci.* **NS-32** (1985), p. 3571.

[80]H. Padamsee in *Proceedings of the Second Workshop on RF-Superconductivity*, Geneva, Switzerland, July 23–27, 1984, p. 339; H. Padamsee, U.S. Patent No. 4,487,637 (1981); P. Kneisel, Cornell Laboratory of Nuclear Studies, Internal Report SRF-840702 (1984).

that dust and other foreign material were emitting sites and that emitter densities on niobium could be reduced by high-temperature annealing.[81]

Better manufacturing procedures also helped solve other problems, such as vacuum voids, a chief cause of thermal breakdown diagnosed by the Cornell group. The problem was caused by the standard electron-beam-welding technique used in fabricating cavities. During cavity fabrication, a focused beam produces a vapor column in the metal while welding, and the weld puddle solidifies with a vacuum bubble present. This bubble immediately below the cavity surface impacts cavity performance by interfering with heat transport. A similar bubble is sometimes opened by chemical processing which leaves sharp edges that enhance fields. Cornell researchers solved the problem using the rhombic raster (4 and 5 kHz) welding technique. The electron beam used for welding is scanned in one direction at 4 kHz and in the other at a mixture of 4 and slightly more than 5 kHz. The rhombic raster thus generated penetrates the material by less than 10 microns. Niobium welded in this fashion is almost indistinguishable from the surrounding metal. Similar results can be obtained with a highly defocused beam, a method first used by Lengeler.[82] Along with researchers at KEK and elsewhere, the Cornell group developed numerous other specialized manufacturing procedures, including improved surface inspection methods, ways to reduce the risk of chemical residues on surfaces by rinsing with demineralized water, and a method of attaching cavities to test stands in dust-free enclosures so that less dust would enter the cavity during mounting.[83] Lengeler stressed the importance of painstaking "clean room assemblies" in the production of workable cavities.[84]

Although researchers experienced difficulties in finding the right cavity shape and manufacturing procedures and developing high-quality niobium, higher-order modes arguably presented the biggest challenge. Sundelin considers the efforts to suppress HOMs as "probably

---

[81]W. Weingarten, "Superconducting Cavities" in *Proceedings of the 15th International Conference on High Energy Accelerators*, Vol. II (Singapore: World Scientific, 1993), p. 679; Ph. Nidermann, N. Sankarraman, R. J. Noer, ø. Fishe, "Field Emission from Broad Area Niobium Cathodes: Effects of High Temperature Treatment," *J. Appl. Phys*. **59** (1986), p. 892.

[82]H. Padamsee, *Proceedings of the Second Workshop on RF-Superconductivity*, Geneva, Switzerland, July 23–27, 1984, p. 339.

[83]Ronald Sundelin, "High Gradient Superconducting Cavities for Storage Rings," 1985 Particle Accelerator Conference, *IEEE Trans. Nucl. Sci*. **NS-32** (1985), p. 3571; W. Weingarten, "Superconducting Cavities" in *Proceedings of the 15th International Conference on High Energy Accelerators*, Vol. II (Singapore: World Scientific, 1993), p. 679.

[84]H. Lengeler, "Recent Developments in Superconducting Linac Structures," in *Proceedings of the 1986 Linear Accelerator Conference*, SLAC, June 2–6, 1986, p. 193.

the biggest part" of the job of developing the Cornell cavity. Major efforts to find ways to suppress HOMs were mounted at CERN and elsewhere. At Cornell, several researchers "worked ... for well over a year to identify all of the important higher-order modes and to make sure that they were adequately coupled." The Cornell work, which built on work done at HEPL, resulted in the use of waveguides on coaxial HOM couplers. Extraction of some HOMs through the fundamental power waveguides was also used.[85]

Revived Hope at the Eleventh Hour: Demonstrations of Maturity, 1982 to 1985

As the SRF research and development effort continued, from 1982 to 1985 evidence for the success of the technology began to trickle in, just a little too late to affect the ongoing decision-making of those planning the new electronuclear accelerator. In 1982, after several years of development work, the five groups vying for funding made the final touches on their proposals and submitted them to the Department of Energy just as the first decisive SRF cavity tests were performed. In April 1982 the first demonstration of a significant accelerating gradient came with the Cornell test of two five-cell, 1500 MHz niobium muffin-tin cavities.[86] In the same month, a single-cell 500 MHz "DORIS" cavity was tested at DESY's PETRA 21 GeV/beam e^+e^- storage ring. Still later in the spring a five-cell, 500 MHz cavity was tested in PETRA by the CERN-DESY collaboration.[87] When the Bromley panel chose SURA's PSR design in 1983, however, the 5 MV/m milestone for a practical SRF cavity had not been achieved. Compelling evidence for the maturity of SRF came only with a series of multicell tests performed at Cornell, CERN, DESY, and KEK from 1983 to 1985, tests that were performed as SURA continued plans

---

[85]Quotation from interview with Ronald Sundelin, March 24, 1994. Also: H. Lengeler, "Recent Developments in Superconducting Linac Structures" in *Proceedings of the 1986 Linear Accelerator Conference*, SLAC, June 2–6, 1986, p. 192; Ronald Sundelin, "Joints, Couplers, and Tuners" in *Proceedings of the Workshop on RF Superconductivity*, Karlsruhe, July 2–4, 1980, pp. 249–252; A. M. Vetter, Stanford Tech. Note, HEPL TN 80-2.

[86]R. Sundelin, J. Amoto, S. Herb, J. Kirchgessner, P. Kneisel, J. Mioduszewski, N. Mistry, K. Nakajima, H. Padamsee, F. Palmer, H. L. Phillips, M. Pickup, R. Siemann, M. Tigner, and E. von Borstel, "Superconducting Cavity Beam Test in CESR," *IEEE Trans. Nucl. Sci.* **NS-30** (1983), pp. 3333–3335.

[87]W. Bauer, A. Brandelik, A. Citron, F. Graf, L. Szecsi (Karlsruhe) and D. Proch (DESY, Hamburg), "Operation of a Superconducting Accelerating Cavity in PETRA," *IEEE Trans. Nucl. Sci.* **NS-30** (1983), pp. 3333–3335.

for building a machine that had triumphed over competitors in large part because of its reliable conventional technology.[88]

One public announcement of the progress made from 1983 to 1985 came in May 1985, just as Hermann Grunder was assuming the CEBAF directorship. In a Particle Accelerator Conference talk that month Sundelin, who would later head the CEBAF SRF group, reported that since August 1983 "four superconducting cavities [had] been tested in storage rings" and that "the average gradient obtained in these four tests was 4 MeV/m, almost twice the average obtained in the four cavities tested in storage rings during 1982–1983." Under laboratory conditions, eight cavities designed for storage ring use exceeded 5 MV/m. The laboratory work included a Cornell test on a 1.5 GHz elliptical cavity system, which achieved record accelerating fields in multicell structures—15.3 MV/m in a fully equipped five-cell cavity coming close to the cavity specifications required for superconducting colliders. Most significantly, one of the four cavities that underwent beam testing—a cavity tested in November 1984 in CESR at Cornell—reached an accelerating gradient of 6.5 MV/m, thus breaking the 5 MV/m barrier under actual beam conditions for the first time.[89] (See Table 2.)

Table 2
Laboratory Results on Multi-cell Cavities Designed for Storage Ring Use

Lab	Yr.	Freq. (MHz)	Cells	E_{max} (MeV/m)	$Q \times 10^{-9}$	E (MeV/m)	Coupling holes	Couplers	Limitation
CERN	83	500	5	5.0	0.74	5.0	Yes	No	Defect,* main coupling hole
CERN	85	352	4	6.0	3.3	5.0	Yes	No	
Cornell	84	1500	5	8.9	7.	8.9	Yes	Yes	Defect, location undetermined
Cornell	84	1500	5	8.0	3.	8.0	Yes	Yes	Defect, cell 3, 2.5 cm from eq.
Cornell	84	1500	5	15.3	2.	15.3	Yes	Yes	Defect or field emission, cell 1
DESY	83	1000	9	6.6	0.9	6.6	No	No	Defect or electron loading
DESY	83	1000	9	>6.7	0.9	6.7	No	No	Available power
KEK	83	508	3	>5.2	0.5	4.0	Yes	No	Available power

* Spots of high resistivity, such as weld imperfections, inclusions, impurities, or surface contamination.

Source: CEBAF, *Scientific and Technological Assessment Report* (STAR) *on the Superconducting CW Linac Design for CEBAF*, November 1985.

[88]Ronald Sundelin, "High Gradient Superconducting Cavities for Storage Rings," 1985 Particle Accelerator Conference, *IEEE Trans. Nucl. Sci.* **NS-32** 1985, p. 3571; CEBAF, *Scientific and Technological Assessment Report (STAR) on the Superconducting CW Linac Design for CEBAF*, November 1985, p. 4-3.

[89]Ronald Sundelin, "High Gradient Superconducting Cavities for Storage Rings," 1985 Particle Accelerator Conference, *IEEE Trans. Nucl. Sci.*, **NS-32**, pp. 3570–71.

The 1984 Cornell beam test held particular promise for CEBAF. Cornell researchers had demonstrated that they had the technological capability and experience necessary to create cavities with the required *Q* and gradient and that beam could be accelerated through a series of such cavities reliably over time. In the process they demonstrated that the scourge of regenerative beam breakup caused by HOMs had been overcome. As later described, the Cornell device is a "five-cell, 1500 MHz, niobium accelerating cavity." It "has an active length of 0.5 meters," uses "rectangular waveguides for input coupling, and has two waveguide high-order-mode output couplers oriented perpendicularly to one another and to the beam axis. All coupling is done along the beam line outside of the cavity itself to avoid disrupting the field pattern within the cells. The inner surface of the five cells, when looked at in axial cross section, is composed of elliptical segments."[90] (See Figure 2.) In Sundelin's words, since this cavity had been created with the hope of achieving the relatively high beam current needed for an electron storage ring, using the design at CEBAF was "duck soup."[91]

Thus, at the eleventh hour—with Grunder on the way to Newport News with the intent to build the PSR—thanks to perseverance and international collaboration, the technical problems preventing an SRF electronuclear accelerator had been solved. Grunder's challenge, after discovering the news, would be to champion the technology so that CEBAF could overcome the remaining obstacles to the development of such a machine: the continuing skepticism of physicists and Department of Energy officials, who were still in the sway of the technology's long and sometimes painful past.

---

[90]CEBAF, *Scientific and Technological Assessment Report* (STAR) *on the Superconducting CW Linac Design for CEBAF*, November 1985, p. 3-7.

[91]Ironically, by this time plans had been dropped for Cornell's CESR II storage ring, the project for which the cavities had originally been developed. CESR II was abandoned because top priority was placed on funding for the Superconducting Super Collider, which was subsequently canceled in 1993. Interview with Ronald Sundelin, March 24, 1994.

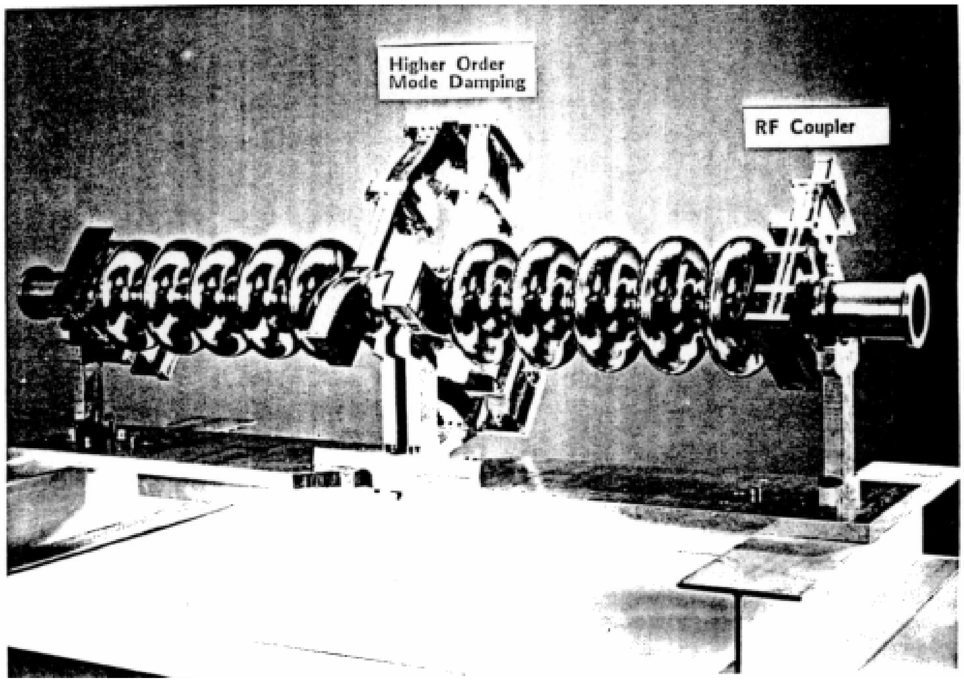


Figure 2. A pair of CEBAF-Cornell cavities as depicted in CEBAF, *Scientific and Technological Assessment Report* (STAR) *on the Superconducting CW Linac Design for CEBAF*, November 1985.