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The Founding of CEBAF, 1979 to 1987

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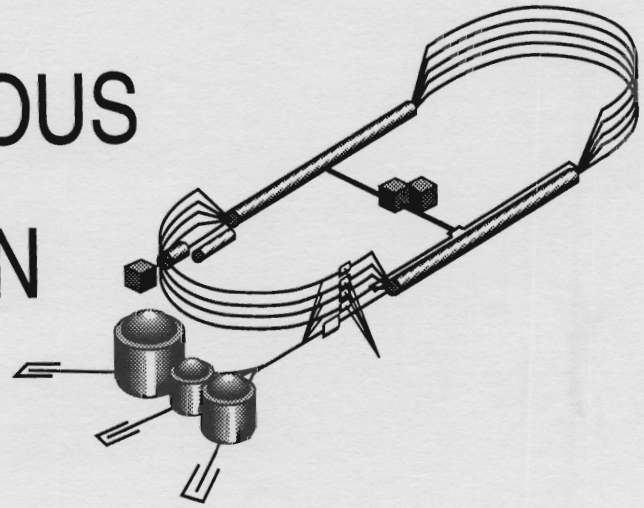

CONTINUOUS
ELECTRON
BEAM
ACCELERATOR
FACILITY

SURA ***Southeastern Universities Research Association***

CEBAF
The Continuous Electron Beam Accelerator Facility

Preface

In separate conversations at Fermilab in summer 1990, Beverly Hartline told two of my colleagues on the Fermilab history team — Mark Bodnarczuk and Lillian Hoddeson, the Fermilab historian — that CEBAF was interested in developing a history program. After hearing about my qualifications, Hartline suggested that I call Steven Corneliussen, who had conceived plans for a CEBAF history program the previous year. After receiving information from Corneliussen, CEBAF Director Hermann Grunder asked me to direct the program. With further help from Corneliussen, who had already formulated a solid, preliminary plan based on his experience with NASA history, in 1991 I joined the laboratory as consulting historian. While expanding the CEBAF archives and creating for it a library of oral history interviews, I wrote this history of how CEBAF was founded. This history, and the history program from which it arises, contributes to the growing effort to preserve, record, and better understand the history of Department of Energy laboratories.[1]

As a professional historian employed by the institution she is studying, I am obliged to explain my methodology and the conditions under which I work.[2] When writing this history I first sought information from letters, technical reports, meeting descriptions, and other historical documents. Elois Morgan, the CEBAF librarian, had already collected some material from the early days of the laboratory. James McCarthy, Hans von Baeyer, Franz Gross, Harry Holmgren, and others instrumental in the founding of CEBAF readily provided additional letters, technical reports, meeting summaries, and notes. From these documents I pieced together a chronology of events. With my chronology in hand, I conducted a series of interviews to learn more about what happened and to gather information that would help me evaluate the historical record. Since I had access primarily to documents and individuals from the Southeastern Universities Research Association (SURA), the

[1]Book-length DOE laboratory histories include J. L. Heilbron and Robert W. Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory* Vol. I (Berkeley: University of California, 1989) and Lillian Hoddeson, Paul W. Henriken, Roger A. Meade, and Catherine Westfall, *Critical Assembly: A Technical History of Los Alamos During the Oppenheimer Years, 1943–1945* (Cambridge: Cambridge University Press, 1993).

[2]American Historical Association, "Statement on Standards of Professional Conduct."

group that won the bid to design the accelerator, my investigation focused on their efforts, which I set into the wider context of scientific, technical, and government decisionmaking, as revealed in available (mostly published) sources. I next sent a series of drafts to participants and historians, successively refining my understanding and interpretation of events on the basis of criticism, suggestions, and new information.

While writing this history I enjoyed complete intellectual freedom. Although Grunder and others on the CEBAF staff provided information and opinions for my consideration, I drew my own conclusions. Any errors in judgment or fact are therefore mine.

Many people contributed to my initial effort to preserve and document CEBAF history. In particular, I would like to thank Larry Cardman and those already mentioned who provided documents on the history of CEBAF. I am also indebted to Elois Morgan for documents and her help in locating information and to Steve Corneliussen and Curtis Brooks for unflagging intellectual, editorial, and collegial support. Many thanks also go to Patricia Stroop, Donna Lewis, and the rest of the Director's Office staff for performing all the necessary administrative chores so that I could work productively. I would also like to thank my reviewers: Sam Austin, Joan Bromberg, Larry Cardman, James Coleman, Peter Demos, Franz Gross, Beverly Hartline, Lillian Hoddeson, Harry Holmgren, Nathan Isgur, Robert Johnson, Stanley Kowalski, Christoph Leemann, James Leiss, Gerald Peterson, Robert Seidel, William Turchinetz, Hans von Baeyer, Roy Whitney, and Richard York. My thanks also go to Anne Stewart, Henry Whitehead, and Stephen Hickson for help in finding and obtaining photographs. Finally, I am grateful for the help of Gary Westfall, who cheerfully produced all the figures in this paper and did numerous computer-related chores, including production of all drafts of the manuscript.

All documents and oral history interview transcripts cited are in the CEBAF archives, Newport News, Virginia. All correspondence cited resides in the papers of James McCarthy, located in the CEBAF archives. All interviews and conversations were conducted by the author, unless otherwise specified.

Catherine Westfall
Newport News, Virginia
September, 1994

THE FOUNDING OF CEBAF, 1979 TO 1987

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1 Genesis of the Proposal, 1970 to Fall 1982

In early 1979 a group of physicists assembled at the University of Virginia (UVa) for a conference entitled "Future Possibilities for Electron Accelerators." In the audience sat an organizer of the conference, UVa professor James McCarthy. While listening to talks by Gregory Loew of the Stanford Linear Accelerator Center (SLAC) and Roger Servranckx of the University of Saskatchewan, McCarthy got very excited. Both discussed new approaches to producing an almost continuous stream of electrons with improved designs for pulse stretcher rings that could be built within a reasonable budget. McCarthy saw the possibility of realizing a dream.[1]

This dream had its origins in the 1950s, when Robert Hofstadter, McCarthy's thesis advisor, made groundbreaking discoveries at Stanford's High Energy Physics Laboratory (HEPL) about the internal structure of nuclei and nucleons. For these experiments Hofstadter used Mark III, the most advanced in a series of electron accelerators designed by William Hansen, who pioneered methods of high frequency acceleration of electrons.[2] The work by Hofstadter and Hansen led to two productive lines of inquiry. One

[1] Interview with James McCarthy, December 11, 1991.

[2] Hofstadter won the Nobel Prize in 1961 for his work on the internal structure of the nucleon or nuclei. The major articles from the Hofstadter era are collected in Robert Hofstadter, *Electron Scattering and Nuclear and Nucleon Structure* (New York: Benjamin, 1963). Also see: Robert Hofstadter, "A Personal View of Nucleon Structure as Revealed by Electron Scattering," in Laurie Brown, Max Dresden, and Lillian Hoddeson (eds.), *Pions to Quarks: Particle Physics in the 1950s* (Cambridge, Mass.: Cambridge University Press, 1989) pp. 126–143. For more information about Hansen's pioneering work, which began in the 1930s, see John Blewett, "The History of Linear Accelerators," in Pierre M. Lapostolle and Albert L. Septier (eds.), *Linear Accelerators* (Amsterdam: North-Holland,

group of researchers studied particle production using electrons at higher energies, which led to the construction in the 1960s of SLAC at Stanford. Another group of researchers, which included McCarthy, investigated nuclear structure with more modest increases in energy accompanied by increases in the duty factor[3] of the electron beam. This line of inquiry, electro-nuclear physics, led in the 1960s and 1970s to a succession of accelerators, including a $7.2 million high duty factor 400 MeV linear accelerator (linac) completed in 1972 at the Bates Laboratory at the Massachusetts Institute of Technology (Bates-MIT), and ambitious attempts to develop untried technologies to further boost energy and duty factor, most notably the effort to develop superconducting radiofrequency (srf) accelerating technology at HEPL.[4]

By 1979 electro-nuclear physics had attracted a considerable following.[5] The growing electro-nuclear physics community was eager to find a scheme to permit virtually continuous acceleration, which would greatly improve the capability of performing coincidence experiments.[6] In the words of the UVa

1970) pp. 1–17 and 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, 1992) pp. 47–77.

[3]Duty factor is the ratio of the time the beam is on to the elapsed time.

[4]U.S. Department of Energy, "Nuclear Physics Accelerator Facilities" (Washington D.C.: U. S. Department of Energy, 1985) p. 17. Electrons are useful probes because they interact with matter with the best understood elementary process, pass through nuclei without distorting features of nuclear structure, and respond sensitively to local current and charge densities. The interest in high duty factor accelerators originated with the realization that such machines could detect, along with the scattered electron, one (or more) of the reaction products. Doing so allows a more complete reconstruction of the interaction, and as a consequence, more detailed determinations of the internal structure of nuclei and nucleons.

[5]Growing interest in the field can be measured by the increased attention paid to the field in panels. See: 1966, National Academy of Sciences, National Research Council, "Physics Survey and Outlook"; 1972, National Academy of Sciences, Nuclear Physics Panel of the Physics Survey Committee; 1974, AEC/NSF, "Report to the Atomic Energy Commission and the National Science Foundation"; 1977, National Academy of Sciences, "Future of Nuclear Science," G. Friedlander, chairman; 1977, Department of Energy/National Science Foundation, "Role of Electron Accelerators in U.S. Medium Energy Nuclear Science," R. S. Livingston, chairman.

[6]Previously, accelerators in this energy range typically used pulsed beams instead of continuous beams. In a pulsed beam, all the particles come close together in time, making very high instantaneous reaction rates. Between pulses there are no beam particles at all

conference proceedings, this experimental capability promised to "open entire new areas of nuclear physics." Convinced that he could be the one to design the necessary groundbreaking machine after hearing the ideas of Loew and Servranckx, McCarthy began gathering a small accelerator building team.[7]

McCarthy was an unlikely candidate for the job — although he was an experienced experimentalist, he was, in his own words, "basically an amateur" when it came to accelerator building.[8] From the 1930s to the 1950s, experimentalists routinely conceived accelerator projects. For example, Nobel Laureates Edwin McMillan and Luis Alvarez led the construction of accelerators and then used them for research at the first major U.S. accelerator laboratory, built by Ernest O. Lawrence in Berkeley.[9] By the 1960s, however, major accelerators like the one McCarthy planned were usually conceived and constructed by physicists who had devoted their careers to the art and science of accelerator building. Against all odds, McCarthy's pipe dream resulted in the construction of a major accelerator laboratory, the Continuous Electron Beam Accelerator Facility (CEBAF).[10] The founding of CEBAF is a tale of luck, perseverance, the triumph of flexible amateurism over rigid

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and the detectors sit idle waiting for reactions. In the high duty factor environment of a continuous beam, the particles are spread out in time, making a much lower instantaneous rate in the detectors for the same total beam current. Electro-nuclear physicists were particularly interested in mounting coincidence experiments, those in which more than one particle is detected in each event, since such experiments could provide important information. This type of experiment can only be done efficiently in a high duty factor environment due to the accidental coincidence counting rate limitations experienced by detectors in a low duty factor environment. Interviews with James McCarthy, December 11, 1991, Franz Gross, October 16, 1991, Larry Cardman, November 4, 1991, and James Leiss, December 9, 1991.

[7]Quote from University of Virginia, *Proceedings: Conference on Future Possibilities for Electron Accelerators* (Charlottesville: University of Virginia, 1979) p. iii. Interview with James McCarthy, December 11, 1991.

[8]Interview with James McCarthy, December 11, 1991.

[9]McMillan won the Nobel Prize in 1951; Alvarez won it in 1968. For information on Lawrence's laboratory before World War II, see John Heilbron and Robert Seidel, *Lawrence and His Laboratory* (Berkeley: University of California Press, 1989).

[10]The new Virginia laboratory had two names. It was originally called the National Electron Accelerator Laboratory (NEAL), but in June 1983 the name was changed to the Nuclear Science Electron Accelerator Laboratory (also NEAL), since an advisory board to the DOE had recently advised against forming new national laboratories to save funds for support of existing facilities. In July 1983, DOE renamed the laboratory CEBAF. To avoid confusion, CEBAF is called by its current name throughout.

professionalism, and ironically, the potential of amateurs when supported by a thoroughly professional international network with well-defined methods for organizing and building accelerators. The CEBAF tale also has a surprise ending, for at the last minute, McCarthy's pipe dream was radically transformed by Hermann Grunder, who would direct the construction of the project. The twists and turns of this tale reveal many lessons about what aids and what detracts from the success of a large, federally sponsored scientific project.

Plans for future accelerators and accelerator improvements were also being discussed in the heavy ion and meson nuclear physics communities by 1979. Which of these projects would best advance nuclear science? Answering this question was the job of the newly formed Nuclear Science Advisory Committee (NSAC), which had been formed by the two major funding agencies for physics, the National Science Foundation (NSF) and the Department of Energy (DOE). NSAC was modelled on the High Energy Physics Advisory Panel (HEPAP), a permanent committee created in 1967 to formalize the tradition of ad hoc advisory committees that had been convened since the 1950s.[11] Fortuitously, those in favor of an electro-nuclear accelerator had no competition: neither meson nor heavy ion physicists wanted to build a new facility immediately. In its December 1979 report, NSAC, then chaired by Herman Feshbach of MIT, gave its stamp of approval to the proposed new machine and suggested the construction of an intermediate energy machine in 1981 and the construction in 1985 of an accelerator in the 1 to 2 GeV range, which would be a "national facility," the first such facility in the electro-nuclear physics community.[12]

Encouraged by the NSAC recommendation and the enthusiastic support from such eminent theorists as J. Dirk Walecka and Stanley Brodsky, groups around the country began to compose proposals for a new machine.[13] Four of the groups — the National Bureau of Standards (NBS), the University of Illinois, Argonne National Laboratory (ANL), and MIT-Bates — were established centers of electro-nuclear physics with machine building expertise and affiliated researchers. MIT-Bates, which had a sizable and well-known re-

[11] At first, the Committee was called "NUSAC." For the sake of consistency, the later name NSAC is used throughout. NSAC was formed in 1978.

[12] NSAC report, "A Long Range Plan for Nuclear Science," December 1979.

[13] Interviews with James McCarthy, December 11, 1991; Richard York, November 9, 1993; Larry Cardman, November 4, 1991; and J. Dirk Walecka, September 25, 1990.

search community and the most powerful electro-nuclear physics accelerator in the U.S., was considered by most to be the frontrunner.[14]

The group of amateurs that McCarthy had formed to design his dream machine was considered the dark horse of the competition. McCarthy immediately took steps to offset the group's lack of accelerator building experience. He hired two young physicists, Blaine Norum and Richard York, who were willing to undergo the training necessary to become accelerator experts. Norum was sent to Saskatchewan to learn the details of Servranckx's design, then to Stanford Linear Accelerator Center (SLAC), where he was soon joined by York. In York's words, they used SLAC "as a skills resource." According to York, working at SLAC was "magic," thanks to the collegial environment shaped by Wolfgang "Pief" Panofsky. Experts, such as Loew, graciously advised the two young men about how to proceed with the design, answered questions, and tracked down specialized information.[15]

Hans von Baeyer, professor at the College of William and Mary (W&M) and newly appointed director of the Virginia Associated Research Campus (VARC), heard about McCarthy's plans. Von Baeyer had been looking for a use for the Space Radiation Effects Laboratory (SREL), the former NASA cyclotron facility at VARC in Newport News, Virginia. Von Baeyer and his W&M colleague Robert Siegel realized that VARC and the SREL building "would be a logical site" for McCarthy's accelerator. By now excitement was mounting, both at W&M and UVa. Other physicists from both universities, including W&M theorist Franz Gross, joined von Baeyer and McCarthy to discuss plans for a new accelerator laboratory, which came to be called the National Electron Accelerator Laboratory (NEAL). The group quickly realized that the new facility would be large and important enough to serve a national community of researchers. In von Baeyer's words, everyone agreed that "one university couldn't manage this thing," because "this was big science."[16] To obtain the manpower, financial resources, and political

[14]By the early 1980s, Bates would house a 400 MeV, 1.8% duty factor linac and a 720 MeV single-turn recirculator. DOE/NSF Nuclear Science Advisory Committee, "Report of the Panel on Electron Accelerator Facilities," April 1983, p. 46.

[15]Another young physicist, John Sheppard, worked for McCarthy at SLAC for a short time. Interview with Richard York, November 9, 1993; Roger Servranckx to James McCarthy, March 29, 1979; and James McCarthy to F. Netter, June 3, 1980.

[16]Quotes from interview with Hans von Baeyer, October 15, 1991. Also: Hans von Baeyer, "Proposal to Use the SREL Site for the National Electron Accelerator Labora-

support necessary to plan such a facility from scratch, the group decided to form a consortium of universities, extending the tradition begun in the 1940s, when the Associated Universities Incorporated (AUI) was formed to manage Brookhaven National Laboratory, and continued in the 1960s, when the Universities Research Association (URA) was founded to manage Fermi National Accelerator Laboratory (Fermilab).

From the beginning the group demonstrated a characteristic willingness to draw on outside help. For general pointers, von Baeyer called on Norman Ramsey, who had helped form both AUI and URA. The URA by-laws were used as an organizational model. To obtain local support for the project, McCarthy convened a breakfast meeting at a local Howard Johnson's restaurant that included Virginia State Senator Elmon Gray and other legislators as well as representatives from UVa, Virginia State University, and W&M.[17] McCarthy's description of ongoing plans inspired enthusiastic support from UVa president Frank Hereford, UVa graduate school dean Dexter Whitehead, and W&M president Thomas Graves, who began to recruit other university presidents to the cause. In the meantime, Gray successfully obtained "seed money" to start the consortium.[18]

While momentum built for organizing the consortium, the accelerator building effort also continued to draw on outside expertise. In addition to the training York and Norum received at SLAC, McCarthy's group received help from a number of other accelerator building experts. Since McCarthy had to devote his time to leading the accelerator design effort, preparation of the scientific justification for the project was led by Gross. A scientific advisory panel, convened by the eminent particle physicist Robert Marshak, provided general advice. McCarthy also periodically called on accelerator building experts, including microtron expert Helmut Herminghaus from Mainz, the well-known Cornell physicist Maury Tigner, and Robert Wilson, who gained considerable large scale accelerator experience while constructing Fermilab.[19]

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tory," November, 1980.

[17]Frank Hereford to Terry Sanford, June 3, 1980; interviews with Hans von Baeyer, October 15, 1991; Harry Holmgren, "SURA's First Decade."

[18]For example, McCarthy's files reveal that in June and July 1980, Hereford alone sent 28 letters to university presidents asking them to join the consortium or providing information about it. Interviews with Hans von Baeyer, October 15, 1991, and James McCarthy, December 11, 1991; Harry Holmgren, "SURA's First Decade."

[19]McCarthy's files are full of correspondence between SURA representatives and ex-

By fall 1980, plans for the consortium fell into place. In October Hereford, Whitehead, and representatives from nine other universities met and voted to form the Southeastern Universities Research Association (SURA). At the organizational meeting of the board of trustees held in December, representatives from 13 founding universities chose board members. Harry Holmgren was elected president, McCarthy vice president, von Baeyer secretary, and Dana Hamel treasurer.[20] Von Baeyer noted that Hamel was picked because he was "a very distinguished man in Virginia education circles." In addition, according to McCarthy, "his knowledge of the intricacies of Richmond was invaluable in developing the project."[21] Holmgren, a distinguished nuclear physicist from the University of Maryland, was a good choice for president because of his experience and because he helped to counterbalance the preponderance of Virginia researchers. The four were, in von Baeyer's words, the "four Musketeers." The group felt the project was a long shot — Holmgren remembers guessing that "the chance of success ... was something in the order of three to five percent." Nonetheless, von Baeyer remembers that monthly meetings were full of "high spirits and camaraderie."[22]

Plans for the accelerator also progressed quickly, in large part, according to group members, because of their congenial working relationship. Since McCarthy had little professional interest in the details of accelerator design — he simply wanted a workable accelerator with improved capabilities for electro-nuclear physics research — he gave York and Norum a great deal of freedom, which they enjoyed.[23] For his part, McCarthy liked working with York and Norum because they were young, resourceful, and enthusiastic. "The attitude [was] ... if it hasn't been done before [it's] a challenge." As

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perts. For example, see Hans von Baeyer to Robert Hofstadter, January 21, 1981, Harry Holmgren to Robert Wilson, July 9, 1981, Maury Tigner to Harry Holmgren, June 26, 1981 and James McCarthy to Helmut Herminghaus, March 15, 1982. Interviews with Hans von Baeyer, October 15, 1991, and Harry Holmgren, October 12, 1991; "SURA's First Decade."

[20] "South Surprises Scientific Circles," *Daily Press*, Newport News, Virginia, September 3, 1991; SURA Executive Board Minutes, December 19, 1980.

[21] Interviews with Hans von Baeyer, October 15, 1991, and James McCarthy, December 11, 1991.

[22] Interviews with Hans von Baeyer, October 15, 1991, and Harry Holmgren, October 12, 1991.

[23] Interview with Richard York, March 25, 1994.

young men, York and Norum were "willing to talk to anybody and make mistakes, try anything and work very hard."[24]

As technical work continued in 1981 and 1982, the "four Musketeers" promoted the project to citizens' groups, to politicians, to university presidents, and to physicists, who were in turn asked to lobby for the project within their own universities. "All of us made innumerable speeches; I was talking to Rotary and Kiwanis clubs, engineering professional groups and legislative groups in Richmond," von Baeyer explained. A June 1981 letter from Holmgren to Hereford outlined a strategy for "developing political support at the national level" for the project by lobbying the Virginia congressional delegation and by exploiting Hereford's "personal contacts within the DOE and the NSF." By 1982, the group had gained the attention of Virginia Republican Senator John Warner, who added a note in his own hand on a July letter: "I'm seriously interested." Another key supporter was Virginia's Secretary of Education, John Casteen, who helped arrange a meeting between SURA representatives and Virginia Democratic Governor Charles Robb on November 5, 1982. Von Baeyer remembers that although it took him and Hamel a year to get "the ear of Governor Chuck Robb ... once we had it, we had it [and] he caught fire." By fall 1982, McCarthy confided to Herminghaus: "I believe that SURA has the strongest political support of all the competitors."[25]

A key to SURA's success was the decision, made early on, to keep the accelerator a regional effort. The notion of a regional facility matched two converging agendas. Von Baeyer remembered "a lot of moaning and groaning" among physicists. Even though physicists from the Southeast felt their universities had by this time developed the intellectual and financial resources for supporting a major facility, the region "never got any of the goodies," which went instead "to the North and the West, and Midwest."[26] Having a regional facility also fit with the agenda of politicians,who readily exploited

[24]Interview with James McCarthy, December 11, 1991.

[25]Quotes from interview with Hans von Baeyer, October 15, 1991, Harry Holmgren to Frank Hereford, June 4, 1981, and James McCarthy to Helmut Herminghaus, October 21, 1982. Also: John Warner to Hans von Baeyer, July 8, 1982, Hans von Baeyer to John Casteen, November 9, 1982.

[26]Interview with Hans von Baeyer, October 15, 1991.

this evidence of "the impoverished Southeast" as a way of obtaining greater resources for their constituents.[27]

The proposals for the electro-nuclear physics accelerator that emerged in 1982 presented competing technologies and accelerator building goals. MIT-Bates and SURA each wanted to build a linac coupled with an electron storage ring operating as a pulse stretcher. In this scheme, electrons are accelerated, then injected into a ring, and extracted slowly and uniformly so that a continuous beam is produced. The MIT-Bates accelerator included a linac with a recirculator; the SURA accelerator combined a single-pass linac and a pulse stretcher ring. Illinois, ANL, and NBS wanted to build different types of racetrack microtrons, or electron cyclotrons, in which electrons are accelerated through a linac and recirculated using a beam transport system designed so that the beam is synchronous and continuous. The proposed NBS effort was an extension of an ongoing R&D project started at the instigation of noted electro-nuclear physicist James Leiss to explore the potential and evaluate the difficulties of microtron technology, including the problem of recirculating beam breakup, a primary obstacle to the improvement of electron accelerators.[28]

SURA and ANL wanted to build 4 GeV electron machines, while MIT-Bates, Illinois, and NBS wanted to build machines that would accelerate

[27]In the early 1960s a similar coalition was formed in the Midwest, between the Midwestern Universities Research Association, a group of physicists who felt they had been unfairly deprived of a first-class facility, and politicians who felt their region had not received its fair share of federal research funding. For more information on politics and its effect on academia in the Midwest, see Leonard Greenbaum, *A Special Interest* (Ann Arbor: University of Michigan Press, 1971). For information on MURA and its effect on the founding of Fermilab, see Catherine Westfall, "The Site Contest for Fermilab," *Physics Today* 42 (1989), pp. 44–52.

[28]Illinois wanted to build a system of three microtrons that would operate in a cascade. ANL wanted to build a modified microtron design to provide "a very high energy gain per turn (105 MeV) and a large orbit separation (17 cm) without requiring an accelerator mode number greater than 1, long accelerating field wavelengths, or unreasonably high dipole fields in the microtron beam transport system." In this way, the microtron concept could be "extended to energies not previously considered possible." NBS wanted to build a second 1 GeV microtron "using technologies developed for the first stage" of their R&D project. Nuclear Science Advisory Committee, "Report of the Panel on Electron Accelerator Facilities," April 1983. Quotes from pp. 35, 51. Also: pp. 22–26, 41, 46, 51, 55; interview with Larry Cardman, November 4, 1991.

electrons to 2 GeV or less.[29] The 4 GeV electron accelerator offered the possibility of opening a new frontier, the study of the role of quarks in nuclei.[30] Although the group at MIT-Bates and their supporters agreed that a higher-energy machine should be built eventually, they felt a lower energy machine should be built first. As Peter Demos, who headed MIT-Bates in 1982, recently summarized: "The MIT philosophy was that it is best to proceed methodically in modest steps determined by a confident estimate of the physics requirements and the available resources. From our point of view, 2 GeV, not 4 GeV, was the next reasonable step." MIT-Bates group members advanced a number of arguments to support their claim. In the words of MIT-Bates researcher Stanley Kowalski, "there was plenty of beautiful physics to do" in the 2 GeV range. In particular, as his colleague T. W. Donnelly noted at the time, this energy range "would permit a significant fraction of the few-body, complex nucleus and hypernucleus electromagnetic nuclear physics" identified as having top priority within the electro-nuclear physics community. Also, existing facilities at MIT could be modified to reach such energies. The MIT-Bates group also argued that the immediate construction of a 4 GeV electron accelerator would encourage a rush of poorly conceived experiments at the expense of the careful, precise techniques and measurements that would truly advance the field.[31]

[29] As Larry Cardman, leader of the University of Illinois group, points out, although Illinois and NBS proposed machines with energies 1 GeV and below, both groups saw their accelerators as complementary in capability to the 4 GeV machine, which they hoped would be built immediately, alongside their projects. Interview with Larry Cardman, March 13, 1991.

[30] Although the high energy physics community had accelerators with much higher energies, for example the 50 GeV SLAC machine, with their lower duty factor, some critical coincidence experiments would be difficult and others would be impossible to perform on these accelerators.

[31] Quotes, respectively, from telephone conversation with Peter Demos, June 22, 1994, interview with Stanley Kowalski, October 25, 1991, and T. W. Donnelly, "Conclusions To Be Drawn From The Workshop on Future Directions in Electromagnetic Physics," in the *Proceedings of the Workshop on the Use of Electron Rings for Nuclear Physics Research in the Intermediate Energy Region*, Lund, Sweden, 1982. The latter document is in the G. A. Peterson Papers, University of Massachusetts, Amherst, MA. The author wishes to thank Dr. Peterson for this document. Also: MIT, "Addendum to the MIT Proposal," March 1983; SURA, "Response to the MIT-Bates Addendum of March 3," March, 1983; H. Jackson to D. A. Bromley, March 28, 1983; interviews with Larry Cardman, November 4, 1991, Franz Gross, October 16, 1991, and James McCarthy, December 11, 1991.

Despite these arguments, the opportunity to explore the new frontier of the quark structure of the nucleus was appealing to many; the crux of the issue therefore hinged on whether electro-nuclear physicists could really explore this frontier with a 4 GeV electron machine. As Kowalski recently noted, many wondered: "What was magic about 4 GeV? Why not talk about 10 GeV or more?" Although a 10 GeV machine seemed out of the reach of available technology, one solution was to exploit superconducting radiofrequency. But the serious difficulty experienced at HEPL and other research centers with exploiting this tricky technology convinced Kowalski, Larry Cardman, head of the Illinois group, the SURA group, and most others that superconducting radiofrequency was too immature to be useful. If quarks could not be studied adequately with a 4 GeV accelerator, why not build a lower energy machine, which could be constructed quickly at half the cost, or less? One counterargument, emphasized by Gross and others in SURA, was that almost everything physicists could study at 2 GeV could be better studied at 4 GeV, since the probability of interactions is higher at higher energy. In addition, some phenomena, such as the final states of nuclei with large internal momenta, could only be studied at 4 GeV. Those in favor of the lower energy machine argued that these possibilities did not justify the time and money that would be spent for the higher energy machine. In the opinion of some participants, the debate was complicated by the reluctance some nuclear physicists felt about the difficult task of studying quarks, which thus far had been the province of high energy physics.[32]

From the beginning, Gross and other SURA physicists advocated a 4 GeV accelerator. Arguments for the importance of obtaining 4 GeV were developed by physicists at SURA universities during a series of meetings held in 1980 to prepare for the December 1980 SURA proposal, the first to be submitted. Such arguments were strengthened during another series of meetings held in 1981 and early 1982, which led to a revised SURA proposal in 1982. SURA physicists also aired their arguments at general gatherings, such as meetings instigated by MIT that were held in May and August 1980 and at a conference hosted by UVa in April 1982. Gross later summarized that SURA members threw themselves into "an extensive effort to document to

[32]MIT, "Addendum to the MIT Proposal," March 1983; SURA, "Response to the MIT-Bates Addendum of March 3," March, 1983; H. Jackson to D. A. Bromley, March 28, 1983; interviews with Stanley Kowalski, October 25, 1991, Larry Cardman, November 4, 1991, Franz Gross, October 16, 1991, and James McCarthy, December 11, 1991.

the national community that the southeastern group [included] major players in the field, and that 4 GeV was necessary."[33]

McCarthy remembers that SURA's campaign for 4 GeV prompted NSAC in August 1981 to convene the Subcommittee on Electromagnetic Interactions, chaired by Peter Barnes of Carnegie-Mellon University.[34] In any event, the Barnes committee's charter was to review "the current status and future direction" for electro-nuclear physics and assess "the need for facilities to pursue the highest priority" research. In the end, the subcommittee agreed with SURA. The subcommittee's report, which was presented to and accepted by NSAC in April 1982, gave "highest scientific priority" to a high duty factor accelerator "able to achieve an electron energy of about 4 GeV."[35]

2 Choosing a Design, Fall 1982 to April 1983

By fall 1982, SURA, MIT-Bates, ANL, Illinois, and NBS had presented proposals to DOE for design funding. To assess the proposals, NSAC in January 1983 convened the Panel on Electron Accelerator Facilities, chaired by D. Allan Bromley of Yale University. Seven of the thirteen members of this panel formed a technical subpanel, which visited sites to assess the technological feasibility of the various designs. (See Figures 1 through 5.)[36] Those presenting proposals were also involved in the assessment process. Each group was asked to submit a written critique of each competing proposal

[33]Quotes, respectively, from DOE/NSF Nuclear Science Advisory Committee, "The Role of Electromagnetic Interactions in Nuclear Science," 1982, p. 43, and Franz Gross to Catherine Westfall, December 10, 1993. Also: "Proposal to the National Science Foundation," December 31, 1980; SURA, "Proposal for a National Electron Accelerator," October 1, 1982; E. C. Booth, T. W. Donnelly, E. Hadjimichael, G. A. Peterson, and P. Stoler, (eds.) *Future Directions in Electromagnetic Nuclear Physics* (Troy: Rensselaer Polytechnic Institute, 1981).

[34]Interview with James McCarthy, March 25, 1994.

[35]The Barnes panel included: P. Barnes, S. Brodsky, D. A. Bromley, G. Brown, P. Debevec, E. Henley, P. Morton, J. O'Connell, R. Pollock, A. Richter, I. Sick, and C. Dover. DOE/NSF Nuclear Science Advisory Committee, "The Role of Electromagnetic Interactions in Nuclear Science," April 1982. Quotes pp. ii, 3; also, pp. 1–2.

[36]Bromley panel members were: S. Austin, D. A. Bromley, J. Cerny, K. Erb, H. Grunder, E. Henley, S. Koonin, A. McDonald, P. Morton, R. Neal, E. Rowe, I. Sick, and A. van Steenbergen. The technical subpanel included: Austin, Erb, Grunder, Morton, Neal, Rowe, and van Steenbergen.

and to respond to comments from the other groups. In July 1983, Bromley prophetically told *Physics Today* that "this was going to be a highly visible, highly political competition."[37]

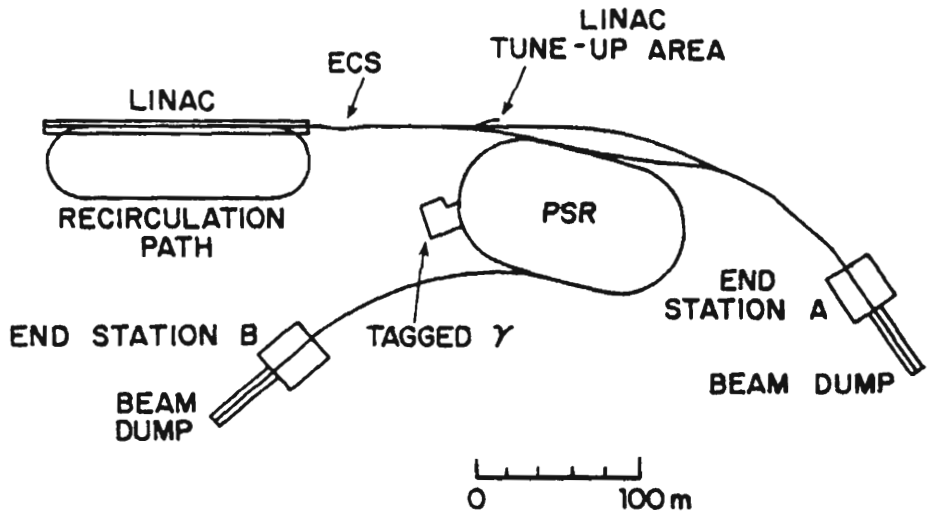

Figure 1. SURA 4 GeV pulse stretcher ring. From R. C. York, J. S. McCarthy, B. E. Norum, "Multi-GeV Electron Linac-Pulse Stretcher Design Options," *IEEE Transactions on Nuclear Science* 47 (1983), p. 3259.

In February 1983, the Bromley panel staged what participants later called a "shoot-out," a public meeting for airing all comments and criticisms of the competing designs, and for further debate of the related issue of 2 GeV vs. 4 GeV. York remembers that the setting was "very dramatic," with Bromley chairing the meeting on a stage, directing the proceedings like a fierce "traffic cop." McCarthy and his team again demonstrated their willingness to seek outside help, a tactic that impressed some panelists. For example, when the committee asked difficult questions, they found the nearest pay phone and called Alex Chao at SLAC and Grahame Rees in Daresbury, England.[38]

[37]Quote from *Physics Today*, July 1983, p. 57. DOE/NSF Nuclear Science Advisory Committee, "The Role of Electromagnetic Interactions in Nuclear Science," April 1982, p. 2; H. Holmgren, "SURA's First Decade."

[38]Interviews with Richard York, November 9, 1993, Hermann Grunder, February 27, 1992 and James McCarthy, December 11, 1991; Ron Meek, "Report of the Panel on Electron Accelerator Facilities, Reporter's Transcript," February 22, 1983.

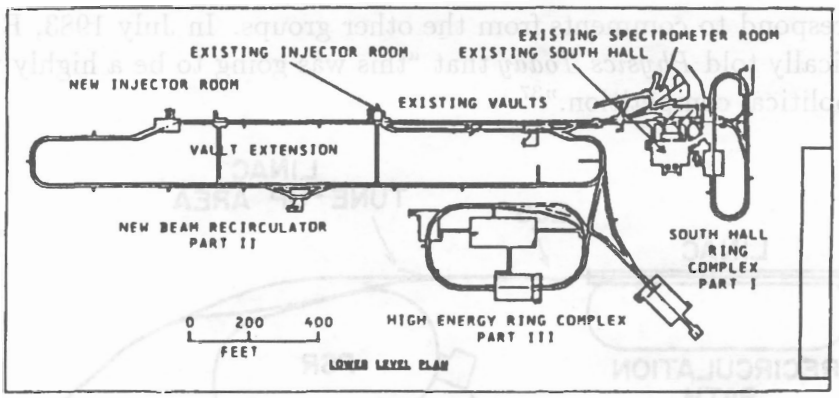

Figure 2. MIT-Bates expansion. Part II and Part III are 2 GeV and 4 GeV, respectively. From C. P. Sargent, "Accelerator Development at Bates," *IEEE Transactions on Nuclear Science* 47 (1983), p. 3263.

Representatives of the five competing proposals made presentations, and questions were posed by panelists as well as by the others at the meeting, which included proponents of competing designs. Illinois proposed a 750 MeV microtron, while NBS planned to finish their ongoing efforts to build a 200 MeV microtron and add a second microtron stage to achieve 1 GeV. Since technological limitations of their existing machine designs prevented obtaining higher energies, hopes at Illinois and NBS hinged on the possibility, which now seemed dim in light of continuing federal budget limitations, that two new machines would be built. The MIT-Bates proposal contained a surprise. MIT-Bates group members had declared their independence from the dictates of the 1982 Barnes panel, refusing to give top priority to a 4 GeV accelerator. As Kowalski recently noted: "You can have a panel [but] not necessarily agree with everything the panel says. No one had convinced us. We still felt that anything much under 10 GeV provided about the same physics." Drawing on their own best judgment about how to proceed, they presented plans to upgrade the MIT-Bates accelerator to 1, then 2, and finally 4 GeV over a period of years. Most of the group's attention had been focused on designing the 1 and 2 GeV upgrades; their design for the future 4 GeV machine was sketchy and incomplete, a weakness highlighted by panelist Grunder. ANL and SURA had taken the opposite approach. Congruent with the Barnes panel recommendation, ANL and SURA had focused on the

design of higher-energy machines, presenting thoroughly researched, detailed designs for a 4 GeV microtron and a 4 GeV linac stretcher, respectively.[39]

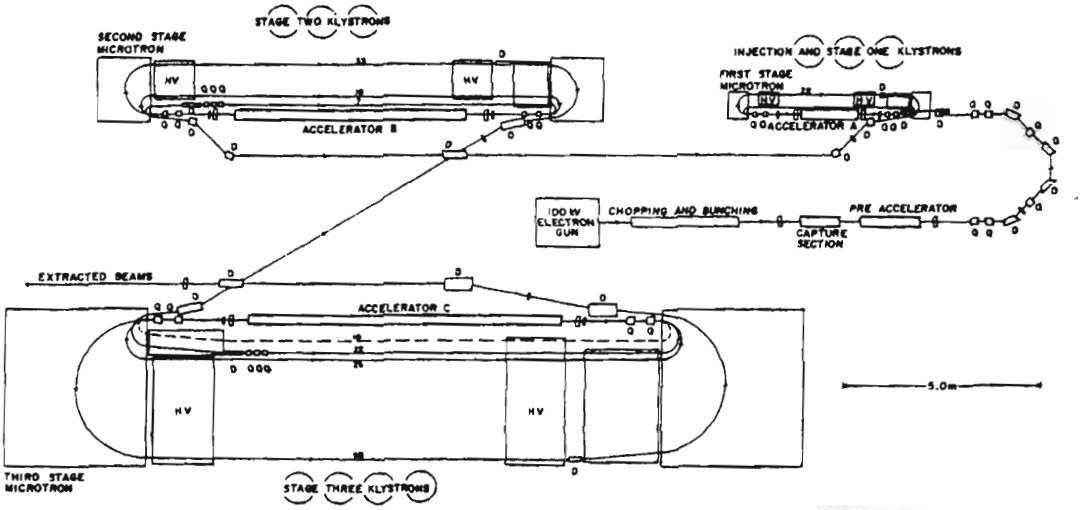

Figure 3. The University of Illinois cascade microtron. From L. S. Cardman, "A Proposal for a High Duty Factor, Intermediate Energy Electron Accelerator," *IEEE Transactions on Nuclear Science* 47 (1983), p. 3267.

Gross remembers feeling that the MIT case was weakened by the failure of MIT representatives to attend the SURA presentation, which preceded theirs. "Every time Bromley asked for a reason why it was necessary to go to 4 GeV (instead of 2 GeV) I gave an answer which no one disagreed with." Although the group from MIT-Bates later presented their arguments, by the time they arrived "they were too late to have any impact on the public discussion. I have often thought that if the MIT people had arrived earlier and argued for 2 GeV," they might have won the day. York remembers feeling heartened by the concern about beam stability in the machine described by ANL.[40]

[39] Argonne presented a design for 2 GeV microtron that could be upgraded to 4 GeV; the 4 GeV version was complete and detailed. Interview with Stanley Kowalski, October 25, 1991. Nuclear Science Advisory Committee, "Report of the Panel on Electron Accelerator Facilities," April 1983, pp. 30–60; interview with Larry Cardman, November 4, 1991.

[40] Peter Demos remembers that several MIT-Bates researchers were at the SURA presentation, but that they simply were not inclined to make strong arguments before their own presentation. Conversation with Peter Demos, June 7, 1994. Also: Franz Gross to Catherine Westfall, January 21, 1993, and interview with Richard York, March 25, 1994.

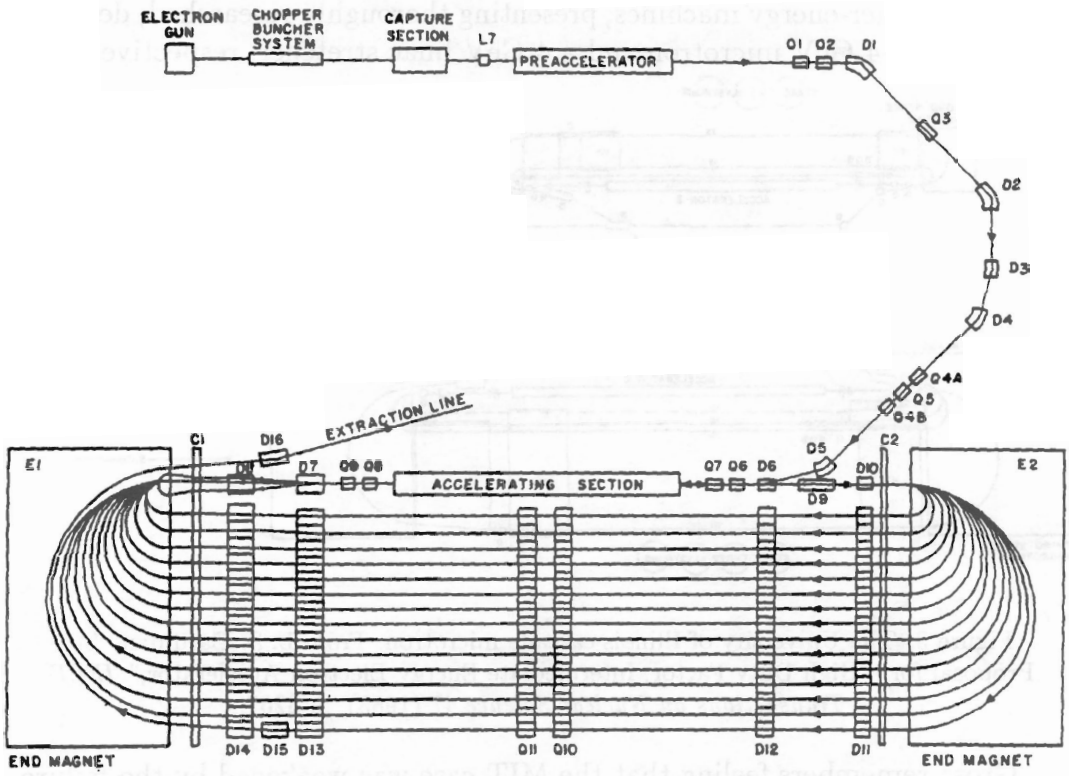

Figure 4. NBS racetrack microtron. From S. Penner, "The NBS Proposal for a One GeV CW Racetrack Microtron Facility," *IEEE Transactions on Nuclear Science* 47 (1983), p. 3280.

In April the Bromley panel issued a report passing judgment on the five competing designs. As the report explained, since the panel decided, in line with the Barnes panel, "to recommend construction of a 4 GeV accelerator as its highest priority, major attention was focused on a comparison of the two fully developed 4 GeV proposals."[41] Not only were the University of Illinois and NBS cut from the competition, but frontrunner MIT-Bates was also out. By valuing their own expertise over the advice of others, MIT-Bates had lost, despite the high reputation and ample resources of their laboratory.

[41] Nuclear Science Advisory Committee, "Report of the Panel on Electron Accelerator Facilities," April 1983, p. 3.

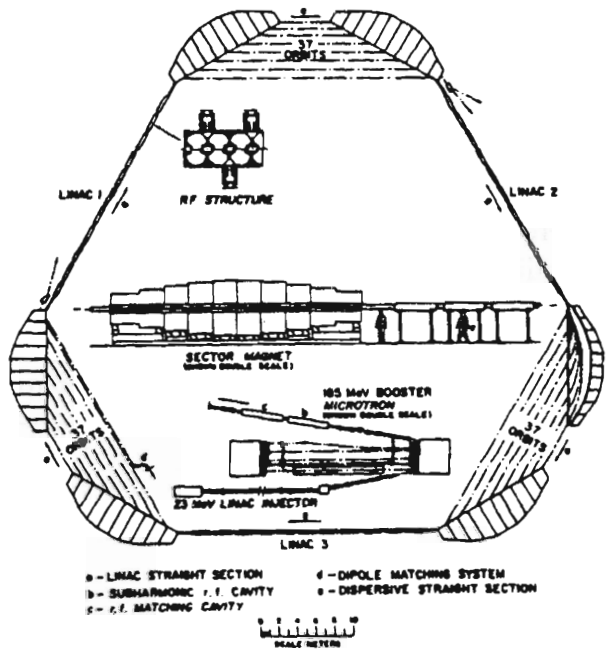

Figure 5. Argonne National Laboratory 4 GeV electron microtron. From R. L. Kustom, "GEM, ANL 4-GeV CW Electron Microtron Design," *IEEE Transactions on Nuclear Science* 47 (1983), p. 3286.

Passing over the MIT-Bates proposal was a shrewd decision based on several realizations. As Bromley panel member Sam Austin pointed out in a 1993 interview, although that panel had few experts in electro-nuclear physics, the Barnes panel was packed with such experts. "It would have been presumptuous, not to mention intellectually indefensible, for us to support a proposal aimed at achieving less than 4 GeV. Such a decision would not have been credible within the nuclear science community."[42] Moreover, as Bromley panelist Hermann Grunder noted, supporting such a proposal would also have risked problems in Washington. The new electro-nuclear physics machine had to vie with numerous other projects for a place in the DOE budget, then face a skeptical Congress at a time when basic physics research funding had reached a plateau. (See Figure 6.) In an environment so unfavorable to new construction, the new machine, like all nuclear physics

[42]Interview with Sam Austin, July 9, 1993.

projects, depended on strong support from NSAC, the official group of advisors responsible for setting priorities for the field. Without a high rating from a creditable advisory group, a project stood little chance of survival in the sometimes hostile funding environment of the 1980s. The panel also felt that by challenging the NSAC-sponsored Barnes panel, MIT-Bates had challenged the credibility of NSAC. By breaking ranks, MIT-Bates revealed that nuclear physicists were not unified, thus compromising their own chance for success as well as funding prospects for other nuclear science projects.[43] To minimize the damage, the Bromley panel upheld the recommendation of the Barnes panel, seeking a technologically defensible 4 GeV proposal capable of winning wide support from the electro-nuclear physics community.

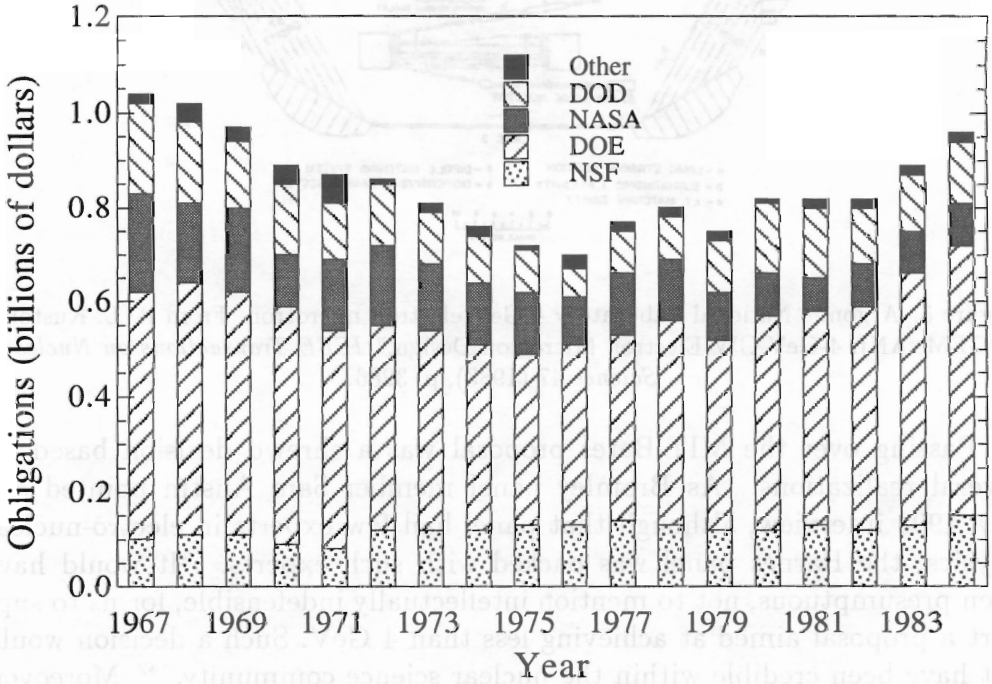

Figure 6. Funding for basic physics research from 1967 to 1984 comparing NSF, DOE, NASA, DOD, and others, and given in constant fiscal year 1983 billions of dollars. From Physics Survey Committee, *Physics Through the 1990s: An Overview*, Washington, D.C.: National Academy Press, 1986.

Ironically, the contest between the two remaining competitors, ANL and SURA, would actually result in further divisiveness as 1983 wore on, arguably

[43]Interview with Hermann Grunder, February 27, 1992.

due, in part, to the diplomatic intent of the Bromley panel. In its April 1983 report, the panel admitted that both groups had feasible designs "and that either could very well form the basis for an extremely powerful national facility." The costs for the two designs did not differ greatly: the panel estimated ANL and SURA could build 4 GeV machines for $143.3 million and $146.9 million, respectively. After mentioning "the different problems facing the two groups," which included the ANL design's beam instability problems and concern about the lack of a close university connection and the proximity of the water table at SURA's Newport News site, the group recommended that SURA build the new facility. The dark horse of the competition had come in first.[44]

Instead of focusing on ANL's disadvantages, the report diplomatically stressed three advantages that clinched SURA's victory. First, thanks to the strong support of universities and politicians in the Southeast, SURA was able to pledge 35 new tenured or tenure-track positions in experimental and theoretical nuclear physics at SURA universities, which went "a long way toward providing assurance that there [would] be the continuing flow of active young scientists required to exploit fully" the electro-nuclear physics community's first national facility.[45] Second was "the recognition that the SURA design could readily be extended in energy to 6 GeV, or above ... while the ANL design could not." Third, although ANL's design was more innovative, since it described a new type of microtron, many technical subpanel members had "significant reservations about the potential beam loss in the ANL accelerator." In contrast, SURA had produced a "conservative" design more likely to assure successful operation of the machine.[46] Hermann Grunder re-

[44]Nuclear Science Advisory Committee, "Report of the Panel on Electron Accelerator Facilities," April 1983, pp. 32, 62.

[45]In some cases, the new positions were offered through universities. In other cases funds were earmarked for new positions at particular universities by southeastern states. Interview with Harry Holmgren, October 12, 1991.

[46]One of the factors that worked against ANL was that the laboratory lacked substantial experience in building electron machines, despite its resources and long experience in building other types of accelerators. Subpanel members worried, specifically, about "beam centering errors and quantum fluctuation phenomena at the higher energies as well as about the geometric stability of the large dipoles and other accelerator components to the accuracy required for successful long term operation." Nuclear Science Advisory Committee, "Report of the Panel on Electron Accelerator Facilities," April 1983, p. 62. Quotes, pp. 61–62.

members that the panel downplayed its concern about beam problems in the written report out of respect for the ANL designers, unwittingly facilitating later controversy.[47]

The three member band of UVa designers, backed by a fledgling consortium, had gone up against experienced accelerator building professionals at established research facilities, and won. McCarthy credits the group's success, in part, to "serendipity." Although he was an amateur, the sort of design he initially favored had winning characteristics, and he was lucky to find congenial, talented group members to help him work out design details and present a convincing scientific proposal. (In fact, in the course of developing the SURA design, both York and Norum had become quite expert.) Clearly, luck played an even greater role. SURA was fortunate to have individuals well suited to the task at hand. The "four Musketeers" had the enthusiasm to work tirelessly, despite the bleak initial prospects, and periodic funding shortages (at one point, for example, McCarthy's group worked without pay).[48] In addition, they were skillful at obtaining and maximizing the value of regional support, which provided the long list of new faculty positions attractive to the Bromley panel and would later help funding prospects. Perhaps most important, SURA benefitted from the willingness of group members to seek and take seriously advice of experts at all stages of the project, from forming the consortium, to improving the design, to organizing the scientific contributions of physicists in the Southeast. Indeed, those willing to abide by the advice of the Barnes panel and answer all questions at the shoot-out were in a far better position than others with greater expertise who kept their own counsel and refused to tailor plans to fit the general scientific and political environment.

[47]Interview with Hermann Grunder, March 22, 1994. Concern about ANL beam problems is more apparent in the transcript of the meeting held by the Bromley panel on February 22 to discuss their report. Ron Meek, "Report of the Panel on Electron Accelerator Facilities, Reporter's Transcript," February 22, 1983.

[48]SURA's Executive Committee of the Board of Trustees reported in October 1982 that McCarthy's group had no funding. SURA, Minutes of the Executive Committee of the Board of Trustees, October 1, 1982. Interview with James McCarthy, December 11, 1991.

3 CEBAF in Trouble, April 1983 to May 1985

Despite SURA's triumph, the future of the new laboratory was far from secure. Amateurism had brought the project this far, but the next step was funding the project, a task that required the help of those with sufficient experience to meet a series of daunting challenges. One serious difficulty was that times were lean. In line with the funding trend of basic physics research, since the late 1970s federal expenditures for nuclear physics had decreased or remained constant. (See Figure 7.) In the eyes of Washington insiders, the image and traditions of nuclear physics presented additional obstacles in this unfavorable funding environment. For example, Ralph DeVries, the assistant director for general science in President Ronald Reagan's Office of Science and Technology Policy, explained in a 1984 speech that nuclear physics did "not have the 'exciting frontier' image in Washington" enjoyed by "high energy physics, molecular biology, or astrophysics," and thus funding was more difficult to obtain. Like Bromley panelists, DeVries also noted the dangers of the lack of consensus within the nuclear physics community. In his opinion, members of this community too often demonstrated the sort of independent approach used by the MIT-Bates group. In his opinion, nuclear physicists needed to follow the example of astronomers and high energy physicists, who agreed upon a priority list of projects, which was "then supported by the entire community in its public relations and in contacts with the executive branch and Congress." He warned: "Without that unanimity, budget cutters will easily succeed in stopping new projects."[49]

[49]Ralph M. DeVries, "Nuclear Physics and Current U.S. Science Policy: Remarks to the Conference on Instrumentation for Heavy Ion Nuclear Research," Oak Ridge National Laboratory, Oak Ridge, Tennessee, October 23, 1984. The author wishes to thank Robert Johnson for providing this document. In recent interviews, James Leiss, then associate director of the Office of Energy Research for High Energy and Nuclear Physics for DOE, and Hermann Grunder, who worked with Leiss in the early 1980s as the special assistant for advanced facilities, independently presented similar views about the difficulties of obtaining nuclear physics funding during this period. Interviews with James Leiss, December 9, 1991, and Hermann Grunder, March 22, 1994.

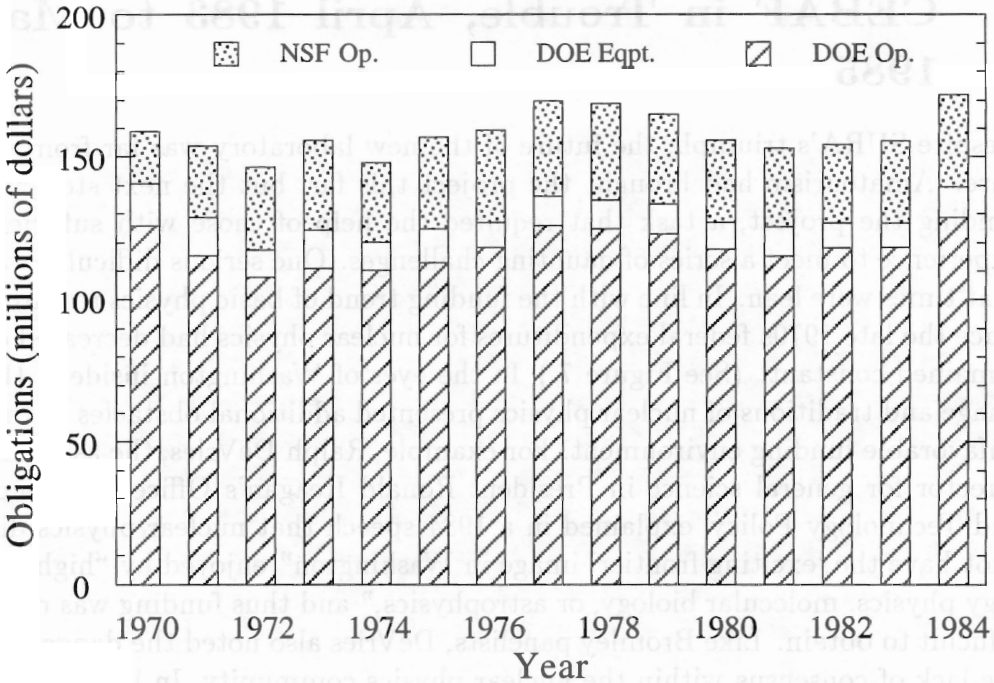

Figure 7. Funding for basic research in nuclear physics from 1970 to 1984 separated into DOE operations, DOE equipment, and NSF operations, and given in constant fiscal year 1983 millions of dollars. Construction funds are excluded. From Physics Survey Committee, *Physics Through the 1990s: An Overview*, Washington, D.C.: National Academy Press, 1986.

CEBAF provided dramatic evidence for DeVries' argument: in the next two years, CEBAF's funding prospects were repeatedly compromised by the lack of unanimous support for the project. The first blow came immediately after the announcement of the Bromley panel recommendations. Argonne director Walter Massey was incensed by the news that ANL had lost at a time when the laboratory desperately needed a new project due to the harsh economy. The temperate wording of the Bromley panel report gave him an opening. In a July 1983 *Physics Today* article he pointed out that the panel had not harshly criticized the ANL design and argued that therefore the report did "not lead logically to the conclusion." Massey did not stop with this volley. In May 1983 he compared costs for building the new accelerator at the home site versus the cost at the winning site and concluded that the SURA machine could be built at ANL for $42 million less than at SURA's

site in Newport News, Virginia. Massey was quick to publicize his cost comparison. When Illinois Senator Charles Percy heard the news, he met with Secretary of Energy Donald Hodel and asked him to choose the ANL proposal or allow the midwestern laboratory to build the SURA design at ANL. Hodel also received a letter signed by six midwestern governors, from Illinois, Indiana, Michigan, Minnesota, Ohio, and Wisconsin, that repeated Massey's arguments. Major midwestern corporations, including Sears, Ford, and FMC Corp., also pledged their support for ANL's fight.[50]

By attempting to overturn NSAC's decision in favor of SURA, ANL incurred the difficulties that the Bromley panel had sought to avoid by rejecting MIT-Bates. A measure of the impact of ANL's defiance can be taken from the testimony of James Leiss. Leiss, the instigator of improvements in electro-nuclear accelerators at NBS, played a key role in 1983 as associate director of DOE's Office of Energy Research for High Energy and Nuclear Physics in the effort to add the new accelerator to DOE's budget in line with the Bromley panel recommendations. In his opinion, this effort was impeded by the perception that Massey's challenge would decrease the likelihood that the project would obtain congressional approval. "Congressmen don't like to burn political chips to support a project only to learn that the whole community isn't behind the idea," Leiss explained. "This causes controversy which makes them look bad and they feel like their efforts are wasted." Massey's challenge and the fear of the resulting congressional response also complicated DOE's battle to obtain approval for the project from the executive branch watchdog, the Office of Management and Budget (OMB), since the traditionally budget-conscious OMB was reluctant to agree that the DOE budget should include such a costly, controversial item. As before, the best strategy was to minimize the damage wrought by defiance by adhering to the recommendations of the advisory panel. Accordingly, DOE upheld the Bromley

[50]Both McCarthy and York remember that the Argonne designers insisted that it was Massey's idea, not theirs, to build the SURA machine at Argonne. York remembers, in fact, that the notion "embarrassed" the Argonne designers. Von Baeyer remembers that Robb ordered his own economic analysis and "of course our estimate came out in our favor." Hans von Baeyer to Catherine Westfall, December 17, 1993. Interviews with James McCarthy, December 11, 1991, and Richard York, November 9, 1993. Argonne National Laboratory, "Comparative Economic Analysis of Alternate Site for an Electron Accelerator Facility," May 1983; H. Holmgren, "SURA's First Decade"; *Physics Today* July 1983, pp. 57, 59.

panel's choice of SURA, after insisting upon a re-evaluation of the Newport News site. In Leiss's opinion, ANL's late bid for the project simply "caused a lot of controversy and threatened the continuation of the project."[51]

By this time, however, CEBAF's funding prospects had sustained considerable damage that would only get worse as the crack in consensus begun by MIT-Bates and deepened by ANL split further, hastened by new scientific evidence and the partisan scramble for limited funding. In March 1984, a *Physical Review Letters* paper by Nathan Isgur of the University of Toronto and C. H. Llewellyn Smith of Oxford suggested that some of the theoretical estimates used to show how quarks might be observed with the SURA accelerator were overly optimistic.[52] DOE officials admitted to *Physics Today* later that year that in addition to continued complaints from MIT-Bates and Argonne, they heard "from scientists who want[ed] to bypass the SURA machine and go directly to a heavy ion facility," or "support a higher energy for SLAC."[53] Leiss remembers that representatives from LAMPF argued that the SURA accelerator should be scrapped in favor of increased funding to upgrade their machine.[54]

By mid-1983, the battle had been won within DOE, thanks to Leiss's efforts and the strong support of Warner and Robb, who personally petitioned Hodel. In July, DOE announced that initial funding for the $225 million project would be included in its request for fiscal year 1985.[55] Even though the Virginia delegation championed CEBAF in Congress, the behind-

[51]Interview with James Leiss, December 9, 1991. The initial choice of the Newport News site resulted from a competition held by SURA that included five sites; two main contenders were UVa and the W&M site at Newport News. In 1981, SURA chose the Newport News site. Due to the reservations of the Bromley panel about the Newport News site, the sites in Virginia were re-evaluated by a technical subpanel in May 1983. At this time advocates of the Newport News site successfully argued that William and Mary was close enough to provide a good university connection and that the site was not too swampy for accelerator construction. The next month the SURA Executive Committee reaffirmed the choice, which was accepted by the technical subpanel and DOE. Harry Holmgren, "SURA's First Decade."

[52]*Physical Review Letters*, March 26, 1984.

[53]*Physics Today* 37 (1984), p. 56.

[54]Interview with James Leiss, December 9, 1991.

[55]Inflation had already driven the original $146.9 million cost to $170 million; then it was raised by the addition of contingency costs required by DOE but omitted from the Bromley panel estimate. *Physics Today* 37 (1984), p. 56; Tom Adams to Buddy MacKay, July 7, 1983. H. Holmgren, "SURA Chronology."

the-scenes lobbying against CEBAF had created, in DeVries' words, "noise in the system" which "swamped the project for [fiscal year] 1985" in Congress.[56] As feared, the controversy provoked a critical response in Congress. The strongest criticism came from powerful Senators Mark Hatfield of Oregon and J. Bennett Johnston of Louisiana, who questioned whether there was really a scientific need for the project, in light of continued squabbling among physicists. Unfortunately for CEBAF, at just this point critics gained further ammunition from the demise of a high energy physics accelerator project, often called Isabelle, which DOE had recently killed on the basis of a High Energy Physics Advisory Panel review. For example, Johnston, who was chairman of the Senate committee that initially approved Isabelle, openly questioned whether the CEBAF proposal was a similar request from the same sort of group. In a June 1983 appropriation hearing he asked SURA: "Are you giving us another Isabelle?"[57] The outlook in Congress became even worse in late 1983, when an NSAC committee chaired by John Schiffer of Argonne charged with updating the long range plan for nuclear physics emphasized the importance of building a 30 GeV relativistic heavy ion collider. Although the committee, which included Bromley, later insisted that it had failed to stress the importance of CEBAF only because the project had been approved already, the omission of CEBAF confirmed fears held by some congressmen that the project lacked unanimous support from the nuclear physics community.[58]

As debate continued, CEBAF supporters worried that the project would be cut from the budget, dashing hopes that McCarthy's dream machine would become reality. At this juncture, the efforts of supporters skilled in the ways of Washington were crucial. As before, the Virginia delegation lobbied hard for the project. The project also benefitted from the support of a powerful ally: George Keyworth, President Ronald Reagan's science advisor.

[56]Ralph M. DeVries, "Nuclear Physics and Current U.S. Science Policy: Remarks to the Conference on Instrumentation for Heavy Ion Nuclear Research."

[57]Quote from *Physics Today*, September 1984, p. 55; interview with William Wallenmeyer, October 18, 1991.

[58]In addition to Schiffer, the Schiffer panel included: S. M. Austin, G. Baym, F. Boehm, D. A. Bromley, H. Chen, D. Cline, K. A. Erb, H. Grunder, C. M. Hoffman, A. Kerman, M. H. McFarlane, P. G. Roos, A. J. Schwartzchild, I. Sick and E. W. Vogt. *Physics Today* 37 (1984), pp. 55–56, 59; DOE/NSF Nuclear Science Advisory Committee, "A Long Range Plan for Nuclear Science," December 1983; interviews with James Leiss, December 9, 1991, and Hans von Baeyer, October 15, 1991.

Perhaps due to his ties to the southeast — Keyworth had obtained his Ph.D. from Duke University — he had been quite accessible to SURA and had periodically met to advise board members about strengthening their proposal. In these meetings, SURA representatives found him to be "actually quite sympathetic" to their cause. By mid-1984, he had become a staunch advocate of the project. As he would explain six months later, his "vision" was that CEBAF was destined to become "a gem" for American science.[59] To help neutralize congressional concern about the project, in July 1984 Keyworth asked DOE to convene yet another advisory group. The resulting NSAC subcommittee was chaired by Erich Vogt of the University of British Columbia and included long-time CEBAF supporter J. Dirk Walecka as well as Bromley and Schiffer. The subcommittee was charged with determining whether "the original recommendation, to build a 4 GeV CW electron accelerator" was "still the most effective strategy for nuclear physics, and, especially, for exploring this important frontier of the field."[60]

By late summer 1984, the labor of Keyworth, Leiss, and the Virginia delegation began to bear fruit. In August, as the Vogt subcommittee deliberated, Congress appropriated the first funding for CEBAF. The appropriation represented a mixed success, however. Whereas SURA had requested $2 million for construction and $5 million for R&D in fiscal year 1985, Congress approved only $3.5 million for R&D.[61] Since the appropriation of construction, not R&D, funding traditionally signaled federal commitment to a project, CEBAF supporters still worried that the project would be cancelled.[62]

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[59]Quotes, respectively, from interviews with Harry Holmgren, October 12, 1991, and Robert Johnson, April 6, 1994. SURA, "Minutes of the Meeting of the Executive Committee," April 15, 1982. Keyworth's comments were made to Hermann Grunder and others, when Grunder was being considered for the directorship. Johnson reiterated these statements after reading the notes made by Grunder and himself at the time.

[60]In addition to Vogt, Walecka, Bromley, and Schiffer, the panel included G. Baym, G. Farrar, S. Koonin, J. Negele, and I. Sick. DOE/NSF Nuclear Science Advisory Committee, "Report of the NSAC Ad Hoc Subcommittee on a 4 GeV CW Electron Accelerator for Nuclear Physics," September 27, 1984, p. 1.

[61]Construction was deferred "without prejudice," and eventually Congress was persuaded to allow DOE to reprogram an extra $1 million for fiscal year 1984. H. Holmgren, "SURA's First Decade," p. 5.

[62]As the cancellation of the Superconducting Super Collider in 1993 shows, even authorization of construction funding does not assure a federal commitment to complete a project.

More good news came from Washington the next month. At a public meeting there on September 24, 1984, the Vogt subcommittee announced its recommendations. As explained in the group's final report, "there is no known sharp threshold for new physics above 2 GeV but one gains kinematic flexibility, which can increase both the rate at which experiments can be carried out and the information they provide." After re-evaluating the scientific priority of the experiments possible at higher energies, the subcommittee reaffirmed "a 4 GeV CW electron facility as the first major construction project for nuclear physics." CEBAF's future still appeared murky, however. McCarthy, who had been appointed acting director of CEBAF in July, noted: "I'm encouraged by the Vogt Report, but I know there's still a long way to go."[63]

As he recently noted, by this time McCarthy felt that he was no longer "suited to be a director," due to his lack of experience and disinclination to lead a major laboratory.[64] For over a year, SURA had searched in vain for a permanent director. The group had offered the job to Paul Reardon, a seasoned accelerator specialist who had helped build Fermilab, but he had refused.[65] At this point, Keyworth again intervened. Holmgren remembers that in October 1984, the science advisor called a meeting "with a few of the senior people of SURA," emphasized the urgency of recruiting a permanent director with sufficient technical, political, and administrative experience to steer the project onto a secure course, and offered to help with the search.

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[63]Quotes, respectively, from DOE/NSF Nuclear Science Advisory Committee, "Report of the NSAC Ad Hoc Subcommittee on a 4 GeV CW Electron Accelerator for Nuclear Physics," September 27, 1984, pp. 6, 9, and as quoted in *Physics Today* 37 (1984), p. 59. Also: Executive Board Minutes, July 11, 1984.

[64]Interview with James McCarthy, December 11, 1991.

[65]Holmgren remembers favoring the eventual selectee, Hermann Grunder, early on. After the January 1983 shoot-out, the Bromley panel, which included Grunder, called Holmgren into an executive session that followed the public session. Bromley asked him if SURA would be able to offer competitive salaries and whether they would seek someone outside SURA to be director. He answered yes to both questions, noting that "as a matter of fact, people sitting at the table could well be candidates." Although Grunder didn't know it, Holmgren "had one [candidate] in mind and that was Hermann." Interview with Harry Holmgren, October 12, 1991. Also: SURA, "Minutes of the Meeting of the Board of Trustees," April 27, 1984; SURA, "Minutes of the Executive Committee of the Board of Trustees," July 11, 1984; H. Holmgren, "SURA Chronology." Interviews with James Leiss, December 9, 1991, Hans von Baeyer, October 15, 1991, and Harry Holmgren, October 12, 1991.

He subsequently "personally ... twisted the arms of" influential people, to persuade them to join the search committee.[66] The committee, which was chaired by Edward Knapp, a former NSF director, decided that the strongest candidate was Hermann Grunder, who had established a solid reputation as a builder of heavy ion accelerators at Lawrence Berkeley Laboratory (LBL), acted as a special assistant in DOE's Office of Energy Research for High Energy and Nuclear Physics, and was the LBL deputy director in charge of developing large scale projects.[67]

Although Grunder was initially reluctant to leave Berkeley, he was urged to accept by a number of prominent officials, including DOE Director of the Office of Energy Research Alvin Trivelpiece. As Grunder told *Physics Today* in 1986: "Both Al Trivelpiece and Jay Keyworth twisted my arm so hard I thought it would fall off. When Ed Knapp took hold of my other arm, I knew it was time to give in."[68] Grunder signed a contract with SURA in April, and joined CEBAF in May 1985. He immediately recruited a core of key LBL management and technical staff, thereby bringing to Virginia the skills and expertise of professionals trained at the oldest major U.S. accelerator laboratory.[69]

4 A New Director and a New Direction, May 1985 to 1987

By summer 1985, Grunder led CEBAF into an unexpected turn. The path to this twist began in early 1985 when Grunder considered the directorship. Grunder, who not been deeply involved with the electro-nuclear physics community aside from his duties on the Bromley panel, interviewed numerous electro-nuclear physicists to learn their views and assess CEBAF's scien-

[66]Interview with Harry Holmgren, October 12, 1991.

[67]SURA, "Minutes of the Meeting of the Board of Trustees," March 15, 1985; Hermann Grunder, curriculum vitae; CEBAF, "CEBAF Chronology," April 1989.

[68]Irwin Goodwin, "CEBAF Wins Praise for Design, but Its Future Is Uncertain," *Physics Today* 39 (1986), p. 52.

[69]Key staff members included Beverly Hartline, the scientific assistant to the director, and Christoph Leemann, who headed accelerator physics. James Coleman, who was working elsewhere but had served at LBL, was recruited to lead administration. Interview with Hermann Grunder, February 27, 1992; CEBAF, "CEBAF Chronology," April 1989.

tific prospects. Contrary to the repeated advisory panel support for a 4 GeV machine, Grunder often heard the argument raised two years earlier by MIT-Bates at the "shoot-out" — that adequate study of quarks required a machine with much higher energy. As he recently explained: "There was the recurring accusation that [CEBAF's] energy range had to be above 10 GeV to make a difference."[70]

These accusations were ringing in Grunder's ears when he asked Christoph Leemann, who headed accelerator physics, to assemble as many experts as possible to conduct a technology review in summer 1985. Such a review was customary. Accelerator builders usually reassess a design before beginning construction of a major accelerator project. This preconstruction review allows them to consider new materials or technologies that have become available since the design was drafted and to propose more detailed information about budgets, deadlines, and procedures that are tailored to the time and place of construction and the accelerator building team. Grunder knew that they would have to push the SURA design to its technological limit to obtain a machine capable of achieving more than 6 GeV. Since he had spoken to various experts and "had an inkling" that superconducting radiofrequency (srf) accelerating technology might have matured sufficiently to be useful, the new director asked Leemann to investigate the technology at the review. Although srf had eluded successful application for over 20 years, in principle a superconducting linac would have intrinsic advantages, which were outlined in a report later that year: "inherently continuous beam with 100% duty factor, improved beam quality, considerable power savings during operation, ability to deliver simultaneous continuous beams at two energies, conceptually simpler design ..., and potential for significant upgrades in energy and operational flexibility."[71]

At first Leemann was skeptical about the prospects for srf. Like most accelerator experts, he had deep reservations about the technology due to the host of past difficulties that had previously prevented its application. His opinion changed, however, after visiting Ronald Sundelin at Cornell, where Sundelin and his group had successfully tested two elliptical superconducting radiofrequency cavities in November 1984. This impressive demonstra-

[70]Quote from interview with Hermann Grunder March 22, 1994. Also: interview with Robert Johnson, April 6, 1994.

[71]CEBAF, "Scientific and Technological Assessment Report (STAR) on the Superconducting CW Linac Design for CEBAF," 1985, pp. 1–7.

tion, which represented an important milestone in proving the utility of srf, was possible due to a multitude of improvements made through meticulous, painstaking work at Cornell and by groups at CERN, Wuppertal, Karlsruhe, Genoa, and Stanford.[72]

Leemann brought the news back to the technology review. In July and August experts convened in Newport News to intensively debate the feasibility of srf, also considering the merits of McCarthy's original design and a new, state-of-the-art design with room-temperature radiofrequency cavities. Their results were presented at a technology workshop held in mid-August, 1985. At the conclusion of the workshop, Grunder was convinced that a viable srf accelerator could be built, and by early October, he and the SURA board of trustees announced that CEBAF would adopt a superconducting design.[73]

Although a design is usually altered on the basis of a technology review, Grunder's decision to introduce a radically different design based on a problematic technology was unprecedented. In Grunder's words, "two arguments" convinced him to make the change: the technology was superior, and by applying it, CEBAF could meet the needs, and thereby circumvent the criticism, of the electro-nuclear physics community. "This double argument gave me the moral courage to take this drastic step."[74]

The change to an srf accelerator made the project more attractive in some quarters. Since CEBAF would be developing a cutting-edge technology, the decision was bound to please those concerned with the development of accel-

[72]Leemann summarized that these improvements included "finding a special shape to avoid a particular runaway form of secondary electron emission, multipacting; improved manufacturing, processing, and diagnostic techniques to achieve fewer surface imperfections; the development of ultra-pure niobium and advanced welding techniques; the design of optimal cavity coupling geometries; and the use of advanced computer codes to solve Maxwell's equations in complicated geometries." C. Leemann, talk given at symposium for Hermann Grunder, December 9, 1991, p. 8. Also: W. Weingarten, "Superconducting Cavities," in J. Rossbach, ed., *Proceedings of the XVth International Conference on Accelerators*, Vol II (Singapore: World Scientific, 1993), pp. 678–685, and interviews with Christoph Leemann, September 27, 1993, and Ronald Sundelin, March 25, 1994.

[73]Hermann Grunder, "The CW Linac: A Review Paper," September 1985; Irwin Goodwin, "CEBAF Wins Praise for Design, but Its Future Is Uncertain," *Physics Today* 39 (1986), p. 52; interviews with Hermann Grunder March 22, 1994, and Roger Miller, March 2, 1994.

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

erator technology and make recruitment of accelerator experts easier. Also, many electro-nuclear physicists were likely to enjoy the prospect of better quality, higher energy beams. However, Grunder was flirting with disaster in Washington. After all, the Bromley panel, which had included Grunder, had chosen the SURA design in part because it was unproblematic and conservative. When he suddenly appeared two years later with a completely different, risky design, Grunder not only lacked the panel's stamp of approval, but also took the gamble of looking ridiculous to DOE officials. "I was afraid I would appear to be criticizing myself," he recently admitted. Grunder also worried that the news of the switch to srf would be greeted with ire in Congress, where the decision to grant construction funding still hung in the balance. In short, in Grunder's opinion, the decision to adopt srf technology placed him in "an absolute quagmire" as he struggled to optimize funding prospects. Others shared Grunder's assessment. Hugh Loweth, head of the Energy and Science Division of the Office of Management and Budget remembers feeling "nervous about it .... If [Grunder] couldn't sell it, there could be a backlash" in Congress. According to *Physics Today*, Trivelpiece feared that "altering the design at this stage ... could give rise to untimely political questions." Leiss and William Wallenmeyer, then director of the Division of High Energy Physics, remember worrying that the proposed switch to srf would be judged illegal and the project killed.[75]

"But I was not to be talked out of [the switch]," Grunder recently explained. Grunder remembers insisting in a tense meeting with Trivelpiece that he "couldn't live with" the notion of returning to the SURA design. After some argument, Grunder obtained permission to produce a new design based on srf technology. However, Trivelpiece warned Grunder that he was taking a big risk. "He told me that due to the budget squeeze, 'if you don't make the [fiscal year] 1987 budget, your window [of opportunity] is closed."' Grunder was unlikely to have a second chance, if the decision to switch to srf technology proved unwise. And at this point he could not be certain that the CEBAF design team would meet the technological challenge of developing srf cavities any more than he could be certain that he could successfully defend the design change to Congress. "My only certainty was that if we

[75]Quotes, respectively, from interview with Hugh Loweth, July 26, 1993, and Irwin Goodwin, "CEBAF Wins Praise for Design, but Its Future Is Uncertain," *Physics Today* 39 (1986), p. 53. Also: interview with William Wallenmeyer, October 18, 1991, and telephone conversation with James Leiss, May 4, 1994.

built a room-temperature machine, we were making a desperate mistake." The fledgling project again faced formidable challenges. Meeting them would require full use of the technological expertise of the group of seasoned accelerator builders gathering to work with Leemann as well as the experience Grunder had gained in planning and promoting large scale projects at LBL.

From fall 1985 through mid-1986, Grunder and his staff rose to the occasion. The most crucial initial tasks were to produce a cost-effective conceptual design of an accelerator using srf cavities and to convince DOE representatives, accelerator experts, and electro-nuclear physicists that CEBAF was poised to build the right machine. At the same time, the CEBAF staff also had to convince DOE that appropriate plans had been laid for building and administering the laboratory. As administrative planning proceeded, the CEBAF design team, which by then included such experts as Norum, York, and Sundelin, drafted and refined a new design and made construction cost estimates. A series of technology, management, and budget reviews was held to assess these efforts.[76] In September 1985, a group of experts called the National Advisory Board reviewed the laboratory, and in November DOE reviewed the laboratory's Scientific and Technological Assessment Report (STAR). In December, the design was further refined into a Preconceptual Design Report, which DOE assessed, along with the budget. In the same month a special visiting committee of eminent scientists, which included several DOE laboratory directors, NSAC chairman John Schiffer and Bromley, visited the laboratory. In January 1986 the National Advisory Board visited again, and in February the laboratory produced the official conceptual design, which was again reviewed, along with cost estimates, by DOE.[77]

[76]Sundelin made regular visits to the laboratory starting in 1984 as a Cornell employee; in 1987 moved to Virginia and joined the CEBAF staff.

[77]The National Advisory Board included: E. Henley, R. Kropschot, L. Bollinger, R. Briggs, L. Cardman, H. Jackson, S. Koonin, S. Kowalski, J. Leiss, H. Lengeler, J. Lightbody, B. McDaniel, R. Miller, R. Minehart, E. Moniz, H. Piel, I. Sick, R. Sundelin, and P. Arend. The visiting committee also included J. Browne, Robert Hughes, Fermilab director Leon Lederman, SLAC director Burton Richter and LBL director David Shirely. CEBAF, "Scientific and Technological Assessment Report (STAR) on the Superconducting CW Linac Design for CEBAF," 1985, p. A-2, and Irwin Goodwin, "CEBAF Wins Praise for Design, but Its Future Is Uncertain," *Physics Today* 39 (1986), p. 53. Also: CEBAF, "CEBAF Chronology," April, 1989, Committee on Science and Technology, U.S. House of Representatives, "Fiscal Year 1987 Department of Energy Authorization," 99th Congress, 2nd Session (Washington D.C.: GPO, 1986) pp. 179–180, and, interview with

The workload was considerable. As secretary Anne Stewart recently remembered: "We worked every single weekend ... . And we had people at work until 1 o'clock in the morning, almost every night, including some with very large titles, who were doing very menial work, like xeroxing and stapling." James Coleman, leader of the Administration Division, added that spouses and children often pitched in, working for free. Stewart and Coleman agree that although the atmosphere was sometimes stressful, it was also exciting.[78]

In February 1986, the CEBAF Conceptual Design Report was presented to DOE. By this time, CEBAF was receiving consistently favorable review comments. For example, as reported in *Physics Today*, a group led by David Hendrie, director of DOE's Division of Nuclear Physics and L. Edward Temple, director of DOE's Construction, Environment and Safety Division, concluded that srf was "appropriate for the CEBAF design goals," adding that the CEBAF staff was "an impressive group." The visiting committee concluded: "The superconducting design appears to be a major improvement over the original design."[79]

By that time, the project also enjoyed continued success with recruitment and an early start on the development of industrial prototype srf accelerating cavities, achievements that once again underscored the importance of strong regional support. Although DOE construction funding was not available in 1985 and through most of 1986, during that period Grunder had access to $3 million from Virginia, and $1 million from SURA, in addition to the roughly $10 million the laboratory had received from DOE ($5 million had been granted for fiscal year 1985). In March 1986 Grunder reported to Congress that he had recruited "76 employees, including 47 professionals, who bring the appropriate mix of talents, experience, and knowledge to launch the project." Leemann remembers that the money from Virginia gave Grunder a significant recruitment advantage because it allowed him to offer top management people, such as Leemann himself, a "parachute" provision which would provide a tenured professorship at a state university if

James Coleman, July 29, 1993.

[78]Interviews with Anne Stewart, September 29, 1993, and James Coleman, July 29, 1993.

[79]Quotes as quoted in Irwin Goodwin, "CEBAF Wins Praise for Design, but its Future Is Uncertain," *Physics Today* 39 (1986), p. 54. Also: CEBAF, "CEBAF Conceptual Design Report," February, 1986.

the project failed. The supplementary money from SURA and Virginia also allowed the laboratory to start the industrial prototyping of srf cavities. By March, Grunder was able to report that four vendors had been contracted to fabricate seven prototypes and that "two cavities from two vendors have been tested, and both exceed our specifications." This early progress not only advanced the project, but also demonstrated to reviewers that various companies had the interest and capability to construct the specialized srf cavities.[80]

Grunder, who made a good impression on Capitol Hill, even with Senator Johnston, gained further respect when the news of the project's progress reached Washington. With staunch support from DOE and the electro-nuclear physics community, the project's stock rose in Congress, where the switch to srf technology sparked little controversy, despite earlier fears.[81]

Grunder's gamble paid off. In October 1986, Congress appropriated $16.5 million in construction funds for fiscal year 1987. By spring 1987, with funding success, the start of construction, a growing staff, and the onset of planning for the experimental program under the direction of eminent theorist J. Dirk Walecka, who had been appointed Scientific Director in May 1986, support for CEBAF in the electro-nuclear physics community blossomed. With great excitement, researchers planned experiments to explore a new frontier in nuclear physics. Under Grunder's leadership, CEBAF was growing, after a long, sometimes painful gestation, into a robust new research facility. (See Figure 8.) [82]

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[80]The group led by Hendrie and Temple noted that "the recent progress in developing suppliers of the proposed 1500-MHz superconducting Nb cavities ... along with the planned R&D [makes] possible an FY 1987 construction start." Irwin Goodwin, "CEBAF Wins Praise for Design, but Its Future Is Uncertain," *Physics Today* 39 (1986), p. 53. Quotes, respectively, from Committee on Science and Technology, U.S. House of Representatives, "Fiscal Year 1987 Department of Energy Authorization," 99th Congress, 2nd Session (Washington D.C.: GPO, 1986) pp. 175 and 183. Also: see pp. 162 and 1272 of the latter source; Department of Energy, "Nuclear Physics Program Plan," (Washington, D.C.: Department of Energy, 1985), p. 794; and conversation with Christoph Leemann, June 23, 1994.

[81]Leiss remembers that Johnston was deeply impressed with Grunder. Interview with James Leiss, December 9, 1991. For a record of Grunder's presentation during the fiscal year 1987 budget hearings, see Committee on Science and Technology, U.S. House of Representatives, "Fiscal Year 1987 Department of Energy Authorization," 99th Congress, 2nd Session.

[82]H. Holmgren, "SURA's First Decade," H. Holmgren, "SURA Chronology," interviews

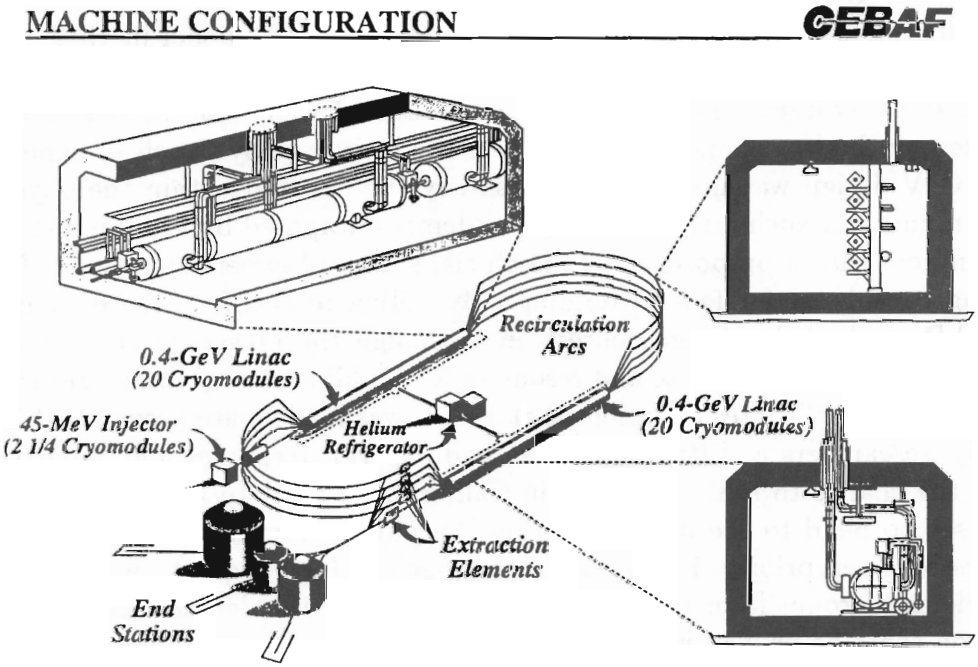

Figure 8. The final design of the five-pass CEBAF superconducting recirculating accelerator.

5 Conclusion: Lessons in the Founding of CEBAF

Those who founded CEBAF had experiences common to founders of other large, federally sponsored accelerator laboratories. Such projects tend to succeed when ways are found to respond constructively to the pressures that arise from the physics community, the funding environment, and scientific and technological developments. The story of the founding of CEBAF is particularly instructive because its founders were forced to respond to *all* of these pressures as it developed from a pipe dream into an official DOE construction project.

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with H. Grunder and W. Wallenmeyer.

By designing a 4 GeV accelerator, SURA was aligned with the consensus of scientific opinion that was established by the Barnes panel in 1982. Although this consensus was challenged by scientific findings, for example the 1984 *Physical Review Letters* paper by Isgur and Llewellyn Smith, it was reaffirmed by the Vogt panel in 1984. Given the insistence by panels of experts, a 4 GeV design was needed both to satisfy the demand within the physics community for such a machine and the demand imposed by the funding environment that a proposal have the official stamp of expert approval before being considered for federal funding. By failing to satisfy these demands, MIT-Bates lost the design contest, even though the Massachusetts laboratory had superior expertise and resources for building the machine, and even though the MIT-Bates argument that 4 GeV was inadequate prevailed. Similarly, researchers at LBL, which also had superior resources and expertise, lost the chance to build Fermilab in California in the late 1960s when they refused to bend to the demand within the physics community that outside users be given priority in the use of the machine (although the weakness of inside user groups later proved a disadvantage) and the demand from Washington that the project be cost-effective.[83] The importance of obtaining the support of officially proclaimed experts to meet the demands of the funding environment is also illustrated by Massey's failed attempt to have the SURA design built at ANL. Although ANL had accelerator building experience and resources superior to SURA's, SURA, not ANL, had obtained the official approval that would facilitate DOE's efforts to obtain funding for the project. Again, having resources and experience is less important than responding to the demands of the funding environment.

As in the CEBAF case, amateurism won over rigid professionalism in the founding of Fermilab, which was built in Illinois by the cost-conscious Robert Wilson, who had only built much smaller accelerators previously. However, in the case of Fermilab, it was the reliable but costly design of the LBL experts that won initial approval in Washington. Wilson's cost-effective design responded to Washington's later demand, inspired by a declining economy, for a less expensive machine.[84] This demonstrates, yet again, that success

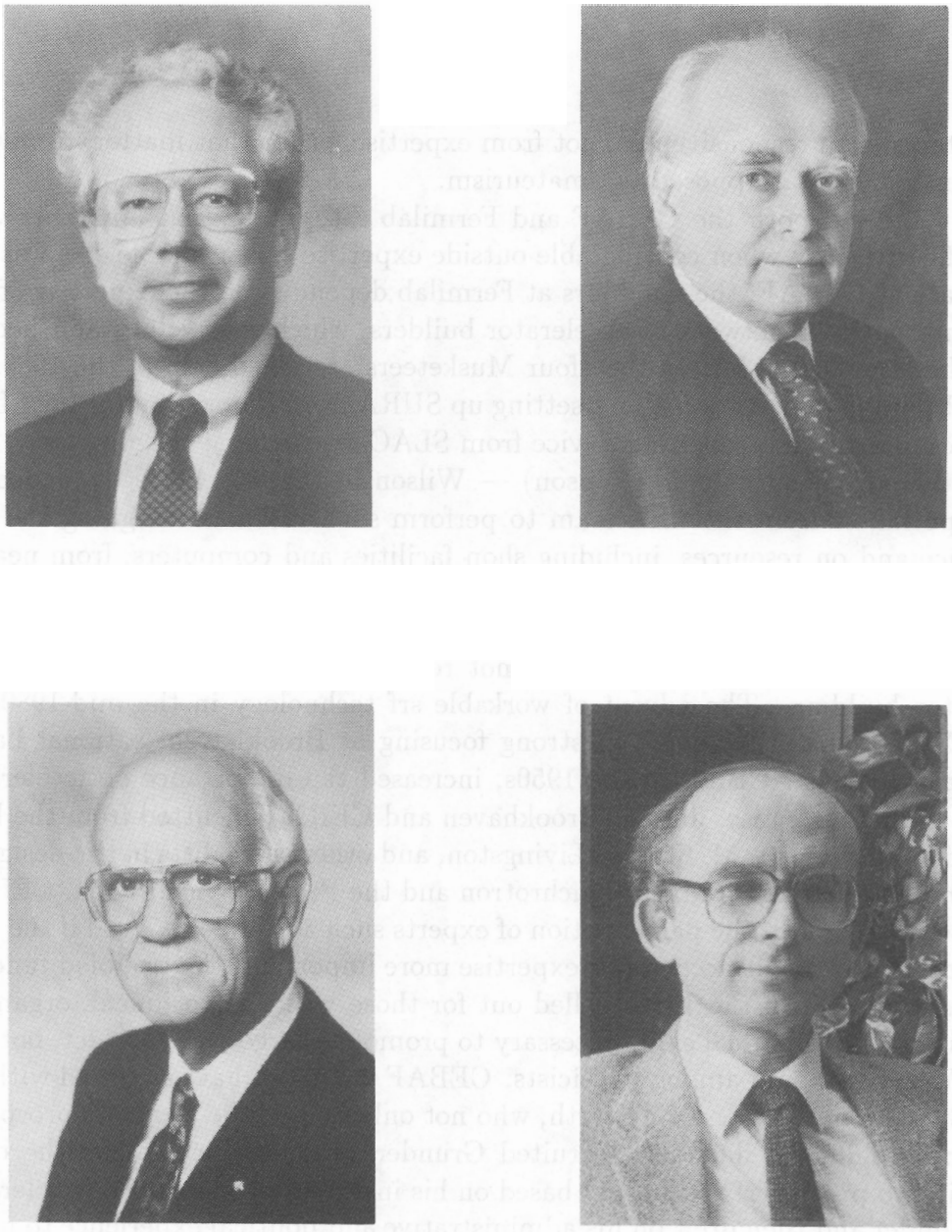
[83]Catherine Westfall, "The Site Contest for Fermilab," *Physics Today* 42 (1989), pp. 44–52.

[84]For more information on this episode, see Catherine Westfall and Lillian Hoddeson, "Frugality and the Founding of Fermilab, 1960–1972," Fermilab PUB-93/283.

stems from responsiveness, not from expertise, or for that matter, from professionalism as opposed to amateurism.

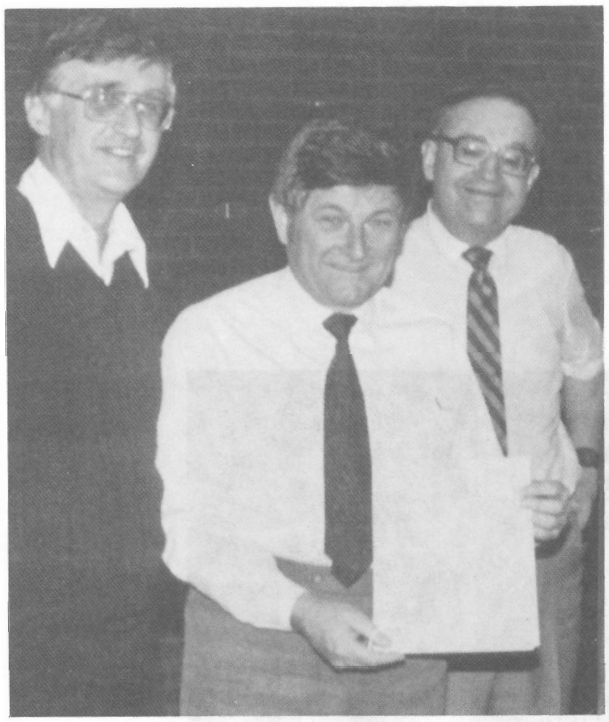
Indeed, both the CEBAF and Fermilab cases show that amateurs were effective only when considerable outside expertise was available. As was the case at CEBAF, the amateurs at Fermilab depended on the expertise of the international network of accelerator builders, which was well established by the late 1960s. Just as the "four Musketeers" received help in the 1980s — obtaining information about setting up SURA from Ramsey and using SLAC equipment, and soliciting advice from SLAC researchers, Herminghaus, and other experts (including Wilson) — Wilson in the late 1960s depended on specialists from the LBL team to perform such tasks as designing the lattice and on resources, including shop facilities and computers, from nearby Argonne.

The Fermilab machine could be built in the late 1960s by a relative amateur only because the task did not require major innovations in accelerator building. The advent of workable srf technology in the mid-1980s at CEBAF, like the advent of strong focusing at Brookhaven National Laboratory and at CERN in the 1950s, increased the importance of accelerator building expertise. Just as Brookhaven and CERN benefitted from the help of John Blewett, M. Stanley Livingston, and other specialists in the design of the Alternating Gradient Synchrotron and the Proton Synchrotron, CEBAF benefitted from the participation of experts such as Sundelin. And if the new technology made accelerator expertise more important, the troubled funding environment of the 1980s called out for those with the technical, organizational, and political skills necessary to promote a large scale project, both in Washington and among physicists. CEBAF could not have survived without the help of Leiss and Keyworth, who not only helped the project's prospects in Washington, but also recruited Grunder, who, in turn, made the decision to pursue srf technology based on his instincts as a seasoned accelerator builder and then drew on his administrative and political experience to make this decision into a winning proposition for the laboratory. The successes at CEBAF, Fermilab, Brookhaven, and CERN all demonstrate that, ultimately, such large projects depend on the skills, experience, and good judgment of leaders, who must make the adjustments necessary for satisfying the complex requirements of science, technology, and politics, both inside and outside the physics community.



Clockwise from top left, four of the principal figures in CEBAF's founding: James McCarthy, Harry Holmgren, Hans von Baeyer, and Dana Hamel.

Hans von Baeyer, center, and Virginia Associated Research Campus employees celebrate the Bromley panel's April 1983 recommendation that SURA be chosen to build the needed new national nuclear physics facility.



With Hermann Grunder, center, displaying the CEBAF construction authorization, are DOE CEBAF Site Office manager Ron Hultgren (left) and Dirk Walecka.

Left to right on the bulldozer used at CEBAF's February 1987 groundbreaking ceremony: SURA vice president Dana Hamel, Congressman Herb Bateman, Newport News mayor Jessie Rattley, Hermann Grunder, and DOE Energy Research director Alvin Trivelpiece.