Thinking Small in Big Science: The Founding of Fermilab, 1960–1972

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> "Money and effort that would go into an overly conservative design might better be used elsewhere . . . A major component that works reliably right off the bat is, in one sense, a failure—it is overdesigned" (ROBERT WILSON)

What happens when science grows larger, increasingly complex, and more expensive?[1] This intriguing question has prompted numerous inquiries. Not surprisingly, much of this literature has focused on the increase in scale, and in particular on the innovations resulting from expansion. Recent scholarship has addressed, among other questions, the issue of how increased federal funding has led to radically new regulatory procedures, values, and definitions for scientific subfields, and how the growing size of instrumentation and

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[1]This article is based on work supported by the National Science Foundation under grant no. Dir-90 15473. The federal government has certain rights in this material. Any opinions, findings, and conclusions or recommendations expressed here are those of the authors and do not necessarily reflect the views of the National Science Foundation.

All interviews and documents cited herein are located in the Fermi National Accelerator Laboratory History Collection, Batavia, Illinois, unless otherwise noted. The following abbreviations are used: DOE Archives (United States Department of Energy Archives, Germantown, Maryland); Green Papers (files of G. Kenneth Green, Brookhaven National Laboratory, Upton, New York); LBNL (Lawrence Berkeley National Laboratory, Berkeley, California), Lofgren Papers (files of Edward J. Lofgren, LBNL); Malamud Papers (files of Ernest Malamud, Fermi National Accelerator Laboratory, Batavia, Illinois); McMillan Papers (files of Edwin McMillan, Lawrence Berkeley National Laboratory Archives); Mills Papers (files of Frederick Mills, Fermilab, Batavia, Illinois); Seaborg Papers (files of Glenn T. Seaborg, LBNL).

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0040-165X/96/3703-0004$01.00

teams has altered various types of scientific practice.[2] The story of the Fermi National Accelerator Laboratory (Fermilab) cuts against the grain of this historiographical trend. Recognition in the 1960s that the number of high-energy physics facilities would likely fall by about one-half, due to the enlarged scope of Fermilab and a diminishing federal budget, led to new management and siting procedures. Those building this one-of-a-kind accelerator at unprecedented expense were preoccupied with making innovations that would reduce costs, an impulse that often led them to emulate old-fashioned models. In short, the oft told story of expansion driven by innovation eclipses a more complicated reality: sometimes the development of big science is driven by contraction and shaped by a reversion to custom.

Fermilab was born in the late 1960s, when President Lyndon B. Johnson was lecturing his White House staff about reducing electricity bills as the U.S. economy strained under the burden of the Vietnam War. Like other large federally sponsored projects, Fermilab was molded by the financial constraints of the time. The disparity between a stringent funding environment and the cost of the facility forced builders to find ways to reduce the size and expense of individual components. In the process, the standard of reliability born of abundant funding in the 1950s gave way to a style that celebrated taking risks for the sake of economy and stressed the small, the modest, the underdesigned.

This ideology reflected the background of Robert R. Wilson, Fermilab's first director. Wilson had worked at the Radiation Laboratory in Berkeley during the 1930s and at Los Alamos during World War II. At Cornell in the 1950s, he developed his own cost-effective means of building accelerators, combining elements of the research and machine-building style of Berkeley's Ernest Lawrence with the methods developed under J. Robert Oppenheimer for the wartime atomic bomb project. To motivate workers to buy into his comparatively old-fashioned approach, Wilson invoked a quintessentially American model: the pioneer, that rugged individual with a zeal for conquering the unknown using native force and enduring perseverance.

New Management and Siting Plans: 1960–1967

The story of Fermilab begins with the rich funding environment fueled by World War II and the cold war.[3] This powerful cash flow

[2] Peter Galison and Bruce Hevly, eds., *Big Science: The Growth of Large-Scale Research* (Stanford, 1992) and Arnold Thackray, ed., "Science after '40," *Osiris*, vol. 7 (1992).

[3] For a comparison of the founding of Fermilab and the Japanese accelerator laboratory KEK, see Lillian Hoddeson, "Establishing KEK in Japan and Fermilab in the

led to dozens of elementary particle discoveries, numerous Nobel Prizes, international prestige, and the founding of a new scientific specialty, high-energy physics, so called because the investigation of elementary particles of smaller mass requires accelerators of ever higher energy. By 1960, the United States had a growing and illustrious community of high-energy physicists confident of continued research success and government support. This atmosphere of expectation and enthusiasm gave rise to competing proposals for proton synchrotrons several times more powerful and expensive than existing machines: a 100 to 300 GeV accelerator proposed by the Western Accelerator Group (WAG), a consortium of universities organized by the California Institute of Technology (Caltech); a 300 to 1000 GeV machine proposed by Brookhaven National Laboratory (hereafter Brookhaven); and a 200 GeV machine proposed by the Lawrence Radiation Laboratory at Berkeley (hereafter Berkeley).[4]

The million-dollar-plus price tags of the proposed accelerators put these three groups in a competition that favored Berkeley and Brookhaven. Berkeley had a glorious accelerator-building track record dating from 1930, when Lawrence and his graduate student M. Stanley Livingston built the first successful cyclotron. Wartime contacts and successful postwar lobbying by the founders of both Berkeley and Brookhaven had brought generous Atomic Energy Commis-

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US: Internationalism, Nationalism and High Energy Accelerator Physics during the 1960s," *Social Studies of Science* 13 (1983): 1–48. For an account that provides more detail on the preconstruction phase of Fermilab from 1945 to 1967, see Catherine Westfall, "The First 'Truly National Laboratory': The Birth of Fermilab" (Ph.D. diss., Michigan State University, 1988); for a preliminary summary of this dissertation see Westfall, "Fermilab: Founding the First US 'Truly National Laboratory,' " in *The Development of the Laboratory: Essays on the Place of Experiment in Industrial Civilization,* ed. Frank A. J. L. James (London, 1989). For an account of political considerations in the history of Fermilab, see Anton J. Jachim, *Science Policy Making in the United States and the Batavia Accelerator* (Carbondale, Ill., 1968). Participant accounts include M. Stanley Livingston, *Early History of the 200-GeV Accelerator* (Batavia, Ill., 1968) and Norman F. Ramsey, "History of the Fermilab Accelerator and URA." The subject is also covered in Theodore J. Lowi and Benjamin Ginsberg, *Poliscide* (New York, 1976).

[4]Matthew Sands, "A Proton Synchrotron for 300 GeV," (Pasadena, Calif., 1960), CTSL-10. Lawrence Radiation Laboratory, "Extract from LRL FY1963 Budget Submission, submitted 4-21-61," Lofgren Papers. Luke C. L. Yuan and John P. Blewett, "Experimental Program Requirements for a 300 to 1000-BeV Accelerator," (Brookhaven, N.Y., 1961). For an overview of high-energy physics in the 1950s, see Laurie M. Brown, Max Dresden, and Lillian Hoddeson, *Pions to Quarks* (Cambridge, Mass., 1989). Electron volt (eV) is a unit of energy describing the energy obtained by an electron accelerated through one volt of potential difference. GeV stands for billion electron volts.

sion (AEC) funding allotments to these labs since the late 1940s.[5] As a result, accelerator-building at Berkeley and Brookhaven had evolved from an amateur activity plied by small groups of physicists to a highly specialized profession practiced by experts adept in methodically solving technological problems and producing reliable machines. This approach contrasted with that employed at Caltech and other relatively small, cost-conscious institutions, where risks were taken in the interest of cost-effective innovation and the prewar practice of building accelerators on a shoestring budget endured. Until the mid-1960s, however, this alternate approach had been used only for smaller machines; the elite teams at Berkeley and Brookhaven built the nation's largest accelerators in the grand style they had developed in the 1950s.

Feeling they could not compete, representatives of the Western Accelerator Group dropped their proposal and Brookhaven subsequently agreed to postpone plans for a machine in the 200 range in favor of later building a 1000 GeV accelerator.[6] The Berkeley group won a further victory in 1963, when an AEC advisory panel headed by Norman Ramsey of Harvard gave top funding priority to Berkeley's proposal, rating it more highly than a proposal for a 12.5 GeV high-intensity accelerator submitted by the Midwestern Universities Research Association (MURA), a group that had been striving for a decade to obtain a first-class accelerator for the Midwest.[7]

[5]See J. L. Heilbron and Robert W. Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory* (Berkeley, 1989) for a history of the Berkeley laboratory in the pre–World War II period. For more information on postwar fund-raising, see Robert Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," *Historical Studies in the Physical Sciences*, 13, no. 2 (1983): 376–92, and Allan A. Needell, "Nuclear Reactors and the Founding of Brookhaven National Laboratory," *History Studies in the Physical Sciences*, 14, no. 1 (1983): 95–100.

[6]Alvin Tollestrup and Robert Walker, interview by Westfall, May 4, 1985. Hayden Gordon, Edward J. Lofgren, "Notes on a Meeting to Discuss the Organization of a Study of a Super High Energy Accelerator," January 2, 1962, Lofgren Papers. Lofgren, "Conference with Haworth in Washington," September 25, 1962, Lofgren Papers.

[7]High-energy machines, like the synchrotron proposed by WAG, Berkeley, and Brookhaven, accelerate particles to particularly high energies; high-intensity machines, such as MURA's Fixed Field Alternating Gradient, accelerate an especially large number of particles per second. U.S. Congress, Joint Committee on Atomic Energy, *High Energy Physics Program: Report on National Policy and Background Information* (Washington, D.C., 1965), pp. 85, 103–6.

MURA was an organization of physicists from Midwestern universities that in the 1950s and 1960s developed innovative accelerator systems and pressed for a large colliding-beams accelerator in the Midwest. For an account of the conditions in the Midwest from which MURA rose, see Leonard Greenbaum, *A Special Interest* (Ann Arbor, Mich., 1971). For an account of the political considerations that led to

Despite AEC efforts and fervent lobbying by midwestern physicists and politicians, Johnson decided in early 1964 to cut the MURA proposal from his budget. The AEC approved the Berkeley 200 BeV proposal, and it was added to the president's budget without complaint. After hearing the news, Berkeley researchers proceeded to design the 200 BeV accelerator in their customary style.[8]

By this time, however, changes that would transform accelerator-building had been set into motion. In the early 1960s growing concern over the social value of research, the role of scientists in policy making, and in particular the rising cost of large scientific projects prompted criticism of high-energy physics and increased congressional skepticism about the value of supporting expensive accelerator projects.[9] President Johnson, already committed to costly Great Society programs, needed to finance the Vietnam War, and the war was too unpopular to fund through taxation. In the resulting press for frugality, funding prospects declined for all large federally sponsored projects.[10] At the same time, physicists faced changing conditions within the physics community. In the mid-1950s, the United States had supported multiple similar accelerators, associated either with individual universities or (as in the case of Brookhaven) with regional university consortia. From 1958 to 1963, however, the number of high-energy physics accelerators had declined from fifteen to seven, in large part because high-energy physicists stopped patronizing local, smaller accelerators to reserve personnel and other resources for building and using the largest possible machines.[11] Based on this trend, high-energy physicists could expect that their facilities would again be reduced roughly by half in another few years. Thus, in 1964 those planning the 200 BeV project faced an unfamiliar and

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MURA's demise, written shortly after the event, see Daniel Greenberg, *The Politics of Pure Science* (New York, 1967). For participant accounts of the history of MURA, see "Accelerators and the Midwestern Universities Research Association in the 1950s," in Brown et al., pp. 202–12, and Francis Cole, "Oh Camelot: A Memoir of the MURA Years," 1993.

[8]At the time one billion electron volts was commonly abbreviated "BeV." Since the project was widely known by this name, this abbreviation is used when referring to the project. The use of BeV in quotes has also been left intact. Glenn Seaborg record of conversation July 17, 1963; Lyndon Baines Johnson to Hubert Humphrey, January 16, 1964, Secretariat, DOE Archives, Box 1424.

[9]Michael D. Reagan, *Science and the Federal Patron* (New York, 1969). U.S. Joint Committee on Atomic Energy, Subcommittee on Research, Development, and Radiation, *Hearings*, 89th Cong., 1st sess., Appendix 17, pp. 752–53, 756.

[10]Doris Kearns, *Lyndon Johnson and the American Dream* (New York, 1976), p. 296. Robert W. Smith, *The Space Telescope* (Cambridge, Mass., 1993) p. 72.

[11]U.S. Joint Committee on Atomic Energy, *High Energy Physics Program: Report* (n. 7 above), pp. 140, 163.

unsettling prospect: the large, expensive machine would be the only one of its kind, and no other high-energy physics facility would be built for some time.

Under the circumstances, active experimentalists and AEC officials alike were adamant that the new accelerator become, in the words of Columbia's Leon Lederman, a "Truly National Laboratory" (TNL), one "accessible *as a right* to any physicist bearing a competitively acceptable proposal."[12] For Berkeley Director Edwin McMillan, however, the TNL concept was foreign, since inside users had been favored in the apportioning of accelerator time at Berkeley for over 30 years.[13]

Faced with the worsening economic climate, tensions arising from MURA's defeat, the pressure for equitable accelerator access, and McMillan's unwillingness to accommodate such pressure, Frederick Seitz, president of the National Academy of Sciences (NAS), worked with AEC Chairman Glenn Seaborg and others to cast new management plans.[14] In early 1965, a National Academy of Sciences group

[12]Such sentiments were not unusual. Those planning other big science projects, such as ground-based astronomical studies sponsored by the Association of Universities for Research in Astronomy (AURA) and ASTRA, an orbiting space telescope project built by the National Aeronautics and Space Administration (NASA), also wanted the broadest possible base of users to channel available resources and help build coalitions to lobby for funding for their projects. See Smith for more information on ASTRA; on AURA, see Merton England, *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945–57* (Washington, D.C., 1982). Smith points out that in the case of space sciences, technical, scientific, and political factors all encouraged the recruitment of a broader base of users as projects grew larger. For example, in the case of orbiting astronomical observatories, as spacecraft grew larger and more expensive they tended to become more permanent and as a result required a larger group of users both to perform research and to form the constituency necessary for obtaining funding. It is interesting to note that at this stage, high-energy physicists were relatively unconcerned about building a coalition to lobby for funding. The main issue was not whether a large accelerator would be funded, but who would build and use the next big machine. Since high-energy physicists were confident that they would obtain funding and that users would flock to the largest accelerator, their push for a broader base of users for the new laboratory was motivated primarily by the concern for equitable allocation of resources. Leon Lederman, "The Truly National Laboratory," in *1963 Super-High Energy Summer Study*, Brookhaven National Laboratory, AADD-6, p. 10. Lederman used TNL as a pun on BNL (for Brookhaven National Laboratory), which he felt was not functioning as a truly national facility.

[13]Edwin McMillan, interview by Westfall, May 16, 1984.

[14]MURA's defeat and its effect on the political background for the 200 BeV accelerator has been noted by many writers, including Daniel Greenberg in *The Politics of Pure Science* (New York, 1967), Jachim (n. 3 above), and Lowi and Ginsberg (n. 3 above). All three writers, however, ignore the major contribution that tensions with outside users played in setting this background. Unsigned draft to Edwin McMillan,

convened by Seitz organized a new nationally based management organization, later called the Universities Research Association (URA), modeled on the Associated Universities Incorporated, the consortium of Northeastern universities that ran Brookhaven. At the same time, the AEC organized a site selection contest, collecting over 126 proposals recommending 200 sites.[15]

With site selection and management plans out of their hands, Berkeley researchers focused their attention on the design of the new accelerator, which they presented to the AEC in June 1965. For an unprecedented $348 million, they proposed a machine with an intensity of 3×10^{13} protons per pulse that would accelerate protons to 200 GeV in four stages (see fig. 1).[16] As the report noted, the design was "basically conservative" aimed at ensuring "reliable performance, rapid construction, and predictable costs."[17]

It quickly drew criticism. In a much publicized letter to McMillan, Wilson, an advocate of the shoestring approach who had built a series of electron synchrotrons at Cornell, called the design "much too conservative" and "lacking in imagination" and voiced concern that such an inflated price tag would kill the entire project.[18] Although Berkeley researchers won the support of Seitz and Seaborg and successfully defended their style in a series of debates among physicists, the design subsequently took a direct hit from the general Vietnam-era funding squeeze.[19] At the insistence of the Bureau of the Budget, watchdog of the executive branch budget, in December 1965 the AEC reluctantly asked Berkeley to estimate the cost of a

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October 7, 1964, McMillan Papers, Box 5. William Fry to G. Kenneth Green, November 3, 1964, Green Papers, Box I. Fry to McMillan, October 30, 1964, Lofgren Papers.

[15]Frederick Seitz, interview by Hoddeson, February 7, 1980. W. B. Fowler, "Meeting at National Academy of Sciences, January 17, 1965. Summary of Notes Taken by Theodore P. Wright," April 13, 1965. Seitz, "National Academy of Sciences Meeting of University Presidents, January 17, 1965," in U.S. Joint Committee on Atomic Energy, *Hearings* (n. 9 above), pp. 8–9; Seaborg to Seitz, March 2, 1965, Secretariat, DOE Archives, Box 1425. Leonard L. Bacon, "Minutes of First Meeting of Board of Trustees of Universities Research Association, Inc.," September 16, 1965, Lofgren Papers. Atomic Energy Commission, "Wide Distribution Shown in AEC List of Proposals for 200 BeV Accelerator," July 9, 1965, press release, Seaborg Papers.

[16]Lawrence Radiation Laboratory, *200 BeV Accelerator Design Study*, vol. 1 (Lawrence Radiation Laboratory, June 1965), pp. I-5, I-8.

[17]Ibid., p. XVI-1.

[18]Robert Wilson to McMillan, September 27, 1965.

[19]Glenn Seaborg, "Meeting of Board of Trustees, Universities Research Association, Inc.," December 12, 1965, Seaborg Papers. Seitz to Norman Ramsey, December 14, 1965.

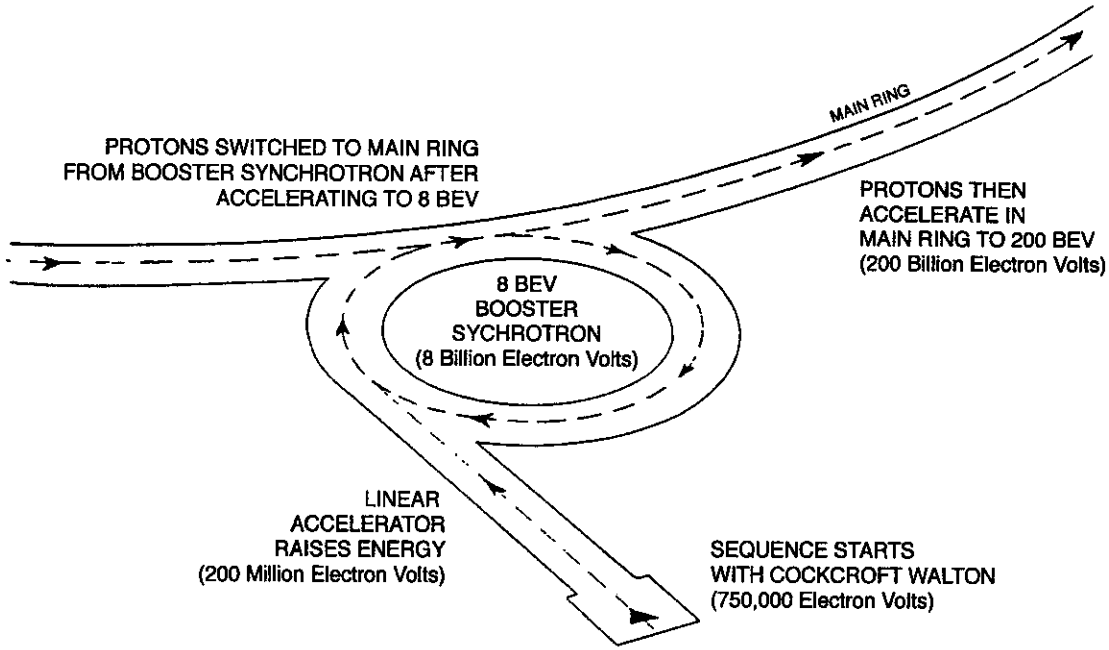

FIG. 1.—Schematic showing four-stage acceleration to 200GeV. (Adapted from Lawrence Radiation Laboratory, *200 BeV Accelerator Design Study*, Volume I [Lawrence Radiation Laboratory, June 1965]. All drawings by Gary Westfall.)

scaled-down accelerator. Within the month the Berkeley design group presented two schemes in which economy was achieved by reducing capability.[20]

In 1966 site selection took center stage as a fierce contest erupted for community prestige and economic benefit. In the course of the year, amid considerable press coverage and particularly strong lobbying by Midwestern politicians, six finalist sites were chosen, including the Berkeley group's California site and Weston, an Illinois site.[21] The latter drew the ire of the National Association for the Advancement of Colored People (NAACP), which argued that the accelerator should not be built in Illinois since the state had not passed legislation to enforce open occupancy laws and had a history of housing discrimination.[22] But pressured for a Midwestern accelerator by politicians and physicists, and facing the push for outside users' rights, the commissioners nonetheless chose the Illinois site on December 7, 1966.[23]

Two crucial questions remained before the project could proceed to the construction phase: would the project be funded, and if so, who would build the machine? Timing was against those promoting the project. Funding approval required the support of the Joint Committee on Atomic Energy (JCAE), which was scheduled to consider construction funding in the wake of the site decision and the resulting loud complaint from the NAACP and politicians who had championed losing sites. In addition, at just this juncture, Edward Lofgren, head of the Berkeley design team, turned down the URA offer to direct the new laboratory, in part because of his displeasure with the Illinois site.[24] At such a sensitive moment, the project could ill afford to have another candidate refuse the directorship. This turn of events led to another change in management plans. Wilson, who was unexpectedly available because he had finished the Cornell electron synchrotron ahead of schedule, was willing to build the accelerator in Illinois and within budgetary constraints. After some de-

[20]In one scheme intensity was decreased, in the other, repetition rate was decreased. Edward J. Lofgren, "On the Costs of an Accelerator with Reduced Initial Capabilities," December 13, 1965, Lofgren Papers; Lofgren to Paul McDaniel, December 14, 1965, Lofgren Papers.

[21]National Academy of Sciences Site Evaluation Committee, *Report,* March 1966.

[22]Glenn Seaborg, record of conversation, July 13, 1966, Seaborg Papers; Henry Traynor to Seaborg, James Ramey, and Gerald Tape, July 29, 1966, Secretariat, Box 7741, DOE Archives.

[23]On how the site contest was decided see Catherine Westfall, "The Site Contest for Fermilab," *Physics Today* 42 (1989): 44–52. Seaborg record of conversation, December 7, 1966, Seaborg Papers.

[24]Lofgren to Ramsey, January 12, 1967, Lofgren Papers.

liberation, the URA approached Wilson about the position of laboratory director.[25]

At first the AEC commissioners were unenthusiastic about the choice of Wilson, remembering with irritation his 1965 criticism of the Berkeley design. His risk-taking, cost-cutting approach to accelerator building was diametrically opposed to the more conservative and careful Berkeley style of the 1950s. In addition, the commissioners questioned whether the physics community, and Berkeley in particular, would accept Wilson's appointment.[26]

In a series of informal meetings, Seaborg and Ramsey, who had been named URA president, ironed out difficulties between Wilson and the Berkeley group and introduced Wilson to key JCAE members.[27] In the subsequent congressional hearings, the AEC successfully defended its choice of the Weston site and promoted the idea of a machine that could later be expanded in size.[28] The latter was a shrewd maneuver: with an expandable machine, the JCAE could feel it was getting the accelerator originally promised and the AEC could still keep within the $250 million budget set by Bureau of the Budget.[29]

These efforts paid off. The JCAE recommended in June authorization of $7 million of the $10 million requested to begin construction. Congress eventually passed the bill to Johnson, who signed it into law on July 26, 1967. In granting the $7 million, the federal government signalled a commitment to build the new laboratory. In return, Congress expected an expandable machine for $250 million. The stage was set for a major change in U.S. accelerator-building.[30]

[25]Wilson officially accepted the directorship on March 1, 1967. Wilson to Ramsey, March 1, 1967. See also Norman Ramsey, interview by Hoddeson, February 26 and 27, 1980, and Edwin Goldwasser, interview by Westfall, July 10, 1985.

[26]Glenn Seaborg, Diary, typescript, January 16, 1967, Seaborg Papers.

[27]Glenn Seaborg, Diary, typescript, February 14, 1967, Seaborg Papers. D. Keefe, "Report on Meeting Between LRL Personnel and the Atomic Energy Commission," February 14, 1967, McMillan Papers, Box 2.

[28]U.S. Joint Committee on Atomic Energy, *Hearings*, 90th Congress, 1st sess., pp. 97, 22–394.

[29]Although Berkeley designers had considered ideas for expandability since 1964 and were about to publish a well-honed expandability scheme, the idea now became associated with Wilson, since the commissioners had just introduced him to the JCAE and were stressing his willingness to apply innovative ideas and produce an expandable machine within stringent budget limitations. Al A. Garren, Glen Lambertson, Edward Lofgren, and Lloyd Smith, "Extendible Energy Synchrotron," *Nuclear Instruments and Methods* 54 (1967): 223–25.

[30]The granting of construction funding for an accelerator project was taken as assurance of continued federal funding. The 1993 cancellation of the Superconducting Super Collider reversed this trend. U.S. Joint Committee on Atomic Energy,

The Initial Wilson Design: 1967–1968

In 1967, a 200 GeV-range accelerator could be achieved without any major advance in accelerator technology. Wilson's challenges were of a different sort. Many physicists questioned whether an expandable 200 GeV accelerator could be built for $250 million. The disappointed Berkeley designers, as well as other experts, doubted Wilson's ability to succeed at his difficult task, citing his lack of experience in building large proton synchrotrons and his reputation for taking risks. In this atmosphere of budgetary constraint and skepticism, Wilson had to assemble a team and build the world's largest synchrotron at a site many considered unappealing. He built the accelerator using an approach previously employed for smaller machines that he tailored to accommodate his own aesthetics, including a preference for a holistically conceived, spare design. This style was particularly evident in building the main ring, our focus in the following discussion. The main ring, the largest, most expensive component of the accelerator, marks the success of the machine, since protons are accelerated there last.

Wilson, a practicing sculptor as well as a physicist, began designing the machine by sketching and then holding in his mind an image of the whole machine. This holistic approach was the first of many distinctions that would separate his work from that done at Berkeley and Brookhaven. As he recently explained, "[in the 1960s and thereafter], most professionals," for example, the expert builders at Berkeley and Brookhaven, "[would] divide the work into parts and then sit down and do a professional job on each part." This tactic made as little sense, he thought, as creating the sculpture of a human head by parceling out the design of the eyes, the forehead, and the nose, and then sticking the parts together. Wilson felt that good design required developing each part to enhance the development of every other part. He also tried to keep his design fluid, so that innovative ideas could be incorporated.[31]

When executing his early sketches, Wilson drew a number of circles on a map of the site to find the largest possible diameter. He considered accelerating protons to 200 GeV as having been "Berkeley's job . . . from the beginning I was thinking about 1000 GeV."[32] However, he did not relinquish the ideal of frugality. Whereas the

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Atomic Energy Commission Authorizing Appropriations, FY 1968, 90th Cong., 1st sess., pp. 36, 57–59; Lyndon Johnson to Glenn Seaborg, July 26, 1967, Seaborg Collection, Box 170, DOE Archives.

[31] Wilson, interview by Westfall, April 1, 1987.

[32] Wilson, interview by Hoddeson, January 23, 1981.

Berkeley expandability scheme called for leaving out a substantial fraction of the magnets, which would later be inserted to reach 400 GeV, Wilson planned to incorporate expandability into the initial design by using magnets capable of being ramped up to higher fields so that the same ring could later reach at least 400 GeV.[33] Since more than a thousand magnets would be needed in the main ring to carry out this initial plan, and since magnet costs consume a large part of any accelerator's budget, Wilson's scheme forced designers to conceive of innovative ways to achieve maximum capability at minimal cost, a demand that drove the entire construction effort.

Wilson combined his aesthetic preference for the open, fluid, and spare with a complementary emphasis on creativity, frugality, and lack of specialization. Plans for the 1967 summer study design effort proceeded in line with this value system. Wilson went out of his way to choose attractive quarters for the design study, which could not be held on the Weston site because the land was not yet available. The environment at the rented offices at Oak Brook, a western suburb of Chicago, reflected the state of the design discussions, for the unfinished quarters were wide open—without walls, partitions, furniture, nothing but an expanse of tile floor reaching to the windows with their panoramic view of Illinois farmland ten floors below. Wilson hoped that the view, the intellectual freedom, and the mix of accelerator-builders who came to the workshop from all over the country, including Berkeley and Brookhaven, would encourage creative approaches to designing the accelerator he wanted. To counterbalance the focus on accelerator-building concerns and emphasize the predominant importance of high-energy physics research, Wilson convinced the prominent theoretician Robert Serber to give lectures on relevant theoretical topics, a service Serber had also performed in the 1940s in the midst of the design of the atomic bomb at Los Alamos and the design of accelerators at Berkeley. To emphasize that the laboratory would serve the entire national community of high-energy physicists, Wilson named the new facility the National Accelerator Laboratory (NAL).[34]

Wilson also firmly believed in the benefits of speed. For him, speedy construction of an accelerator facility encouraged creativity,

[33] The later addition of a ring of superconducting magnets would then allow reaching 1,000 GeV. Lillian Hoddeson, "The First Large-Scale Application of Superconductivity: The Fermilab Energy Doubler, 1972–1983," *Historical Studies in the Physical and Biological Sciences,* 18, no. 1 (1987): 25–54.

[34] Goldwasser, interview by Westfall, May 15, 1987. Robert Serber, interview by Westfall, February 24, 1986. Wilson, interview by Hoddeson, January 12, 1979. Don Getz, May 1977, untitled manuscript. Hoddeson (n. 3 above), p. 20.

saved personnel costs, and facilitated timely research progress. The design study therefore proceeded with the hurried pace associated with an earlier era in American physics, and in particular with the 1930s Berkeley Laboratory and with wartime Los Alamos. As a result, major features of the main ring emerged in the first weeks at Oak Brook. In late July, about a month into the summer study, Wilson froze these features so the ring could be completed, as scheduled, by early 1968.[35]

Wilson's leadership style was calculated to encourage rapid progress; he set clear priorities, dominated decision making, and sometimes made decisions abruptly. For example, after preliminary calculations those working on design of the the main ring agreed that the radius would need to be about 1,000 meters but expected that they would have some leeway in developing the design. Instead Wilson announced that the radius would be exactly 1,000 meters, explaining that a round number would be easier for everyone to remember. All discussion on the issue was then suspended. As Wilson wrote a few years later, "one soon finds that a bad decision is better than no decision, for even a bad decision is a basis of action and eventually it can be corrected."[36]

Wilson's emphasis on action was characteristic of an underlying ethic, rooted for him in a pioneering past. Wilson remembers encountering this pioneering ethic as a boy in Frontier, Wyoming. Individualism, a passion for attacking the unknown, and the expectation that persistence would vanquish any obstacle became part of his scientific heritage while he was Lawrence's graduate student at Berkeley in the 1930s. As he explained: "I learned from Lawrence to define what you want and then, damn, make it come out that

[35] Heilbron and Seidel note that in the 1930s Lawrence was known for instituting "the California habit of speed," and that this tendency was characteristic of American science at the time; Heilbron and Seidel (n. 5 above), pp. 36, 264. For a discussion of speed with reference to research at Los Alamos, see Hoddeson, "The Los Alamos Implosion Program in World War II: A Model for Postwar American Research," in *The Restructuring of Physical Sciences in Europe and the U.S. 1945–1960,* ed. Michelangelo de Maria, Mario Grilli, and Fabio Sebastiani (Singapore, 1989) pp. 32–41, and Lillian Hoddeson et al., *Critical Assembly: A History of Los Alamos during the Oppenheimer Years, 1943–1945* (Cambridge, Mass., 1993). See also Wilson, interview by Westfall, April 1, 1967, and Wilson, "National Accelerator Laboratory Synchrotron," January 23, 1967.

[36] Robert Wilson, "My Fight against Team Research," *Daedalus* (Fall, 1970): 1076–87. See also Wilson, interview by Westfall, May 25, 1987; Drasko Jovanovic, interview by Westfall and Hoddeson, November 29, 1989; Francis Cole, interview by Westfall, March 13, 1987; and Thomas Collins, interview by Westfall and Hoddeson, November 29, 1989.

way . . . you don't think about what other people regard to be impossible . . . you work as hard as you can . . . you don't ever say no, ever." During World War II, Wilson experienced this ethic again at Los Alamos. Since he saw his career as a pioneering endeavor, Wilson was quick to cast his staff in the role of pioneers. Building on the prairie setting of Illinois, Wilson, a charismatic storyteller, evoked pioneering images, thus tapping into a powerful American idiom to motivate workers as they struggled to meet his demand for maximum energy at minimal cost.[37]

Wilson's preference for nonspecialization and the importance he assigned to the role of the research physicist reflected a bygone era when researchers routinely built accelerators. In stark contrast to the elite at Berkeley and Brookhaven, who saw accelerator-building as a profession best practiced by experts, Wilson insisted that research physicists "could design things that would be much prettier, sparser, and cheaper than the things the . . . professionals would design" because they were more likely to focus their attention on research goals rather than technologically impressive innovations. Although Wilson was happy to make use of the skills of some specialists—such as Berkeley's Alper Garren, who had considerable expertise in the exacting calculations necessary for designing the placement of components in the main ring—Wilson criticized the orderly, highly specialized Berkeley design effort, which in his opinion showed the undue influence from the specialists he called "engineering types." The European Organization for Nuclear Research (CERN) accelerator laboratory provided an even more striking example of the consequences of the strong influence of engineering values. As the CERN historians explain, since engineers there at this time were the only ones with expertise with large scale equipment, they were often "the 'real bosses' of the laboratory." Since their predominant professional concern was engineering innovation, not physics, "they remained detached from the urgency of research and the needs growing from it."[38]

Much of Wilson's old-fashioned approach to accelerator-building—his focus on research priorities during the design process, his dominating leadership style, his rapid pace, and his emphasis on frugality—hearkened back to his graduate student years with Law-

[37] For a discussion of the pioneer ethic and how it has driven American technology, see Eugene S. Ferguson, "The American-ness of American Technology," *Technology and Culture* 20 (1979): 3–24. Wilson, interview by Westfall, May 25, 1987.

[38] Wilson, interview by Westfall, May 25, 1987. Dominique Pestre and John Krige, "Some Thoughts on the Early History of CERN," in Galison and Hevly (n. 2 above), p. 95.

rence. As he later noted: "I had a notion about how to proceed with a design that was derived . . . for a different time . . . it came directly from Lawrence." In that preprofessional era, accelerators were built at breakneck speed, using whatever resources were available, by the enthusiastic cadre of experimentalists led by Lawrence, the undisputed driving force at the Berkeley laboratory.[39]

Wilson spurred the Oak Brook designers to design an accelerator that incorporated his values. One distinctive feature of the eventual product, contributed by Brookhaven physicist Gordon Danby, was the "separated function" design for the main ring magnets. This scheme used different—cheaper—magnets for bending the beam.[40] Although the Berkeley designers had earlier rejected Danby's suggestion, the decision to use a separated function design for the main ring was made "almost without discussion," since the designers realized that for a large synchrotron this approach would allow achieving higher magnet fields.[41] To further efforts to achieve the highest field and at the same time reduce the size of the main ring bending magnets, Wilson hit upon the idea of modifying a conventional "window frame" coil construction by fitting added coil into the unoccupied space on either side of the poles, where the radiation level is high and the potential radiation damage to insulation is substantial. This design therefore risked radiation damage of the coil at the expense

[39]Wilson, interview by Westfall, May 25, 1987. Wilson's style contrasted sharply with Lawrence's in one important aspect. Lawrence characteristically built the largest, most powerful accelerator he could afford. For example, as LBNL historians explain, the rectangular magnet design for Lawrence's sixty-inch cyclotron "was wasteful of material." Lawrence justified its size simply by noting, "we can get the money for [it]." While for Lawrence, larger was better, for Wilson, smaller was better. As Thomas Collins, a key NAL accelerator designer, explains: "It was Wilson's style to whittle." If two designs looked equally promising "he'd take the smaller one, every time." For Wilson, a spare, clean design was both virtuous and aesthetically pleasing. See Heilbron and Seidel (n. 5 above), pp. 283–84, and Collins, interview by Westfall and Hoddeson, November 29, 1989.

[40]Separate function magnets were independently proposed in late 1952 by Toshio Kitagaki in Japan and by Milton White at Princeton, after they realized that intermittent focusing is sufficient in a strong focusing accelerator. Toshio Kitagaki, "A Focusing Method for Large Accelerators," *Physical Review* 89 (1953): 1161–62, and M. G. White, "Preliminary Design Parameters for a Separated-Function Machine," Princeton, N. J., March 3, 1953.

[41]Arie Van Steenbergen, "200–400 BeV Accelerator Summer Study," July, August 1967, p. 7. The separated function magnets could achieve 15–20 percent higher central-orbit fields for the same peak fields in the aperture than combined function magnets. Francis Cole, "Progress Report on the NAL Accelerator," *Particle Accelerators* 2 (1971): 1–11. See also Denis Keefe, Glen Lambertson, and L. Jackson Laslett, interview by Westfall, December 22, 1986, and Wilson, interview by Westfall, April 1, 1987.

of containing the beam very precisely, an economizing feature since beam extraction in the new accelerator could then be limited to only one point, a novel feature for a synchrotron. To reduce the risk of radiation damage, Wilson insisted that the magnets be assembled with as little epoxy as possible, since epoxy is particularly sensitive to such damage.[42]

Wilson continued to press for higher energy as the summer study proceeded. He got encouraging news when Garren, with the help of Lee Teng, finished the design of the lattice, the arrangement of components in the main ring. Garren and Teng calculated that a 1,000 meter ring filled with the new bending magnets run at 18 kilogauss, interspersed with a few quadrupole magnets, would allow 400 GeV acceleration.[43] This news whet Wilson's appetite for even higher energies. In a characteristic move, he pushed the goal higher: if his staff could stretch performance of the bending magnets to 22.5 kilogauss, the machine could accelerate beam to 500 GeV.[44]

To achieve his goal of maximum energy at minimal cost, Wilson aimed for the most compact overall dimensions of the bending magnets and simplified methods for coil insertion and fabrication. The NAL staff chose a high-field H-shaped magnet rather than the larger C-shaped magnet in the Berkeley design. Also, they sized the magnet aperture to match beam properties, which allowed a smaller vertical aperture for half of the bending magnets. At Wilson's insistence, the coil insertion was also novel and cost-effective. Although previous proton synchrotrons used coils that were mounted mechanically in the magnet core, Wilson planned to save manufacturing costs by using glued-in coils. Fabrication was also simplified; drawing on his Cornell experience, Wilson planned to fabricate both types of bend-

[42]The innovative beam extraction system was possible because of the development by Alfred Maschke of the electrostatic septum, which extracts protons using an electric field and then deflects them out of the accelerator with a magnet. This device, along with the long straight section, formed an extraction system with an efficiency of 99 percent, much higher than that achieved in previous accelerators. James A. Sanford, "The Fermilab National Accelerator Laboratory," *Annual Review of Nuclear Science* 26 (1976): 151–98. See also National Accelerator Laboratory (NAL), *Design Report,* January 1968, p. 5-3; Wilson, interview by Westfall, April 1, 1987; Robert Wilson, "National Accelerator Laboratory Synchrotron," July 23, 1967; Robert Wilson, "Some Aspects of the 200 GeV Accelerator" (paper presented at the Sixth International Conference on High Energy Accelerators, Cambridge, Mass., September 12, 1967), p. 4.

[43]The lattice, refined by Collins when the components were installed, also contained straight sections for beam handling and radio frequency acceleration.

[44]Wilson, interview by Westfall, April 1, 1987. National Accelerator Laboratory, *Design Report,* pp. 4-1, 5-5. Livingston (n. 3 above), p. 21. Wilson, "Some Aspects of the 200 GeV Accelerator," p. 5.

ing magnets (B1 and B2) and quadrupole magnets using die-stamped laminations.[45] The use of pared-down magnets was inherently risky. While less copper and iron meant lower cost, it also compromised the potential of the magnet, since the greater amount of steel in larger magnets produces higher fields more reliably. But the simplifications bought considerable savings. Cost estimates later that year showed that although the NAL design called for almost twice the number of main ring magnets needed in the Berkeley design, the Berkeley magnets required almost 10 tons more steel per magnet and cost a total of almost $6 million more.[46]

Other cost-cutting features in NAL's main ring design, as presented to the AEC on schedule in January 1968, included a smaller and less expensive magnet enclosure tunnel with no cranes or air conditioning (see figs. 2 and 3). In addition, although previous accelerator magnets were secured with caissons, Thomas Collins had designed the magnets to sit on glacial till. Wilson planned to align magnets using a controversial system of stretched wires. And while the Berkeley group had allowed seven years for construction, Wilson planned to cut personnel costs by completing the accelerator by June 1972, in just four and a half years. Thanks to such reductions, Wilson was able to present a design for a $248 million 200 GeV accelerator expandable to as much as 500 GeV.[47]

Wilson's cost-effective design followed a larger pattern emerging about this time for large, federally sponsored projects. As historian Robert Smith has noted in another context, for the most part "to 1969, NASA designed to get the job done; after 1969, NASA designed to cost." Like the scientists at NASA, Wilson responded to congressional pressure to reduce costs by tailoring the budget to fit an expected figure—in Wilson's case the $250 million set by the Bureau of the Budget in 1967 hearings. In doing so, Wilson strengthened the new laboratory's funding prospects; Congress was more likely to

[45] B1 magnets have $1^1/_2$ inch vertical gaps while the B2 magnets have 2 inch gaps. The sizes differ to take into account the changing dimensions of the beam. See also Livingston, p. 21; Wilson, "Some Aspects of the 200 GeV Accelerator," pp. 4–5; Ernest Malamud and James K. Walker, "Progress and Prospects at the National Accelerator Laboratory," December 1970, p. 3; Collins, interview by Westfall and Hoddeson, November 29, 1989.

[46] The Lawrence Radiation Laboratory (LRL) magnets were estimated to weigh 19,415 tons total and cost $26.6 million, while the NAL magnets were estimated to weigh 9,750 tons and cost $20.9 million. Lawrence Radiation Laboratory, *200 BeV Accelerator Design Study* (n. 16 above), pp. III-9, III-10, XVI-4; National Accelerator Laboratory, *Design Report*, pp. A-4, 16-3.

[47] Lawrence Radiation Laboratory, *200 BeV Accelerator Design Study*, p. XVI-16; National Accelerator Laboratory, *Design Report*, p. 16-11.

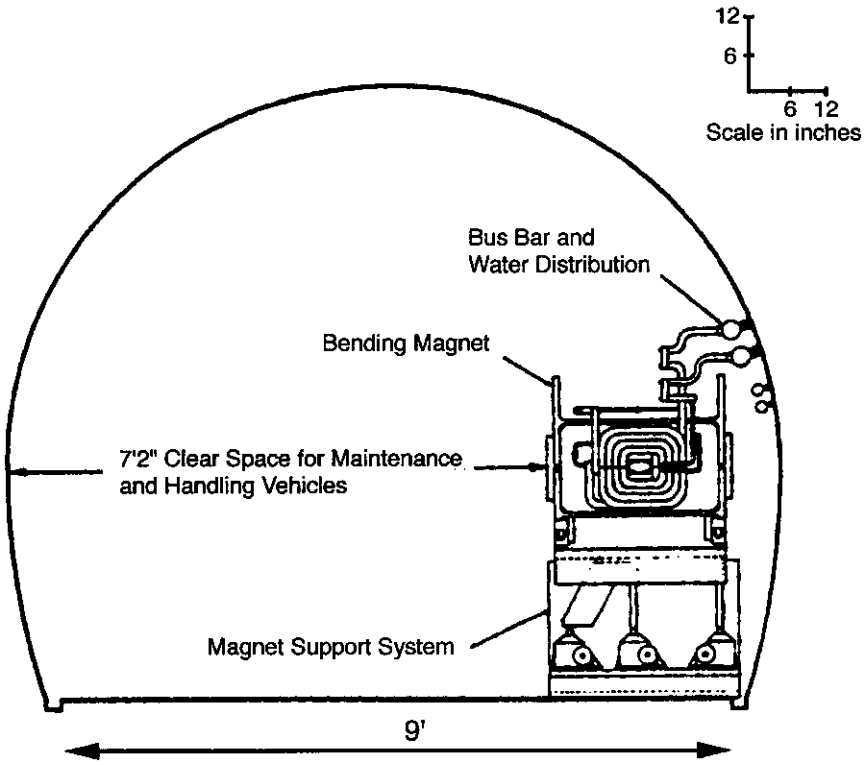

FIG. 2.—Diagram of Wilson's NAL design for the magnet enclosure. Note that this enclosure is approximately half the size of the Berkeley design (fig. 3). (Adapted from National Accelerator Laboratory, *Design Report*, January 1968.)

fund a project that met its expectations and the JCAE and the AEC were more likely to champion a project positioned for congressional approval. The JCAE and AEC were also more likely to support a budget-conscious project since physics funding, which had been rising steadily since the end of World War II, had reached a plateau due to the economic strain of the Vietnam War. High-energy physics funding was 40 percent below the level projected by the AEC in 1965.[48]

[48]Smith (n. 10 above), quote on p. 392. See also Spencer Weart, "The Physics Business in America, 1919–1940: A Statistical Reconnaissance," in *The Sciences in the American Context: New Perspectives*, ed. Nathan Reingold (Washington, D.C., 1979), p. 328; High Energy Physics Advisory Panel, "The Status and Problems of High Energy Physics Today," January 1968, p. 38.

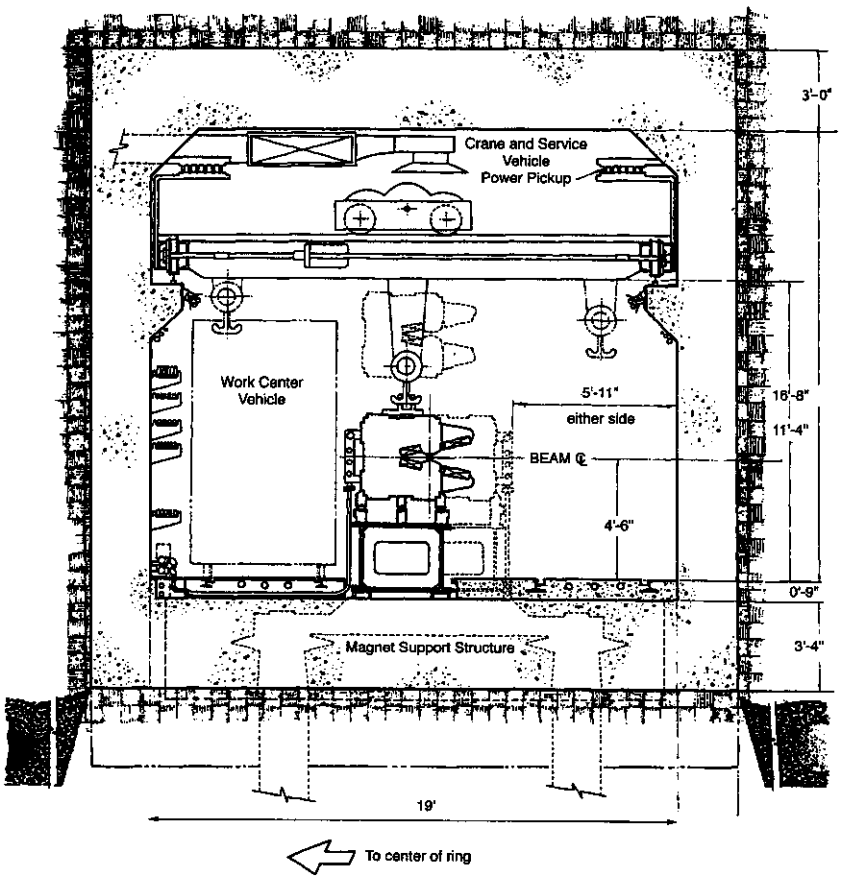

FIG. 3.—Diagram showing the Berkeley proposed design for a magnet enclosure. (Adapted from National Accelerator Laboratory, *Design Report*, January, 1968.)

The new attention to budgets in big science did not come without other costs. Short-term cost reductions led to higher long-term expenses; in the case of Wilson's design, less copper in the magnets reduced construction costs but promised to increase operating costs. And curtailed design meant greater risks taken, some so radical that failure was assured. As URA president Ramsey later explained, Wilson took chances on about twenty aspects of the design, saving about $5 million per risk. "We knew something would fail," he noted, "but we figured it would be much less expensive to fix the failure than to play it safe with all 20 items." Critics disagreed, calling Wilson's

approach unacceptable in view of the cost and importance of the project.[49]

Despite such criticism, Wilson eventually found acceptance within the physics community. By early 1968 Wilson and his deputy director, Edwin Goldwasser, had successfully recruited a staff with a wide range of expertise. Recruitment was easier than usual because MURA had been disbanded and funding was running low at both the Cambridge Electron Accelerator and the Princeton-Pennsylvania Accelerator. At Wilson's insistence, the AEC had granted approval authority to a local AEC office, to avoid red tape and expedite construction. With the necessary staff, URA, AEC, and JCAE approval, and streamlined federal administrative procedures, Wilson was poised to build the world's most powerful accelerator, his way.[50]

Modeling the Main Ring: 1968–1969

Building an accelerator typically follows the standard stages of a large construction project. After the preliminary design is done the focus shifts to model-building, in which aspects of the design are built (often in miniature) or computer-modeled to test basic characteristics. After refinements in design, prototypes are constructed to test full-size components. Then, following further development, construction specifications are frozen and building begins.[51]

Model-building dominated the attention of the main ring group until mid-1969. One distinctive feature was the early construction of full-size models (either prototypes or structural models) to check overall dimensions. For example, a model of a section of the main ring tunnel was installed that included full-scale mockups of magnets, vacuum system, and water and power connections. Model-making encouraged the holistic approach that Wilson favored, because full-sized models helped staff members envision how parts fit together and anticipate fabrication problems.[52] Although the em-

[49] Ramsey, interview by Hoddeson, February 26 and 27, 1980. See also Don Getz, untitled report, May 1977. "Questions Raised on the Design of the 200 BeV Accelerator," no date, Mills Papers.

[50] Wilson to Seaborg, February 28, 1967; Ramsey, interview by Hoddeson, February 26 and 27, 1980.

[51] M. Stanley Livingston, "Design Progress at the National Accelerator Laboratory," June 1969, p. 1. Malamud, interview by Westfall, March 12, 1987.

[52] Malamud, interview by Westfall, March 12, 1987; interview by Westfall and Hoddeson, October 24, 1989. Cole, "Main Ring Group Meeting," March 27, 1968; "Monthly Report of Activities," August 1, 1968, p. 4, and September 1, 1968, pp. 4–5.

phasis on model-making showed Wilson's influence on the main ring group during this period, the pace and tone of the effort were set by Frank Shoemaker, a veteran accelerator-builder on leave from Princeton, known for his careful, deliberate style.[53]

Because of the lack of facilities that resulted from Wilson's frugality, Shoemaker, like the other group leaders, used the resources of other laboratories, in particular the shop space and computing facilities at nearby Argonne National Laboratory.[54] Various means were found to provide inexpensive, easily constructed work space when the group moved onto the site. To continue work on the magnets and begin on the vacuum system, the main ring group purchased and installed in June 1968 an inflatable building, dubbed the "air building." The series of problems that developed with this building demonstrates the difficulties faced by the NAL staff when working in the primitive conditions of the new laboratory. In August, a portion of the vacuum chamber model had to be extended through a hole, since the structure was not long enough to house the entire unit. On one windy day the building collapsed, fortunately causing no damage. The Illinois winter brought additional problems. The group discovered that the building stayed only 30 degrees Fahrenheit warmer than the ambient temperature, not the 50 degrees advertised by the manufacturer. Only the installation of gas heaters prevented freezing temperatures inside the building. Although Shoemaker hoped to replace the air building with a more substantial metal structure by the next summer, because of the continued shortage of funding and space the group used the air building until early 1970.[55]

The main ring group made various innovations during this period. In the summer of 1968 a fixture was developed for welding together flanges of adjacent magnets. Since it was assumed that magnets would occasionally need replacement, Walter Pelczarski developed a clever means for cutting apart these flanges, an innovation

[53]Philip Livdahl, "A Brief Summary of Fermilab during Initial Construction Years," November 1983, pp. 9, 15.

[54]Early vacuum chamber and model work was done at the University of Wisconsin, and computer modeling was sometimes performed by a data telephone link to New York University. James Maclachlan, interview by Westfall, November 27, 1989. Cole, "Monthly Report of Activities," April 1, 1968, p. 2, and June 1, 1968, pp. 3, 4, 6; Wilson, "Some Aspects of the 200 GeV Accelerator" (n. 42 above), p. 5.

[55]"Main Accelerator Section Monthly Report," November 1968 and December 1968. Cole, "Monthly Report of Activities," September 1, 1968, p. 6. Ryuji Yamada, interview by Hoddeson and Westfall, October 25, 1989. "Minutes of Staff Meeting Main Ring," August 27, 1969. "Minutes of the Main Ring Staff Meeting," January 28, 1970.

that would later prove to be much more useful than anyone imagined at the time.[56] Engineer Richard Cassel developed an innovative magnet power supply system. Previous large proton synchrotrons had used motor-flywheel generator sets. In contrast, the NAL design employed power supplies that were equipped with solid-state, controlled-thyristor rectifiers. This system, one of the first computer control systems for accelerator magnets, was controlled by program-activated firing circuits. Another unusual feature of the power supply was the energy storage system. After studying and costing the conventional method of local storage, they decided to use the Commonwealth Edison Company's network as a power storage system.[57]

By the spring of 1969 the "protomain"—a prototype of the tunnel large enough to contain one complete cell, consisting of eight bending magnets and two quadrupoles—had been built and the final lattice design had been refined. After some cost-cutting design modifications, the group was ready for the prototype stage.[58]

Despite the group's accomplishments, some felt the main ring effort from 1968 to mid-1969 had a relaxed, academic atmosphere at odds with Wilson's stress on speed and economy. Visible progress in other groups added pressure; in April 1969 the group building the linear accelerator produced the laboratory's first beam with the prototype preaccelerator, and construction was underway on the linear accelerator building and the booster enclosure.[59] Just as Wilson was beginning to worry that the main ring effort would lag behind the rest of the project, Shoemaker announced that he would return to Princeton. Wilson seized this opportunity to redirect the main ring group. In an irregular move he appointed himself as Shoemaker's successor. To help alleviate the administrative burden of simultaneously acting as laboratory director and a group leader, he formed a main ring management "troika," including Ernest Malamud, Cassel, and Henry Hinterberger.[60]

[56]Cole, "Monthly Report of Activities," June 1, 1968, p. 6, and October 1, 1968, p. 3. "Main Accelerator Section Monthly Report," March 31, 1969.

[57]Livingston (n. 3 above), p. 21; Wilson, interview by Westfall, February 13, 1990. Sanford (n. 42 above), p. 165; Cole, "Progress Report on the NAL Accelerator" (n. 41 above).

[58]Cole, "Monthly Report of Activities," April 30, 1969, p. 6. Livingston, "Design Progress" (n. 51 above), pp. 11–12.

[59]Malamud, interview by Westfall, October 24, 1989. Yamada, interview by Hoddeson and Westfall, October 25, 1989. Cole, "Monthly Report of Activities," May 31, 1969, p. 2.

[60]Livdahl (n. 53 above), p. 10. Wilson, interview by Westfall, May 25, 1987.

The First Flush of Success: 1969–1971

About the time he appointed himself main ring group leader, Wilson made another eccentric decision. Although standard procedure called for freezing the design after prototyping began—and prototyping and even construction had begun on various parts of the accelerator—Wilson asked staff members to reassess accelerator plans as part of a laboratory-wide campaign against "heavy-footed overdesign." In Wilson's view, keeping the design fluid at this late stage would spur creativity aimed at achieving the overriding goal of maximum energy at minimum cost. He was explicit about the consequences of such design practice: the "money and effort that would go into an overly conservative design might better be used elsewhere . . . Failure should be designed into a successful machine . . . A major component that works reliably right off the bat is, in one sense, a failure—it is overdesigned."[61]

Under Wilson's direct leadership, the main ring group responded promptly to this challenge. From May to December 1969 the group made numerous design changes to simplify or lower the cost of the main ring magnets. They devised a more compact design of both quadrupole and bending magnets and developed a new method of fabricating bending magnet coil in which inner coils and outer coils were constructed separately. Outside manufacturers could then continue fabricating outer coils, while the more exacting inner coil window frame fabrication could proceed under the supervision of NAL staff.[62] Other aspects of main ring design were also made more economical; the stretched wire system used to help align the quadrupoles, for example, was replaced with a survey system using a laser beam.[63]

A prime example of the effect of Wilson's preference for nonspecialization can be found in the work of Ryuiji Yamada, who successfully boosted the capability of the accelerator from 400 to 500 GeV. In late 1969, the accelerator theory group still considered the prevailing bending magnet design, which yielded an 18 kilogauss field, to be optimal. Yamada, then a young experimentalist, reassessed the calculations to see if a higher field could be achieved. Assuming the

[61] Wilson, "Sanctimonious Memo #137," July 11, 1969. See also Malamud, interview by Westfall and Hoddeson, October 24, 1989.

[62] Cole, "Monthly Report of Activities," May 31, 1969, p. 5; "Monthly Progress Report," September 30, 1969, pp. 8, 9; "Progress Report on the NAL Accelerator," p. 5. "Monthly Report Main Ring Section," October 1969, p. 2.

[63] Cole, "Monthly Report of Activities," November 30, 1969, p. 9, and January 31, 1970, p. 6. "Minutes of the Main Ring Staff Meeting," January 7, 1970.

role of theorist, he explored alternate calculations of design. Assuming the role of the engineer, he then decided how the new design should be fabricated. Next, assuming the role of the accelerator physicist, he constructed a short model based on his calculations, made magnetic measurements of the model, and plotted the data. He then repeated the process, creating a further revision based on the results and after many iterations emerged with a new, tapered design in which the decrease in the magnetic field at the edge of the magnet was offset by saturation effects due to the shape of the pole tips.[64] Yamada's approach proved remarkably effective. By December, calculations predicted that the field shape with Yamada's design would be "acceptable without further correction up to 21 kilogauss." In the next few months tests showed that the magnets were in fact good at fields up to 22.5 kilogauss.[65]

The main ring group provided other examples of what happened when Wilson's design style was put into practice. Despite Wilson's stated prejudice against engineers, they played key roles in the project. Two engineers in the troika, Cassel and Hinterberger, made critical contributions to the main ring design. Hinterberger devised an unconventional, cost-effective "box girder" design for main ring magnets.[66] William Hanson, who headed the magnet factory, solved a last-minute minor crisis in magnet production by setting up a vacuum impregnation system capable of accommodating a 20-foot, 15-ton magnet.[67] Anthony Glowacki, who designed the magnet

[64] "Minutes of the Main Ring Staff Meeting," December 3, 1969, and December 17, 1969. Yamada, interview by Hoddeson and Westfall, October 25, 1989.

[65] Cole, "Monthly Progress Report," December 31, 1969, p. 8. See also Cole, "Minutes of the Main Ring Staff Meeting," December 3, 1969; "Monthly Progress Report, April 30, 1970," pp. 11–12.

[66] In the 1968 design, the steel yokes, which were made of thousands of thin laminations, were secured with a steel I-beam. When the twenty-foot structural models were built according to two alternate designs using this plan, they sagged. Hinterberger realized they could solve the problem by inserting angle girders around the four outside corners of the magnet, eliminating the I–beam. He remembers "pointing out that the laminations themselves" could secure the yoke because "the structure was strong enough to support itself." In this "box girder" design, the iron laminations weighed less and magnetic properties improved because the girders became part of the magnetic flux return circuit. Henry Hinterberger and Robert Wilson, tape recording, June 21, 1982. See also Livingston, "Design Progress" (n. 51 above), p. 13; National Accelerator Laboratory, *Design Report* (n. 42 above), p. 5-1; Cole, "Progress Report on the NAL Accelerator" (n. 41 above), p. 8.

[67] Yamada's tapered pole tip left an empty space, since the original design had been square, and the main ring group had been filling the space with plaster. Malamud remembers discovering that the plaster was "sopping wet." Magnet production halted. Hanson's solution provides another example of the importance of engineering skill to the main ring effort. He suggested that the void be filled with epoxy by vacuum impregnation, a technique that had been used successfully on booster

water cooling system and made other key contributions, was also an engineer. However, these engineers were unlike Berkeley's specialists or the semiautonomous technological perfectionists at the European laboratory, CERN. Wilson wanted the laboratory to be built in line with research priorities, but successful implementation of his building style required the participation of specialists—such as Alper Garren and Lee Teng, who designed the main ring lattice—as well as the participation of engineers. In practice, the accelerator-builders who came closest to Wilson's ideal were those who could blend skills from engineering, research physics, and accelerator science.

Comparison with contemporary events at CERN places Wilson's style in an international perspective. When analyzing why the European laboratory in the 1950s and 1960s lagged behind American high-energy physics laboratories, CERN historians note that a great gap separated the CERN engineers, with their focus on technological perfection, and the physicists, who urgently needed "an 'imperfect' piece of equipment ready at the right moment" rather "than a 'perfect' one ready when the dust of the battle had settled." They argue that in the United States this gap closed sometime between the 1930s and 1960s due to a peculiarly American process, a "profound symbiosis previously unknown in basic science, a fusion of 'pure' science, technology, and engineering." They then describe "a new kind of researcher" who sounds much like Wilson's ideal scientist, someone "who can be described at once as a physicist, i.e., in touch with the evolution of the discipline and its key theoretical and experimental issues," who is also a "conceiver of apparatus, [an] engineer, i.e., knowledgeable and innovative in the most advanced techniques . . . and [an] entrepreneur, i.e., capable of raising large sums of money, of getting people with different expertise together, of mobilizing several kinds of human, financial, and technical resources."[68]

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magnets. Holding to the view that the use of epoxy was undesirable because of the increased danger of radiation damage due to the magnet design, Wilson vehemently opposed the change. Hinterberger and Robert Sheldon agreed. However, at Malamud's suggestion, Hanson was given two weeks, which included the Christmas holidays, to show that his scheme would work. Laboring day and night, Hanson set up a vacuum impregnation system capable of accommodating a twenty-foot, fifteen-ton magnet. In the end, Wilson was convinced, and magnet construction was altered to include vacuum impregnation. Malamud, interview by Westfall and Hoddeson, October 24, 1989. See also Malamud to Mrs. Hanson, March 7, 1980, Malamud Papers.

[68]Pestre and Krige (n. 38 above), p. 90; Dominique Pestre, "Some Characteristic Features of CERN in the 1950s and 1960s," in *History of CERN,* vol. 2, *Building and Running the Laboratory,* ed. Armin Hermann et al. (Amsterdam, 1990), p. 799.

Wilson's focus on economy continued to suit the times. As historian Spencer Weart has noted, if all figures are adjusted to reflect 1972 dollars "federal support for physics peaked at about $350 million in 1967 and was only 70 percent of this in 1975, rising only a little thereafter."[69] In this funding environment, the laboratory consistently received smaller allocations than requested. When NAL received formal approval for the project in early 1971, the laboratory was short $93 million of its construction budget.[70] Although there was little threat at this point that funding would be terminated, limited allocations forced further economies to keep the entire project under construction. Cost overruns, even if they were caused by delays beyond Wilson's control, would have chipped away at the considerable political capital he had accumulated by designing to cost and thus thrown future funding into jeopardy.

Always one to seize opportunity, Wilson turned the necessity of budget limitations into a virtue, using fiscal pressure as an excuse to squeeze further efficiencies from his staff. He insisted that the project could not be completed within the budget if time was wasted. To add to the pressure, he made goals more ambitious or shortened the deadlines whenever possible. For example, when Wilson became main ring group leader in 1969, he advanced the deadline for the installation of main ring components from January 1, 1972, to July 1, 1971. The stress from ambitious schedules augmented that caused by constant risk-taking, primitive working conditions, and the pressure to find further economies. Surprisingly, staff members found working on the main ring to be stimulating and fun. Wilson used adversity to build esprit de corps and continued to use imagery to reinforce the theme that staff members were pioneers struggling with nature to advance a new frontier. In staff meetings and public presentations he characterized the project as a "kind of adventuresome thing," and a "pioneering effort."[71]

Rapid progress in the next year and a half displayed the power of Wilsons's strategies. In October 1969, ground was broken for the

[69]Weart (n. 48 above), pp. 327–28.

[70]Transcript, Second User's Meeting, December 2, 1968. U.S. Joint Committee on Atomic Energy, *Hearings*, 91st Cong., 1st sess., p. 1214. Cole, "Monthly Report of Activities," July 31, 1969, p. 1.

[71]For a full explanation of the strategies Wilson used to recruit and motivate his staff during the construction years see Catherine Westfall, "Vision, Rhetoric, and Persuasion: Robert Wilson and the Creation of Fermilab," 1994, Fermilab Pub. 94/162. Wilson, "Wilson to Whole Staff," October 1, 1969. See also James Maclachlan, interview by Westfall November 27, 1989; Jovanovic, interview by Westfall and Hoddeson, November 29, 1989; Malamud, interview by Westfall and Hoddeson, October 24, 1989.

main ring enclosure. Spurred by the ambitious new deadline of having eight bending magnets and two prototype quadrupoles installed and operating in the protomain by March 20, 1970, the group forged ahead, working frantically to prepare for rapid, cost-effective magnet production, skipping from model-building to production whenever possible, stimulating competition between contractors to lower bids, and implementing efficient factory systems.[72]

By January 1970, arrangements had been made for factory production of magnets. Although Wilson's desire to cut costs and leave the design flexible as long as possible did not allow for full-scale mass production, the December 1969 coil design did enable partial mass production.[73] By this time, the necessary materials for magnet construction had been procured—a nontrivial task, since orders for some items, such as coils, saturated the international market of producers.[74] At this stage, Wilson was confident enough of the group's overall progress to publicly announce to NAL users in April that the laboratory would "have an accelerated proton beam by mid-1971, a year earlier than the originally scheduled date." He also noted that the machine would soon thereafter reach energies close to 500 GeV. Just as magnet assembly began on site in May, Wilson announced a new milestone: by October 1, the group was to have one-sixth of the main ring ready to handle an accelerated beam. In the next four months the group scurried to meet this goal. By August, 200 magnets and most of the water piping and magnet power system had been installed in the tunnel. By the end of September, the milestone had been reached.[75]

At this point, the booster was partially completed and the linac could accelerate protons to 139 MeV (million electron volts). Wilson faced a problem, however. He had to this point actively encouraged the groups working on individual components to have a strong group loyalty. With the whole accelerator almost ready to function, Wilson decided to set up a new technical challenge, to force coordination among groups and identify technical weak spots. The Sep-

[72] Cole, "Monthly Progress Report," October 31, 1969, p. 8, and March 31, 1970, p. 6.

[73] Wilson, interview by Westfall, May 25, 1987. "Minutes of the Main Ring Meeting," December 10, 1969. For more on the further development of mass-produced magnets at Fermilab, see Hoddeson (n. 35 above), pp. 25–54.

[74] Cole, "Monthly Progress Report," December 31, 1969, p. 8. Malamud, interview by Westfall, March 12, 1987.

[75] Wilson, "Statement Made by R. R. Wilson at the Second Annual Meeting of the NAL Users Organization on Friday, April 10, 1970." See also "Minutes of the Main Ring General Meeting," May 28, 1970; Cole, "Monthly Report of Activities," August 31, 1970, p. 12, and September 30, 1970, p. 3.

tember monthly progress report announced the goal, dubbed Oktoberfest, of "accelerating protons to 139 MeV in the linac and transporting them through half the Booster and into the Main Accelerator."[76] While some thought the Oktoberfest little more than an ineffectual stunt—the components were not designed for such low energies and the expected beam tests could not be performed—others insisted that the exercise had been worthwhile, since a large amount of equipment was installed in record time and the entire NAL staff was forced to work as a team for the first time since the beginning of construction.[77]

The main ring group spent the next five months working feverishly to complete installation. By the following March, 800 magnets had been installed and about half of the power supply modules had been tested. Despite various difficulties, including the need to wade through mud in the main ring tunnel to install magnets in the fall of 1970, in April 1971 the last magnet was installed. In the course of installation, the group enjoyed one pleasant surprise. Advances in the technology of thyristors, used as rectifiers in the magnet power supply, coupled with unexpected savings in the cost of transformers, allowed the laboratory to install a power supply capable of achieving 500 GeV for less than that previously estimated for 200 GeV.[78]

Despite minor crises, Wilson was confident about the laboratory's prospects as the mid-1971 deadline approached. But a series of technical disasters soon cast his building style into doubt and destroyed the dream of building the world's most powerful accelerator one entire year ahead of schedule.

Problems with the Main Ring: 1971

The NAL and URA leaders expected parts of the accelerator to fail. But they were not prepared for the traumatic summer and fall of 1971. Because of the tight schedule, the spare magnets had been installed in the midst of severe winter weather. In spring, the ventilating system brought in humid, warm air, causing water to condense on the cold magnets. Some magnets eventually contained "as much as a quart of water." When the NAL staff tried in May to bring the more than 1,000 main ring magnets into operation under these con-

[76]Cole, "Monthly Report of Activities," September 30, 1970, p. 1. See also Wilson, interview by Westfall, April 1, 1987.

[77]Malamud, interview by Westfall, March 12, 1987. Helen Edwards, interview by Westfall, March 13, 1987. Cole, "Monthly Report of Activities," October 31, 1970, p. 1.

[78]Wilson, "Statement," "Main Accelerator Monthly Report," October 1970. Cole, "Monthly Report of Activities," March 31, 1971, p. 1, and April 30, 1971, p. 1.

ditions they found, to their horror, that a high percentage shorted. By summer 1971, magnets were failing at an alarming rate, putting the entire project in jeopardy.[79]

The staff were not sure what was causing the trouble. Using the trial and error empiricism characteristic of the rushed atomic bomb effort, they tried a succession of improvements.[80] In the meantime other difficulties surfaced, the product of haste and untried technology. Among the most disabling were minor but chronic linac and booster problems, which complicated main ring problems since beam studies could only be performed when both accelerators were operating well. There were problems as well with power supplies and with obstacles in the main ring vacuum tube.[81]

Disadvantages of Wilson's style now became apparent. The emphasis on action often led to unproductive chaos. Researchers at CERN were inclined to proceed more slowly and cautiously, and as a result, in the opinion of researchers familiar with the construction of both machines, when confronted with accelerator problems the European laboratory employed a methodical approach better suited for solving complex technical problems. Physicists at CERN were also quick to conclude that the NAL's magnet problems revealed the superiority of their own customary emphasis on technological perfection; as Leon Lederman recalls, CERN officials openly gloated over Wilson's misfortunes. Problems at the NAL also seemed to lend credence to Berkeley and Brookhaven criticism of Wilson's design style. Outside users also complained, since many had arranged sabbatical leaves to allow them to prepare experimental equipment based on Wilson's optimistic projections. Rumors surfaced of a campaign to convince the URA to remove Wilson from the directorship.[82]

[79]Cole, "Monthly Report of Activities," June 31, 1971, p. 2. See also Cole, "Monthly Report of Activities," May 31, 1971, p. 2.

[80]Hoddeson et al., *Critical Assembly* (n. 35 above). Cole, "Monthly Report of Activities," October 31, 1971, p. 2. William Hanson to distribution, October 18, 1971. Malamud, interview by Westfall and Hoddeson, October 24, 1989.

[81]To remove the obstacles, researchers first tried, unsuccessfully, to train a ferret, affectionately named Felicia, to drag a harness through the tube. Eventually they developed a mechanical spear capable of pulling a cord through 2,650 feet of vacuum tube. Cole, "Monthly Report of Activities," August 1, 1971, p. 2, and October 31, 1971, p. 3. Malamud to all members of Main Ring Section, August 2, 1971. "Steering Meeting," September 23, 1971. Wilson, interview by Westfall, April 1, 1987, p. 14.

[82]Pestre and Krige (n. 38 above). Jovanovic, interview by Westfall and Hoddeson, November 29, 1989. J. Richie Orr, interview by Westfall, March 12, 1987. Wilson, interview by Westfall, April 1, 1987. Goldwasser, interview by Westfall, May 15, 1987. Leon Lederman, interview by Westfall, November 26, 1990.

In an attempt to rally the laboratory staff and solve the main ring problems, Wilson again drew on an old-fashioned model, devising a new management plan that in its flexibility and disregard for hierarchy resembled tactics used at Los Alamos during World War II.[83] Merging the various groups working on the accelerator, he diverted workers from the experimental areas to help with main ring problems. Next he appointed three "strong Managers," J. Richie Orr, Richard Lundy, and Philip Livdahl and, in parallel, assigned each component of the accelerator to a commissioner. Wilson himself tightly coordinated the entire effort. Orr remembers that the resulting work assignments focused the efforts of technical experts on technical problem solving, leaving organizational decisions to those with managerial skills.[84]

Fifteen years later, experts still do not agree about the cause of the various main ring problems, or even on their relative severity. The standard explanation for the magnet failures blames the epoxy insulation, which had been made very thin to decrease the possibility of radiation damage. This too-thin insulation developed tiny cracks that allowed water to be absorbed and short the magnet. But veteran accelerator-builder Collins blames the decision to glue in the coils, arguing that thermal and mechanical stress from temperature cycling cracked the epoxy in the glued-in coils, allowing water to enter.[85] Collins feels that the B1 magnets were simply too small; Drasko Jovanovic suspects that the power supply was the major source of difficulty. The consensus is that fragmentary bookkeeping, inadequate diagnostic tools, and insufficient understanding of the underlying physics prevent a complete explanation of the problems that plagued the main ring in 1971.[86]

Whatever their cause, the problems began to disappear once the three managers focused the entire laboratory on them. In time much of the main ring was filled with reconditioned or newly built magnets that displayed a lower failure rate. Although some magnets

[83] On problem-solving strategies at the wartime atomic bomb project, see Hoddeson et al., *Critical Assembly*.

[84] Wilson to Ramsey, October 29, 1971. Orr, interview by Westfall, March 23, 1989. Wilson, "Formation of the Accelerator Section," memorandum to the staff, October 21, 1971.

[85] Goldwasser, interview by Westfall, May 15, 1987. Wilson, interview by Westfall, May 25, 1987. Collins, interview by Westfall and Hoddeson, November 29, 1989.

[86] Collins, interview by Westfall, June 4, 1990. Yamada, interview by Westfall and Hoddeson, October 25, 1989. Maclachlan, interview by Westfall, November 27, 1989. Collins, interview by Westfall and Hoddeson, November 29, 1989. Jovanovic, interview by Westfall and Hoddeson, November 29, 1989. Malamud, interview by Westfall and Hoddeson, October 24, 1989.

still failed (and continue to fail, twenty years later), workers learned how to replace them quickly, a job facilitated by Pelczarski's pipe cutting scheme. During the crisis approximately 350 magnets failed, which caused six months to be lost. About 10 percent (approximately $2 million) of the original cost of the magnets was spent to overcome difficulties.[87]

The Triumph of Frugality: 1972

Throughout the rest of the winter the NAL staff made steady progress, and on January 22, 1972, they produced a 20 GeV beam that seemed stable from pulse to pulse. The beam energy continued to rise. On February 11, a 100 GeV beam broke the world's record for proton energy; 200 GeV was achieved on the afternoon of March 1, 1972, and the champagne flowed. Wilson triumphantly announced to the AEC and JCAE that the project had come in ahead of schedule and under budget, even though NAL had not yet received $50 million of its construction budget.[88]

Wilson's announcement was well received in Washington, where hard times for large federally funded projects continued. That year planners of the Hubble Space Telescope decided to cut the project from $700 million to around $300 million to produce "a cheaper, and thus politically more feasible, telescope." To meet this goal they employed many of the budget-cutting tactics used by Wilson: competitive bidding with contractors, skipping the model-building phase, designing cost-effective components, and transferring costs from the construction stage to the operation stage. Unlike Wilson, but like Berkeley researchers, they also considered design changes that would reduce research capability. When the final $400 million to $500 million price tag came up on Capitol Hill in June 1974, however, Congress denied funding, throwing the project into crisis and delaying its completion.[89] The NAL was positioned more favorably in Washington, in large part because Wilson had produced what Congress expected and stayed within the budget, no matter what

[87] Orr, interview by Westfall, March 12, 1987. Cole, "Monthly Report of Activities," February 30, 1972, p. 2. U. S. Joint Committee on Atomic Energy, Subcommittee on Research and Development and Radiation, *Hearings*, 92nd Cong., 2d sess., p. 1435.

[88] The project finished $20 million under budget, money that Wilson used to support research on building a superconducting accelerator. Hoddeson (n. 3 above), p. 35. See also Cole, "Monthly Report of Activities," January 31, 1972, p. 1, and March 1, 1972. U. S. Joint Committee on Atomic Energy, Subcommittee on Research and Development and Radiation, *Hearings*, Appendix 3, p. 1731.

[89] For the story of how the project was saved see Smith (n. 10 above), chaps. 4 and 5.