



Fermi National Accelerator Laboratory

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Catherine Westfall

*Michigan State University, Lyman Briggs School
East Lansing, Michigan 48824*

Lillian Hoddeson

*University of Illinois at Urbana-Champaign, Department of History and Physics
Urbana, Illinois 61801*

*Fermi National Accelerator Laboratory
P.O. Box 500, Batavia, Illinois 60510*

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FRUGALITY AND THE FOUNDING OF FERMILAB, 1960-72

Catherine Westfall

Michigan State University

Lyman Briggs School

East Lansing, MI, 48824, USA

Lillian Hoddeson

University of Illinois at Urbana-Champaign

Departments of History and Physics

Urbana, Illinois, 61801, USA

and

Fermi National Accelerator Laboratory

P.O. Box 500

Batavia, Illinois 60501

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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 over-designed.” Robert Wilson

1 Introduction

Fermi National Accelerator Laboratory (Fermilab) was born in the 1960s, at just the time 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 the stringent funding environment and the unprecedented expense of the new facility made the traditional standard of reliability in building particle accelerators give way to an ideology that celebrated taking risks for the sake of economy and stressed the small, the modest, and the underdesigned.²

Robert R. Wilson, Fermilab’s first director, 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 tradition that Ernest

Lawrence established in Berkeley with the methodology developed under J. Robert Oppenheimer for the wartime atomic bomb project. Fermilab's early years reflected the merging of these earlier traditions. All three drew on a quintessentially American cultural idiom: the pioneer, that rugged individual with a zeal for conquering the unknown using native force and enduring perseverance.

2 FUNDING AND THE PLANNING YEARS— 1960-1967

2.1 Genesis of the Proposal, 1960-1964

Enthusiasm and confidence in the high energy physics community, bred in part by the bounteous funding and physics breakthroughs of the 1950s, was percolating by the early 1960s into plans for new accelerators of unprecedented energy and expense.³ Several ideas for machines in the several 100 GeV range emerged. Matthew Sands of the California Institute of Technology (Caltech) generated the first such plan at a 1959 summer study sponsored by the Midwestern Universities Association (MURA).⁴ At this meeting MURA physicists challenged colleagues to design an economically feasible fixed-target machine (300 GeV) capable of producing the same energy at the point of collision as their proposed colliding-beam machine, the Fixed Field Alternating Gradient Synchrotron (FFAG).⁵ Most physicists at this time felt that colliding beam machines were the only practical route to high energies, since the principle of Alternating Gradient, or “strong” focusing, put forth in 1952 at Brookhaven National

Laboratory (BNL) by M. Stanley Livingston, Ernest Courant and Hartland Snyder, had not yet been demonstrated.⁶

Sands took up the MURA group's challenge and designed a fixed-target 100 to 300 GeV pulsed strong focusing synchrotron consisting of several accelerators. Higher energy was to be achieved by cascading accelerated beam from one machine into another: a linear accelerator ("linac") would inject protons into a "booster" synchrotron, which in turn would send the protons into a larger synchrotron of relatively small magnet aperture.⁷ Sands estimated that building this several hundred GeV machine would cost "of the same order" as a 10 GeV FFAG accelerator.⁸

In collaboration with Caltech colleagues Alvin Tollestrup and Robert Walker, Sands worked further on the design of the 300 GeV proton accelerator. In September 1960 they estimated the cascade machine would cost \$77 million. Since the project was too expensive for Caltech alone, the Caltech group joined in January 1961 with groups from several other California Universities – the University of California, Los Angeles, the University of California, La Jolla (now San Diego) and the University of Southern California – to form the Western Accelerator Group (WAG). In April, WAG submitted a proposal to the Atomic Energy Commission (AEC) for \$593,000 to support a fifteen-month study of the design for the new proton synchrotron.⁹

By this time, the Alternating Gradient Synchrotron (AGS) at Brookhaven National Laboratory (BNL) and the Proton Synchrotron (PS) at CERN, the European high-energy laboratory in Geneva, had proved the feasibility of strong focusing.

This demonstration spurred plans for large strong focusing synchrotrons at both the Lawrence Radiation Laboratory (LRL) and at Brookhaven. In April 1961, LRL director Edwin McMillan submitted a proposal to the AEC to design a machine in the 200 GeV range. Berkeley's design study, estimated for \$3 million for a two-year period, was five times more expensive than WAG's. A Brookhaven group under John Blewett also began to prepare a preliminary design study for the U.S. part of a two-nation study for an international accelerator in the 300-1000 GeV range.¹⁰

The expense of the accelerator proposals put these three groups in a competition, which favored Berkeley and Brookhaven, even though WAG had prepared the most complete, innovative, and cost-effective proposal. Berkeley had a long, glorious accelerator building tradition dating from 1930, when Lawrence and his graduate student Livingston built the first successful cyclotron. Wartime contacts and successful post-war lobbying by the founders of both LBL and Brookhaven had brought generous AEC funding allotments to these labs since the late 1940s.¹¹ As a result, accelerator building at LRL 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. The Berkeley and Brookhaven teams were the uncontested elite in the building of large accelerators. Unsure of Caltech support and convinced they couldn't compete against the elite accelerator builders, WAG representatives decided in late 1961 to drop their proposal.¹²

Berkeley's believed that its claim to the 200 GeV machine superceded Brookhaven's. They argued that, since in the 1950s LRL and BNL had alternated in building the largest machines and BNL had recently completed the 30-GeV AGS, LRL should have the next turn.¹³ After negotiations with both the AEC and LRL, BNL representatives agreed in early 1962 to continue the policy of alternating large accelerator projects with LRL. With the understanding that they would submit a proposal for a 1000 GeV accelerator for the longer term future, BNL withdrew from the competition for an accelerator in the 200 GeV range.¹⁴

Having triumphed over Brookhaven and WAG, the LRL proposal still faced competition from other accelerator projects vying for AEC support. Following the precedent set by the National Science Foundation (NSF) in the 1950s, the AEC convened a panel of prominent physicists with experience in making science policy to evaluate all the proposals.¹⁵ Early in December 1962 this panel, headed by Norman Ramsey of Harvard, began to consider proposals, including the LRL 200 BeV proposal, as it was known in the vernacular of the time, the BNL proposal for a 1000 GeV machine, and MURA's proposal for the 12.5 GeV high-intensity FFAG.¹⁶ In the end, on 26 April 1963, the panel recommended that the AEC authorize the 200 BeV design proposal and that BNL design a 600 to 1000 GeV range accelerator. It gave MURA's FFAG only conditional approval, specifying that the project be continued "without permitting this to delay the steps toward higher energy."¹⁷

In the parlance of advisory committees, such conditional support meant lack of

support; as Ramsey later admitted, the recommendation was MURA's "kiss of death." Despite AEC attempts to save the FFAG and the fervent lobbying of Midwestern physicists and politicians, who were convinced that the Midwest was being deprived of its fair share of research funding, Johnson decided in early 1964 to cut the MURA proposal from his budget. The AEC approved the LRL and BNL proposals and both were added to the President's budget without complaint.¹⁸

Early planning for the 300 GeV Super Proton Synchrotron (SPS) at CERN reflects a less efficient decision making process. As the CERN historians reveal, serious discussion about CERN's 300 GeV machine began just after the CERN Proton Synchrotron (PS) began operation in 1960, about the same time discussions about 200 to 300 GeV machines began at BNL and LRL. However in Europe momentum for planning the new machine was slowed because CERN physicists lacked managerial and long-term planning skills and were reluctant to request funding for a new accelerator before the research potential of the PS had been more thoroughly exploited. In the early 1960s, planning proceeded at a slow pace for an "ideal" program, which included the SPS, considered top priority, and two intersecting storage rings (ISR) to be added to the PS. In 1964, when the problem of high accelerator costs and limited budgets for accelerators became apparent in Europe, simultaneous construction of both projects proved financially impossible. Since physicists followed "the decision-making process more episodically and from a greater distance" than machine builders, who wished to build the technologically more interesting ISR, the choice between the two projects

was not made on the basis of scientific priorities. As a result, CERN administrators and financial authorities decided to first build the less expensive ISR, delaying plans for the SPS.¹⁹

The funding of LRL's 200 BeV design proposal continued a tradition that had predominated in the U.S. during the 1950s. But MURA discontent along with budgetary and political pressures would soon force the 200 BeV supporters to search their repertoire for a new approach to building large accelerators. An alternate approach had, in fact, already taken root during the 1950s at smaller institutions where builders struggled to compete with fewer resources. It celebrated risk taking in the interest of cost-effective innovation and continued the prewar practice of building accelerators on a shoestring budget. Elements of this alternate approach were present in MURA's FFAG proposal, which included innovations such as colliding beams and radiofrequency beam manipulation, in the accelerator building effort at Cornell, where Wilson completed a series of innovative electron synchrotrons under budget, and in WAG's proposal for the 300 GeV machine. This more cost-effective tradition became increasingly appealing as the 1960s wore on.

2.2 New Management and Siting Plans, 1964-mid-1965

Growing concern in the early 1960s over the social value of research and the role of scientists in policy making prompted close scrutiny of science funding by a number of Congressional committees. At the same time, the rising cost of high energy physics

bred complaints from those working in other fields. In a 1963 *Minerva* article, Alvin Weinberg of Oak Ridge presented a method for assessing the relative value of scientific programs; he rated high energy physics poorly. Such sentiment focused public attention on the funding of high energy physics and heightened Congressional skepticism about the value of supporting expensive accelerator projects.²⁰

In the meantime, physicists grew concerned about management of the new laboratory. Up to this time, accelerators had been associated either with individual universities or (as in the case of Brookhaven) with a regional consortium of universities. However, by the mid-1960s, the government expected large projects, for example, ASTRA, an orbiting telescope project built by National Air and Space Association (NASA), to serve a national community of users.²¹ Seaborg noted in 1964 that the Stanford Linear Accelerator (SLAC), which had been funded in 1960 as a single-university facility, was “the last of its kind.” Although in theory McMillan supported the idea of equitable accelerator access, in practice he found it difficult to devise a management scheme that would satisfy the demands of active experimentalists, such as Columbia’s Leon Lederman, who insisted that the new facility should be a “Truly National Laboratory” (TNL), one “accessible *as a right* to any physicist bearing a competitively acceptable proposal.”²²

Severe anti-Berkeley sentiment had been prompted by LRL’s practice of favoring inside users in apportioning accelerator time, a practice begun in the 1930s, when Lawrence granted requests for accelerator time from researchers at other institutions

only on an invitational basis. MURA physicists, who were bitterly disappointed after the demise of the FFAG, forcefully endorsed the argument for outside user rights and fair allocation of resources. Lederman and others serving on an AEC-sponsored advisory committee insisted that the new laboratory be managed by a corporation with nationwide representation. However McMillan vehemently opposed such a plan.²³

Amid complaints about the rising cost of high energy physics, tensions arising from MURA's defeat, and the pressure for equitable accelerator access, Frederick Seitz, President of the National Academy of Sciences (NAS), worried that continued bickering between McMillan and his advisory committee would doom funding prospects for the new 200 GeV machine. When McMillan and his advisory committee were unable to agree upon a management scheme after a year of negotiation, Seitz invited 25 university presidents, AEC Chairman Glenn Seaborg, and other federal officials to a January 1965 meeting to cast plans for the new laboratory. While LRL was finishing the technical design for the new accelerator, this NSF group organized a new nationally based management organization, later called the Universities Research Association (URA), modeled on the Associated Universities Incorporated (AUI), 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.²⁴

By mid-1965, Berkeley's prospects for the future were not nearly as bright as they

had been two years earlier. The realization that only one large accelerator could be built had fueled the movement for outside user rights and killed the MURA proposal. Furthermore, resulting tensions had led to the formation of URA and the site selection contest.

2.3 Berkeley's Design and Its Critics, June 1965-January 1966

In June 1965, as site proposals were flooding the AEC, Berkeley presented its preliminary design for the new machine. 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, from a Cockcroft-Walton preinjector, into an Alvarez-type linear accelerator, then to a rapid-cycling injector synchrotron, and finally through the main synchrotron.²⁵

Although favoring the California site chosen by Berkeley, the design report mentioned that the facility would “be a national facility ... open to all on the basis of the scientific merit of the experiments proposed.” The designers estimated that “approximately 70% of the experimental program” would be conducted “by visitors and 30% by the resident staff.” LRL also appeared willing to cooperate with the URA. But in most other ways the design reflected a vision congruent with Berkeley's grand tradition of the 1950s. For example, the design called for extensive experimental areas and costly large C-shaped magnets set on substantial girders. As the report itself aptly summarized, the design was “basically conservative” aimed at ensuring

“reliable performance, rapid construction, and predictable costs.”²⁶

Within a few months, the Berkeley design fell victim to heavy objection. Wilson wrote McMillan that, although details were “most professionally worked out,” the design was “much too conservative,” and “lacking in imagination.” In addition, it had been made “without enough regard for economic factors.” He noted that “as someone who helped disassemble radios for parts in the old days at the Radiation Laboratory,” he was “offended” by the Berkeley cost estimate, which he characterized as “ridiculously high.” Wilson worried that such an inflated price tag would both kill the 200 BeV project and endanger the future of the 600-1000 GeV machine.²⁷

In an attached paper, “Some Proton Synchrotrons, 100-1000 GeV,” Wilson proposed a rough alternate design consistent with his philosophy of modest accelerator designing. He urged that all components be kept “simple and understandable.” For example, he featured H-shaped magnets, which were smaller and therefore less expensive than C-shaped magnets. The paper announced: “There is no reason to have fancy cranes, etc., nor should the tunnel be too comfortable on the few occasions when one might have access to it.” Spartan guidelines also applied to the design of experimental facilities and research equipment. The estimate for components of the 200 GeV was only \$70 million. With research equipment and shielding, Wilson’s proposed machine totaled “less than 100 million dollars,” with completion in only 3 years.²⁸

However, in debates over the Berkeley design at two meetings held in January 1966,

Berkeley's tradition triumphed. Both Seitz and Seaborg were opposed to alternate proposals, which they feared would divert the course of the 200 BeV project.²⁹

2.4 Construction Funding and Selection of Director, mid-1965-mid-1967

As the debate over the Berkeley design emerged in mid-1965, President Johnson, faced with the expenses of the escalating Vietnam war and costly Great Society programs, began to stress frugality. The AEC, NASA, and other federal agencies felt the squeeze. At just this point, the AEC faced the task of obtaining the first allotment of construction funding for the new accelerator. Although Berkeley had lost its bid to manage the new facility, in early 1966 the California laboratory still hoped that its site would be chosen for the accelerator. Over the next year, LRL would completely lose hold of the project.³⁰

The 200 BeV proposal was too expensive to avoid attention as the funding environment grew more austere. The Bureau of the Budget (BOB), the watch-dog of the executive branch budget, pressured the AEC to pare down its accelerator project. Thus, in December 1965 the Commission reluctantly asked LRL to estimate the cost of a scaled down accelerator. Within the same month, the LRL design group presented two alternate schemes. In one, the injector would produce one tenth the intensity; in the other the radiofrequency and magnet power supply systems would have a reduced repetition rate. The estimated reductions were \$88 million for the first scheme and \$84 million for the second. In both, economy was achieved by reducing capability.³¹

Site selection then took center stage as a fierce contest erupted for community prestige and economic benefits. Seitz and Seaborg arranged for the AEC and NAS to collaborate in evaluating the large number of site proposals. In September 1965, after President Johnson, a Texan, intervened in favor of a site in Austin, Texas, the AEC forwarded a list with 85 sites to the NAS Site Evaluation Committee. In March 1966, the NAS committee submitted a list of six finalist sites to the AEC Commissioners, who would chose the winner. The list included the California site and Brookhaven; the latter had lost hope of building the 1000 GeV accelerator in light of the tightening budget. The other sites were Ann Arbor, Michigan; Denver, Colorado; Madison, Wisconsin; and two Illinois sites – South Barrington and Weston.³²

For the first time, political considerations outside the physics community were a factor in planning an accelerator. Local response to the site contest reached a near frenzy as citizens groups and politicians lobbied for each site, showering the AEC with telegrams, letters, and petitions. Midwestern politicians pushed particularly hard since MURA's defeated FFAG proposal had become a cause célèbre for those lobbying for equitable geographical distribution of federal research funds. Ironically, Illinois, which had the strongest Congressional support according to an AEC tally, was forced to withdraw the South Barrington site due to local opposition. More trouble came for Illinois in late June 1966, when Seaborg received a letter from Clarence Mitchell, director of the Washington Bureau of the National Association for the Advancement of Colored People (NAACP), complaining that Illinois had not passed legislation to

enforce open occupancy laws and had a history of housing discrimination. In response, the AEC mounted an extensive campaign to ensure civil-rights compliance at finalist sites, a move that prolonged the contest until late 1966.³³

The highly publicized site contest actually increased the funding prospects by gaining large-scale political support for the project. However, this advantage did not come without cost. Many physicists found the public spectacle distasteful and the loss of control disturbing, especially since many felt that the site of this one-of-a-kind facility would be an important factor in its success. The contest also increased divisiveness within the physics community, since physicists tended to rally behind regional proposals. Since funding prospects could be compromised if the entire physics community did not support the expensive project, AEC Commissioners could not ignore pressures inside the physics community any more than they could ignore external pressures. Faced with the combined pressure for a Midwestern accelerator from politicians and physicists and the push for outside users rights, the Commissioners chose the Illinois Weston site on December 7, 1966. The commissioners remember that they were concerned about the civil-rights problem, but ultimately decided that Weston's advantages, including easy accessibility, more than offset this disadvantage.³⁴

In the midst of vigorous complaint from the NAACP and politicians who had championed losing sites, planning proceeded for the new laboratory. In late December 1966 the URA received a temporary contract from the AEC and offered the job of design director of the accelerator to the logical candidate – Edward Lofgren, head

of the LRL design team. However in January Lofgren declined the post, objecting to his job description, which seemed to preclude directing the completed laboratory, and expressing doubt that he could assemble the necessary staff in Illinois “and develop an organization having the enthusiasm and spirit needed to make the project a distinguished success.”³⁵

Now the URA was in a tight spot. In just a month, the Joint Committee on Atomic Energy (JCAE) would hear testimony at the annual AEC budget hearing to decide whether to authorize the first allocation of construction funding. The federal budget was more stringent than ever and criticism was mounting over the site decision. At this sensitive moment the project could ill-afford to have another candidate refuse the directorship.

At this juncture, Wilson was unexpectedly available because he finished a 10 GeV electron synchrotron at Cornell ahead of schedule. Wilson, who had a well established reputation as an accelerator builder, had constructed the Cornell project on budget and was willing to take on the 200 GeV project given budgetary constraints. After some deliberation, the URA approached Wilson about the position of Laboratory Director, making it clear this time that the position included overall responsibility for the laboratory.³⁶

At first the AEC Commissioners were unenthusiastic about the choice of Wilson, remembering with irritation his 1965 criticism of the Berkeley design. His risky cost-cutting approach to accelerator building was diametrically opposed to the more

conservative and careful LRL style of the 1950s, which had suited an era of financial ease. The commissioners questioned whether the physics community, and LRL in particular, would accept Wilson's appointment. According to Ramsey, then URA president, although most of the LRL team had been angry at Wilson since 1965 and refused to work with him, most physicists outside the Berkeley circle favored the appointment.³⁷

Seaborg next organized a series of meetings. At a decisive conference on February 14, Lofgren estimated that the accelerator would cost \$53 million more to build at Weston than at the California site. An AEC representative countered that the Commission expected only a \$5 million cost difference. Ramsey interceded to stress that cooperation was necessary to win the machine. It was finally agreed that the project's first task would be to produce both an updated design and thorough cost estimates. Terms were negotiated for a transition from California to Illinois that allowed continued, though limited, LRL participation. Later that day, Seaborg, Gerald Tape, and Ramsey took Wilson to meet California Representatives Chester Holifield and Craig Hosmer, and their fellow JCAE member Melvin Price, an Illinois Congressman. Tape felt that by the end of the meeting Wilson had "made a hit with them." This was the beginning of a convivial relationship between the JCAE and Wilson, the laboratory director committed to cutting costs.³⁸

In between these negotiations, the AEC faced the JCAE in authorization hearings which were to shape the future Congressional expectations of the new laboratory. To

blunt accusations from Mitchell of the NAACP, who charged that the Commission had been insensitive to civil rights violations, and from New York and California Congressmen who insisted that the decision was unfair, the AEC made a point-by-point analysis of the site selection process, emphasizing that the decision had been made on the basis of established criteria. The AEC's case would have been seriously weakened if LRL leaders had released their estimate of the cost difference for building at Weston. However, when Lofgren and McMillan appeared before the subcommittee, they limited negative comments to vague warnings about the necessity of proceeding with caution. Thanks to behind-the-scenes meetings between the LRL leaders and the AEC, the physics community publicly displayed unified support for the project at a time when divisiveness could have crippled funding prospects.³⁹

The AEC was also able to turn the discussion on reduced scope to the advantage of the project. Although John Pastore, recently appointed JCAE chairman and Senator from Rhode Island, and Hosmer sharply criticized the plan, JCAE member William Bates of Massachusetts pointed to CERN plans for the 300 GeV machine and asked why the U.S. was "settling on a second-rate" accelerator. Bates' question gave the AEC the perfect opportunity to bridge the gap between JCAE expectations and BOB restrictions by touting the idea of a machine that could later be expanded to reach higher energies. With an expandable machine, the JCAE could feel they were getting the accelerator they originally promoted and the AEC could still keep within the \$250 million budget set by BOB. Although LRL 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.⁴⁰

The tireless efforts of the AEC and URA paid off. Although Pastore suggested that the project be deferred because of the lack of open housing in Illinois, the JCAE recommended in June that \$7 million of the \$10 million requested be authorized 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 made a commitment to build the new laboratory. In return, Congress expected an expandable machine for \$250 million. For the first time since World War II frugality would shape the technical decision making of a major national laboratory.⁴¹

3 CONSTRUCTION OF THE MAIN RING

3.1 Adventures at Oak Brook: The Initial Wilson Design, summer 1967-early 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 LRL designers, as well as other experts, were pessimistic about Wilson's ability to succeed at his difficult task because of 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 accomplished the latter task using an approach previously employed only for smaller machines. This style was particularly evident in building the main ring, our focus in the following discussion. The main ring is the largest, most expensive component, and it marks the success of the machine, since protons are accelerated there last.

Wilson, a sculptor as well as a physicist, began designing an accelerator by sketching and then holding in his mind an image of the whole machine. As he recently explained: "Most professionals," for example the expert builders at Berkeley and Brookhaven in the 1960s, "divide the work into parts and then sit down and do a professional job on each part." This tactic made as little sense to Wilson as creating the sculpture of a human head by parsing 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. Wilson also tried to keep his design fluid, so that innovative ideas could be incorporated.⁴²

Whereas the elite at LRL and BNL saw accelerator building as a profession best practiced by experts, Wilson thought accelerators should be built by research physicists. In his opinion, 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. Wilson criticized the orderly, highly specialized LRL design effort, which in his opinion had been overtaken by “engineering types.” CERN provided an even sharper contrast than LRL or BNL to Wilson’s ideal accelerator building environment. As the CERN historians explain, since engineers at CERN at this time had the only available 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.”⁴³

Beginning his accelerator design work in the same spirit as a painter preparing a large canvas, he 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.”⁴⁴ However, Wilson did not relinquish the ideal of frugality, so prominent in his 1965 design. Whereas the LRL 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.⁴⁵ 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 expandability scheme forced designers to conceive of innovative ways to achieve maximum capability at minimal cost, a demand that drove the entire

construction effort.⁴⁶

To encourage creativity, Wilson went out of his way to choose attractive quarters for the summer 1967 design effort, 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 open-ended quality of the design discussions, for the quarters were not yet finished and were totally 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. Accelerator designers came to the workshop from all over the country, including Berkeley and Brookhaven. The length of visits varied, with about 25 participants present at a time. To encourage a research-oriented atmosphere, Wilson convinced Robert Serber, a prominent theoretician, to give lectures on relevant theoretical topics, a service he 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).⁴⁷

The designing proceeded with a speed reminiscent of that at the Radiation Laboratory in the 1930s and at wartime Los Alamos. For Wilson, speed encouraged creativity and also saved personnel costs. 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 1968.⁴⁸

Brookhaven physicist Gordon Danby brought to Oak Brook a “separated function” design for the main ring magnets, a design that used different – cheaper -- magnets for bending than for focusing the beam.⁴⁹ Although the LRL designers had earlier rejected Danby’s suggestion, the decision to use a separated function design for the main ring was made at Oak Brook “almost without discussion,” since the designers realized that for a large synchrotron this approach would allow achieving higher magnet fields.⁵⁰

Casting for ways to achieve the highest field while reducing 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 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 possible radiation damage, Wilson insisted that the magnets be assembled with as little epoxy as possible, since epoxy is particularly sensitive to such damage.⁵¹

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 the main ring agreed that the radius would need to be about 1000 meters, but expected that they would have

some leeway in developing the design. Instead Wilson announced that the radius would be exactly 1000 meters, explaining that a round number would be easier for everyone to remember. All discussion on the issue was then suspended. As Wilson expressed publicly 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.”⁵²

Wilson’s emphasis on action was consistent with the pioneering ethic he encountered 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 recently explained: “I learned from Lawrence to define what you want and then, damn, make it come out that 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. By evoking it at NAL, he was able to tap into a powerful American idiom to motivate his staff as they struggled to meet his demand of achieving maximum energy at minimal cost.⁵³

Other aspects of Wilson’s accelerator building style, including his celebration of the role of the research physicist in accelerator building, his focus on research priorities during the design process, his dominating leadership style, and his rapid pace, all harkened back to his graduate student years with Lawrence. As Wilson recently

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 pre-professional era of accelerator building, machines were built at break-neck speed by the enthusiastic cadre of experimentalists led by Lawrence, the undisputed driving force at the Berkeley laboratory.⁵⁴

However 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 the LBL historians explain, the rectangular magnet design for Lawrence’s 60-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.⁵⁵

Wilson continued to press for higher energy. When Alper Garren and Lee Teng designed the lattice, the arrangement of components in the main ring, they calculated that a 1000 meter ring filled with the new bending magnets run at 18 kilogauss, interspersed with a few quadrupole magnets, would allow 400 GeV acceleration.⁵⁶ This happy news whet Wilson’s appetite for even higher energies. In a characteristic move, he pushed the goal higher: if they could stretch performance of the bending magnets to 22.5 kilogauss, the machine could accelerate beam to 500 GeV.⁵⁷

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 design in the LRL 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, inserted hot (90 degrees) and held in place in the cooled magnet due to the difference in shrinkage properties of copper and steel. Fabrication was also simplified; drawing on his Cornell experience, Wilson planned to fabricate both types of bending magnets (B1 and B2) and quadrupole magnets using die-stamped laminations that were aligned and mounted in self-supporting girders.⁵⁸

The use of pared-down magnets was inherently risky, since larger magnets produce 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 LRL design, the LRL magnets required almost 10 tons more steel per magnet and cost a total of almost \$6 million more.⁵⁹

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 tunnel with no

cranes or air conditioning. In addition, although previous accelerator magnets were secured with caissons, Collins had designed the magnets to sit on glacial till. Wilson planned to align magnets using a controversial system of stretched wires. While the LRL group had allowed 7 years for construction, Wilson planned to cut personnel costs by completing the accelerator by June 1972, in just four and a half years. As Ramsey later explained, Wilson took risks on about 20 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, insisting that Wilson had taken unacceptable risks, considering the cost and importance of the project.⁶⁰

The streamlined NAL design was tailored to the funding environment. In early 1968, physics funding, which had been rising steadily since the end of World War II, had reached a plateau; high energy physics funding was 40% below that projected by the AEC in 1965. Although the AEC Commissioners, who had just finished their annual budget request, realized that the funding environment would not improve in the next year, Congress still expected an expandable accelerator. Both the Commissioners and JCAE were inclined to support Wilson, since both groups had sponsored initial funding and therefore had a political stake in the project. Wilson strengthened the support of both group by presenting a design for a \$248 million 200 GeV accelerator expandable to as much as 500 GeV.⁶¹

Despite criticism of his design, Wilson also found acceptance within the physics

community by early 1968. By this time 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 (CEA) and the Princeton-Pennsylvania Accelerator (PPA). 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.⁶²

3.2 Modeling the Main Ring: An Academic Start, early 1968-mid-1969

Building an accelerator typically follows the standard stages of a large construction project. After preliminary designing, the primary focus is model building, in which aspects of the design are built (often in miniature) to test basic characteristics.⁶³ After refinements in design, prototypes are constructed to test full-size components. Then, following further development, construction specifications are frozen and building begins.⁶⁴

In the main ring group model building dominated attention until mid-1969. One distinctive feature was the early building of full-size models (either prototypes or structural models) out of easily constructed materials to check overall dimensions. The practice encouraged the holistic approach that Wilson favored, because full-sized

models helped staff members envision how parts fit together and anticipate fabrication problems.⁶⁵

Although Wilson influenced main ring decision making during this period, the pace and tone was set by Frank Shoemaker, a veteran accelerator builder on leave from Princeton, known for his careful, deliberate style. The accelerator theory group, led by Teng throughout the construction years, also aided in the conceptual design of the main ring, computer modeling of the performance of accelerator components, and interpretation of measurements of accelerator properties. A technical services group, under Henry Hinterberger, provided engineering support as well as machine shop and drafting services.⁶⁶

Shoemaker, like the other group leaders, was faced with a lack of facilities. Since the Oak Brook offices could not accommodate either model-building or computing and the space was not available on site, the new laboratory had to rely on other institutions. The first models of a vacuum chamber and the two-foot H-magnet were constructed at the Physical Sciences Laboratory in the former MURA headquarters at the University of Wisconsin. Most of the early magnet models were built and tested at Argonne National Laboratory (ANL). Computer modeling for beam dynamics was done using a data telephone link to New York University (NYU) or at ANL.⁶⁷ From mid-1968 to mid-1969 the main ring group built and tested both prototypes and structural magnet models. In February 1969, a model of a section of the main ring tunnel was installed in the recently completed shop, run by professional model-maker

Jose Poces; the section was complete with full-scale mockups of magnets, vacuum system, and water and power connections.⁶⁸

Since land and construction funding took longer to acquire than anticipated, Shoemaker still faced the problem of finding 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 frontier-like atmosphere 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. Then, 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 instead of 50 degrees, as 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, the group used the air building until early 1970 due to the continued shortage of funding and space. By this time the group, which was gearing up for full-scale magnet production, had spread to several buildings on site.⁶⁹

The design of the vacuum system also advanced in this period. After the model built at the University of Wisconsin was reassembled in the air building, work began in

summer 1968 on the development of a fixture for welding together flanges of adjacent magnets. Since it was assumed that occasionally magnets would need replacement, Walter Pelczarski developed a clever, efficient means for cutting apart these flanges, an innovation that would later prove to be much more useful than anyone imagined at the time. By September the 50-foot vacuum model had been equipped with small ion pumps and the hardware necessary to isolate the straight sections and tests had begun of various roughing systems. In March 1969, a month after the mockup vacuum system was set up in the model tunnel, the vacuum chamber model performed as expected during pulsed-field magnetic measurements using digital recording and analyzing equipment.⁷⁰

Meanwhile, arrangements were being made for an innovative magnet power supply system designed by engineer Richard Cassel. Previous large proton synchrotrons had used motor-flywheel generator sets to provide the necessary slowly pulsed current from ordinary 60-Hertz alternating current power. 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 that determined the shape of the power cycle delivered to the magnets. 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, pumping electrical energy from the grid during the rise of the

guide field and pumping it back when the field fell.⁷¹

By spring 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. The design for long straight sections was modified to standardize the quadrupoles and the design of the medium straight sections was simplified to reduce cost. The group was ready for the prototype stage.⁷²

Despite the group’s accomplishments, some felt the main ring effort had been too relaxed in the 1968 to mid-1969 period. Ernest Malamud and Ryuji Yamada remember that the main ring effort had an “academic” atmosphere, which seemed at odds with Wilson’s stress on speed and economy. Malamud notes: “We spent endless hours philosophizing.” Visible progress in other groups added pressure; in April the linac group accelerated the laboratory’s first beam with the prototype preaccelerator and construction was underway on the linac building and the booster enclosure.⁷³

Just as Wilson was beginning to worry that the main ring effort would lag the rest of the project, Shoemaker announced 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 Malamud, Cassel, and Hinterberger.⁷⁴

3.3 The First Flush of Success: Mass Production, Prototypes, and Filling the First Superperiod, mid-1969-mid-1971

Although the standard procedure called for freezing the design after prototyping began, Wilson injected fresh design ideas as part of a laboratory-wide campaign against “heavy-footed over-design.” This campaign lasted until the first production dies were made and stamping had begun. A July 1969 memo boldly celebrated taking risks: 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 over-designed.”⁷⁵

Thus inspired, the main ring group made numerous design changes from May to December 1969 to simplify or lower the cost of the main ring magnets. For example, a more compact design of both quadrupole and bending magnets was devised, the magnet water and power manifolding was streamlined, and the group developed a new method of bending magnet coil fabrication in which inner coils and outer coils were constructed separately. In this way, outside manufacturers could continue fabricating outer coils, while the more exacting inner coil window frame fabrication could proceed under the supervision of NAL staff.⁷⁶ Other aspects of main ring design were also made more economical: the water and power distribution system inside the tunnel and the welded joints in the vacuum system were simplified and, to replace the expensive and

awkward stretched wire system used to help align the quadrupoles, a survey system was developed that used a laser beam.⁷⁷

Wilson's pressure for a spare, clean design is reflected in magnet modifications made in May 1969 by Hinterberger, who, as Wilson recently noted, had a sense of designing that was "both elegant and economical." In the 1968 design, the steel yokes, which were made of thousands of thin laminations, were secured with a steel I-beam. When the 20-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.⁷⁸

Engineers, as well as physicists, could live up to Wilson's ideal of an accelerator builder. Two engineers in the troika, Cassel and Hinterberger, played key roles in the main ring design. William Hanson, who headed the magnet factory, and Anthony Glowacki, who designed the magnet water cooling system and made other key contributions to the construction of the main ring, also were engineers. However, these engineers were unlike LRL's specialists or CERN's semi-autonomous technological perfectionists, for Wilson's nonspecialized building style required workers who could blend skills from engineering, research physics, and accelerator science.

The possibilities of Wilson's nonspecialized approach to accelerator building are illustrated by Yamada's successful effort to boost the capability of the accelerator from 400 to 500 GeV. Although in late 1969, the accelerator theory group considered the prevailing bending magnet design, which yielded a 18 kilogauss field, to be an optimal design, Yamada, then a young experimentalist, reassessed the calculations to see if a higher field could be achieved. Assuming the 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.⁷⁹

Yamada's approach proved remarkably effective. In a December 3 staff meeting Wilson noted that the resulting "pole shape was very similar to the ideal pole shape worked out many years ago by H. A. Bethe." Wilson later concluded that Yamada's improvement, which depended on the earlier decision to place the coils inside the gap, was "one of the biggest innovations" of the entire accelerator. 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.⁸⁰

Wilson's focus on economy continued to suit the times. As historian Spencer Weart explains, 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." In this funding environment, the laboratory consistently received smaller allocations than requested. In 1968, the laboratory requested \$75 million, battled against a \$7.1 million allocation, and received \$14.7 million.⁸¹ In 1969, the laboratory requested \$102 million and received \$70 million. Although formal approval for the entire budget was obtained in July 1969, in early 1971 NAL had still had not received \$93 million of its construction budget.⁸² Although there was little threat at this point that funding would be terminated, limited allocations forced Wilson to economize in order to keep the entire project under construction. In addition, cost overruns would have made the project unpopular in Washington and likely have jeopardized future funding.

Wilson used the budgetary limitations to help justify tactics aimed at obtaining optimum efficiency 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 1 January 1972 to 1 July 1971. The stress from ambitious schedules was augmented by the constant risk taking, primitive working conditions, and the pressure to find further economies. Accelerator theorist James Maclachlan

remembers that Wilson explicitly reinforced the mythic image of the effort by using terms such as “heroic” and “adventure” to describe their task. “Sometimes it felt like a search for the holy grail.”⁸³

Observations by the CERN historians place Wilson’s style in an international perspective. When analyzing why CERN in the 1950s and 1960s lagged behind American high energy physics laboratories, they note that a great gap separated the CERN engineering staff, with its focus on technological perfection, and staff 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 U.S. 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, and engineer, i.e., knowledgeable and innovative in the most advanced techniques... and 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.”⁸⁴

Rapid progress in the next year and a half displayed the power of Wilson’s strategies. In October 1969, ground was broken for the 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 20 March 1970, the group forged ahead with the prototype program. They had to implement efficient factory and procurement systems and work frantically to make the other preparations for magnet production.⁸⁵

Large-scale industrial mass-production of magnets was both necessary for the main ring, which contains more than 1000 magnets, and appealing to Wilson because he wanted to keep the permanent NAL staff as small as possible, both as an economy measure and because he felt people worked more efficiently in smaller groups. However his tendency to leave the design flexible as long as possible and his insistence on economy did not fit standard full-scale mass production, which requires exact specifications that are costly to modify. This difficulty was in part overcome after the modification of the coil design allowed a partial mass production scheme to emerge by December 1969.⁸⁶

Economically and rapidly procuring the services and material to produce main ring components was no trivial task. Malamud remembers that he, Hinterberger, and Robert Sheldon traveled all over the world to award coil contracts, saturating the international market for coil manufacture. In the never-ending quest for efficiency and speed, Wilson devised a clever administrative gambit. As he explained to his staff in 1970, they would “give 1/3 to one producer; 1/3 to another; whoever gets through first gets the remaining 1/3.”⁸⁷

By January 1970, arrangements had been made both for factory production and materials procurement and 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 could reach energies close to 500 GeV "at reduced intensity not long after the synchrotron is brought into operation." Just as magnet assembly had begun on site in May, Wilson announced a new milestone: by October 1, the group was to have the first superperiod, 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.⁸⁸

At this point, the booster was partially completed and the linac could accelerate protons to 139 MeV. Wilson faced a problem, however. He had until 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 September 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."⁸⁹ Some were annoyed because the scheme caused artificial technical problems, for the components

were not designed for that low an energy. In any event, Oktoberfest was not a complete technical success. Although the 139 MeV beam reached the main ring on 9 October, proton intensity was too low to perform beam tests, as expected. While some judged the Oktoberfest as little more than an annoying stunt, others insisted 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.⁹⁰

The main ring group spent the next five months working feverishly to complete installation. By the following March 800 magnets had been installed, the water and power bus systems were complete at the tunnel level, and about half of the power-supply modules had been tested. In April, the last magnet was installed.⁹¹ 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.⁹²

The group suffered various trials during this period that ranged from merely inconvenient incidents to short-lived crises. For example, meeting notes from the installation of the protomain reported “one black eye and several sore heads” resulting from the repair of water leaks that unexpectedly erupted from the power/water manifold system after the magnets were in place. More distressing was wading through

water and mud in the main ring tunnel to install magnets in the fall of 1970. However installation continued, aided by portable dehumidifiers, so that no time was lost.⁹³

Another crisis born of haste came in late December when Malamud noticed a problem with magnet construction. 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. The solution, from engineer Hanson, provides another example of the importance of engineering skill to the main ring effort. Hanson suggested that the void be filled with epoxy by vacuum impregnation, a technique that had been used successfully on booster 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 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 20-foot, 15-ton magnet. In the end, Wilson was convinced, and magnet construction was altered to include vacuum impregnation.⁹⁴

Despite such minor crises, Wilson reported that he was quite "confident" about the laboratory's prospects as the mid-1971 deadline approached. But the laboratory would soon encounter technical disasters that would cast his building style into doubt and destroy the dream of building the world's most powerful accelerator one entire

year ahead of schedule.⁹⁵

3.4 Main Ring Problems, Spring 1971-December 1971

Since so many risks had been taken, NAL and URA leaders expected aspects of the accelerator to fail. But they were not prepared for the traumatic summer and fall of 1971. Due to the tight schedule, the magnets had been installed in the midst of severe winter weather, which made them cold. In spring 1971, the ventilating system brought in humid, warm air, causing water to condense on the magnets. As a result, as a progress report noted, “as much as a quart of water” could be “removed from a wet magnet.” When the NAL staff tried in May to bring the more than 1,000 main ring magnets into operation under these conditions, they found, to their horror, that a high percentage shorted. By summer 1971, magnets were failing at an alarming rate. Unless the magnet problems were solved, the entire project would fail.⁹⁶

NAL staff were not sure what aspect of the design was causing trouble. Using the trial and error empiricism characteristic of the time-pressed wartime atomic bomb effort, they tried empirically, in the absence of theoretical guidance, a succession of improvements.⁹⁷

In the meantime, other difficulties surfaced, the product of haste and untried technology. For example, Malamud identified 11 electrical and mechanical problems that likely interfered with main ring performance in July, including misaligned magnets, malfunctioning ion pumps, a piece of copper in the beam pipe, and a plastic cap found

in a quadrupole. Also, as the August progress report admitted, the linac and booster were running “with an efficiency of about 50%.” Although this figure was considered “reasonable for such new accelerators,” linac and booster problems complicated main ring problem solving since beam studies could only be performed when both accelerators were operating well. The staff also experienced problems with power supplies, and with obstacles in the main-ring vacuum tube. 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.⁹⁸

Many disadvantages of Wilson’s style were becoming apparent. The main ring crisis caused considerable trouble for outside users, who had arranged sabbatical leaves to allow them to prepare experimental equipment based on Wilson’s optimistic projections. Also, the emphasis on action often led to nonproductive chaos. CERN, which was bound in “a kind of conservatism,” in the words of the CERN historians, was able to employ a more productive approach when crisis erupted. As Drasko Jovanovic remembers, the calm atmosphere at CERN allowed physicists to keep “cooler heads”; they shut down the accelerator and formed a coordinated effort to study the root of the problem. This approach facilitated a methodical assessment better suited for solving complex technical problems. CERN physicists were also quick to conclude that NAL magnet problems revealed the superiority of their traditional emphasis on technological perfection; as Lederman remembers, CERN officials openly gloated over

Wilson's misfortunes. NAL's problems also seemed to give credence to LRL and BNL criticism of Wilson's design style. Wilson and Goldwasser remember hearing rumors that a campaign was being mounted to convince the URA to remove Wilson from the directorship.⁹⁹

In an attempt to rally the laboratory staff and solve the main ring problems, Wilson devised a new management plan, which in its flexibility and de-emphasis on hierarchy resembled tactics used at Los Alamos during World War II.¹⁰⁰ Wilson first formed an Accelerator Section, headed by himself, merging the groups responsible for accelerator theory, operations, radiofrequency, and beam transfer. 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." The Commissioners, as Wilson explained at the time, were "expected to identify work problems and to come to one of the Managers to get the work force ... to do the actual work" As Orr recently noted, the resulting work assignments focused the efforts of technical experts on technical problem solving, leaving organizational decisions to those with managerial skills. Wilson met daily with the three managers and other key leaders so that the effort was tightly coordinated. To further expedite problem solving, he pulled workers from other groups, such as those working on the experimental areas, to increase the pool of those working on main ring problems.¹⁰¹

After 15 years, experts do not agree about the cause of the various main ring problems or even on their relative severity. The standard explanation for the mag-

net 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 which allowed water to be absorbed and short the magnet. However veteran accelerator builder Collins blames the decision to glue in the coils. He concludes that the considerable thermal and mechanical stress that resulted from temperature cycling cracked the epoxy in the glued-in coils, allowing water to get in the cracks.¹⁰² Collins feels that the most central problem was that the B1 magnets were too small, which caused “field quality and systematic errors.” Jovanovic suspects 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 1971.¹⁰³

Whatever their cause, the problems began to abate once the three managers focused the entire laboratory on problem solving. In time much of the main ring was filled with reconditioned or newly built magnets that displayed a lower failure rate. Although some magnets 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 6 months to be lost, and about 10% (approximately \$2 million) of the original cost of the magnets was spent to overcome the difficulties.¹⁰⁴

3.5 The Triumph of Frugality: Setting the Indoor Proton Speed Record, Jan-March 1972

Throughout the rest of the winter, the NAL staff made steady progress and on January 22, 1972 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 held by the USSR. 200 GeV was achieved on the afternoon of March 1, 1972. Champagne was uncorked. 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.¹⁰⁵

Wilson's announcement was well received in Washington for times were hard for large, federally funded projects. That year planners of the Large 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: stimulating competition between contractors to lower bids, skipping the prototype phase, designing cost-effective components, and transferring costs from the design stage to the operation stage. Nonetheless, in June 1974 Congress denied funding for the telescope, throwing the project into jeopardy.¹⁰⁶

NAL was more fortunate. Not only was the project alive and well in the early 1970s, but the laboratory and its director had become the toast of Washington. In 1971, JCAE members joked about considering Wilson for sainthood. In 1972 JCAE

member Hosmer reassured Wilson that magnet troubles and delays didn't tarnish "a bit the brilliance with which this whole effort was conceived and constructed." All four experimental areas were in operation by 1974, when the laboratory was renamed Fermi National Accelerator Laboratory, in line with a 1969 AEC decision to honor Enrico Fermi. A full-scale research program was underway by 1975, with routine operation at 400 GeV, a beam intensity of 1.84×10^{13} , and only 28% unscheduled downtime. In May 1976, the first 500 GeV beam was accelerated.¹⁰⁷

Orr remembers someone sarcastically commenting that Wilson's efforts in early 1972 were merely aimed at "trying to set the indoor proton speed record." The laboratory's next challenge would be to prove that a successful experimental program could be established with the resources born of frugality.¹⁰⁸

4 Conclusion

Ironies proliferate in the Fermilab story. The big scientists who built accelerators, like those who built telescopes at NASA, were forced to think small in the 1960s as large federally funded projects grew much larger. Although LRL lost the 200 GeV machine, NAL was built in a Lawrencian style. And the new value of the egalitarian TNL took its research model from wartime Los Alamos, a military laboratory which influenced Fermilab by its flexible institutional structure.

Every aspect of planning and building the 200-500 GeV-range accelerator responded to funding considerations. The machine's unprecedented expense in the

worsening funding environment meant that only one large accelerator could be built. Consequently, unified support from the physics community was crucial in obtaining a federal commitment to fund the project. But obtaining such support required addressing the demand for fair access to the accelerator by the nation-wide community of experimentalists, as well as the complaints of Midwestern physicists, who felt that their region had been unfairly deprived of a first-class accelerator. The accelerator's price tag also attracted the attention of the public and politicians. In the tense environment in 1965, the URA, a unique, nationally-based management organization, was established to ensure equitable access to outside users. A nation-wide site contest doomed the hopes of LRL physicists to continue its glorious accelerator-building tradition into the next decade.

The new management, the novel siting arrangement and, above all, continued budgetary pressure, provided a climate receptive to an accelerator-building style previously used only for smaller machines. After Lofgren declined to lead the project, URA members offered the job to Wilson, a leader not only willing to accept the site and budgetary constraints, but with a reputation for building accelerators ahead of schedule and under budget. Faced with a continuing tight budget during 1967-1972, Wilson introduced the value of frugality into accelerator building; transforming it into a virtue, he motivated and brought cohesion to his staff. Tight deadlines, primitive working conditions and goals set just beyond reach, turned the job of building the accelerator into an adventure. Taking risks to save money became a celebrated activ-

ity. Wilson's building style proved flexible enough to accommodate mass production and institute necessary organizational changes. This style allowed meeting the goal of producing maximum capability at minimal cost while maintaining the aesthetic preference for a spare design.

The main ring magnets provide a clear illustration. Using a separated function lattice, modified window frame coil construction, H-shaped bending magnets, and tapered pole tips, the NAL staff achieved high fields with impressively compact, and therefore inexpensive magnets. Simplified fabrication and coil installation procedures gained further savings as did coils having fewer turns. The design, with its box-girder laminations and compact H-shape, had clean, graceful lines. However the risk of the pared-down magnets was augmented by other risks, which in some cases, compounded each other. For example, restriction on the use of epoxy, made necessary by the risk of placing the coils close to the beam, led to thin, easily cracked coil insulation, the likely cause of widespread magnet failures that brought on a year-long crisis, threatening the entire project.

Few would characterize Fermilab's main ring as an elegant, precision instrument, for its minimal magnets have relatively poor field quality and need to be replaced periodically. Interruptions in the operation of the machine introduce uncertainty in scheduling experiments. Also, the accelerator costs more to run than it would have if the initial investment in copper had been greater. Due to escalating electricity costs since the mid-1970s, this disadvantage has been greater than anticipated. However,

despite these drawbacks, the main ring, as well as the booster and linac, did function as a world class accelerator at almost twice the expected energy.

Wilson met the challenge of building a workable \$250 million 400 GeV accelerator by drawing on a nexus of traditions – trial and error empiricism, the pioneer of the American West; the ebullient, home-grown, combined engineer-scientist-industrialist; and flexible family-style organization. As a graduate student under Lawrence at Berkeley in the 1930s, as a group and division leader under Oppenheimer at Los Alamos in the 1940s, and as director of NAL in the 1960s and 1970s, Wilson experienced how each of these laboratories drew on this American research tradition. All three were new institutions formed in response to conditions in their times. All three arose from the ambitions, dreams and sense of urgency of their strong charismatic leaders who were also their founders. All pressed for speedy progress.¹⁰⁹ The organizations, not yet rigidly defined, were able to respond flexibly and quickly to crisis and change. All three operated in the shadow of external crisis -- too little money in the case of the Radiation Laboratory and NAL, and a war in the case of Los Alamos – which set constraints that helped to define, consolidate, and motivate the efforts. All shared a sense of adventure and optimism that the impossible could be conquered and that the search would be pleasurable, feelings that resonated with the American pioneering ethic. The workers were, on the whole, young, relatively inexperienced and multitalented scientists, who had not yet made a commitment to any particular approach or scheme of organization.

The distinctly different environment at Lawrence Radiation Laboratory in the early 1960s shows the range of the American accelerator building repertoire. LRL was an established laboratory with a set institutional structure, a laboratory led by a second director struggling to follow in the footsteps of Lawrence. The workers were well-seasoned experts with defined specialties and a sense of commitment to shared values and techniques of the profession. In the 1950s, such a laboratory had major advantages; the institutional structure, the collection of experts, and the availability of money helped when pathbreaking technical developments were needed. Also, its tradition of excellence and reliability was valuable in a time when basic research money went to those with high reputation, especially those who, like LRL researchers in the 1950s, made contributions to national defense. Many of these factors turned to disadvantage in the 1960s when the established laboratory faced the problem of its own inertia in adapting to a changed external context. The lack of money, the TNL philosophy, and the geographical distribution of research funding all flew in the face of the established LRL tradition. By this time, LRL was no longer being supported for defense reasons. Maintaining a reputation and a grand tradition had become too heavy a burden to bear in those turbulent times; the 1950s LRL approach to accelerator building was eclipsed.

A final irony would become evident in the latter part of the 1970s, when Wilson's legacy of frugality threatened to undermine the very program that Fermilab was built to conduct. The ideal that small and less are beautiful did not in the 1970s and 1980s

fit the needs of either the physics or the technology programs (like developing superconducting magnet accelerators). Celebration of the small quickly done clever effort by a nonspecialist did not prepare researchers for the expensive, time-consuming, meticulous, larger-scale experiments that would characterize particle physics research in this later period. CERN, which had suffered in the 1960s from the disadvantages of a tradition favoring slow, careful, methodical administrative and technical decision making, had the advantage in the 1970s and 1980s. Fermilab's outside-user focus, necessary in the earlier part of laboratories history, also became a disadvantage in the mid-1970s. The more complex experiments of this era would have been easier to implement had the laboratory been able to offer the support of stronger inside groups like those at SLAC, which was organized before pressure grew for a TNL. By the 1980s, Fermilab's leaders were again examining their repertoire for alternate ways of building and using accelerators.

Footnotes

1. This material is based upon work supported by the National Science Foundation under Grant No. Dir-90 15473. The Government has certain rights in this material. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation.

All interviews and documents are located in 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); LBJ Library (Lyndon Baines Johnson Library, Austin, Texas); LBL, (Lawrence Berkeley Laboratory, Berkeley, California), LBL Archives, (Lawrence Berkeley Laboratory Archives, LBL); Lofgren Papers, (files of Edward J. Lofgren, LBL); Malamud Papers (files of Ernest Malamud, Fermi National Accelerator Laboratory, Batavia, Illinois); McMillan Papers, (files of Edwin McMillan, Lawrence Berkeley Laboratory Archives); Mills Papers, (files of Frederick Mills, Fermilab, Batavia, Illinois); Salsig Papers, (files of William Salsig, LBL Archives); Seaborg Papers, (files of Glenn T. Seaborg, LBL).

2. 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., 1971). A comparison of the history of Fermilab and the Japanese

accelerator laboratory KEK can be found in Hoddeson, "Establishing KEK in Japan and Fermilab in the US: Internationalism, Nationalism and High Energy Accelerator Physics During the 1960s," *Social Studies of Science*, 13 (1983), pp. 1-48. Participant accounts include M. S. Livingston, *Early History of the 200-GeV Accelerator* (Batavia, Ill., 1968) and N. 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).

3. For an overview of particle physics in the 1950s, see L. Brown, M. Dresden and L. Hoddeson, *Pions to Quarks* (Cambridge, Mass., 1989). Also: Robert R. Wilson, "Ultrahigh Energy Accelerators: Summary of a Discussion Held at Rochester, N. Y., August 28, 1960."

4. MURA was an organization of physicists from Midwestern universities, which in the 1950s and 1960s developed innovative accelerator systems and pressed for a large colliding beams accelerator in the Midwest.

5. In a fixed-target machine, a single accelerated beam is shot against a "target" material. In a colliding beams machine, two accelerated beams collide against each other compounding their energy.

6. For an account of the discovery of strong focusing, see E. Courant, "Early History of the Cosmotron and AGS at Brookhaven," in Brown, et al., (n. 3 above), pp. 180-184.

7. Sands gives credit to Marcus Oliphant and T. A. Welton for conceiving cascade

schemes. Hoddeson, (n. 2 above), p. 14, points out that F. Heyn and Lee Teng had also made similar suggestions.

8. Quote from Matthew Sands, "Ultra High Energy Synchrotrons," in Midwestern Universities Research Association, "1959 MURA Summer Study," MURA Report 465, p. 1, LBL Library.

9. Matthew Sands, "A Proton Synchrotron for 300 GeV," (Pasadena, 1960), CTSL-10; form letter from Robert Bacher and Matthew Sands, January 12, 1961; Sands to Robert Bacher, January 30, 1961; Westfall interview with Alvin Tollestrup and Robert Walker, May 4, 1985; "A Proposal to the Atomic Energy Commission for the Support of the Accelerator Design-Study Program of the Western Accelerator Group," April 1961.

10. For early planning on accelerators in the 25 to 200 GeV range at Berkeley see David L. Judd, "The Development from 1952 to 1960 of Accelerator," September 29, 1960. 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).

11. See J. L. Heilbron and R. 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*, 13:2, (1983): 376-392 and Allan A. Needell, “Nuclear Reactors and the Founding of Brookhaven National Laboratory,” *Historical Studies in the Physical Sciences*, 14:1, (1983): 95-100.

12. Westfall interview with Alvin Tollestrup and Robert Walker, May 4, 1985.

13. Edward J. Lofgren to Edwin McMillan, April 6, 1961, Lofgren Papers; Westfall interview with McMillan, May 16, 1984.

14. 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.

15. The NSF convened panels in 1954, 1956, and 1958, the AEC convened panels in 1958 and 1960. 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, 106. Also: appendices 2-6.

16. At this time 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. High energy machines are those which accelerate particles to particularly high energies; high intensity machines are those which accelerate a large number of particles per second.

17. *Ibid.*, pp. 103-104.

18. Hoddeson interview with Norman Ramsey, 22 January 1980; Westfall interview with Edwin Goldwasser, 10 July 1985; Paul McDaniel to Maurice Goldhaber, 2 April 1963, Green Papers; Glenn Seaborg record of conversation 17 July 1963; Lyndon Baines Johnson to Hubert Humphrey, 16 January 1964, Secretariat, DOE Archives, Box 1424.

19. Quotes, respectively, from Dominique Pestre, “Monsters’ and Colliders in 1961: The First Debate at CERN on Future Accelerators,” in Frank A.J.L. James ed. *The Development of the Laboratory* (London, 1989), p. 238 and Pestre “The Second Generation of Accelerators for CERN, 1956-1965” (Amsterdam, 1990), in Armin Hermann, John Krige, Ulrike Mersits and Pestre, with L. Weiss, *History of CERN: Building and Running the Laboratory* Volume II (Amsterdam, 1990), p. 760.

20. Michael D. Reagan, *Science and the Federal Patron* (New York, 1969); Donald R. Fleming, “The Big Money and High Politics of Science,” *Atlantic Monthly*, (August 1965); “Appendix 17,” in U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, p. 753. *Ibid.*, pp. 756, 752.

21. Robert W. Smith, *The Space Telescope* (Cambridge, Mass., 1989) p. 66.

22. Lederman used TNL as a pun on BNL, which he felt was not functioning as a truly national facility. Quotes, respectively, from Glenn Seaborg record of conversation, March 2, 1964, Seaborg Papers and Leon Lederman, “The Truly National Laboratory,” in “1963 Super-High-Energy Summer Study,” Brookhaven National Labo-

ratory, AADD-6, p. 10. Also: Edward Lofgren to Edwin McMillan, April 6, 1961, Lofgren Papers.

23. MURA's defeat and its effect on the political background for the 200 BeV has been noted by many writers, including Daniel Greenberg in *The Politics of Pure Science* (New York, 1967), Jachim, (n. 2 above), Lowi and Ginsberg, (n. 2 above). All three writers, however, ignore the major contribution that outside user tensions played in setting this background. Westfall interview with Edward Lofgren May 3, 1984; Westfall interview with Leon Lederman, July 20, 1984; Westfall interview with William Wenzel, May 2, 1984; unsigned draft to Edwin McMillan, 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.

24. Hoddeson interview with Frederick Seitz, 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. Congress, (n. 20 above), pp. 8-9; Glenn 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.

25. Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume

I, pp. I-5, I-8.

26. Quotes, respectively, from *ibid.*, pp. I-6, I-7, and XVI-1.

27. Robert Wilson to Edwin McMillan, September 27, 1965.

28. Quotes from Robert Wilson, "Some Proton Synchrotrons, 100-1000 GeV," September 22, 1965.

29. One of the meetings was hosted by the Brookhaven managing consortium, Associated Universities Incorporated (AUI) and held at the Biltmore Hotel in New York City on January, 15, 1965. The other was hosted by the AEC and held in Washington on January 24, 1965. Glenn Seaborg, "Meeting of Board of Trustees, Universities Research Association, Inc.," December 12, 1965, Seaborg Papers; Frederick Seitz to Norman Ramsey, December 14, 1965; Seaborg, "Meeting of Board of Trustees, Universities Research Association, Inc." December 12, 1965.

30. Doris Kearns, *Lyndon Johnson and the American Dream* (New York, 1976), p. 296; Smith, (n. 21 above), p. 72.

31. 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.

32. Glenn Seaborg to Frederick Seitz, March 2, 1965; Seaborg, Diary, TS, September 1, 1965, Seaborg Papers; Seaborg to Frederick Seitz, September 13, 1965, Seaborg Papers; Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966.

33. John Erlewine, "Summary of Proposers' Written and Oral Commitments Re the 200 BeV Accelerator Project," July 25, 1966, Secretariat, Box 7741, DOE Archives; Glenn Seaborg, record of conversation, July 13, 1966, Seaborg Papers; Henry Traynor to Seaborg, James Ramey, Gerald Tape, July 29, 1966, Secretariat, Box 7741, DOE Archives.

34. Although the Commissioners acknowledge that choosing a Midwestern site was responsive to prevailing pressures, they all insist that they would not have chosen a Midwestern site unless they sincerely believed it possessed superior qualities. Despite continued assertions, many feel that the Commission did not make the decision. These arguments are refuted in Westfall, "The Site Contest for Fermilab," *Physics Today*, 42 (1989): 44-52. Westfall interviews with Glenn Seaborg, February 4, 1983 and February 16, 1984; Westfall interview with Gerald Tape, November 21, 1986; Westfall interview with James Ramey, November 21, 1986; Westfall interview with Samuel Nabrit, October 6, 1987; Westfall interview with Bernard Waldman, February 4, 1983; Seaborg record of conversation, December 7, 1966, Seaborg Papers.

35. Quote from Edward Lofgren to Norman Ramsey, January 12, 1967, Lofgren Papers. Also: Universities Research Association, "Proposal for Continuing Studies for a 200 BEV Accelerator Facility," December 23, 1966; Norman Ramsey to Edward Lofgren, December 30, 1966, Lofgren Papers.

36. Wilson officially accepted the directorship on March 1, 1967. Robert Wilson to Norman Ramsey, March 1, 1967. Also: Hoddeson interview with Norman Ramsey,

February, 26, 27, 1980; Westfall interview with Edwin Goldwasser July 10, 1985.

37. Robert Wilson, tape recording, "1967 Berkeley Meeting" Glenn Seaborg, Diary, TS, January 16, 1967, Seaborg Papers; Hoddeson interview with Norman Ramsey, February 26, 17 1980; Westfall interview with Denis Keefe, Glen Lambertson, and Jackson Laslett, December 22, 1986.

38. Quote from Glenn Seaborg, Diary, TS, February 14, 1967, Seaborg Papers. Also: Seaborg, Diary, TS, 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; William Salsig, D. G. Eagling, J. A. Burt, E. Eno, R. O. Haglund, F. M. Johnson, W. Popenuck, H. A. Wollenberg, "Preliminary Estimate of Cost Differentials Between the Weston Site and the Reference Site (Sierra) for the Design Study Accelerator," February 6, 1967, Salsig Files.

39. Quotes from U.S. Congress, Joint Committee on Atomic Energy, United States Congress, Hearings. 90th Congress, First Session, (Washington D.C., 1967), pp. 97. Ibid., pp. 22-394.

40. Ibid., pp. v, 24-28, 31-32, quote from p. 24. Also: Al A. Garren, Glen Lambertson, Edward Lofgren, and Lloyd Smith, "Extendible-Energy Synchrotron," *Nuclear Instruments and Methods* 54 (1967): 223.

41. U.S. Congress, Joint Committee on Atomic Energy, "Atomic Energy Commission Authorizing Appropriations, FY 1968," 90th Congress, First Session, (Washington D.C.: GPO, 1967) pp. 36, 57-59; Lyndon Johnson to Glenn Seaborg, July 26,

1967, Seaborg Collection, Box 170, DOE Archives.

42. Westfall interview with Robert Wilson, May 25, 1987.

43. Quotes, respectively, Robert Wilson, "My Fight Against Team Research," *Daedalus*, (Fall, 1970): 1086; Westfall interview with Robert Wilson, May 25, 1987; and Dominique Pestre and John Krige, "Some Thoughts on the Early History of CERN," in Peter Galison and Bruce Hevly eds., *Big Science: The Growth of Large-Scale Research* (Stanford, 1992), p. 95.

44. Hoddeson interview with Robert Wilson, January 23, 1981.

45. The later addition of a ring of superconducting magnets would then allow reaching 1000 GeV. Hoddeson, (n. 45 above), pp. 25-54.

46. Westfall interview with Robert Wilson, April 1, 1987.

47. Westfall interview with Edwin Goldwasser, May 15, 1987; Westfall interview with Robert Serber, February 24, 1986; Hoddeson interview with Robert Wilson January 12, 1979; Don Getz, May, 1977, untitled manuscript; Hoddeson, (n. 2 above), p. 20.

48. As Heilbron and Seidel note, in the 1930s Lawrence was known for instituting "the California habit of speed." As quoted in Heilbron and Seidel (n. 11 above), p. 264. For a discussion of speed in reference to research at Los Alamos, see Hoddeson, "The Los Alamos Implosion Program in World War II: A Model for Postwar American Research," in *Proceedings of Rome Internal Conference on the Restructuring of Physical Sciences in Europe and the United States, 1945-1960*, 19-23 September

1988 (Singapore, 1989). Also: Westfall interview with Robert Wilson, April 1, 1967; Robert Wilson, "National Accelerator Laboratory Synchrotron," July 23, 1967.

49. 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. T. Kitagaki, "A Focusing Method for Large Accelerators," *Physical Review* 89 (1953): 1161-2 and M. G. White, "Preliminary Design Parameters for a Separated-Function Machine," Princeton, N. J., March 3, 1953.

50. Quote from Arie Van Steenbergen, "200-400 BeV Accelerator Summer Study," July, August 1967, p. 7. The separated function magnets could achieve considerably higher central-orbit fields (by 15 to 20%) for the same peak fields in the aperture than combined function magnets. Francis Cole, "Progress Report on the NAL Accelerator," *Particle Accelerators* 2 (1971): 3. Also: Westfall interview with Denis Keefe, Glen Lambertson, and L. Jackson Laslett, December 22, 1986; and Westfall interview with Robert Wilson, April 1, 1987.

51. 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%, much higher than that achieved in previous accelerators. James A. Sanford, "The Fermilab National Accelerator Laboratory," *Annual Review of Nuclear Science*

26 (1976): 169; Cole, *ibid.*, p. 2. Also: National Accelerator Laboratory, "Design Report," (National Accelerator Laboratory, January 1968), p. 5-3; Westfall interview with Robert Wilson, April 1, 1987; Robert Wilson, "National Accelerator Laboratory Synchrotron," July 23, 1967; Robert Wilson, "Some Aspects of the 200 GeV Accelerator," presented at the VI International Conference on High Energy Accelerators, Cambridge Massachusetts, September 12, 1967, p. 4.

52. Quote from Robert Wilson, "My Fight Against Team Research," *Daedalus*, (Fall, 1970): 1083. Also: Westfall interview with Robert Wilson, May 25, 1987; Westfall and Hoddeson interview with Drasko Jovanovic, November 29, 1989; Westfall interview with Francis Cole, March 13, 1987; and Westfall and Hoddeson interview with Thomas Collins, November 29, 1989.

53. For a discussion of the pioneers 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. Quote from Westfall interview with Robert Wilson, May 25, 1987.

54. Westfall interview with Robert Wilson, May 25, 1987.

55. Quotes, respectively, from Heilbron and Seidel (n. 11 above), pp. 283, 284 and Westfall and Hoddeson interview with Thomas Collins, November 29, 1989.

56. The lattice, refined by Collins when the components were installed, also contained straight sections for beam handling and rf acceleration.

57. Westfall interview with Robert Wilson, April 1, 1987; National Accelerator

Laboratory, (n. 51 above), Report,” pp. 4-1, 5-5; Livingston, (n. 2 above), p. 21; Robert Wilson, “Some Aspects of the 200 GeV Accelerator,” (n. 51 above), p. 5.

58. B1 magnets have 1 1/2 inch vertical gaps while the B2 magnets have 2 inch gaps. The sizes are different to take into account the changing dimensions of the beam. Also: Livingston, (n. 2 above), p. 21; Wilson, “Some Aspects of the 200 GeV Accelerator,” (n. 51 above), pp. 4-5; Ernest Malamud and James K. Walker, “Progress and Prospects at the National Accelerator Laboratory,” (National Accelerator Laboratory, December 1970,), p. 3; Westfall and Hoddeson interview with Thomas Collins, November 29, 1989.

59. The LRL magnets were estimated to weigh 19,415 tons and cost \$26.6 million while the NAL magnets were estimated to weigh 9,750 tons and cost \$20.9 million. Lawrence Radiation Laboratory, (n. 25 above), pp. III-9, III-10, XVI-4; National Accelerator Laboratory, (n. 51 above) pp. A-4, 16-3.

60. Quote from Hoddeson interview with Norman Ramsey, February 26, 27, 1980. Also: Don Getz, untitled report, May, 1977; “Questions Raised on the Design of the 200 BeV Accelerator,” no date, Mills Papers; Lawrence Radiation Laboratory, *ibid.*, p. XVI-16; National Accelerator Laboratory, (n. 51 above), pp. 16-11.

61. Spencer Weart, “The Physics Business in America, 1919-1940: A Statistical Reconnaissance,” in Nathan Reingold, ed., *The Sciences in the American Context: New Perspectives* (Washington D. C., 1979) p. 328; High Energy Physics Advisory Panel, “The Status and Problems of High Energy Physics Today,” January, 1968, p.

38; Atomic Energy Commission, "Atomic Energy Commission Summary Notes of 200 BeV Accelerator Briefing," September 1, 1967, Seaborg Files.

62. Robert Wilson to Glenn Seaborg, February 28, 1967; Hoddeson interview with Norman Ramsey, February 26, 27, 1980.

63. By the late 1960s, some modeling of the accelerator, for example the design of the magnet lattice, was done with computer programs.

64. M. Stanley Livingston, "Design Progress at the National Accelerator Laboratory" (National Accelerator Laboratory, June 1969) p. 1; Westfall interview with Ernest Malamud, March 12, 1987.

65. Westfall interview with Ernest Malamud, March 12, 1987.

66. Philip Livdahl, "A Brief Summary of Fermilab During Initial Construction Years," (Fermilab, November 1983), pp. 9, 15; Francis Cole, "Monthly Report of Activities," February 28, 1969, p. 1.

67. Although the accelerator theory group obtained a PDP-10 as a gift when the Princeton-Pennsylvania Accelerator closed, the group continued to use the more powerful computing facilities at NYU and ANL for many years. Westfall interview with James Maclachlan, November 27, 1989. Francis Cole, "Monthly Report of Activities," April 1, 1968, p. 2; Cole, "Monthly Report of Activities," June 1, 1968, p. 3, 4, 6; Wilson, "Some Aspects of the 200 GeV Accelerator," (n. 51 above), p. 5.

68. Westfall and Hoddeson interview with Ernest Malamud, October 24, 1989; Francis Cole, "Main Ring Group Meeting," March 27, 1968; "Monthly Report of

Activities,” August 1, 1968, p. 4; Cole, “Monthly Report of Activities,” September 1, 1968, pp. 4-5; Cole, “Monthly Report of Activities,” November 1, 1968, p. 3; Cole, (n. 66 above), p. 9.

69. For more information on land acquisition and funding difficulties, see Westfall, “The First ‘Truly National Laboratory’: The Birth of Fermilab,” (Ph.D. diss., Michigan State University, 1988), Chapter 6. Also: “Main Accelerator Section Monthly Report,” December 1968; Francis T. Cole, “Monthly Report of Activities,” September 1, 1968, p. 6; Hoddeson and Westfall interview with Ryuji Yamada, October 25, 1989; “Monthly Report Main Accelerator Section,” November 1968; “Minutes of Staff Meeting Main Ring,” August 27, 1969; “Minutes of the Main Ring Staff Meeting,” January 28, 1970.

70. Cole, (n. 67 above), p. 6; Francis Cole, “Monthly Report of Activities,” October 1, 1968, p. 3; “Main-Accelerator Section Monthly Report,” March 31, 1969.

71. Livingston, (n. 2 above), p. 21; Westfall interview with Robert Wilson, February 13, 1990; Sanford, (n. 51 above), p. 165; Cole, (n. 50 above), p. 5.

72. Francis Cole, “Monthly Report of Activities,” April 30, 1969, p. 6; Livingston, (n. 64 above), pp. 11, 12.

73. Quote from Westfall interview with Ernest Malamud, October 24, 1989. Also: Hoddeson and Westfall interview with Ryuji Yamada, 25 October 1989; Cole, *ibid.*; Cole, “Monthly Report of Activities,” May 31, 1969, p. 2.

74. Livdahl, (n. 66 above), p. 10; Westfall interview with Robert Wilson, May

25, 1987.

75. Quotes from Robert Wilson, "Sanctimonious Memo #137," July 11, 1969. Also: Westfall and Hoddeson interview with Ernest Malamud, October 24, 1989.

76. Cole, (n. 73 above), p. 5; Cole, "Monthly Progress Report," September 30, 1969, pp. 8, 9; Cole, (n. 50 above), p. 5; "Monthly Report Main Ring Section," October, 1969, p. 2.

77. Francis Cole, "Monthly Report of Activities," November 30, 1969, p. 9; Colc, "Monthly Report of Activities," January 31, 1970, p. 6; "Minutes of the Main Ring Staff Meeting," January 7, 1970.

78. Quotes, respectively, from Westfall interview with Robert Wilson, May 25, 1987 and Henry Hinterberger and Robert Wilson, tape recording, June 21, 1982. Also: Livingston, (n. 64 above), p. 13. Also: National Accelerator Laboratory, (n. 51 above), p. 5-1; Cole, (n. 66 above), p. 8.

79. "Minutes of the Main Ring Staff Meeting," December 3, 1969; "Minutes of the Main Ring Staff Meeting," December 17, 1969; Hoddeson and Westfall interview with Ryuji Yamada, October 25, 1989.

80. Quotes, respectively, from "Minutes of the Main Ring Staff Meeting," December 3, 1969; Westfall interview with Robert Wilson, April 1, 1987; and Francis Cole, "Monthly Progress Report," December 31, 1969, p. 8. Also: Cole, "Monthly Progress Report, April 30, 1970," pp. 11-12.

81. Quote from Weart, (n. 61 above), p. 328. *Ibid.*, p. 327.

82. Transcript, Second User's Meeting, December 2, 1968; U.S. Congress, Joint Committee on Atomic Energy, Hearings. 91st Congress, First Session, (Washington D.C.: GPO, 1971), p. 1214; ; and Francis T. Cole, "Monthly Report of Activities," July 31, 1969, p. 1.

83. Quotes from Westfall interview with James Maclachlan, November 27, 1989. Also: Westfall and Hoddeson interview with Drasko Jovanovic, November 29, 1989; Westfall and Hoddeson interview with Ernest Malamud, October 24, 1989.

84. Dominique Pestre, "Some Characteristics Features of CERN in the 1950s and 1960s" in Hermann, et al. (see note 19), p. 799.

85. Francis Cole, "Monthly Progress Report," October 31, 1969, p. 8.

86. Westfall interview with Robert Wilson, May 25, 1987; "Minutes of the Main Ring Meeting," December 10, 1969. For more information on the further development of mass-produced magnets at Fermilab, see Hoddeson, (n. 45 above), pp. 25-54.

87. Quotes, respectively, from Westfall and Hoddeson interview with Malamud, October 24, 1989 and Robert Wilson, "Notes on Talk to Employees," June 4, 1970. Also: Francis Cole, "Monthly Progress Report," December 31, 1969, p. 8; Westfall interview with Ernest Malamud, March 12, 1987.

88. Quote from Robert Wilson, "Statement Made by R. R. Wilson at the Second Annual Meeting of the NAL Users Organization on Friday, April 10, 1970." Also: "Minutes of the Main Ring General Meeting," May 28, 1970; Francis T. Cole, "Monthly Report of Activities, August 31, 1970, p. 12; Cole, "Monthly Report of

Activities,” September 30, 1970, p. 3.

89. Quote from Francis Cole, “Monthly Report of Activities,” September 30, 1970, p. 1. Also: Westfall interview with Robert Wilson, April 1, 1987.

90. Westfall interview with Ernest Malamud, March 12, 1987; Westfall interview with Helen Edwards, March 13, 1987; Francis T. Cole, “Monthly Report of Activities,” October 31, 1970, p. 1.

91. “Main Accelerator Monthly Report,” October 1970; Francis T. Cole, “Monthly Report of Activities,” March 31, 1971, p. 1; Cole, “Monthly Report of Activities,” April 30, 1971, p. 1.

92. Wilson, (n. 88 above).

93. Quote from “Minutes of Main Ring Section Meeting on the Protomain,” March 16, 1970. Also: Hoddeson and Westfall interview with Ryuji Yamada, October 25, 1989.

94. Quote from Westfall and Hoddeson interview with Ernest Malamud, October 24, 1989. Also: Ernest Malamud to Mrs. Hanson, March 7, 1980, Malamud Papers.

95. U.S. Congress, Subcommittee on Research and Development and Radiation of the Joint Committee on Atomic Energy, Hearings. 92nd Congress, Second Session, (Washington D.C., 1972), p. 1433.

96. Quote from Francis T. Cole, “Monthly Report of Activities,” June 31, 1971, p. 2. Also: Cole, “Monthly Report of Activities,” May 31, 1971, p. 2.

97. L. Hoddeson, P. Henriksen, R. Meade, and C. Westfall, *Critical Assembly:*

A History of Los Alamos During the Oppenheimer Years, 1943-1945 (Cambridge, Mass., 1993). Francis Cole, "Monthly Report of Activities," October 31, 1971, p. 2; William Hanson to distribution, October 18, 1971; Westfall and Hoddeson interview with Ernest Malamud, October 24, 1989.

98. Quotes from Francis T. Cole, "Monthly Report of Activities," August 1, 1971, p. 2. Also: Ernest Malamud to all members of Main Ring Section, August 2, 1971; "Steering Meeting," September 23, 1971; Westfall interview with Robert Wilson, 1 April 1987, p. 14; Francis Cole, "Monthly Report of Activities," October 31, 1971, p. 3.

99. Quote from Westfall and Hoddeson interview with Drasko Jovanovic, November 29, 1989. Also: Westfall interview with J. Richie Orr, March 12, 1987; Westfall interview with Robert Wilson April 1, 1987; Westfall interview with Edwin Goldwasser, May 15, 1987; Westfall interview with Leon Lederman, November 26, 1990.

100. For more information on problem solving strategies at the wartime atomic bomb project, see L. Hoddeson, et al., (n. 97 above).

101. Quotes, respectively, from Robert Wilson to Norman Ramsey, October 29, 1971 and Westfall interview with J. Richie Orr, March 23, 1989. Also: Robert Wilson, "Formation of the Accelerator Section," memorandum to the staff, October 21, 1971.

102. Westfall interview with Edwin Goldwasser, May 15, 1987; Westfall interview with Robert Wilson, May 25, 1987; Westfall and Hoddeson interview with Thomas Collins, November 29, 1989.

103. Quote from Westfall interview with Thomas Collins, June 4, 1990. Also: Hoddeson and Westfall interview with Ryuji Yamada, October 25, 1989; Westfall interview with James Maclachlan, November 27, 1989; Westfall and Hoddeson interview with Thomas Collins, November 29, 1989; Westfall and Hoddeson interview with Drasko Jovanovic, November 29, 1989; Westfall and Hoddeson interview with Ernest Malamud, October 24, 1989.

104. Westfall interview with J. Richie Orr, March 12, 1987; Francis Cole, "Monthly Report of Activities," February 30, 1972, p. 2; U.S. Congress, (n. 95 above), p. 1435.

105. When construction was completed, the project was \$20 million under the budget. Wilson used the \$20 million to support research on building a superconducting accelerator. Hoddeson, "The First Large-Scale Application of Superconductivity: The Fermilab Energy Doubler, 1972-1983," *Historical Studies in the Physical and Biological Sciences*, 18:1 (1987): 35. Also: Francis T. Cole, "Monthly Report of Activities," January 31, 1972, p. 1; Cole, "Monthly Report of Activities, March 1, 1972; Appendix 3 in U.S. Congress, (n. 95 above), p. 1731.

106. Eventually the telescope was funded. For the story of how the project was saved, see Chapters 4 and 5 in Smith, (n. 21 above). Quote from p. 87. *Ibid.*, pp. 89, 90, 99, 100, 109, 115.

107. Quote from U.S. Congress, (n. 95 above), p. 1438. Also: U.S. Congress, Joint Committee on Atomic Energy. Hearings. 92nd Congress, First Session (Washington

D.C.: GPO, 1971), p. 1196; Universities Research Association, Annual Report 1974, January 24, 1975, pp. 2, 5; Atomic Energy Commission, "AEC Names 200 BeV Accelerator in Honor of Enrico Fermi," April 29, 1969, press release, Seaborg Papers; Universities Research Association, Annual Report 1975, January 15, 1976, p. 1; Universities Research Association, Annual Report 1976, January 1, 1977, p. 1.

108. Westfall interview with J. Richie Orr, March 12, 1987.

109. Continuities in personnel among the three laboratories included: Oppenheimer, who was both at Lawrence's laboratory in the 1930s and at Los Alamos, where he served as director; Wilson, who was at all three laboratories; and Priscilla Duffield who served as the director's secretary at all three.