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THE FIRST "TRULY NATIONAL LABORATORY": THE BIRTH OF FERMILAB

Volume I

By
Catherine Westfall

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ABSTRACT

THE FIRST "TRULY NATIONAL LABORATORY": THE BIRTH OF FERMILAB

By

Catherine Westfall

Growing intellectual excitement, technological advances in physics, and a new alliance between the physics community and the federal government after World War II created a powerful momentum for accelerator building. This momentum led in 1960 to plans for a multi-million dollar accelerator laboratory. In 1972, Fermi National Accelerator Laboratory, or Fermilab for short, was christened. This dissertation charts the birth of Fermilab. It first explores the scientific, technological, and social environment from 1945 to 1960 that allowed the laboratory's conception, Next, it examines the development of the multi-hundred million dollar project from planning to construction against the backdrop of the changing economic and political environment of the 1960s and 1970s. This examination shows how cost and resulting pressures led to the formation of the first U.S. "Truly National Laboratory," a centrally located accelerator laboratory, managed by a national corporation, and built with maximum capability at minimum cost. It also explains how the multi-hundred million dollar project was funded, despite budgetary pressures and growing public skepticism about the value of science and technology.

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INTRODUCTION

Fueled by the powerful momentum for accelerator building generated in the fifteen years after World War II, physicists began in 1960 to plan a multi-hundred million dollar accelerator laboratory, the first designed to serve the entire community of high energy physics. As planning proceeded, the U.S. economy worsened and public skepticism about the value of science and technology rose. In this environment, two questions emerged. Would cost and resulting pressures force changes in the way a major accelerator project was planned, organized, and constructed? Would the federal government agree to fund a multi-hundred million dollar project even though the economic and social climate of the 1960s was less favorable to science funding than that of the 1950s?

To assess what the birth of Fermilab reveals about the evolution of accelerator laboratories and the federal commitment to accelerator building, this study examines the twelve-year birth of Fermilab, analyzing the decisions that formed the course for the new laboratory. In some ways, the new laboratory was shaped by tradition. For example, the decision to build the expensive new machine arose from the post-World War II momentum of accelerator building. The intellectual excitement of high energy physics in the 1950s whetted the desire for a more powerful tool for elementary particle exploration. Thanks to the successful discovery and implementation of phase stability and strong focusing, such an accelerator could be built. Thanks to the postwar alliance between the federal government and the physics community, such an accelerator could be funded.

Cost, growing public skepticism about the value of science and technology, budgetary pressures, and political tensions inside and outside the physics community dictated that other decisions were made along nontraditional lines. The worsening economy and increasing costs of accelerators led to the complaint that accelerators were edging more useful projects, such as reactors, from the Atomic Energy Commission (AEC) budget. The shrinking budget had other consequences: longstanding plans for a Midwestern accelerator were cancelled, giving rise to political pressure from Midwestern physicists and politicians for a Midwestern accelerator. Added pressure also came from the unprecedented cost of the new accelerator. Realizing that the budget could support only one such machine, young, active experimentalists lobbied for assurance that the accelerator would be accessible to outside users. To respond to these pressures, which threatened to erode support for the accelerator, National Academy of Sciences (NAS) President Frederick Seitz and AEC Chairman Glenn Seaborg abolished the traditional prerogative of designers to manage and build an accelerator at the site of their choice. Seitz formed the nationally represented Universities Research Association, Inc. to manage the accelerator, and Seaborg held a competition which chose an Illinois site for the accelerator.

Aided by the accrued capital of prestige and interest in high energy physics, the AEC secured the project's first large funding allocation in 1967, an allocation which signaled a federal commitment to complete the project. By this time, the economic pressure of the planning years had influenced another decision. Faced with a tightening budget and the political need to build an expandable machine for $250 million, Robert Wilson, an accelerator builder with a reputation for

producing maximum capability for minimum expense, was chosen as the laboratory's first director.

By 1972, when the new accelerator reached its design energy and Fermilab was christened, the nation had a centrally located accelerator laboratory, managed by a national corporation, and built with maximum capability at minimum cost. With these innovations, this first "Truly National Laboratory" set a new course for planning, organizing, and constructing future accelerator laboratories. In addition, the funding of the laboratory affirmed the federal decision to continue its investment in ever-larger accelerators.

This analysis of the birth of Fermilab is composed of 6 chapters. The first two chapters examine how momentum was generated for building the tools designed to explore the fundamental nature of matter in the period from 1945 to 1960. These chapters address only those issues directly affecting accelerator development, and since the period has not been thoroughly analyzed by historians, the discussion leans heavily at points on a handful of definitive studies which have emerged in the last few years. The remaining chapters examine the development of the new accelerator project against the backdrop of the changing economic and political environment of the 1960s and 1970s. Chapter 3 explores the beginning of planning efforts for the new accelerator and traces the emergence of the design proposal. Chapters 4 and 5 analyze how arrangements were made to manage, locate, and fund the new accelerator, while Chapter 6 explains how the laboratory was established and how the accelerator was designed and constructed.

Additional information has been placed at the end of the text. A glossary of names identifies major characters and a glossary of terms provides more thorough explanations of scientific terms. To make the latter glossary more easy to use, glossary terms are added parenthetically in the text at their first occurrence. Five appendices give three site proposal lists, provide AEC site criteria, and detail the institutions and officers involved in the early years of the Universities Research Association, Inc. In addition, a bibliograpy has been added.

Chapter 1: Physics After World War II--1945-1950

Introduction

In June, 1947 a group of physicists left Manhattan for a trip by bus and ferry to attend a conference on Shelter Island, a small stretch of land just off the coast of Long Island. The conference, which was convened to assess the status and problems of quantum theory, included such luminaries as J. Robert Oppenheimer, scientific head of the atomic bomb project, and Victor F. Weisskopf, known by many as the oracle of Los Alamos. After being stopped by a motorcycle trooper, the bus was escorted to Suffolk County. Amid the blaring of the police siren, the startled scientists speculated that they were the victims of an unexpected security screening. Instead of a screening, however, they were treated to dinner at Greenport and a speech. John C. White, president of the Greenport Chamber of Commerce and host for the evening, explained that their abduction was done in a spirit of appreciation. As a Marine fighting in the Pacific, he explained, he might not have made it home had it not been for the atomic bomb. The assembled townspeople cheered. (1)

With uplifted spirits, the group proceeded to the island for three days of discussion, which Oppenheimer later characterized with dignified understatement as "rather unexpectedly fruitful." From the meeting emerged a self-consistent theory of quantum electrodynamics, a scheme for explaining sub-atomic matter that one historian calls "the most powerful and accurate theory known to physical man." Although the

Atomic Energy Commission had not yet been formed by the time of the 1947 meeting, an accelerator of unprecedented power and expense had already gone into operation at Berkeley to facilitate further sub-atomic exploration. This was only the first step in a full-speed postwar march to machines of ever-higher energy, a march that would eventually lead to a $250 million dollar accelerator laboratory named Fermilab. (2)

The birth of Fermilab was possible because of the momentum for accelerator building generated in the fifteen years after World War II. Many of the patterns that would form the course of American physics grew out of the first five years after World War II. To understand these patterns, three key questions must be examined. First, how did the funding structure capable of financing expensive accelerators evolve and what new possibilities and obligations were incurred in the resulting alliance between the federal government and the physics community? Second, what scientific discoveries and accomplishments stimulated interest in the study of the fundamental structure of matter? Third, how did the first postwar accelerator projects develop and how did this development set the pace for the future?

The Postwar Science Funding Structure

One of the many changes forged in World War II with successful atomic bomb and radar projects was large-scale U.S. government support of science. Although attempts to continue funding started in World War I foundered due to lack of government interest and the scientific community's wariness of political control, during World War II the

average annual federal research and development investment increased from less than $50,000,000 to $500,000,000. (3)

This wartime investment left rich new resources, including new facilities. In particular, the Manhattan Engineer District (MED), which administered the atomic bomb project, had undertaken massive construction. The MED built new laboratories for uranium enrichment at Oak Ridge, Tennessee and plutonium production at Hanford, Washington, facilities for reactor design at Argonne Forest near Chicago, and a laboratory to develop the bomb at Los Alamos, New Mexico. In addition, new equipment and buildings were added to a number of existing academic institutions, most notably to the Berkeley Radiation Laboratory, where research was conducted on electromagnetic separation of uranium, and to the University of Chicago, where the first working pile was constructed as part of the wartime Metallurgical Laboratory.

The war also substantially increased the size of the American scientific community since large numbers of scientists and engineers were trained for war projects. In addition, the nation had gained a number of talented scientists who emigrated to the United States to escape the political and religious persecution of Axis nations. Physicists, who had been culled primarily from academia, played a crucial role in wartime projects and the investment in training and facilities for physical research was particularly heavy.

Research into the fundamental nature of matter had virtually halted under the pressure of war, but when this pressure lifted physicists again turned their attention to the confusing and intriguing problems that had accumulated just before the war. As explained in the next

section, physicists were eager to study mesotrons, unusual phenomenon spotted in 1937 in cosmic rays, which promised to provide information both about the atomic nucleus and about cosmic rays. This item on the research agenda provided a powerful stimulus for improving accelerators, the instrument designed for exploring subatomic matter. As in the past, physicists were eager to employ this powerful tool so that the desired phenomenon could be more copiously produced than in nature and studied under more controlled conditions. Since existing accelerators were unable to achieve or maintain the energies necessary for mesotron production, physicists were eager to devise a new, more powerful type of accelerator. By December 1945 a review article was able to list six new accelerator designs. (4)

In the heady postwar era, American physicists were in a position to secure more funding for such projects than ever before. The citizens of Greenport were not alone in their admiration for the development of the atomic bomb and there was general agreement that the new energy source needed further development. As historian Daniel J. Kevles explains, after the atomic bomb was dropped "military officials, civilian analysts, congressmen and thoughtful observers across the partisan and ideological spectrum agreed: In the atomic age, the United States could not do without a national policy of scientific, especially nuclear, research." In an atmosphere friendly to the support of science, three separate plans emerged for the postwar funding structure for physical research. Those active in the wartime research and development effort, in particular, Vannever Bush, worked to establish a research agency and an atomic energy agency. At the same time, the U.S. Navy took steps to provide a research agency of its own. (5)

In the Navy, the conviction had grown that a permanent plan was needed to provide naval research. Before the war ended a group of bright young naval officers, who came to be called "Bird Dogs," began promoting the idea that the Navy would benefit from a peacetime research program. Their cause was championed by Vice Admiral Harley G. Bowen, who wanted to pioneer the development of nuclear powered naval vessels. At the instigation of Bowen, legislation was prepared and by August, 1946 a bill was signed to establish the Office of Naval Research (ONR), dedicated to fostering both basic and applied research. ONR assuaged initial academic suspicion by minimizing red tape and granting a great deal of freedom to professors in proposing, conducting, and publishing research and subsequently the ONR became an important patron of university-based research. (6)

In contrast with the speed with which the ONR was established, plans for the basic research agency took a long time to materialize. Some observers were already offended that wartime funding had been channeled into a small number of corporations and universities. While this practice followed physics community tradition, which extolled meritocracy and encouraged merit through concentration of funding, it contradicted liberal notions of fair play. Concern over such issues found a spokesman in Senator Harley M. Kilgore, a West Virginia Congressman, who proposed a program in 1945 for a national science foundation under the direct control of the president. The program focused on socially useful goals and included funding for basic research in the social sciences and civilian-controlled research aimed at military applications. According to Kilgore's plan, funding would be distributed, at least in part, on the basis of geographical equity, and

patents stemming from federally funded research would usually be awarded to the government. (7)

The most vigorous opponent to the plan was Vannever Bush. Bush, the son of a clergyman and native of Boston, was a formidable foe. After receiving his Ph.D. in engineering jointly from MIT and Harvard, he accepted a professorship at MIT, where he invented a mechanical computer. He served as vice-president of MIT, president of the Carnegie Institution, and served on the short-lived Science Advisory Board, which attempted to establish federal support for science in the 1930s. Armed with his administrative experience and fueled by his ambition, considerable technical reputation, and determination to mobilize the nation's scientific resources for national defense, he became the principal architect of the Office for Scientific Research Development (OSRD) during World War II. (8)

When President Franklin D. Roosevelt requested his opinions on the postwar support of science in fall 1944, Bush was determined to present an alternate to Kilgore's plan. Bush, a shrewd-eyed, flinty man, believed in the economic value of technological benefits and opposed government constraints on business or science. His politically conservative and elitist views were vividly apparent in Science the Endless Frontier, his 1945 response to Roosevelt's request. The document suggested the formation of an independent foundation meant to fund federal research. Although it would receive government support and approval, the foundation's decision-making and administration would be free from direct political control. In contrast to Kilgore, he proposed no remedy for the patent system, omitted consideration of the social

sciences, and avoided mention of either geographical distribution of funds or plans for socially useful research. As an OSRD aide noted, Bush's plan would be "a new social invention--of government sanction and support but professional guidance and administration." (9)

At Bush's direction the OSRD drafted a bill which was then submitted to Congress by Senator Warren G. Magnuson. When hearings were held in the fall of 1945 to debate the Magnuson bill and legislation presented by Kilgore, the most hotly contested issue was the advisable amount of presidential control. Whereas the Kilgore plan called for a foundation director chosen by and responsible to the president, the Bush plan proposed a presidentially appointed part-time board composed of private citizens, primarily scientists, with the power to elect a director responsible only to themselves. (10)

When Bush refused to compromise on major issues, resistance continued, both from Congress and from President Harry Truman, who was less responsive than Roosevelt to the prominent physicist. Legislation for the foundation was consequently delayed for more than three years. In 1950, however, Bush's efforts were rewarded. The bill establishing the National Science Foundation (NSF) described an institution embracing his major recommendations, with the exception that the new Foundation had a presidentially appointed director and had no jurisdiction over fundamental military research. (11)

While Bush labored along traditional lines to establish a basic research agency largely independent of government control, younger leaders, in particular Ernest O. Lawrence and Isidor I. Rabi, were prepared to lose such independence to ensure that physics funding levels

remained high. By this time, Lawrence and Rabi had forged illustrious careers as both scientists and leaders of government projects. Lawrence, whose parents were Norwegian immigrants, showed an early proclivity for hard work and ambitious goals. As his mother repeatedly noted: "Ernest was born grown up." After ardently pursuing graduate studies in physics at the University of Minnesota and obtaining his Ph.D. at Yale, the lanky, energetic Lawrence accepted a professorship at the University of California, Berkeley. There, with the help of his graduate student, M. Stanley Livingston, he constructed the first successful cyclotron in 1931. In the ensuing decade, Lawrence directed the construction of a series of accelerators. Before one could be finished, the indefatigable Lawrence had plans for the next, more powerful machine. In the process, Lawrence built a world-renowned research facility, thanks to his technical and administrative talents and a knack for obtaining private financing. In recognition of his contributions to accelerator development, Lawrence won the Nobel Prize in 1939. In World War II, Lawrence bolstered his reputation and forged new contacts by leading the effort to develop the electromagnetic method for enriching uranium and serving as a top OSRD administrator. (12)

Rabi's ascent to prominence resembled Lawrence's in some important ways. Like Lawrence, Rabi was born to emigrant parents; Rabi's parents were orthodox Jews from Austria. Like Lawrence, Rabi worked hard and had high goals as a youngster. After enrolling in a predominantly gentile high school to broaden his cultural and educational horizons, he went to Cornell and majored in chemistry, becoming a top student in the department. Unlike Lawrence, Rabi confronted prejudice after receiving his degree in 1920 that prevented immediate advancement. Like many of

his Jewish classmates, he was refused a graduate fellowship and could not land a good job. Refusing to be deterred, the short, intense Rabi supported himself with a full-time instructorship at New York's City College while earning his Ph.D. in physics from Columbia in 1927. After traveling to Germany, where he gained considerable acclaim for a clever method for deflecting molecular beams, Rabi became the first Jew to be appointed to Columbia's physics department. After further research with molecular beams, Rabi won the Nobel prize in 1944 for his contributions to atomic physics. Like his fellow Nobel laureate Lawrence, Rabi assumed a leadership role in World War II research. Rabi made contributions both to the radar project, heading a radar group that investigated methods for shortening wavelengths, and to the atomic bomb project, acting as a consultant to Los Alamos. (13)

Rabi never used accelerators in his own research. In the words of one biographer, Rabi felt physics was "too personal" to be conducted in large groups and thus avoided "Berkelitus." Nonetheless, as World War II ended he knew that Lawrence was poised to rapidly advance accelerator development at Berkeley, a move that would certainly win scientific acclaim for the Berkeley laboratory. As Rabi's former graduate student, Norman Ramsey, later admitted: "Rabi and myself . . . were rather unhappy, and I must confess, slightly jealous," of the bright prospects at Berkeley and at Chicago, where Arthur Compton, former head of the Metallurgical Laboratory, had recruited Enrico Fermi and other top physicists. Thus, when planning began in 1945 for the demoblization of MED facilities for the postwar atomic energy program, Ramsey and Rabi promoted the idea of establishing a new laboratory to accommodate the considerable scientific talent returning to universities on the East

Coast, where no MED facility had been built. Feeling that such a facility would need the support of a number of universities to compete with existing MED laboratories, East Coast scientists had already begun to rally their forces to press for a new laboratory. (14)

Before any official steps were taken on the atomic energy program, however, Lawrence took more direct action, making a special plea to MED leader Leslie R. Groves, who admired Lawrence for his wartime accomplishments. Lawrence argued that he wanted his laboratory to resume work postponed by wartime service. In particular, the 184-inch cyclotron, which had been converted before completion to help develop the electromagnetic process, required costly changes before it could be used as a basic research tool. He also asked for funds and surplus equipment to develop the two new accelerator schemes suggested by his proteges, Edwin M. McMillan and Luis W. Alvarez, who would return from wartime service to his laboratory. Support for the three machines would require a thirty-fold increase over the prewar laboratory budget. (15)

Although Lawrence intended that the three accelerators be devoted to basic research, he shrewdly realized that this was the right time to make the extravagant request. He was currently serving, along with other scientists important to the war effort, on the Scientific Panel of the Interim Committee, a panel charged with drafting recommendations for the new atomic energy program. In September 1945, just after Lawrence submitted his special request to Groves, the panel submitted a report to the General which strongly urged that basic research be supported and specifically suggested funding for accelerators. To justify its emphasis on basic research, the panel explained: "The atomic bomb derives its

energy from the splitting of the atomic nucleus, yet we do not understand the forces which hold the protons and neutrons in the nucleus together." Thus, any program lacking research into this basic problem "would have practically no foundation at all." (16)

The views of the Scientific Panel were consonant with the vision of the time. On the heels of the war, the idea of supporting accelerators through the atomic energy program found support for many reasons. Because accelerators played an important role in the atomic bomb project it was easy for leaders in government and the physics community to see them as a natural part of the nation's atomic energy arsenal and to feel confident that the public would support such a view. In addition, as Lawrence, Rabi, and other leaders were quick to see, the atomic energy program was sure to have a large budget due to postwar anxieties and hopes for the applications of nuclear energy. Although accelerators would primarily be used for basic, not applied research, the devices were bound to become more expensive as they became more powerful; thus it made sense to secure the funding of this important tool through a well-endowed agency.

Although professional self-interest was undoubtedly involved in such thinking, it is also likely that top physicists sincerely believed that basic nuclear research and research equipment were crucial for a strong atomic energy program. At a time when most agreed that a strong atomic energy program was crucial to the welfare of the nation, at least some leaders in the physics community took defense responsibilities very seriously. For example, as explained in Chapter 2, Lawrence would again deflect the energies of his laboratory from basic research to the

development of an accelerator-based research program aimed at bolstering the nation's defense in the midst of the Cold War. (17)

Another factor that encouraged support for large accelerators through the atomic energy agency was a change in attitude, at least in some circles, fostered by the wartime experience. While some in Congress focused on elitist OSRD practices, which they wished to prevent in the postwar basic research agency, the attention of military leaders focused on OSRD accomplishments, which they wished to emulate quickly by enlisting academic support, as shown by the formation of ONR in 1946. At least in the eyes of many scientists, this put them in a prime position to request funding. As Alvarez, Lawrence's protege, has remarked: "Right after the war we had a blank check from the military because we had been so successful." As Lawrence and Rabi sensed in 1945, a particularly lucrative avenue of military sponsorship was available through the MED. (18)

A change in postwar government attitudes was not the only factor that opened the possibility of federal support for accelerators. Just as important, attitudes within the physics community had changed. Although funding basic research through an atomic energy agency did not ensure the independence Bush thought so important for the basic research agency, younger leaders, such as Rabi and Lawrence, emerged from the successful, large-scale, federally sponsored radar and atomic bomb projects with new attitudes. While Lawrence, Rabi, and others of their generation had not sought large-scale government funding before the war, they were now ready to accept such funding and direct the new fleet of ambitious researchers, such as Alvarez and McMillan, who had spent their

early careers in wartime laboratories. Although Bush had played a crucial personal role in establishing the wartime projects, he retained the views of an earlier generation. Warmed by wartime successes, many younger physicists were less fearful of dependence on the federal government and more eager to grasp the opportunities made possible by government support, especially since exciting research questions had recently opened that could best be explored with expensive accelerators. (19)

In October, 1945, as a bill was framed to establish the new atomic energy agency and introduced in Congress by Kentucky Congressman Andrew J. May and Colorado Senator Edwin Johnson, Groves gave his tentative approval to Lawrence's program. Although he warned that final approval would be needed from the new agency, he immediately authorized the start of pilot studies for the new accelerators and subsequently transferred $453,000 worth of radar sets and capacitors to aid in their construction. When passage of legislation was delayed, Groves subsequently granted $170,000 to complete the 184-inch cyclotron, $203,000 for construction of an electron synchrotron, as well as additional hefty sums for operation of the laboratory. Thus, before the official mechanism for funding MED laboratories was even in place, Lawrence, with his characteristic talent for making the most of a lucrative situation, had given his laboratory a glorious head start for attacking the leading problems of postwar basic research. Although this head start did not guarantee the subsequent funding of basic research and accelerators by the new atomic energy agency, it set an important precedent. Acting on the basis of self-interest, and probably with good intentions, Lawrence had successfully exploited new opportunities in the

postwar environment to forge a conduit for funding accelerators of unparalleled expense through the atomic energy program, years before the National Science Foundation was formed. (20)

Although a step behind, East Coast physicists were mobilizing by early 1946 to fight for their share of atomic energy funding. In January, Rabi and others from the New York area met with Kenneth D. Nichols, Groves' deputy. At the meeting Nichols agreed, in principle, to the idea of a new laboratory and suggested that the group join forces with a group based in Cambridge, Massachusetts. By mid-1946, the combined East Coast group had convinced Harvard, Cornell, Columbia, Johns Hopkins, MIT, Princeton, the University of Pennsylvania, Yale, and the University of Rochester to form a consortium to support such a project. (21)

Aware of growing East Coast pressures and worried that talented scientists needed for postwar weapons work would permanently sever ties with the languishing MED laboratories, Groves took matters into his own hands when it became clear that legislation for the new atomic energy agency would not be passed in early 1946. Since he was in charge of the MED laboratories until the new agency took the reins, he appointed an Advisory Committee on Research and Development to get advice. By May, they had issued a set of recommendations. In addition to approving the suggestion of the new Eastern laboratory, they recommended incorporating facilities at Argonne Forest into a new laboratory and uniting MED facilities into a national laboratory system. By July, the group of East Coast universities incorporated in the State of New York as Associated Universities, Inc. (AUI), and a site on Long Island was

selected and approved. In September, the new laboratory was officially named Brookhaven National Laboratory (BNL) and throughout the fall MED and BNL officials debated the terms of the contract for the new laboratory. (22)

When the Atomic Energy Commission (AEC) began operation, in January 1947, it inherited a national laboratory system organized by the MED which provided physicists with plentiful resources for basic research. Although the new Commission was not committed to continue plans for basic research initiated by the wartime agency, it had compelling motivation to do so. After all, basic research projects were already underway at Berkeley and at Brookhaven. In addition, the Commission faced the task of organizing an agency devoted to atomic energy at a time when few outside the academic community knew about the energy source. Both precedent and the continuing need for the expertise of academic physicists encouraged continued support of basic research in the case of all three postwar funding agencies. Even if some in government, like Kilgore, remained skeptical of the paramount importance of basic research in the physical sciences, the only experts capable of offering crucial expertise were committed to this idea. As expensive postwar research projects blossomed amid federal attempts to develop high-powered federal science programs, the physics community and the federal government needed each other more than ever before. (23)

This mutual need was nowhere more evident than in the relationship that developed between the AEC and the physics community, where the stakes were highest on both sides. Although the physics community as a whole did not vigorously lobby on behalf of NSF legislation, even when

plans were deadlocked in Congress for three years, a scientists' lobby immediately formed to influence the formulation of the atomic energy agency. Many physicists were deeply concerned about the future use of nuclear energy, particularly nuclear weapons, and this concern would undoubtedly have found political expression, no matter how funding was arranged. The relative lack of concern about NSF, ostensibly the principal funding agency for basic research, implies, however, that the rank and file, as well as leaders such as Lawrence and Rabi, realized that the new atomic energy agency would be a more important donor than NSF, both for applied and basic science projects. (24)

The scientists' lobby opposed the May-Johnson bill, which proposed a military-dominated, politically independent commission and had the support of Lawrence as well as Bush, who suggested some of its features. The lobby, composed almost entirely of lower-ranking scientists, argued that the bill unduly emphasized military over peaceful uses of atomic energy, encouraged military domination of the commission, and allowed excessive security regulations that would interfere with research. Less concerned about political independence than Bush and more concerned about military control than Bush or Lawrence, the lobbyists joined forces with Don K. Price, James R. Newman and other members of the Truman Administration who advocated increased presidential control for the agency. Their cause was championed by Senator Brien R. McMahon, a Democrat from Connecticut who introduced alternate atomic energy legislation in early 1946. (25)

In July, 1946, it was a revised version of the McMahon bill, amended to allow some military influence, that passed in Congress to

create the AEC. The bill described a five-member civilian commission under presidential direction with wide-ranging powers to provide federal "control of the production, ownership, and use of fissionable materials . . . ." The act had specific provisions for sponsoring peaceful applications to atomic energy and for funding both pure and applied research. To allow for a measure of Congressional control, an amendment to the bill created a Joint Committee on Atomic Energy (JCAE), with five members from each House. The JCAE was charged with the task of making studies of Commission activities and "of problems relating to the development, use, and control of atomic energy." Although the relationship between the AEC and JCAE would sometimes be stormy, with personality clashes and shifts in the balance of power between the executive and congressional branches, the physics community had a new, wealthy patron with the AEC, and since JCAE power and influence derived from a healthy AEC program, the physics community had a powerful congressional ally with the JCAE . (26)

Scientists who had attained prominence in wartime projects soon gained considerable influence in the newly formed AEC. In 1947, the Commission formed the General Advisory Committee (GAC), a powerful advisory group which offered advice on major policy issues. In addition to Fermi, Rabi, and Oppenheimer, who was appointed chairman, the group included Hood Worthington of du Pont, who had helped with the plutonium production plant at Hanford, Hartley Rowe, who had been an administrator in the wartime moblization of science and an advisor at Los Alamos, Cyril S. Smith, who had led the metallurgy group at Los Alamos, and Glenn Seaborg, who had led the development of chemical separation schemes for the production plutonium plants. The group quickly moved to

extend early gains in large-scale research projects. In addition to recommendations for weapon and reactor development and radioisotope research, the GAC urged the AEC to take the lead in supporting accelerator research and recommended healthy basic research support for universities, pointing out that such support would help guarantee the future supply of engineers and scientists. To encourage the acceptance of these suggestions, and promote the particular interests of his own laboratory, Lawrence hosted a sumptuous meeting for the Commission, GAC members, and laboratory directors at Bohemian Grove near San Francisco. (27)

In October, 1947, shortly after this meeting, the new Commission agreed to enter a joint program with the Navy to support basic research. It awarded funds to partially completed university accelerator projects and also granted $15 million for new, larger accelerator projects. Although the legislation which created the AEC forbade it to issue research grants, the Commission worked around this limitation by establishing a research division which administered contracts with universities. As the Lawrence Berkeley Laboratory historians relate: "By the end of 1948 AEC research policy had been shaped to assure the future of fundamental nuclear science." (28)

The newly formed atomic energy program bound the physics community and the federal government into a new alliance that was compelling and demanding for both partners. At a time when the promise of military applications of atomic research was a prime rationale for support of basic research, the agency provided ample basic research funding without military domination. In addition, as the successes at Berkeley and

Brookhaven foretold, elitist customs, such as concentration of funding to those with established reputations, had been successfully transferred into postwar practice. At the same time, the federal government had not only gained immediate expertise, but had also provided for training the new team of experts needed for future programs in advanced weapons and nuclear power. In addition, the new agency had been incorporated directly into the federal hierarchy where it was obliged to operate, like any other public agency, in response to political pressure. The stage was set for both rapid growth and later conflict between the physics community and its new patron.

Growth was rapid. By 1950, basic research projects, including increasingly powerful and expensive accelerators, flourished in the new AEC national laboratory system. This system was in turn augmented by funding and additional equipment awarded to universities by ONR, NSF, and other federal sources. These new, rich resources made "big physics" possible. Before World War II, most leading experiments were conducted in university laboratories. Now the national laboratories were best-suited for supporting the expensive accelerators and related equipment necessary for the most ambitious experiments, such as the search for mesotrons. As the equipment in the accelerator laboratories got larger, research teams increased in size and specialization. A large percentage of physics funding, both for professorships and grants, was channeled through universities, where physics departments trained students and provided a large percentage of the necessary staff. Since universities greatly outnumbered national laboratories, the situation encouraged collaboration, a trend already obvious in the formation of Brookhaven. (29)

Although the physics community obtained unprecedented benefits from the postwar funding structure, it also incurred new obligations. Whereas most basic physics research was privately funded before World War II, federal funding dominated after the war. In addition, despite the original intention that the NSF be the major donor of physical research, AEC and ONR funding dwarfed the NSF contribution. While the AEC spent $26.1 million and the ONR spent $29 million for physical research in 1949, the NSF began in 1950 with an appropriation of only $350,000 and the limitation that total yearly budgets could not exceed $15 million. Although few strings were attached to ONR and AEC funding immediately after the war, both agencies were dedicated to achieving practical results. In the absence of a strong agency dedicated to the support of basic research for its own sake, the parameters defined by the new alliance between the AEC and the physics community applied to postwar basic research as a whole. (30)

Summary: How the Postwar Funding Structure Evolved

After successful wartime collaboration, the U.S. Government was more willing to support physical research and the physics community was more eager to accept such support than ever before. As a result, conduits of funding were established through the Office of Naval Research, the National Science Foundation, and the Atomic Energy Commission. The AEC became the sponsor of expensive accelerator projects because accelerators were seen as a natural part of the nation's atomic energy arsenal after the war, large amounts of money were needed to fund new accelerator schemes, and the agency had a large budget due to the fears and hopes inspired by the new energy source.

With this arrangement the physics community gained ample funding and remained free from military domination while the federal government gained immediate expertise and the means for training future experts in advanced weapons and nuclear power. These compelling benefits wed both partners to the new alliance without resolving underlying conflicts between the elitist customs of the physics community, which encouraged merit through concentration of funding, and democratic political practices in the United States, which favored equal distribution of resources.

The Lure of Elementary Particles

Once released from wartime projects, the nation's top physicists were eager to continue prewar research. In the decades before the war, physics research had focused on several productive lines of inquiry. Attempts to extend the knowledge afforded by quantum mechanics, formulate a coherent picture of the atomic nucleus, and explore the nature of cosmic rays all offered clues about the fundamental nature of matter.

Views on elementary particles had undergone rapid transformation. The tidy picture held by most scientists in 1930 that matter was composed of two particles, electrons and protons, was quickly abolished. In 1930, Wolfgang Pauli postulated the existence of the neutrino. The 1932 discovery by James Chadwick of another neutral particle, the neutron, brought the conclusion that the electron existed only outside the nucleus. In 1932, Carl D. Anderson discovered the positron in cosmic rays; Irene Curie and Frederic Joliot subsequently showed that artificially produced radioactive light elements decayed by emitting these particles. By this time several types of accelerators had been devised to explore subatomic matter. (31)

After the successful formulation of quantum mechanics by Werner Heisenberg and Erwin Schroedinger in 1925 and 1926, theorists were eager to extend this powerful theory to develop a quantum mechanical field theory for charged particle interactions. This theory, called quantum electrodynamics (QED), was initiated by Paul A. M. Dirac, who also developed a relativistic theory of the electron. (See Quantum Field Theory) One product of this effort was the growing speculation that

nature contained a new kind of matter, called antimatter, and that each particle had an antimatter counterpart. Such speculation was reinforced with Anderson's discovery of the positron. (32)

Physicists could prove the existence of antimatter by finding the antiproton. Given a choice, physicists would have used an accelerator rather than cosmic rays because an accelerator beam is much more intense than a cosmic-ray shower and thus produces many more events for observation. In addition, physicists have more control over the experimental environment with an accelerator since they know the precise composition of the beam, projectile energies, and the conditions of collision; thus many measurements can be made more accurately. Unfortunately for those interested in confirming antimatter, the antiproton search required an energy in the 6 GeV range which was about a hundred times higher than the energies produced with existing machines. Since such energy ranges seemed out of reach in the short-term future, hopes for confirming antimatter hinged on the unlikely possibility of another lucky cosmic-ray find. (33; See Electron Volt)

Cosmic-ray results soon provided another more immediate goal for accelerator development. By 1937, physicists had discovered both positively and negatively charged particles that were 200 times more massive than the electron. It was now clear that yet another sub-atomic particle had been discovered and it came to be called the mesotron. Some identified the new particle as the particle carrying the nuclear force, as predicted by Hideki Yukawa in a fundamental theory of 1935 aimed at extending Fermi's theory of nuclear beta decay to include an

explanation of the strength and range of the nuclear forces. When researchers investigated further, they found that measured properties of the cosmic-ray mesotron, other than mass, did not match those postulated for the Yukawa particle. Since mesotrons could be created at 200 MeV, only ten times more than the energies produced with existing machines, physicists were eager to develop a 200 MeV accelerator to explore a variety of theoretical speculations, including the suggestion that the particle predicted by Yukawa and the mesotron were two different particles. As explained in the next section, this task required a breakthough in accelerator technology. (34; See Beta Decay)

In the meantime, prewar attempts to calculate QED were plagued by the discovery that certain crucial values, such as the self-mass of the electron, were infinite, indicating that the theory had internal contradictions. Nevertheless, by 1937, QED had proven to be quite useful for explaining the nature of the cosmic rays. Attempts to unravel the mesotron mystery in turn stimulated new theories. Some physicists wanted to develop a meson theory to explain the particle's behavior, while others wanted to develop a version of the theory that would accommodate nuclear forces. Since these theories had even more difficulties with infinities than QED, the desire to vanquish this scourge grew more compelling. (35)

By the early 1940s, physicists had come a long way in the understanding of the fundamental nature of matter. Just as the disruption and demands of WWII intervened, a particularly intriguing set of experimental findings and theoretical problems emerged from the concerted efforts of quantum theorists, and nuclear and cosmic-ray

physicists. Attention focused on two interrelated problems. Physicists wanted to understand the mesotron, which promised to provide information both about the atomic nucleus and about cosmic rays, and to provide a coherent theoretical explanation of known phenomena consistent with quantum mechanics, an effort which required a means to banish infinities with the formulation of a fully self-consistent version of QED. After the war, these intriguing problems attracted more than their share of attention for the nation's top physicists.

As many top nuclear physicists focused their attention on accelerator building, a series of crucial results emerged from cosmic-ray experiments. (See Cosmic-Ray Experiments) Pursuing the productive prewar techniques of cosmic-ray studies, Marcello Conversi, Ettore Pancini, and Oreste Piccioni used an improved array of Geiger-Mueller counters to measure the decay of the mesotron and found that its rate of capture by atomic nuclei was far slower than theory allowed. This result gave credence to the suggestion that the mesotron was not the particle predicted by Yukawa. This suggestion was confirmed, in early 1947, by another cosmic-ray group led by Cecil F. Powell. Using a new, more concentrated emulsion placed at high altitude in the Bolivian Andes, this group observed one charged particle give birth to another. It was later determined that the first particle was the Yukon (now called the pion) and the second was the mesotron (now called the muon). (36)

By this time, more interesting events were emerging from cosmic-ray studies. Taking advantage of such wartime advances as the introduction of flash tube lighting, George D. Rochester and Clifford C. Butler

devised an improved counter-controlled cloud chamber in Manchester, England. With the new chamber they subsequently observed two events, while scanning the many stereoscopic photographs taken in 1946 and 1947, which produced "forked tracks of a very striking character." Rochester and Butler argued that these tracks could not have been caused by a collision process since no similar events occurred in the chamber's 3-cm lead plate where collision would be much more likely to occur. The tracks, they maintained, were due to a spontaneous process which depended on the distance traversed instead of the quantity of matter penetrated. They concluded that the tracks in each case were caused by "the spontaneous transformation in the gas of the chamber of a new type of uncharged elementary particle into two lighter charged particles." Although Rochester and Butler tried to duplicate their results, the events, which came to be called V particles, were not sighted again in Manchester for three years. (37)

While Rochester and Butler waited in uneasy suspense, in 1949 an emulsion group led by R.M. Brown at Bristol, England found another unusual event using an improved emulsion technique. They labeled it the tau plus. The next year, a group led by Anderson spotted 33 V particles after operating a cloud chamber both at sea-level and on White Mountain, California and confirmed the "remarkable conclusion" offered by Rochester and Butler: the cloud chamber tracks indeed represented "the spontaneous decay of neutral and charged unstable particles of a new type." When the V, tau and other unusual events continued to appear in bubble chambers and emulsions on both continents, excitement grew among both cosmic-ray experimenters and many nuclear physicists, since the new phenomena fell within the energy range of accelerators already under

construction. At the same time, riveting theoretical results monopolized the attention of many physicists. (38)

While the mysterious collection of cosmic-ray events grew, theorists achieved a long-awaited breakthrough. The excitement began in 1947. Enlivened by a spirit of camaraderie and self-confidence inspired by the wartime collaboration of many of the participants, a group of eminent physicists convened at Shelter Island, New York. In line with the traditional empirical approach of American physics, experimental data was the center of attention, although the meeting drew the nation's top theorists. Participants were particularly interested in a presentation by Willis E. Lamb, who described a series of experiments performed at Columbia University, using new microwave technology available after the war. In these experiments, Lamb and his co-worker Robert C. Retherford successfully measured a shift, now called the Lamb shift, in electron energy levels in the hydrogen atom. Although such a shift had been predicted earlier, previous theoretical methods had proved inadequate for calculating it. The reliable, precise new measurement, and additional experimental evidence on the structure of deuterium and hydrogen presented at the Conference by Rabi, inspired a flurry of ideas for new theoretical methods. (39)

On the heels of the first calculation of the Lamb shift by Hans A. Bethe in 1947, Julian S. Schwinger and Richard P. Feynman each produced a general theory of QED based on earlier work by Hendrick A. Kramers and others. Although it first seemed that Feynman and Schwinger had created different theories, Freeman Dyson demonstrated in 1949 that the two schemes were equivalent and further extended the range of applicability

of their calculations. (The next year American physicists learned that some of this work had been independently conceived by Sin-itiro Tomonaga in Tokyo, in 1943.) To avoid the weaknesses of prewar QED, theorists had found a way to absorb damaging infinities into physical constants, such as electron mass and charge. An additional theoretical tool was contributed by Feynman, who devised a graphical representation of QED expressions, called Feynman diagrams, which provided a potent language for both qualitative and quantitative calculations. (40; see Feynman Diagrams)

Facilitated by both wartime technological innovation and the American empirical tradition, the development of QED was, as historian Silvan Schweber explains, "a great American success story." In turn, the spirit of self-confident empiricism evident in the development of QED set the pace for further theoretical development. Armed with potent new methods, a tested tradition, and the thrill of success, theoreticians eagerly made plans to apply field theory to the strong and weak interactions. (41; See Fundamental Interactions)

Summary: What Stimulated Interest in Elementary Particles

The intellectual rewards of extending quantum mechanics and investigating nuclear physics and cosmic rays escalated sharply in the years following World War II. As the 1950s began, the surge of theoretical activity evident in the development of QED merged with the excitement of pion and muon discoveries and a growing sense of intrigue about the unexplained cosmic-ray data. Interest in the fundamental particles was stronger than ever before.

The First Postwar Accelerators

While interest in fundamental particles grew, top nuclear physicists worked to design more powerful accelerators to facilitate further exploration. In this endeavor, physicists at the Radiation Laboratory and at Brookhaven clearly had an advantage because of the considerable resources that could be commanded at both laboratories as part of the legacy of World War II. Because of the advance funding obtained by Lawrence, Alvarez and McMillan were the first to begin work on new accelerator ideas; Brookhaven physicists quickly sensed the challenge and soon became competitors in the race for higher energies.

Alvarez and McMillan both began designs for new accelerators in the summer of 1945, before they left the bomb project at Los Alamos. Alvarez first hoped to modify improved radar technology to create a new, more powerful type of power supply for an electron linear accelerator (linac). (42; See Linear Accelerators) However, he abandoned this scheme after hearing one of McMillan's ideas. As McMillan recently remembered, he was "lying in bed thinking about the problem of getting

high-energy particle. . . . " The problem was that acceleration in a cyclotron is limited because mass increases caused when particles approach the speed of light eventually detune such particles from the resonance needed for continued acceleration. (See Resonance Acceleration). McMillan recalls that to find a way around this problem, he "was tracing out in my imagination the motion [of a particle] as it unfolded in time. . . ." Suddenly he felt "like the inventor in a cartoon with a lightbulb flashing on over his head." McMillan had independently hit upon the concept of phase stability, an idea suggested earlier by Vladimir Veksler of the Soviet Union. As he explained in a 1945 report sent to Lawrence, particles could be accelerated to higher energies with a machine "in which the phase of the particles with respect to the high frequency electric field automatically adjusts itself to the proper value for acceleration." In such a machine, now called a synchrocyclotron, particles could be accelerated to energies far beyond the capability of cyclotrons. (43; See Phase Stability)

McMillan discussed the scheme with Lawrence, who came to New Mexico for the first atomic weapon test in mid-July. Some researchers, including Rabi, were worried about the effect of excessive radiation losses, which were later observed in electron synchrotrons and called synchrotron radiation. Nonetheless, the MED support obtained by Lawrence allowed researchers at Lawrence's laboratory to begin several projects. McMillan persuaded Lawrence, who had hoped to pierce the relativistic barrier with brute force using the large cyclotron, to use MED funds to convert the 184-inch cyclotron into a synchrocyclotron. He also received permission to build a 300 MeV electron synchrotron, an accelerator which varied both the magnetic field and frequency to keep

particles in phase, thus initiating a productive new line of accelerators. While McMillan was working on these projects, Alvarez began work on another linear accelerator, gathering a team that included Wolfgang (Pief) Panofsky, a German emigre who had worked with Alvarez at Los Alamos. This accelerator, which accelerated protons to 32 MeV in 1946, was the first high-energy proton linear accelerator and became the model for subsequent proton linear accelerators. (44; See Synchrotrons and Synchrotron Radiation)

In the meantime, synchronous accelerators were also showing quick success. In November, 1946 the newly converted synchrocyclotron accelerated deuterons to 200 MeV. While McMillan continued work on his 300 MeV machine, Frank G. Goward and J. E. Barnes at the Woolwich Arsenal in England completed the first synchrotron, an 8 MeV electron machine, in 1946. The next year a 70 MeV electron synchrotron was built by H. C. Pollack at the General Electric Co. Research Laboratory. Due to the largess of the AEC and ONR, in the next few years synchrocyclotrons were built at Harvard, Columbia, Chicago, Rochester and the Carnegie Institute of Technology and synchrotrons were built at the University of Michigan, Purdue, Cornell, and MIT. The national fleet of accelerators also boasted the University of Illinois Betatron and a number of smaller machines, mostly cyclotrons, at universities throughout the country. (45; See Betatron)

In addition to the stimulus provided by a favorable funding environment and the successful application of phase stability principles, accelerator building received a considerable boost when mesotron research, which had been used to justify the need for larger

machines, compounded the mystery of elementary particles by yielding not one but two new particles. Although cosmic-ray researchers identified the pion and muon before suitable detectors could be devised for the Berkeley synchrocyclotron, after the discovery Berkeley physicists were in a highly enviable position since estimates placed pion production within the energy range of their machine. If they devised a way to make man-made pions, which would decay into muons, Berkeley researchers would have a unique tool for detailed exploration of both muons and pions (collectively called mesons). Many were skeptical of their chances, but they struggled to meet the challenge. (46)

By 1946, three particularly ambitious schemes were taking shape to reach even higher energies. After an accelerator design course for engineers given in the summer of 1946, William Brobeck conceived a 10 GeV proton machine which he hoped to build at Berkeley. When Rabi saw the design, he sent M. Stanley Livingston, Lawrence's former student, on an assignment, determined to secure the best possible scientific reputation for Brookhaven. As Livingston recalls, he was instructed to: "Find the highest high-energy energy machine that can be built." After a trip to Berkeley, Livingston began plans for a synchronous machine on Long Island. Marcus Oliphant, who had visited Berkeley during the war and learned about phase stability, had already begun plans for a 1.3 GeV machine to be built in Birmingham, England, where preliminary plans for a machine with a ring magnet had been made in 1943. The race for higher energies was on, with Berkeley at the lead, and others close behind. (47)

Although estimates placed the mass of the muon within range of the Berkeley synchrocyclotron, skeptics still questioned its ability to produce the more massive pion. Spurred by the desire to secure an even higher energy accelerator for Berkeley, especially in light of the imminent competition of the Birmingham machine scheduled to begin operation in late 1949, Lawrence was determined to maintain Berkeley's lead in accelerator development to ensure that Berkeley would reap future elementary particle discoveries. By fall, 1947, he was shrewdly planning his next accelerator request. Having already abandoned plans to continue development of Alvarez's linear accelerator, since synchrotrons seemed a more promising route to higher energies, he also decided to reduce the maximum energy of the proton synchrotron, figuring the AEC would balk at the cost of a 10 GeV machine. First, he considered a maximum energy of 5 GeV, then settled on 6 GeV at the suggestion of McMillan and Panofsky, who argued that at this energy, the machine could produce antiprotons. (48)

Despite the self-imposed reduction in energy, Lawrence encountered resistance when he submitted the $9.6 million request in November, 1947. GAC members expressed concern that the request might be premature since lower energy machines were not yet complete, and wondered if it was wise to fund such a large project at a university facility. In light of these concerns, Rabi was able to stall the GAC recommendation. (49)

In the months to follow, Berkeley and Brookhaven debated competing ideas for machines in the 2-6 GeV range. As revealed in the minutes of a February 1948 meeting, the GAC had been persuaded by that time to endorse "the policy of extending the range of particle accelerators into

the multi-billion volt range," although members could not agree on how to implement this policy. Rabi promoted Brookhaven's interests and Berkeley chemist Glenn Seaborg promoted Berkeley's interests; Fermi argued that only one of the expensive new tools was needed, but worried that morale would suffer at the unsuccessful laboratory if only one accelerator were funded. Eventually, the group recommended "that two machines aimed at substantially different maximum energies should be built and that the energies and locations should be determined as the result of consultation between the laboratories" and AEC representatives. Although this recommendation was "mutually acceptable to both laboratories," as noted in a 1948 Brookhaven progress report, the important question of who would get the higher energy machine would have to be ironed out within the scientific community. (50)

Just after the GAC released its February recommendations, Berkeley physicists announced that the 184-inch synchrocyclotron, upgraded to produce alpha particles at 380 MeV, had successfully produced pions, which they had detected using a newly perfected emulsion technique. Despite this news, Brookhaven researchers opted to build the 2.5 GeV machine, insisting that the proposal should not prejudice their case to later build an additional machine in the 10 GeV range. At this point Brookhaven saw several advantages to building the lower energy machine. It could be built more quickly and provided the energy necessary for more detailed meson studies, including pion pair production, which would permit further study of nuclear forces. In addition, Brookhaven would have the satisfaction of defending the United States lead over the British rival at Birmingham. (51)

Lawrence boldly grasped this opportunity to extend the energy range for the Berkeley machine. In a March, 1948 proposal he requested a 6-7 GeV machine. In April, the AEC approved both machines. In the final settlement it was agreed that Brookhaven would build a 3 GeV machine, while Berkeley would build a 6-7 GeV machine. (52)

As this was being settled, the impetus for further exploration with the new machines was intensified by the scientific accomplishments of the first synchronous machines. Shortly after achieving the first man-made pions, Berkeley researchers accurately measured the masses of both the pion and the muon with the synchrocyclotron. Later in 1948, the electron synchrotron reached energies of 335 MeV. With these machines researchers conducted a number of important experiments, including the production of the neutral pion, the first elementary particle discovered in an accelerator. (53)

The First Postwar Accelerators and How They Set the Pace for the Future

At just the time when the new alliance with government made it possible to finance the next generation of accelerators, the discovery of phase stability made it technically possible to design a powerful new line of accelerators. As the first synchrotrons were being built in 1946, particularly ambitious accelerator schemes were devised at Birmingham, Berkeley and Brookhaven and fierce rivalry subsequently developed when the two American laboratories simultaneously requested funding from the AEC. The resulting compromise helped America to take the lead over Britain in the race to higher energies, provided for major machines at both BNL and the Radiation Laboratory, and paved the way for

future requests for machines of even higher energy. With the excitement of the first round of accelerator-based meson studies at Berkeley, the edge of competition could only become sharper.

Conclusion: Physics After World War II--1945-1950

In the five years after World War II patterns had been set that would direct the future course of physics research into the fundamental nature of matter. From the fruitful wartime collaboration of scientists and the federal government arose a new alliance between the AEC and the physics community which bound both partners with mutual need and obligation. Government leaders had immediate access to expertise and the means for educating the experts needed for the future. At the same time, physicists had at their disposal new technologies, facilities, and funding at a time when the possibilities and puzzles of the fundamental nature of matter were more compelling than ever before and accelerator advances and accomplishments spurred the desire for ever larger and more expensive machines to facilitate further exploration.

New patterns had also been set within the physics community. As the first round of high energy accelerator proposals indicated, the United States, with superior resources and a lion's share of scientific talent after wartime emigration, was positioned to capture the international lead in physics done at high energies. Within the United States, Brookhaven and Berkeley were squared off as the chief American competitors for the increasingly prestigious research bounty afforded by the most powerful machine. The inevitable disputes in this high stakes game would be mediated within the scientific community, under the

watchful eye of the AEC. With the 1950s, the physics community faced glorious new possibilities as well as new dangers.

Notes for Chapter 1

1. Abraham Pais, Inward Bound: Of Matter and Forces in the Physical World, (Oxford: Oxford University Press, 1986), p. 450.

2. Quotes, respectively, from Pais, Inward Bound, p. 450 and Andrew Pickering, Constructing Quarks: The Road to Unity in High-Energy Physics, Informal Report, p. 51.

3. For an account of a post-World War I attempt to establish federal support for science see Lewis Auerbach, "Scientists in the New Deal: A Pre-War Episode in the Relations Between Science and Government in the United States, Minerva, 111 (1965) pp. 457-482 and Ronald Tobey, The American Ideology of National Science, (Pittsburg: Pittsburg University Press, 1971). Text references: Daniel Kevles, The Physicists: The History of a Scientific Community in Modern America (New York: Alfred A. Knopf, 1978) p. 341.

4. M. Stanley Livingston, Particle Accelerators: A Brief History (Cambridge: Harvard University Press, 1969) p. 39 and L. I. Schiff, "Production of Particle Energies Beyond 200 MeV," Review of Scientific Instruments 17 (1946), p. 6.

5. For a more detailed account of the formation of the Atomic Energy Commission, see Richard Hewlett and Oscar Anderson, Jr., The New World, 1939/1946: A History of the United States Atomic Energy Commission, Vol. I (University Park: Pennsylvania State University Press, 1962). For a more detailed description of the formation of the National Science Foundation, see J. Merton England, A Patron for Pure Science: The National Science Foundation's Formative Years 1945-57 (Washington D.C.: National Science Foundation, 1982) and Kevles, pp. 356-366. Text quote from Kevles, p. 334. Also see James L. Penick, Jr., Carroll W. Pursell, Jr., Morgan Shwerwood, Donald Swain, eds., The Politics of American Science, 1939 to Present, (Cambridge: MIT Press, 1972) p. 14.

6. Throughout this document, the term "basic" research is used to describe research aimed at advancing the conceptual understanding of science. The term "applied" research is used to describe research aimed at solving technological problems. Text references: Kevles, p. 354; Laurie Brown, Max Dresden, Lillian Hoddeson, "Introduction," Pions to Quarks: History of Particle Physics in the 1950s eds. Brown, Dresden, and Hoddeson to be published by Cambridge University Press, 1988, p. 20; and Harvey M. Sapolsky, "The Origins of Office of Naval Research," a presentation to the Sixth Naval History Symposium, Annapolis, Maryland, September, 1983, pp. 2, 27. The author wishes to thank Paul Henriksen for supplying a copy of this manuscript.

7. For discussions of the tradition of concentration of funding and the development of center of excellence as part of the flowering of the American physics community, see Kevles, pp. 185-199; Stanley Coben, "The Scientific Establishment and the Transmission of Quantum Mechanics

in the United States, 1919-32, American Historical Review 76 (1971) 442-66; 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.: Smithsonian Institution Press, 1979), pp. 295-358; and Katherine Sopka, Quantum Physics in America: 1920-1935 (New York: Arno Press, 1980), pp. 2.2-2.26. Text references: Kevles, pp. 342-344 and England, p. 5.

8. For information on the Science Advisory Board, see Lewis Auerbach, "Scientists in the New Deal: A Pre-War Episode in the Relations Between Science and Government in the United States, Minerva, 111 (1965) pp. 457-82. Text reference: Kevles, pp. 293-295.

9. Quote as quoted in Kevles, p. 347. Also see Kevles, p. 293 and England, p. 22.

10. Kevles, p. 356; England, p. 26.

11. Although Kevles and others, including Allan Waterman, the first NSF director, judge that the NSF largely fulfilled the Bush's expectations, Nathan Reingold disagrees. For an alternate interpretation of Bush and Science, the Endless Frontier, see Reingold, "Vannevar Bush's New Deal for Research: Or The Triumph of the Old Order," Historical Studies in the Physical and Biological Science, 17 (1987), pp. 300-344. Text reference: Kevles, pp. 357-358, 362.

12. Quote as quoted in Herbert Childs, An American Genius: The Life of Ernest Orlando Lawrence, (New York: E. P. Dutton & Co., Inc., 1968), p. 23. (This source provides a thorough, if somewhat uncritical, account of Lawrence's life.) Also see Kevles pp. 227-231, Hewlett and Anderson, p. 75, and John L. Heilbron, Robert W. Seidel, and Bruce R. Wheaton, Lawrence and His Laboratory: Nuclear Science at Berkeley, 1931-1961 (Berkeley: Office for History of Science and Technology, 1981), p. 11. (The latter source gives an excellent account of the early history of Lawrence Radiation Laboratory.)

13. John S. Rigden, Rabi: Scientist and Citizen, (New York: Basic Books, Inc., 1987) pp. 60-62, 136-138 and Kevles, p. 213-215. (Rigden provides a thoughtful account of Rabi's life.)

14. Quotes, respectively, from Ridgen, p. 190 and as quoted in Ridgen, p. 183. Also see Alan Needell, "Nuclear Reactors and the Founding of Brookhaven National Laboratory," Historical Studies in the Physics Sciences, 14, Part 1, 96-97. This source gives an excellent account of the early history of the East Coast Laboratory, later named Brookhaven National Laboratory. Additional information and personal accounts can be gained from Norman Ramsey, "Early History of Associated Universities and Brookhaven National Laboratory" (Brookhaven: Brookhaven National Laboratory, 1966) and Brookhaven National Laboratory Associated Universities, Inc., "The Founding of the Brookhaven National Laboratory by Associated Universities, Inc." (Brookhaven: Brookhaven National Laboratory, 1948).

15. Luis Alvarez, "A Proposal for Accelerating Electrons to Very High Energies," AM-2290, Los Alamos National Laboratory Archives, Los Alamos, New Mexico; Robert W. Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," Historical Studies in the Physical Sciences, 13, Part 2, p. 382; Heilbron, Seidel, and Wheaton, p. 47; and Robert Seidel in "The Postwar Political Economy of High Energy Physics," in Brown, Dresden, Hoddeson, eds., p. 4.

16. Quote as quoted in Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," p. 382.

17. Heilbron, Seidel, and Wheaton, pp. 62-65, 72-75.

18. Pais, Inward Bound, p. 19.

19. Kevles, p. 363 and interview with Laurie Brown, August 28, 1986.

20. Hewlett and Anderson, p. 628; Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," pp. 383, 389, 392.

21. Needell, pp. 97-98.

22. Hewlett and Anderson, pp. 633, 634; Needell, pp. 99, 105; Norman Ramsey, "Early History of Associated Universities and Brookhaven National Laboratory," p. 7.

23. Needell, pp. 106-107.

24. Kevles, pp. 350, 363.

25. Kevles, pp. 350-351. Alice Kimball Smith, A Peril and a Hope: The Scientists' Movement in America, 1945-1947, p. 130. Smith's book provides a good look at the views of scientists on the domestic control of atomic energy.

26. Quotes, respectively, from Hewlett and Anderson, Appendix 1, p, 714 and as quoted in Daniel S. Greenberg, The Politics of Pure Science (New York: The New American Library, 1967), p. 231. Also see Kevles, pp. 351-352; and Hewlett and Anderson, p. 507.

27. Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," pp. 377, 393; Hewlett and Duncan, Atomic Shielf, 1947-1952: A History of the United States Atomic Energy Commission, Vol. II (University Park: The Pennsylvania State University Press, 1969), pp. 15-16; Kevles, p. 377; and Heilbron, Seidel, and Wheaton, p. 49.

28. Quote from Heilbron, Seidel, and Wheaton, pp. 49, 48-49. Also see Hewlett and Duncan, p. 246.

29. Brown, Dresden, and Hoddeson, eds. "Introduction," p. 18; and 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.: Smithsonian Institution Press, 1979), p. 329.

30. Kevles, pp. 358-359, 364; Sylvan Schweber, "Some Reflections on the History of Particle Physics in the 1950s," in Brown, Dresden, and Hoddeson, eds.; and Hewlett and Duncan, Appendix 7.

31. Laurie Brown and Lillian Hoddeson, eds. The Birth of Particle Physics, (Cambridge: Cambridge University Press, 1983), pp. 5-6; and Edward Lofgren, "Particle Accelerators," in Robert M. Besancon, ed. The Encyclopedia of Physics, (New York: Van Nostrand Reinhold Company, 1985).

32. Brown and Hoddeson, eds. pp. 10-12; and Steven Weinberg, The Discovery of Subatomic Particles, (New York: Scientific American Library, 1983), p. 162.

33. When physicists contemplate the next step in accelerator development they consider a number of factors, principally the energy needed for particular experiments, technical feasibility, and cost. While such decisions may vary, due to individual differences of opinion and ability to obtain funding, at any given time there is a professionally accepted energy limit for the next step to higher energy. In the 1930s the next accepted step was from tens of MeV to 200 MeV. Despite the interest in searching for the antiproton, the jump to 6 GeV was considered much too big at the time. Text reference: M. Stanley Livingston, Particle Accelerators: A Brief History, p. 39.

34. Brown and Hoddeson, eds., p. 17; Heilbron, Seidel, and Wheaton, p. 53; Peter Galison, "Discovery of the Muon and the Failed Revolution Against Quantum Electrodynamics," Centaurus, 26 (1983), p. 309; and Pais, Inward Bound, p. 488.

35. "Introduction," Brown and Hoddeson, eds., p. 16, pp. 12-18 and private communication, Robert Walker, July 3, 1987.

36. A good historical account of the discovery of the muon is Peter Galison, "The Discovery of the Muon and the Failed Revolution Against Quantum Electrodynamics," Centaurus, 26 (1983) pp. 282. Oreste Piccioni, "The Observation of the Leptonic Nature of the 'Mesotron' by Conversi, Pancini, and Piccioni," in Brown and Hoddeson, eds. pp. 222-229; Heilbron, Seidel, and Wheaton, p. 53; Robert E. Marshak, "The Multiplicity of Particles," Scientific American, 186 (1952), p. 25; C. M. G. Lattes, "My Work in Meson Physics With Nuclear Emulsions," in Brown and Hoddeson, eds. pp. 307-308; and private communication, Robert Walker, July 3, 1987.

37. Quote from George D. Rochester and Clifford C. Butler, "Evidence for the Existence of New Unstable Elementary Particles," Nature, 160 (1947), p. 855. Also see George D. Rochester, "Cosmic Ray Cloud Chamber Contributions to the Discovery of Strange Particles in the Decade 1947-1957," in Brown, Dresden, and Hoddeson, eds., pp. 6-7.

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38. Quote from A. J. Seriff, R. B. Leighton, C. Hsio, E. W. Cowan and Carl. D. Anderson, Physical Review 78 (1950), 290. Also see Luis W. Alvarez, "Recent Developments in Particle Physics," Evolution of Particle Physics, (Stockholm: Nobel Foundation, 1969), p. 4 and A. Pais, "From the 1940s into the 1950s," in Brown, Dresden, Hoddeson, eds. p. 6.

39. For discussion of American empirical tradition see Gerald Holton, "'Success Sanctifies the Means': Heisenberg, Oppenheimer and the Transition to Modern Physics," in Everett Mendelsohn ed., Transformation and Tradition in the Sciences: Essay in Honor of I. Bernard Cohen, (Cambridge: Cambridge University Press, 1984), pp. 164, 168-169; Sopka, p. 3.68; Sylvan Schweber, "Some Reflections on the History of Particle Physics in the 1950s,"in Brown, Dresden, Hoddeson, eds. p. 34; Pais, Inward Bound, pp. 450-451; and Jeremy Bernstein, Hans Bethe: Prophet of Energy, (New York: The New Yorker Magazine, Inc., 1979,) p. 90.

40. Pickering explains that to find a way around the infinities in QED, physicists first noted that "only a small number of distinct types of infinities occur. These can be understood as infinite contributions to the electron mass, charge and so on. If one puts up with this and, at the end of the calculation, sets the apparently infinite mass and charge of the electron equal to their measured values, one arrives at perfectly sensible results." Andrew Pickering, Constructing Quarks: A Sociological History of Particle Physics, (Chicago: Chicago University Press, 1984) pp. 66-67. Also see Pais, Inward Bound, pp. 455-466. Pais gives a more detailed account of the development of QED. For a thorough historical description of the period, see Sylvan S. Schweber, "Shelter Island, Pocono and Oldstone: The Emergence of American Quantum Electrodynamics after World War II, Osiris, Vol. 2 (1986). Text references: Bernstein, p. 90; Freeman Dyson, Disturbing the Universe, (New York: Harper and Row Publishers, 1979), pp. 55-57; Steven Weinberg, "The Search for Unity: Notes for a History of Quantum Field Theory," Daedalus, Fall 1977, pp. 28-29.

41. Schweber, "Some Reflections on the History of Particle Physics in the 1950s," in Brown, Dresden, Hoddeson, eds., p. 33.

42. Luis Alvarez, Adventures of a Physicist, (New York: Basic Books, Inc., 1987) p. 154; Alvarez, "A Proposal for Accelerating Electrons to Very High Energies" AM-2290, Los Alamos National Laboratory Archives, Los Alamos, New Mexico.

43. Quotes, respectively, from Edwin McMillan, "A History of the Synchrotron," Physics Today, 37 (1984), p. 31 and Edwin McMillan, "The Synchrotron," Enclosure to McMillan to Ernest Lawrence, July 4, 1945, Green Papers, Box III, "Early Accelerator History," BNL. Also see M. Stanley Livingston, Particle Accelerators: A Brief History, pp. 18, 39 and Edward J. Lofgren, "Accelerator, Particle," in Robert M. Besancon ed., The Encyclopedia of Physics, (New York: Van Nostrand Reinhold Company, 1985), p. 17. Livingston gives a good overview of the subject; for more detailed information, see M. Stanley Livingston and John P. Blewett, Particle Accelerators (New York: McGraw Hill, 1962).

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46. Heilbron, Seidel, and Wheaton, p. 53 and Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," p. 396.

47. Quote as quoted in John S. Rigden, Rabi: Scientist and Citizen, p. 185. Also see Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," pp. 392-393; and Livingston, Particle Accelerators: A Brief History, p. 50.

48. Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," pp. 394-396; Hewlett and Duncan, p. 234; and Charles Weiner, interview with Edwin McMillan, October 31, 1972, p. 270, Bancroft Library, University of California, Berkeley, California.

49. Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," p. 394. For more detailed accounts of accelerator developments at Brookhaven in the 1950s from the point of view of a participant, see John Blewett, "Accelerator Design and Construction in the 1950s," and Ernest Courant, "Early History of the Cosmotron and AGS at Brookhaven," in Brown, Dresden, Hoddeson, eds.

50. Quotes, respectively, from General Advisory Committee, "Draft Minutes, Eigth Meeting of the General Advisory Committee to the Atomic Energy Commission, February 6-8, 1948," p. 18 and Appendix F, DOE Archives, Secretariat, Box 1217 and Brookhaven National Laboratory, "Scientific Progress Report, January - July, 1948", (Brookhaven: Brookhaven National Laboratory, 1948), p. 16, FHC. (The author wishes to thank Roger Anders for supplying the GAC document.) Also see "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," pp. 395-396 and Hewlett and Duncan, p. 250.

51. Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," pp. 396-397; Heilbron, Seidel, and Wheaton, p. 61; and Courant, "Early History of the Cosmotron and AGS at Brookhaven," in Brown, Dresden, and Hoddeson, eds., p. 3.

52. Seidel, "Accelerating Science: The Postwar Transformation of the Lawrence Radiation Laboratory," p. 397 and Hewlett and Duncan, p. 250.

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Chapter 2: Building Bigger and Stronger--1951-1959

Introduction

At the close of the decade that spanned the tensions of the Oppenheimer hearing and the triumphant crowning of particle physics as a highly prestigious and distinct subfield, Feynman exclaimed to an international audience: "we live in an heroic, a unique and wonderful age of excitement, an exciting age of very vital development in fundamental physics and the study of fundamental law." Attention focused on the population explosion of elementary particles and the powerful new tools designed for their exploration. In this propitious era, large accelerator projects flourished. While the largest accelerators built at the beginning of the decade cost less than $10 million, by the end of the decade several accelerators were proposed which would cost over $100 million apiece. As Rabi later remarked, physicists of the new generation "were fortunate to be the grandchildren of the atomic bomb and the children of Sputnik." (1)

During the 1950s, the alliance between the federal government and the physics community, continuing intellectual motivation, and the growing investment in accelerator projects set the stage for accelerator developments of increasing complexity and expense. To understand this tumultuous decade, three questions must be examined. First, how did the alliance between the scientific community and government change in the midst of great social pressures and how did these changes affect the potential for funding accelerators? Second, what progress was made in

the understanding of fundamental particles and what impact did this progress have on accelerator development? Third, what advancements were made in accelerator technology and what was the status of American accelerator building as the decade ended?

Funding and Tensions Increase

In the 1950s, Americans experienced the Cold War, the Korean Conflict, and the McCarthy era. While all citizens were affected by the change in political climate, the physics community experienced special tensions, both within the physics community and between the physics community and the federal government. One cause of tension was security regulations. As apprehension increased about the Soviet threat, the scientific community, which had lobbied against excessive security regulations and military domination of the AEC, was faced with barbed wire surrounding the Radiation Laboratory at Berkeley and security restrictions covering most work on fissionable materials and reactor research. In light of increasingly strict security regulations in basic research projects, a mid-decade survey of graduate students revealed that almost half would accept a job with less pay if no security clearance was required. (2)

Another cause of tension was differing views among scientists on the arms race. The hydrogen bomb prompted particularly sharp controversy. As GAC chairman, Oppenheimer lobbied against development of the weapon, a stand that was applauded by Rabi and others. Oppenheimer, Rabi, Charles Lauritsen and Jerrold Zacharias advocated an

alternate defense scheme centered around the development of a variety of fission weapons. Lawrence, Edward Teller, Luis Alvarez and other Berkeley physicists responded by lobbying harder for the development of the hydrogen bomb. Lawrence even delayed construction of the Bevatron for two years to plan a giant, high-intensity cyclotron, called the Materials Testing Accelerator (MTA), which was designed to provide fissionable materials for the hydrogen bomb and other nuclear weapons. As one observer noted: "They had all worked together for the A-bomb and differed radically on the H-bomb." (3)

In the midst of this environment emerged the Oppenheimer security hearing, which provided a vivid display of how the new alliance with the federal government caused tensions within the physics community and between the physics community and government. In late 1953, K. D. Nichols, former deputy to General Groves and current AEC General Manager, wrote to Oppenheimer. He noted that after "investigation as to your character, associations, and loyalty, and review of your personnel security file" in light of current government guidelines "there has developed considerable question whether your continued employment on Atomic Energy Commission work will endanger the common defense and security. . . ." Nichols then specifically reported suspicions about the relationship Oppenheimer and members of his family had with communists and the Communist party, and suspicions about the propriety of Oppenheimer's attempts to block development of the hydrogen bomb, including his opposition while GAC chairman from 1946 to 1952. A personnel review board headed by Gordon Gray, president of the

University of North Carolina, was subsequently convened to consider the case. During the hearing, many of Oppenheimer's friends and former colleagues were asked to testify. Most physicists supported him wholeheartedly; Rabi gave a particularly stirring defense. Lawrence, who was ill with the chronic colitis that would kill him four years later, declined to testify; however, particularly damaging testimony came from Teller. As a result of the hearing, the AEC was advised in 1954 not to renew Oppenheimer's security clearance. (4)

After the hearing, many scientists criticized Teller for betraying his former Los Alamos boss. By this time, Teller commanded a huge crash program for developing the hydrogen weapon and many felt his behavior smacked unpleasantly of both political and professional opportunism. As a result, Teller was ostracized by many physicists. For example, Rabi snubbed Teller while the two were attending a meeting at Los Alamos, and Teller refused to return to the laboratory for nine years. Alvarez remembers that as a result of the hearing Berkeley physicists were unfairly accused of plotting against Oppenheimer. As Alvarez recently noted: "All of us who were touched by the hearing were in some way wounded." As individual physicists struggled to conciliate the often conflicting demands of personal advantage, political conviction, and professional loyalty, the physics community increasingly faced the nasty new divisiveness first evident in the Oppenheimer hearing. In the politically turbulent era of the 1950s, nonscientific issues were more complicated, and the economic and political stakes higher, than ever before. (5)

In the aftermath of the Oppenheimer hearing, alarm rose about the the dangers accompanying the new alliance between the physics community and the federal government. Many scientists felt Oppenheimer had been persecuted for his stand on the hydrogen bomb, singled out for a personal vendetta by AEC Commissioner Lewis Strauss. Thoughtful physicists found it disquieting that such an eminent physicist could meet such a fate after exemplary wartime service. In speeches, Bush promoted restoration of "the wholehearted, intimate partnership among scientists, military men, and civilian policy makers." The view that scientists had become alienated from the federal government was bolstered by the perceptible decline in the influence of the GAC which coincided with the rise in power of the JCAE. Although the feeling of alienation passed quickly, the memory of the Oppenheimer trial lingered. As historian Sylvan Schweber notes: "Oppenheimer's downfall . . . made it clear that although physicists had power, power rested with the state." (6)

While tensions were brewing within the scientific community, a change was being forged in the character of support from the AEC, ONR, and other federal sponsors. As Schweber explains: "National priorities and defense needs, in particular, began to determine more directly the areas and directions that were to be sponsored." (7) Complaints about this change joined the uneasiness about Oppenheimer's fate and the specter of tighter security regulations. Life in the physics community was clearly changing; as the 1950s wore on, the obligations contracted with federal support became more apparent. As Robert Seidel has noted,

the government expected three payoffs from its support for expensive particle physics research:

> The first was manpower: young men, attracted to physics by the glamor of probing the limits of the universe, [were] trained with the intention of putting their talents to use in more mundane and sometimes more military pursuits, like nuclear reactors or nuclear weapons. Secondly, those who trained scientific manpower for these purposes, but pursued the higher calling of particle physics, were called upon in times of crises by their patrons to consecrate their talents to these purposes, and served, therefore, as a sort of 'national guard' to reinforce the weapons laboratories. Thirdly, the instruments of discovery, particle accelerators, were turned into instruments of war, when defense seemed to require turning plowshares into swords. (8)

Although such payoffs prompted uneasiness and complaint within the physics community, the motivation to cope with the terms of the contract was high, since the agencies dictating payoffs were the only ones offering plentiful funding for fundamental research. While some leaders, like Lawrence, paid up, others simply went about their research and ignored the reality of the contract. When justifying basic research funding, physicists emphasized their traditional belief that basic research naturally led to useful applications, all the while assuming that government leaders, like physicists, also held the conviction that basic research deserved support primarily because of its intrinsic value. Although naive, this view was undoubtedly easy to maintain in an era when the prestige of science was high outside the physics community. (9)

Conflicting attitudes about the relative importance of basic and applied science remained largely unnoticed in an era of rapid funding increases for both basic and applied science. In 1950, government expenditures for research and development reached the all-time high of $1 billion, and by 1956, this amount had tripled. When the Russians launched Sputnik in October, 1957 the pressure to upgrade science and technology prompted further escalation so that from 1957 to 1961 federal funding for research and development saw a more than two-fold increase. (10)

Physics fared particularly well. As Gaylord P. Harnwell, chairman of the University of Pennsylvania physics department, remarked, the ample funding had created a "bull market" for physicists. After the Korean War, industrial demand for physicists increased sharply. At the same time, national laboratories and other government facilities needed more physicists than ever before. Job registers sometimes contained four jobs for every physicist looking for work. In response to this demand, the number of American physics Ph.D.'s awarded jumped from 275 in 1949 to more than 500 in 1955. After Sputnik, the increase became even more marked. Although average annual increase in physics Ph.D.'s had been 5% for decades, the rate leapt to more than 10% after 1958. From 1959 to 1967 schools offering Ph.D.'s in physics doubled, and because of competition for physicists, salaries increased. (11)

Because more physicists were produced in the 1950s than ever before, by 1960 the profession had a low median age. A disproportionate number of these children of Sputnik who pursued basic research were

recruited to subatomic physics. In addition to high prestige and the glamor of working with large accelerators, young, ambitious physicists were drawn by the high funding levels of particle physics. Even in an era of large overall funding increases, the record for particle physics was impressive. Whereas the subfield received $12.5 million from government agencies in 1950, the figure had jumped to almost $30 million by 1958. Ironically, although funding increases came from the desire to upgrade applied science and technology, in an era when physicists had considerable influence in funding allocation, particle physics flourished, although its prestige within the physics community derived from its ability to address fundamental issues, a characteristic which rendered it particularly remote from application. (12)

Although the future for particle physics looked bright at the end of the decade, the seeds had been sown for future dissension, both within the physics community and between the physics community and its federal patron. Within the physics community, allocation of funding had followed traditional lines, directed by the recommendations of powerful academic scientists. As in the past, a small number of highly regarded institutions received most of the resources. In the early 1950s, for example, 72% of federal support for unclassified fundamental research was given to only 17 institutions. (13)

Those with outstanding prewar scientific credentials were most likely to assume power and influence during the war and this power and influence could be translated after the war into a competitive advantage for winning the most expensive fundamental research projects. These

projects, in turn, brought new research success and prestige, which justified further special treatment. This process was all the more obvious because the professional bonds forged during World War II further encouraged clustering at privileged institutions, creating highly visible cliques. As Rabi had anticipated before the end of World War II, basic research flourished at Berkeley and in the Chicago area at Argonne National Laboratory. As Rabi had ensured, basic research also flourished at Brookhaven. In the favorable funding environment of the 1950s, the poor did not get poorer, but the rich got much, much richer, creating an imbalance that worsened as funding increased.

The first major reaction to this imbalance was the formation of the Midwest Universities Research Association (MURA) in 1954. Part of the impetus to form MURA came from the conviction that a disproportionate amount of research funding went to the East and West Coasts. Another motivation was the frustration that some Midwestern physicists felt with management at Argonne. Bernard Waldman, who became MURA's staff director in 1960, recently explained that "many of us had bitter feelings about Argonne, there's just no question about it." (14)

Argonne was run by the University of Chicago, which had a strained relationship with other, less richly endowed Midwestern universities. According to Waldman, who was a professor for many years at Notre Dame, Argonne's first director, Walter Zinn, "acted like Argonne was a star attraction. He treated sabbatical visitors as if they had no minds of their own." Also, Zinn designated reactor research as the primary focus of the laboratory. As former MURA researcher Frederick Mills recently

noted: "Zinn just wasn't interested in the new particles or in building a top-notch accelerator to explore them." At a time when Lawrence and Rabi championed the best interests of East and West Coast physicists interested in accelerator-based physics, Midwesterners were, in Mills' words "left out in the cold." MURA researchers launched an ambitious accelerator design project, as explained in the next section of this chapter, to establish a niche of their own in the AEC accelerator-building budget. This helped to assuage, but did not defuse, MURA discontent. (15)

As discontent in the Midwest grew, the groundwork was also being laid for future problems in relations between the physics community and federal government. By the end of the decade the issue of the relative value of basic and applied science had become more complicated, and in addition, conflicting trends had become more firmly established under the mask of affluence. As the American response to Sputnik revealed, a Russian display of technological superiority was taken as a political threat. In fact, as Michael Smith has pointed out when explaining the symbolic dimension of the space program, the arms race became "a succession of symbolic deployments" since it was, by nature, a means of psychological warfare. Working from World War II precedent, physicists had promoted the view that accelerators were a natural part of the postwar atomic energy arsenal. In the midst of the Cold War, these instruments quite naturally joined space vehicles as politically important symbols of technological competence. Already convinced of the value of accelerators and predisposed toward competitiveness, physicists

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were quick to embrace the new idiom. After American physicists learned of Soviet accomplishments as part of the mid-decade "Atoms for Peace" program, accelerator proposals stressed the necessity to "beat the Russians" to the next accelerator advance. (16)

Accelerator projects also benefited because they were funded alongside reactors. By mid-decade hopes for nuclear powered electricity generation ran high, prompting the JCAE actively to promote an expanded overall atomic energy program. It was true that the political arena sometimes exacted its price, for example, a $100 million accelerator proposal from Stanford was stalled when it became a pawn in a fight between the JCAE and the Eisenhower Administration. Nonetheless, in general, accelerator projects benefited from JCAE and AEC efforts since both gained increased political power from a well-funded atomic energy program. For example, the AEC assumed sponsorship of the Stanford machine, although Stanford had previously received accelerator support from ONR, and in 1959 formally agreed to be the nation's primary sponsor for future accelerator projects. (17)

Summary: The Evolving Alliance and the Potential for Accelerator Funding

Against the backdrop of increased tensions, the alliance between government and the physics community grew stronger and the already sturdy position of those promoting accelerator development was bolstered. Increased government support was spurred by both the promise of payoffs in manpower and resources for defense and technology, by the self-interest of the AEC and the JCAE, and by the compelling view that

accelerator development symbolized technological, and therefore political, strength. Although all these rationales were grounded in the value of applied science, the subsequent flourishing of accelerator funding provided further evidence to the physics community that a particularly valuable line of basic research could expect rich future support due to its intrinsic worth. Despite conflicting values, the bonds uniting the physics community and the federal government drew tighter through continued mutual benefit, powerful symbolism, and institutional reinforcement.

The Heroic Age of Discovery

Before the new machines at Berkeley and Brookhaven began operation, cosmic-ray researchers had exclusive access to particles in the interesting energy range above the first synchrotrons, and thus led the way in discovering unusual events in the early 1950s. Many of the particles involved came to be called "strange" particles because of their surprisingly long lifetimes and copious production. In 1951, mesons (now called kaons) were found in emulsion. In 1952, a cloud chamber group observed the first cascade decay. In 1953 particles later identified as sigma particles were found in emulsion. Since strange particles were more abundant at high altitudes, cosmic-ray detectors were often carted to mountain peaks. (18)

While strange particles attracted a great deal of attention from experimentalists in the early years of the decade, theoretical interest in the phenomena remained somewhat sluggish, largely because of the

preoccupation with extending QED. Laurie Brown remembers, for example, that during this period Oppenheimer encouraged theorists at the prestigious Institute for Advanced Studies to study the new particles. One of the few to accept the suggestion was Abraham Pais. At about the same time, independent work on the subject was also done in Japan by Kazuhiko Nishijima, Yoichiro Nambu, and others. (19)

By 1952, this separate work had produced a common hypothesis, which later came to be known as associated production. Associated production explains that the strong interaction process which creates a strange particle always results in two or more strange particles and, therefore, cannot decay by the strong interaction due to a lack of energy. Instead, they survive until affected by the much slower weak interaction. (20; See Fundamental Interactions)

Associated production meant that strange particles were produced strongly only in pairs. Since they decayed singly, they had longer lives. However, certain processes that were not forbidden did not seem to occur and more detailed rules were required. In 1953, Nishijima and Tadao Nakano, and independently Murray Gell-Mann, introduced a new quantum number which provided such rules. The strangeness quantum number, as it was called by Gell-Mann, was combined with earlier isotopic spin quantum number with which strongly reacting particles with similar masses but different charges are grouped so that they act as different states of the same particle. Once these groups are identified and assigned predetermined values, it is possible to predict what strong interactions will occur, since the scheme dictates that strangeness and

isospin will always be conserved. The idea governing the assignment of quantum numbers is that the symmetries which define classification reflect the underlying dynamics of elementary particles. As Brown, Dresden, and Hoddeson explain: "The strangeness idea is elegant, economical, and powerful. . . ." After strangeness was announced and cosmic-ray researchers produced a flood of new data with improved nuclear emulsions, theorists enthusiastically focused their attention on strange particles. (21)

To their great fortune, at just this time physicists had new tools to investigate strange particles more efficiently. In 1952, the Cosmotron began operation at Brookhaven, accelerating protons first to 2.2 GeV and later to 3 GeV. Although it had been built with the idea of exploring meson physics, the Cosmotron provided an unexpected dividend--it could create strange particles. Because of the control and intensity limitations faced by cosmic-ray researchers, one of the first opportunities thus opened for Cosmotron researchers was verification of associated production. (22)

Building on the cosmic-ray detector tradition, experimenters devised a new, continuously sensitive high pressure diffusion cloud chamber for use at the Cosmotron. A group led by Ralph P. Shutt exposed this instrument to the machine's negative pion beam and found nine examples of associated production in 1953 and 1954. Physicists were soon convinced that the theory was correct. (23)

Berkeley researchers were quick to join in the fun. In 1954, the Bevatron began accelerating protons to 6.2 GeV. By the end of the year, it produced a mixed beam of pions and kaons; physicists now had an even more powerful tool for exploring fundamental particles. With the Bevatron, physicists discovered literally dozens of particles in the next two decades. (24) In addition, they were quickly able to make detailed, accurate measurements of kaon masses and lifetimes. These studies confirmed the predictions made by the strangeness quantum number and provided important evidence in a growing dilemma called the theta-tau puzzle. (25)

Detector development was crucial to the success of strange particle research. The breakthrough came with the hydrogen bubble chamber. Berkeley researchers, like Cosmotron researchers, borrowed on the tradition of cosmic-ray detectors when devising equipment for the Bevatron. After hearing about a promising new detector that Donald Glaser had invented for use in cosmic-ray experiments, Alvarez quickly made plans in 1953 to adapt the instrument for detecting strange particles at the Bevatron. Convinced that current nuclear emulsion and cloud chamber techniques were inadequate for observing important strange particle events, Alvarez sought to devise a track-recording detector with liquid or solid density, uniform sensitivity, and a rapid cycling time. Liquid hydrogen, with its single proton nucleus, was a promising medium for the new detector. (26)

Conquering a number of technological difficulties, including the safety hazard of the highly flammable liquid, Alvarez made a series of

small bubble chambers. At the end of the decade, after obtaining a special appropriation of $2.5 million from the AEC, he jumped in one big step to a 72-inch chamber which operated with the aid of a computer and hundreds of workers who entered, processed, and interpreted data. Hydrogen bubble chambers, which were built at many accelerator laboratories, greatly extended the experimental capabilities of accelerators; for example, while the cloud chamber produced only nine events after nearly two years of operation at the Cosmotron, with the hydrogen bubble chamber Brookhaven experimenters were able to record 55 associated production events after only two days of operation. (27)

While the large synchrotrons, augmented with sophisticated detection devices, paid an unexpected dividend in facilitating strange particle production, they also came through with the expected benefits. At the Cosmotron, productive studies were made of meson physics and at the Bevatron, the antiproton was discovered.

The antiproton search, in particular, prompted considerable attention. Many groups jumped at the chance to detect the prized new particle after the Bevatron went into operation. As with previous discoveries, devising a suitable detector provided a formidable challenge. Early attempts with cloud chambers and emulsions ended in failure. Berkeley researchers then tried Cerenkov and scintillation counters. (See Cerenkov Counter and Scintillation Counter) One group, including Emilio G. Segre, Owen Chamberlain, Clyde Wiegand and Oreste Piccioni, fashioned a clever experimental arrangement using these counters as well as bending magnets and magnetic focusing lenses to comb

out the one antiproton produced in a stream of 40,000 mesons. By the time Segre and Chamberlain announced the discovery of the antiproton in late 1955, this experimental setup was netting an average of four antiprotons during each hour of Bevatron operation. (28)

In addition to strange particle data and the discovery of the antiproton, accelerators produced other important results by mid-decade. Robert Hofstadter's work at Stanford provides a brilliant example of contributions made with electron linear accelerators. (See Linear Accelerators) As with other researchers, detector development was crucial for Hofstadter; he constructed a large spectrometer to separate electrons scattered at various energies. By later measuring the number of electrons deflected in each direction, Hofstadter was able to obtain detailed information about the electromagnetic properties of the protons and neutrons inside the nucleus. (29)

By the time of mid-1950 accelerator accomplishments, the explosion of elementary particles, which would ultimately number in the hundreds, had forced physicists to revise their view that matter was composed of a small number of elementary particles. Despite the central contributions made by cosmic-ray researchers in the landslide of new particle discoveries in the late 1940s and early 1950s, the Cosmotron and Bevatron quickly eclipsed cosmic rays as a particle source. As one cosmic-ray researcher admitted, although cosmic-ray searches "survived the arrival of the Cosmotron because of the impetus we had" by the time Bevatron began operation two years later "the story was finished." Thereafter, cosmic rays provided information, as they had in the past,

for geophysicists, astronomers, and cosmologists. At a time when the exciting particle explosion rapidly drew new recruits, those interested in particle physics needed access to the Bevatron or Cosmotron, unless, in the words of another experimenter, "you wanted to sit in the corner." (30)

Experimenters were not the only ones profoundly affected in mid-decade by the particle explosion. The long list of new particles both frustrated and challenged theorists. In the midst of the phenomenological chaos of particles, theorists struggled throughout the decade to create elegant, theoretical order by casting the strong and weak interactions into quantized theories with appropriate fields. (See Fundamental Interactions and Quantum Field Theory) While engaged in this task, compelling experimental data inspired a revolution in weak interaction theory. The flood of new data provided by cosmic rays and the powerful new accelerators and the self-confident empirical approach of American theorists combined to produce the exceptionally productive experimental and theoretical interaction underlying the heroic age.

Attempts to devise a field theory for the strong and weak interactions grew progressively harder as the list of particles got longer, since the application of field theory required separate fields for each particle. (See Field Theories) In addition to this complication, which promised an untidy though not insupportable solution, theorists had a deeper problem. When modeling the strong interaction on QED, physicists found that differences between the two interactions made strong interaction calculations much more difficult.

As theorist Steven Weinberg points out: "Any physical process is represented by an infinite sum of Feynman diagrams. . . ." Each of these represents "a particular sequence of intermediate states consisting of definite number of particles of various types." Each diagram "is associated with a numerical quantity . . ." In QED, small quantities are associated with complicated diagrams, and thus the theory can be understood primarily from simple diagrams. In contrast, the strong interaction is so strong that the quantities associated with complicated diagrams are large, and thus complicated Feynman diagrams have as much importance as simple diagrams. "It was not that there was any difficulty in thinking of renormalizable quantum field theories that might account for the strong interactions," Weinberg notes "it was just that having thought of such a theory, there was no way to use it to derive reliable quantitative predictions, and to test if it were true." (31; see Feynman Diagrams)

While efforts to find a field theory of strong interactions continued without success, two developments focused attention on the weak interaction. One was the observation of the neutrino. This neutral, massless particle, which experiences only the weak interaction, was first postulated in 1930 by W. Pauli to explain nuclear beta decay. In 1934, Fermi incorporated it into his formulation of a quantum field theory for weak interactions. Although indirect evidence for the particle mounted in the next two decades, neutrinos were first observed only in 1956 by Frederick Reines and Clyde L. Cowan. To achieve this feat, Reines and Cowan devised a huge detector consisting of thin tanks

filled with water containing cadmium chloride sandwiched between thick tanks of liquid scintillator. They built this detector next to the Savannah River nuclear reactor which produced abundant supplies of neutrinos in the beta decay accompanying uranium fission. In this setup, neutrinos from the reactor interacted with protons in the water solution and the distinctive pulses of energy from subsequent decay processes were recorded by the scintillators. By observing the characteristic pattern of scintillation pulses, Reines and Cowan were able to positively identify the particle. (See Beta Decay; 32)

While this success was being chalked up, pressure grew to understand the theta-tau puzzle. Tentative calculations in the early 1950s revealed that although theta and tau had comparable masses and lifetimes, which implied they were the same particle, they appeared to have different parity. Assuming the conservation of parity, they could thus not be the same particle. Since measurements were uncertain, however, physicists were not seriously alarmed. (See Conservation Laws and Parity; 33)

When the Bevatron began operation, several groups sought to employ its unique kaon beam to clarify the situation. Emulsion and counter experiments on the new machine provided more statistics for kaon decay and the first highly accurate mass and lifetime measurements. Physicists could no longer ignore the theta-tau puzzle: it was now clear that theta and tau indeed had identical masses and lifetimes and different spin parity. (34)

When attempts to employ current theories failed to solve this perplexing puzzle, Chen-Ning Yang and Tsung Dao Lee made the radical suggestion that parity was not conserved in the decay. To their surprise, a subsequent review of earlier experiments revealed no evidence to support parity conservation in weak interactions. Due to convincing evidence in strong and electromagnetic interactions and the aesthetic appeal of space-time symmetry, physicists had failed to design experiments aimed at checking this conservation law. Overthrowing parity was distasteful, even for Yang and Lee. As Yang explains: "This prospect did not appeal to us. Rather we were, so to speak, driven to it through frustration with the various other efforts at understanding the theta-tau puzzle." (35)

Yang and Lee proposed several experiments to check their radical idea, and experimental confirmation soon followed. The first experiment to prove parity nonconservation was performed by Chien-Shiung Wu and others at Columbia in 1957. In this experiment, a large number of cobalt nuclei were polarized, using special low-temperature techniques to preserve their orientation. An electron counter then observed the trajectories of electrons ejected in beta decay in two directions of polarization. The results clearly showed an asymmetry, thus indicating parity violation. With this single blow, parity conservation, a principle accepted without question for generations, was destroyed. (36)

The confirmation of parity nonconservation revolutionized weak interaction theory. Physicists began reexamining earlier experimental

data and theoretical work to find a way to revise their understanding of the weak interaction. The V-A theory arose from this process. For many years interest had focused on a Hamiltonian which emerged from Fermi's 1934 explanation of beta decay. This Hamiltonian had the following schematic form: H = V + A + S + T + P. Each term in this function represents mathematical entities describing the forces or interactions between particles (vector, V; axial vector A; vector, V; scalar, S; tensor, T; or pseudoscalar, P). From the late 1930s to the mid-1950s, efforts were made to determine relative magnitudes for each of the five terms. In its original form, the Hamiltonian assumed parity conservation. (37)

When Yang and Lee announced parity nonconservation, physicists dusted off a previously rejected wave equation from the late 1920s which suggested a simple parity, nonconserving theory of beta decay. When testing this hypothesis, called the two-component neutrino theory, physicists were also eager to substantiate the notion that all weak decays could be understood in terms of a "universal Fermi interaction," an idea suggested in 1948. Resulting efforts led in 1957 to a new Hamiltonian, proposed by Feynman and Gell-Mann and independently by Robert E. Marshak and Ennackel Sudarshan and by Jun Sakurai, which described all weak interactions, not just beta decay, in terms of two equal, opposite terms, V and A. In late 1957, the theory was bolstered when experiments by Maurice Goldhaber and others at Brookhaven demonstrated that the direction of spin is always opposite the direction

of momentum for neutrinos. This negative "helicity" of the neutrino is specifically predicted by the V-A theory. (38)

The new V-A theory gave a concise explanation of a wide range of weak interaction decays. Despite this success, V-A was plagued with infinities; thus, by the early 1960s, theorists attempting to create a rigorous field theory for the weak interactions had reached an impasse and their efforts ground to a halt. With the difficulty of modeling either the strong or the weak interaction on QED, interest in field theory was waning by 1960. When it no longer seemed likely that fundamental understanding of the weak and strong interactions would come from a conventional field theory approach, interest turned to alternate theoretical approaches. (39)

One approach centered on the scattering-matrix, or "S-matrix" idea, which had been considered in the immediate postwar era, largely in response to the problem of infinities in QED, and then forgotten by most physicists until the problems with field theory arose. The S-matrix developed from the realization that no one can directly observe sub-atomic phenomena in experiments, but instead observes the transition of a collection of known particles into some final state. By focusing on the transition probabilities instead of particles as fields, physicists could avoid the impossible task of summing the infinite set of Feynman diagrams. Although early exploration of S-matrix properties was done using quantum field theory, by 1960 ways had been found to pursue S-matrix ideas as an autonomous theory, thanks to the efforts of

Gell-Mann, Geoffrey Chew and others. Ultimately, physicists found that the predictive power of this method was limited. (40)

One outgrowth of the S-matrix approach was the "bootstrap" theory devised by G. Chew in the early 1960s. Chew extracted a series of complicated non-linear equations from S-matrix analytic structure proposing that the solutions described the properties of all strong interacting particles. Chew saw his scheme as picturing a kind of "nuclear democracy" in which all these particles had equal dynamical status. As Pickering explains: "they were supposed to pull themselves up by their bootstraps as a self-consistent solution to S-matrix equations." Eventually, computational problems caused the theory to lapse into obscurity. (41)

Around 1960, however, the S-matrix approach was thrust into the limelight when Tullio E. Regge introduced the mathematical concept of "Regge poles," which Chew and Steven C. Frautschi applied to S-matrix theory. In this theory, particles correspond to Regge poles obeying a mathematical pattern. Using this scheme, physicists found that strongly interacting particles could be classified into a small number of groups based on "Regge trajectories," which aided in some calculations. (42)

Despite the brilliant accomplishments of weak interaction theory and the limited success of S-Matrix theory, by the end of the 1950s theorists were frustrated by the cloudy theoretical picture of fundamental particles. Although they had learned a great deal in the

midst of their heroic age, the dual prods of accomplishment and frustration compelled them to learn much more.

Summary: Research Progress and its Impact on Accelerator Development

With the flood of new particle data, the shock of parity nonconservation in weak interactions, experimental confirmation of the antiproton and the neutrino, and valiant attempts to devise a field theory for the strong and weak interactions, the 1950s were a time of intense intellectual excitement for those investigating the fundamental nature of matter. By 1960, particle physics had become a highly prestigious and distinct subfield. In the mid-1950s, when accelerators eclipsed cosmic rays as the primary source of particles for experiments, cosmic ray and nuclear physicists interested in elementary particles became a single group. In addition, more and more theorists focused attention on the intriguing puzzles of elementary particles.

A measure of the importance of accelerators in the new amalgam of particle physics can be taken by the fact that physicists currently tend to forget the important role of cosmic-ray research in the discovery of new particles. Impressed by exhilarating new capabilities, such as the opportunity for studying strange particles at the Cosmotron and Bevatron, they thought of the accelerator, the device designed to study sub-atomic matter, as the tool that built the heroic age of particle discoveries. Moreover, as the flood of new data brought increasing excitement and new puzzles, the lure of developing even more powerful accelerators was irresistible, since this was the most efficient route

to more complete particle data. As Hoddeson explains, by this time, "the discoveries using high energy machines were furnishing their own dynamics for further increasing the energy of accelerators." (43)

The Flowering of Accelerator Technology

The heightened desire for accelerator development in the 1950s came at a time when American industry was capable of providing more support than even before. In addition to abundant supplies of raw materials, such as steel for magnets, a growing variety of electronics components was available throughout the decade with the expansion of the radio and television industry. Some specialized equipment, such as oscilloscopes, was also commercially available. On this fertile ground, accelerator building blossomed. At a time when every university of even moderate size campaigned for funds to build an accelerator, the fleet of accelerators grew ever larger. By 1960 about 20 universities had machines capable of accelerating particles over 50 MeV. With the demand for even more advanced accelerators and the increasing complexity of accelerator design, accelerator building was professionalized: by the mid-1950s accelerator physicists had separate conferences, publications and a professional identity of their own. (44)

With their superior resources and established reputations, Berkeley and Brookhaven supported a particularly experienced group of accelerator physicists which many considered to be the ruling elite of the new profession. From this elite emerged a new type of synchrotron. (45)

Before the Cosmotron was ready for use, ideas were already brewing at Brookhaven for the next planned accelerator, a 10 GeV proton synchrotron. In anticipation of a visit from European scientists planning a large machine for the newly formed European high energy

physics laboratory, now called CERN, Ernest D. Courant, Stanley Livingston, and Hartland Synder in 1952 began scrutinizing Cosmotron designs to find ways to build a larger accelerator. In the process, they developed the concept of strong focusing. (They later discovered that the idea had been proposed two years earlier in Greece by Nicholas Christofilos.) A strong focusing, or alternating gradient (AG), machine has a series of strong magnetic lenses that focus horizontally, defocusing vertically and then focus vertically, defocusing horizontally. The net focusing effect, which is much stronger than that obtained in previous accelerators, confined the beam to a smaller area, allowing a less expensive accelerator. Prospects for the synchrotron now looked brighter than ever before. Previously, it appeared that synchrotrons more powerful than the Cosmotron and Bevatron would be prohibitively expensive. If a strong focusing synchrotron could be successfully built, accelerators of much higher energy would be economically feasible. (46; See Magnetic Lens)

Stimulated by this new idea, Brookhaven and CERN designers both began designs for proton AG accelerators in the 25 to 30 GeV range. In 1954, the AEC authorized Brookhaven's plans for the Alternating Gradient Synchrotron (AGS). This time there was no immediate competition from Berkeley. After hearing about strong focusing, Berkeley physicists made rough designs for large strong focusing synchrotrons, but further detailed planning was stalled by the diversion of resources to the ambitious Bevatron research program, Lawrence's preference for nuclear physics and its applications, and skepticism, shared by many in the

physics community, about the feasibility of strong focusing for large proton machines. (47)

While CERN and Brookhaven machines were under construction, strong focusing stimulated other innovations. In late 1952, for example, Toshio Kitagaki in Japan and Milton White at Princeton independently proposed another cost-cutting idea: the "separated-function" magnet. Realizing that a circular AG machine would only require intermittent focusing, they suggested using two separate magnets: the standard quadrupole magnet for focusing the beam and a cheaper dipole magnet for bending the beam. In 1954, Robert R. Wilson successfully applied strong focusing for electron acceleration, building the first strong focusing machine, a 1.3 GeV electron synchrotron. (48)

The idea of strong focusing also stimulated MURA researchers, who were determined to find a novel contribution to accelerator building and thereby gain entry into the AEC budget for accelerator building. In 1953, Keith Symon devised a scheme for producing strong focusing which allowed the use of direct current magnets and a much higher beam intensity. Such accelerators were dubbed Fixed Field Alternating Gradient Synchrotron (FFAG) machines. (MURA researchers later learned that researchers in Japan and the U.S.S.R. had independently developed the same concept.) While seeking to understand the acceleration system in FFAG machines, Symon and Andrew Sessler developed a comprehensive theory of the motion of particles in the energy "dimension." They determined that it should be possible to "stack" beams of successively-accelerated particles so as to achieve an intense

circulating beam current at high energy. With this understanding, Donald Kerst realized that sufficient particle densities could be achieved to make practical colliding beam systems. (49)

Although MURA made several preliminary designs, the group eventually focused on FFAG machines in the 10-20 GeV range. These machines were designed to either produce protons beams about a hundred times more intense than the Bevatron or the Cosmotron, or colliding beams equivalent to a fixed target accelerator of several hundred GeV, or both. In addition to pioneering the FFAG concept and colliding beams, the particularly innovative MURA designers made a number of other contributions, including the application of the digital computer to accelerator design and advances in the understanding of the mechanics of particle beams. In light of the skepticism about the feasibility of strong focusing synchrotrons, it seemed quite possible that MURA was blazing a new trail. If conventional strong focusing synchrotrons were unsuccessful, the MURA could well possess a more cost-effective and practical approach to future accelerators, through FFAG colliding beams and through higher intensity, lower energy machines. (50)

MURA scientists were dismayed when the AEC decided in 1955 to fund the Zero Gradient Synchrotron (ZGS) at Argonne. The determination to build a machine quickly "to beat" the Russian 10 GeV synchrophasotron had produced a 12.5 GeV weak focusing design and MURA scientists worried that the relatively mundane machine would kill the FFAG. Bravely proceeding with their plans for their ambitious ultra-modern accelerator, which would undoubtedly take years to design, the MURA

scientists presented their ideas to the AEC and received design funding in 1956. (51)

Although attention centered on the development of circular proton accelerators in the 1950s, the work of Hofstadter and others made it clear that contributions could also be made with large electron linear accelerators. Starting with the work of William Hansen in 1947, a series of electron accelerators of increasing size were built at Stanford. By the late 1950s the enthusiastic Panofsky, who had helped Alvarez with the proton linear accelerator at Berkeley after World War II, led Stanford's High Energy Physics Laboratory and designs were being developed for a two-mile linear accelerator, 30 times larger than the previous Stanford machine. Because of the size of the project, and the estimated cost of $100 million, Stanford scientists felt the project should not be developed and operated by Stanford faculty members alone. In 1957, a proposal for a new national laboratory, the Stanford Linear Accelerator Center (SLAC), was submitted the to the AEC, prompting the AEC decision to assume future funding of large accelerators. After delays caused by political infighting between the JCAE and the Eisenhower Administration, the project was authorized in 1961. (52)

By this time the Commission already had an expensive list of commitments. Since the early 1950s, when the 3 GeV Cosmotron and the 6.2 GeV Bevatron were built for about $10 million a piece, the AEC had agreed to build two proton synchrotrons, the 12.5 GeV ZGS at $27 million and the 33 GeV AGS at $30 million, and the 20 GeV electron linear accelerator at Stanford at $100 million. At the same time, MURA had its

hopes for a top-flight Midwestern facility staked on the 10 GeV FFAG proposal, which would add about $150 million to the AEC bill, if approved. During this period of rapid accelerator development two large accelerator projects were also underway outside the U.S.: in addition to the 10 GeV Russian machine, CERN had authorized the 28 GeV Proton Synchrotron (PS), the machine similar in design to the Brookhaven AGS. Thirteen smaller accelerators with energies above 1 GeV were also being built worldwide. (53)

Summary: Advancements in Accelerator Technology and the Status of American Accelerator Building

As accelerator building became a distinct scientific specialty, accelerator technology advanced rapidly. The discovery of strong focusing opened the way for much larger proton synchrotrons. While the first strong focusing proton synchrotrons were being built at Brookhaven and CERN, many additional innovations emerged from MURA and SLAC. American accelerator builders started the decade with a headstart on international competitors and superior resources. With increased funding, the continued growth and properity of the American physics community, strong industrial support, and abundant new ideas and enthusiasm, the American lead grew throughout the 1950s. By the end of the 1950s the American physics community had an array of planned and approved accelerator projects which outstripped international competition both in the number and capability of machines.

Conclusion: Building Bigger and Stronger--1951-1959

The impressive array of American accelerator projects graphically illustrated the powerful momentum that had been building since World War II. The impetus for constructing ever more powerful accelerators was driven intellectually by the self-reinforcing dynamics of particle physics discoveries and supported financially with a contract cemented by mutual need and obligation. Competing, elite groups of accelerator builders emerged at Berkeley and Brookhaven to exploit the resulting opportunities. With the discovery of phase stability at Berkeley and the invention of strong focusing at Brookhaven, the way opened for building much larger accelerators.

To these intellectual, economic, and technical forces, potent sociological forces were added. As Andrew Pickering remarks in his discussion of particle theory, once physicists have accumulated certain training and skills, they have a strong interest in using and further developing this expertise. (54) In his discussion of the development of the hydrogen bubble chamber, Peter L. Galison makes clear how expertise influences the course of experimental work: experimentalists invest in skills specific to certain instruments and these investments, in turn, influence not only the future style of work, but also future instrumentation choices. During the course of the 1950s, accelerator-based physics became predominant, and as a result, experiments and detecting equipment grew larger and much more complicated and specialization in all facets of particle exploration increased. As Sylvan Schweber points out, the predominance of

accelerators reinforced a tendency begun during World War II: theorists became accustomed to focusing attention on experimental results and eschewed more abstract topics. By the end of the 1950s, large numbers of theorists, experimentalists, and accelerator builders were bound in a professional investment which relied on continued particle investigations with ever-larger accelerators. While most organized human activities share this self-perpetuating tendency, in the case of particle physics the cost was particularly high and constantly increasing in the fifteen years after World War II. (55)

By 1960, the CERN PS and Brookhaven AGS had both passed their performance tests with flying colors. To the amazement of many physicists, these machines not only proved the feasibility of strong focusing, but also produced surprisingly high beam intensities. Almost immediately plans were drafted at three separate American laboratories for strong focusing synchrotrons in the range of 100-1000 GeV. In each case, estimated costs were well over $100 million. Expectations and price tags had never been so high; the stage had been set for the possibilities and tensions of the next decade. (56)

Notes for Chapter 2

1. Quotes, respectively, as quoted in Sylvan. S. Schweber, "Some Reflections on the History of Particle Physics in the 1950s," in Brown, Dresden, and Hoddeson. eds., Pions to Quarks: History of Particle Physics in the 1950s, to be published by Cambridge University Press, 1988; and as quoted in Abraham Pais, Inward Bound: Of Matter and Forces in the Physical World (Oxford: Oxford University Press, 1986), p. 19.

2. Kevles, pp. 382-383, 378, and 380.

3. Although the MTA was left unfinished, Lawrence's efforts led in 1952 to the establishment of a new weapons laboratory at Livermore. Plans for the MTA were dropped after the AEC introduced cost incentives which prompted the discovery of new uranium sources. For more information on the MTA, see John L. Heilbron, Robert Seidel, and Bruce R. Wheaton, Lawrence and His Laboratory: Nuclear Science at Berkeley, 1931-1961 (Berkeley: Office for History of Science and Technology, 1981), pp. 62-75. Text quote as quoted in Herbert Childs, An American Genius: The Life of Ernest Orlando Lawrence, (New York: E. P. Dutton and Co., Inc., 1968), pp. 472-473. Also see Daniel J. Kevles, The Physicists: The History of a Scientific Community in Modern America (New York: Alfred A. Knopf, 1978), p. 383, and John S. Rigden, Rabi: Scientist and Citizen (New York: Basic Books, Inc., 1987) p. 223.

4. Quote from John Major, The Oppenheimer Hearing (New York: Stein and Day, 1971), p. 34. Also see pp. 35-42 in this source; Kevles, pp. 380-381; and Childs, p. 472.

5. Quote from Luis Alvarez, Adventures of a Physicist, (New York: Basic Books, Inc., 1987) p. 181. Also see John S. Rigden, Rabi: Scientist and Citizen, p. 230, Jeremy Bernstein, Hans Bethe: Prophet of Energy, (New York: Basic Books Inc., 1979), p. 101, Kevles, p. 378.

6. Quotes, respectively, as quoted in Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," in Historical Studies in the Physical and Biological Sciences, 18 (1987) p. 177n and in Schweber, "Some Reflections on the History of Particle Physics in the 1950s," in Brown, Dresden, Hoddeson, eds. Also see Kevles, pp. 381-382 and Harold P. Green and Alan Rosenthal, Government of the Atom: The Integration of Powers, (New York: Atherton Press, 1963), pp. 89 and 79.

7. Quote from Sylvan Schweber, "Some Reflections on the History of Particle Physics in the 1950s," in Brown, Dresden, Hoddeson, eds.

8. Robert Seidel in "The Postwar Political Economy of High Energy Physics,"in Brown, Dresden, and Hoddeson, eds.

9. Otherwise perceptive participant accounts reveal this naivete. For example, when explaining the new financial resources after World War II Abraham Pais, Stanley Livingston, and John H. Van Vleck all ignore mention of any payoff. Although they see that funding was prompted by increased appreciation for science or new prestige for scientists, they speak of support as an act of generosity or benevolence, as though this was the patron's sole motivation. In this author's experience, many physicists in basic research are quick to refute Seidel's analysis and seem astonished by the idea that basic researchers are bound in such a contract. Since basic research funding continues to be justified by invoking its value to applied research, the tenacity of such naivete suggests a powerful underlying myth worthy of further investigation. See Daniel S. Greenberg, The Politics of Pure Science (New York: The New American Library, 1967), pp. 28-32 for a discussion of justifications for basic research funding. Text references: Abraham Pais, Inward Bound, p. 19; M. Stanley. Livingston, Particle Accelerators: A Brief History (Cambridge: Harvard University Press, 1969), p. 43; and John. H. Van Vleck, "American Physics Comes of Age," Physics Today, 17 (1964); p. 26.

10. Kevles, pp. 369, 386.

11. Kevles, pp. 370, 387-388.

12. Report by a Special Panel of the President's Science Advisory Committee and the General Advisory Committee to the Atomic Energy Commission, "Piore Panel Report--1958," Appendix 3 in Joint Committee on Atomic Energy, Congress of the United States, "High Energy Physics Program: Report on National Policy and Background Information," (Washington D.C.: GPO, 1965), p. 141; Kevles, p. 374; and Theresa R. Shapiro and Helen Wood, "The New Physicists" Physics Today 6 (1953) pp. 14-15.

13. Kevles, p. 375.

14. Quote from interview with Bernard Waldman, Feburary 4, 1983.

15. For a history of the MURA controversy emphasizing political events, see Greenberg, 207-265. For further insight into the relationship between the Atomic Energy Commission, Argonne National Laboratory, and various Midwestern universities, see Leonard Greenbaum, A Special Interest (Ann Arbor: University of Michigan Press, 1971) pp. 243-265. Text quotes, respectively, from interview with Bernard Waldman, February 4, 1983 and from private communication, Frederick Mills, July 2, 1987. Also see Greenberg, pp. 211-212, Robert Seidel, "A Home for Big Science: The Atomic Energy Commission's Laboratory System," in Historical Studies in the Physical and Biological Science, 16 (1986), p. 143, and private communication, Lawrence Jones, July 9, 1987.

16. The desire to beat the Russians had a particularly strong affect on the development of the Zero Gradient Synchrotron, mentioned in

the last section of this chapter. Text references: private communication, David Allison, August 27, 1985; Michael Smith, "The Selling of the Moon," in Richard W. Fox and T. J. Jackson Lears, eds. The Culture of Consumption, (New York: Pantheon Books, 1983); Greenberg, p. 214; interview with Bernard Waldman, February 4, 1984; and Matthew Sands, "The Making of an Accelerator Physicist," in Brown, Dresden, and Hoddeson, eds.

17. For an acerbic account of the political fight over the Stanford Linear Accelerator, see Greenberg, pp. 231-236. Text references: Green and Rosenthal, p. 5; interview with James T. Ramey, November 21, 1986; and Harold Orlans, Contracting for Atoms, (Washington, D.C.: The Brookings Institution, 1967), p. 220.

18. Luis W. Alvarez, "Recent Developments in Particle Physics," Evolution of Particle Physics, (Stockholm: Nobel Foundation, 1969), p. 4 and Seriff, A. J., Leighton, R. B., Hsio, C., Cowan E. W., and Anderson, C. D., "Cloud-Chamber Observations of the New Unstable Cosmic-Ray Particles," Physical Review, 78 (1950) p. 290.

19. Interview with Laurie Brown, August 28, 1986; Brown, Dresden, and Hoddeson, eds., "Introduction," pp. 39-40.

20. For more detailed accounts of the development of associated production, see Abraham Pais, "From the 1940s into the 1950s," in Brown, Dresden, and Hoddeson, eds. Text references: Murray Gell-Mann and E. P. Rosenbaum, "Elementary particles," Scientific American, 197 (1957), p. 82; and Emilio Segre, Nuclei and Particles, (New York: The Benjamin/Cummings Publishing Co., Inc., 1965), pp. 653-654.

21. For more detailed accounts of this era, see Pais, "From the 1940s into the 1950s," Murray Gell-Mann, "Progress in Elementary Particle Theory, 1950-1964," and Bruno Pontecorvo, "Recollections on the Establishment of the Weak Interaction Notion," in Brown, Dresden, and Hoddeson, eds. Text quote from Brown, Dresden, and Hoddeson, eds., "Introduction." Also see interview with Laurie Brown, August 28, 1986, and Andrew Pickering, Constructing Quarks: A Sociological History of Particle Physics, (Chicago: Chicago University Press, 1984) p. 54.

22. The limitations faced by cosmic-ray researchers were the following. Although nuclear emulsion provides a dense medium that optimizes the chance of detecting a rare event, the particles produced in associated production have long lifetimes and thus appeared at a distance too far apart to be detected by the nuclear emulsions of the time. The cloud chambers of the time were large enough to detect both particles, but they provided a low density medium and standard models did not operate continuously. These characteristics created a severe problem for cosmic-ray researchers who had to rely on the naturally sparse production of strange particles in cosmic-ray showers. Note in the following discussion that Cosmotron researchers still took a long time to collect events despite the dual advantages of a much larger flux

of machine-produced particles and a continuously operating cloud chamber. Interview with Laurie Brown, August 28, 1986. Text references: Brown, Dresden, Hoddeson, eds., "Introduction," pp. 28-29 and Livingston, p. 110.

23. William Fowler, "Strange Particle -- Production by Cosmotron Beams as Observed by Diffusion Cloud Chamber," in Brown, Dresden and Hoddeson, eds. This source gives a more detailed account of the first experimental verification of associated production.

24. At the time physicists called many of the new discoveries "resonances" because they had significantly shorter lifetimes than "particles." In more recent times the distinction between particles and resonances has collapsed and physicists speak only of particles; here the term "particle" is employed to match current usage. Text reference: Livingston, p. 110.

25. Heilbron, Seidel, and Wheaton, pp. 76-81: Allan Franklin, "The Discovery and Nondiscovery of Parity Nonconservation," Studies of the History and Philosophy of Science,(1979), p. 209. Franklin provides a history of the discovery of parity nonconservation in weak interactions.

26. Alvarez, "Recent Developments," pp. 7-8; Heilbron, Seidel, and Wheaton, pp. 86-87; Peter Galison, "Bubbles, Sparks and the Post-War Laboratory," in Brown, Dresden, and Hoddeson, eds. Galison presents an excellent history of the hydrogen bubble chamber in "Bubble Chambers and the Experimental Workplace," in Peter Achinstein and Owen Hannaway eds., Observation, Experiment, and Hypothesis in Modern Physical Science, (Cambridge: MIT Press, 1985).

27. Heilbron, Seidel, and Wheaton, pp. 87-95; Alvarez, pp. 7-20, Peter Galison, "Bubbles, Sparks and the Post-War Laboratory," in Brown, Dresden, and Hoddeson, eds. Cosmotron work described in Jack Steinberg, "A Particular View of Particle Physics in the Fifties," in Brown, Dresden, and Hoddeson, eds.

28. Heilbron, Seidel, and Wheaton, pp. 81-83; Emilio Segre and Clyde Wiegand, "The Antiproton," Scientific American, 194 (1956), p. 41. Segre and Wiegand give a good account by participants of the discovery of the antiproton. Also see Owen Chamberlain, "The Discovery of the Antiproton," in Brown, Dresden, and Hoddeson, eds.

29. Robert Hofstadter, "The Atomic Nucleus," Scientific American, 195 (1956), 64-66. For more details about his Hofstadter's work during this period, see "A Personal View of Nucleon Structure as Revealed by Electron Scattering," in Brown, Dresden, Hoddeson, eds.

30. Quotes, respectively, from Charles Peyrou as quoted in Brown, Dresden, Hoddeson, eds., "Introduction," p. 13 and interview with Frederick Mills, July 13, 1984. Also see Brown, Dresden, Hoddeson, eds., "Introduction" and Pickering, p. 33.

31. Quote from Steven Weinberg, "The Search for Unity: Notes for a History of Quantum Field Theory," Daedalus, Fall 1977, p. 31. Also see Pickering, pp. 58; 57-59.

32. Pickering, 68; Clyde L. Cowan, Jr., Frederick Reines, F. B. Harrison, H. W. Kruse, A. D. McGuire, "Detection of the Free Neutrino: A Confirmation," 124 Science, (1956) p. 103; Kenneth Ford, The World of Elementary Particles," (Waltham: Blaisdell, 1963), pp. 139-141; and Frederick Reines "Detection of the Neutrino," in Brown, Dresden, and Hoddeson, eds.

33. For more detailed accounts, see William Chinowsky, "Strange Particles," Val L. Fitch, "The Tau-Delta Puzzle--an Experimentalist's Perspective," Robert E. Marshak, "Scientific Impact of the First Decade of the Rochester Conference (1950-1960), and Sam S. Treiman, "A Connection Between the Strong and Weak Interactions," in Brown, Dresden, and Hoddeson, eds. Text reference: Franklin, pp. 205-206.

34. Heilbron, Seidel, and Wheaton, p. 81 and Franklin, pp. 205-206.

35. Quote as quoted in Franklin, p. 208. Also see p. 208.

36. E. M. Hafner and Susan Presswood, "Strong Inference and the Weak Interaction," Science, 149 (1965) 506 and Franklin, pp. 210-211.

37. Hafner and Presswood, pp. 504-506.

38. Hafner and Presswood, pp. 504-508; "U.S. Major Accomplishment in High Energy Physics since 1945," author's copy, pp. 5; and private communication, Laurie Brown, February 24, 1988.

39. Pickering, p. 73.

40. Pickering, pp. 73-74 and Weinberg, p. 31.

41. Pickering, p. 74.

42. Pickering, p. 75 and Lillian Hoddeson, "Establishing Fermilab in the U.S. and KEK in Japan: Nationalism and Internationalism in High Energy Accelerator Physics During the 1960s,", unpublished manuscript, p. 25.

43. To gain more complete particle information physicists need accelerators of increasing energy because as beam energy increases, objects of decreasing size can be observed and objects of increasing mass can be produced. Brown, Dresden, and Hoddeson, eds., "Introduction," p. 14. Text quote from Lillian Hoddeson, "Establishing Fermilab in the U.S. and KEK in Japan: Nationalism and Internationalism in High Energy Accelerator Physics During the 1960s," unpublished manuscript, p. 24.

44. Interviews with Matthew Sands, May 29, 1987; Mark Oreglia, August 29, 1986; and Frederick Mills, July 13, 1984; Schweber, "Some Reflections on the History of Particle Physics in the 1950s,"in Brown, Dresden, Hoddeson, eds., p. 36; and Hoddeson, "Establishing Fermilab in the U.S. and KEK in Japan: Nationalism and Internationalism in High Energy Accelerator Physics During the 1960s,", unpublished manuscript, p. 16.

45. Perception of the Berkeley-Brookhaven elite is mentioned often in interviews with accelerator physicists. Examples can be found in the following interviews: Lillian Hoddeson interview with Robert Wilson, January 12, 1979, interviews with John Blewett, November 17, 1986; Robert Walker and Alvin Tollestrup, May 4, 1985; and Frederick Mills, July 13, 1984.

46. After their visit, the CERN delegation, which consisted of Odd Dahl, Frank Goward, and Rolf Wideroe, brought the news of strong focussing back to Switzerland and plans were quickly made to make the first CERN accelerator strong focussing. The impact of strong focussing on CERN is discussed in Armin Hermann, John Krige, Ulrike Mersits, Dominique Pestre, History of CERN, (Amsterdam: North-Holland, 1987), pp. 213, 273-282. Text references: Livingston, pp. 60-62; Brown, Dresden, Hoddeson, eds., "Introduction," pp. 24-25, and private communication, Lawrence Jones, July 9, 1987.

47. For more detailed accounts of the design and construction of the AGS, see John Blewett, "Accelerator Design and Construction in the 1950s," Ernest Courant, "Early History of the Cosmotron and AGS at Brookhaven," in Brown, Dresden, and Hoddeson, eds. and N. V. Baggett, ed. AGS 20th Anniversary Celebration, May 22, 1980, (Brookhaven: Brookhaven National Laboratory, 1980) UC-28. Text references: David L. Judd, "The Development from 1952 to 1960 of Planning for a Larger Accelerator," September 29, 1960, unpublished document, pp. 1-3, Lofgren Papers; interviews with William Wenzel, May 2, 1984; Edward J. Lofgren, May 3, 1984; and Edwin McMillan, May 16, 1984; and Livingston, pp. 70-73.

48. Livingston, p. 73 and Brown, Dresden, Hoddeson, eds., "Introduction," pp. 24-25.

49. 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. The author wishes to thank Lawrence Brown for his lucid summary of MURA's early development. Lawrence Jones, private communication, July 9, 1987.

50. As Brown, Dresden, and Hoddeson explain, although MURA scientists were the first to demonstrate a way to accomplish colliding beams, subsequently scientists at Indiana and (independently) at Princeton developed the concept of storage rings for achieving colliding beams. In fact the storage ring approach has been the method employed

for achieving colliding beams for both electrons and protons. Brown, Dresden, Hoddeson, eds., "Introduction," pp. 27-28. For more information on MURA technical accomplishments, see Donald W. Kerst, "Accelerator Developments of the Midwestern Universities Research Association (MURA) in the 1950s," in Brown, Dresden, and Hoddeson eds. Text references: private communication, Lawrence Brown, July 9, 1987, interview with Frederick Mills, July 13, 1984, and interview with David Judd, May 1, 1984.

51. Hoddeson, Hoddeson, "Establishing Fermilab in the U.S. and KEK in Japan: Nationalism and Internationalism in High Energy Accelerator Physics During the 1960s,", unpublished manuscript, pp. 28-29 and Greenberg, p. 217.

52. For more detailed information on the development of the accelerator at Stanford, see Robert B. Neal, ed. The Stanford Two-Mile Accelerator, (New York: W. A. Benjamin, Inc., 1968). Text references: Wolfgang Panofsky, "The Evolution of SLAC and its Program," Physics Today, 36 (1983), p. 35; Livingston, p. 110; and Greenberg, p. 236.

53. Greenberg, pp. 214, 218, 225; Brookhaven National Laboratory, "Brookhaven AGS," (Brookhaven: Brookhaven National Laboratory); Greenberg, p. 219; Livingston, p. 67, 110; and Sands, "The Making of an Accelerator Physicist," in Brown, Dresden, and Hoddeson eds.

54. This is only part of Pickering's analysis. He notes: "Each scientist has at his disposal a distinctive set of resources for constructive research. These may be material--or they may be intangible--expertise in particular branches of experiment or theory acquired in the course of a professional career . . . . these resources may be well or ill matched to a particular contexts. Research strategies, therefore, are structured in terms of the relative opportunities presented by different contexts for the constructive exploitation of the resources available to individual scientists." Pickering goes on to show how quark theory evolved in the 1960s from a particular intersection of interests. Pickering, p. 11.

55. Peter Galison, "Bubbles, Sparks and the Post-War Laboratory," in Brown, Dresden, and Hoddeson, eds., pp. 1-2; 53 and Sylvan. S. Schweber, "Some Reflections on the History of Particle Physics in the 1950s," in Pions to Quarks: History of Particle Physics in the 1950s, pp. 4-5.

56. Brown, Dresden, and Hoddeson, eds. "Introduction," pp. 24-25.

Chapter 3: Design Study Arrangements--1960-1963

Introduction

Edwin McMillan, the discoverer of phase stability, remembers that in late 1954 Enrico Fermi wanted to see him. Fermi, an eminent physicist some physicists called "the Pope," was on his deathbed, striken with cancer. When McMillan got to Fermi's home in Chicago, Fermi had an urgent request. As the founding of MURA indicated, a terrible rift was developing within the tight-knit brotherhood of physicists grouped around the nation's accelerator laboratories. Would he make it his task to patch things up? McMillan knew the problem: Midwestern physicists interested in exploring elementary particles felt they lacked the first-rate accelerator they deserved. "I told Fermi I would try my best." A few years later McMillan visited Dubna, a high energy research center near Moscow, as part of a small scientific delegation. Although this was the first opportunity in many years to renew contacts with Russian physicists, McMillan remembers best his encounter with fellow American Bernard Waldman. One night over drinks at the hotel, Waldman, who was usually soft-spoken and congenial, "got terribly mad at me. He said that Midwesterners didn't have a Nobel laureate to look out for their best interests. That was their main problem. He was so resentful that I could hardly talk to him." By then it seemed to McMillan that placating his Midwestern colleagues "was hopeless. . . . The lines were so drawn. I talked to people, but it did no good at all." (1)

Four years later McMillan got another commission, this time issued from the deathbed of Ernest Lawrence, soon to die of colitis. On July 1, 1958 Lawrence informed Clark Kerr, President of the University of California, that he wanted McMillan to become Radiation Laboratory deputy director, with the implicit understanding that the younger scientist would succeed him as director after his death. Kerr made the appointment the same day. (2)

McMillan, born in 1907, had grown up in Pasadena, California. Although his father and several uncles were physicians, the young McMillan, who showed an early passion for tinkering with electrical gadgets, resisted family pressure to enter medical school. "I was somewhat solitary," he remembers, "and I preferred something I could do all by myself." As a medical doctor, he would have to get along with people and "be a public man." Instead, he earned a bachelor's degree from the California Institute of Technology in 1928 and a Ph.D. from Princeton in 1932, both in physics. (3)

His star rose quickly. After graduation in 1932, the same year Lawrence achieved 1 MeV with a Berkeley cyclotron, McMillan came to Berkeley, first as a National Research Council Fellow, and subsequently as a permanent member of Lawrence's staff. At a time when cyclotrons were built from cast-off telegraph magnets and sealing wax, Lawrence was eager to promote young men who showed promise, not only in physics, but in designing and coaxing the balky new machines into operation. McMillan, who distinguished himself in the 1930s both in machine tinkering and in investigations of radioactive nuclides, became one of

Lawrence's "boys," as the laboratory director called them. Others so honored were Luis Alvarez, Glenn Seaborg, and Robert Wilson. In 1941 McMillan married Molly Lawrence's sister, Elsie, and became the brother-in-law of the now-famous Lawrence. (4)

By this time, McMillan and Philip Abelson had discovered element 93, later christened neptunium, and McMillan had noted the tell-tale alpha emission which led Glenn Seaborg to the discovery of element 94, plutonium. After serving in the wartime radar and atomic bomb projects, McMillan returned to Berkeley with the sensational announcement of his discovery of phase stability. By late 1948, his electron synchrotron began operation. In the 1950s, he served on the prestigious General Advisory Committee (GAC) of the AEC and became an associate director of the Radiation Laboratory. In 1951 he and Seaborg, who as young scientists had shared quarters at the Berkeley Faculty Club and confidences about element 94, also shared the Nobel Prize in chemistry for discovery of the first two transuranic elements. (5)

When McMillan in 1958 became director of the Radiation Laboratory, which was renamed Lawrence Radiation Laboratory (LRL) the next year in honor of Lawrence, he knew something of the challenge he faced in maintaining the laboratory's reputation in high energy physics. Even before Fermi's 1954 request he knew of the growing resentment of some Midwestern physicists, and he was accustomed to the bitter feelings that sometimes accompanied the rivalry with Brookhaven. Such tensions seemed familiar, in fact, for even in his first years at the Radiation Laboratory, he sensed that others were intimidated by Lawrence and

jealous of his success. He was also acutely aware of the glorious tradition at Berkeley, a tradition he had helped to build and hoped to continue. A colleague remembers that as LRL director McMillan seemed intent upon preserving Lawrence's attitudes and ideas. McMillan remembers approaching the job with some trepidation, feeling he could handle it "but not in the same way as Lawrence. . . . I was more the scientist type. . . .I told everybody I wasn't Lawrence." (6)

Two years after his appointment, McMillan learned that the PS at CERN was performing unexpectedly well. Inspired by the news and anxious to continue Berkeley's tradition as a leader in the exploration of high energies, he was eager to activate plans for a new strong focusing synchrotron at Berkeley. He was not alone in his enthusiasm for further exploration of high energies. More than a year earlier, Matthew Sands and Alvin Tollestrup from the California Institute of Technology (Caltech) attended a conference sponsored by MURA and began pioneering work on designing a "cascade" synchrotron to accelerate protons to an energy 10 times that possible at the PS or AGS. By 1960, researchers at a number of other institutions, including MURA and Brookhaven, were considering a variety of strong focusing accelerators with energies ranging from 10 to 1000 GeV. On August 12, 1960, McMillan convened a committee to begin serious planning for the next Berkeley high energy accelerator. Two weeks later he joined about 30 other physicists at an informal meeting at Rochester, New York for a vigorous discussion of the feasibility and desirability of a new high energy machine. (7)

Leaders of the American physics community began such discussions with confidence and expectations forged in the previous decade. While scaling up accelerator designs prompted great optimism and an onslaught of clever ideas, scaling up the non-technical aspects of the project proved more problematic. As proposals for the new high energy machine emerged in the next two years, both the physics community and the AEC struggled to find ways to plan an accelerator meant to serve the entire community of high energy physicists. Despite signs of change, arrangements for designing the 200 BeV accelerator were made in 1963 along traditional lines, with Berkeley researchers taking sole control of the design study with the expectation that LRL would construct and manage the accelerator. To understand how the new accelerator was conceived, it is necessary to address two questions. First, how did serious discussion of the next American high energy accelerator begin and how did this discussion affect accelerator planning? Second, how did Berkeley emerge in sole possession of an approved design proposal? (8)

Serious Discussion Begins

By mid-1960, the time was ripe to discuss the feasibility and desirability of a new high energy accelerator. By this time, the physics community was buzzing with the news that AGS and PS were producing beams that were easier to use and of higher intensity than expected. In addition, the preliminary design work at Caltech provided a focus for technical discussions. Also, against the backdrop of lively

interaction among U.S., Western European, and Soviet researchers in the High-Energy Physics Commission of the International Union of Pure and Applied Physics, the Soviets expressed interest in pursuing a proposal for a joint US-USSR accelerator project. This was one of several cooperative ventures in unclassified atomic energy research first discussed in a US-USSR information exchange agreement signed by AEC Chairman John A. McCone and his Soviet counterpart Vasily S. Emelyanov in November 24, 1959. In July 1960, McCone was contacted by Emelyanov who suggested: "it would be convenient to take advantage of the presence of our high energy physics specialists in the USA for the Rochester Conference to conduct a preliminary exchange of views" on the subject of accelerator collaboration. (9)

After learning of the Soviet interest following a visit to the USSR, Robert Wilson organized an informal meeting on his own initiative to discuss the desirability and feasibility of a new accelerator. Wilson, formerly one of Ernest Lawrence's "boys," along with McMillan and Seaborg, was now a professor at Cornell. As Emelyanov had suggested, the meeting, which was held on August 28, coincided with the prestigious Rochester Conference on High Energy Physics. In addition to Soviet scientists, the meeting drew a substantial number of top physicists from the United States and Europe. While Soviet physicists remained largely silent throughout the meeting, perhaps as a result of the mercurial diplomatic climate between the two nations, the lively exchange of views from other participants provides a revealing glimpse of accelerator discussions of the time. (10)

According to notes taken by participants R. F. Mozley and W. D. Walker, physicists first discussed the desirability of building the new machine. After some preliminary statements, Wilson introduced the eminent Werner Heisenberg, who remarked that cosmic ray data had shown little of interest at higher energies and therefore they might "find nothing of great interest" by going to higher energies with an accelerator. In his summary of the meeting, Wilson remarked that others saw disappointing cosmic ray evidence as "inconclusive" because of the limitations of cosmic ray detection techniques and the small number of events examined. He went on to quote a statement prepared by Oppenheimer: "The clearest reason for a super-high energy machine is the same reason that motivated the present generation [of] accelerators. . . we do not know what we shall find . . . ." He further explained that at the proposed energy it was "likely that essential novelty will appear. . . . We are confident that a knowledge of what does in fact occur in this domain will take us a long way" in understanding subatomic matter. Despite this pledge of general optimism for the research potential at higher energies, Wilson admitted that "the range of opinion between optimism and pessimism is fairly uniformly populated by physicists -- but shaded a bit toward those who are optimistically minded." (11)

Meeting notes reveal that discussion topics also included various designs for 100 to 1000 GeV machines and their cost, and possible experimental instrumentation. Design discussions focused on linear accelerators, colliding beam machines, and proton synchrotrons. Even MURA representatives, who felt their high intensity FFAG machine would

be funded in addition to the new high energy machine, were willing to give advice on the best route to high energies. For example, in his summary, MURA researcher Keith Symon judged that both proton and electron linear accelerators would be costly alternatives. Although colliding beams would provide more energy at the point of collision than fixed target machines, such machines required detectors with microsecond time resolution, which excluded the primary detector of the time, the bubble chamber. (See Colliding Beams; Bubble Chamber) In addition, current colliding beam schemes could not produce high-energy secondary beams, such as neutrinos, which were of particular research interest. (See Secondary Beams) "For these reasons," Symon concluded, "it is clear that colliding beam devices cannot compete with conventional accelerators in an energy range accessible to both." Physicists also considered scaling up designs along the lines of the AGS and PS. According to Wilson, the group agreed that for about "100 million dollars -- or at most 200 million -- it would be feasible to push the design of a conventional alternating gradient proton synchrotron to 100 Gev or even higher and this might also cover the first round of experiments." By pushing the necessary tolerances they could hope to attain "1000 Gev and at a cost of less than one billion dollars--really a bargain of course." (12)

The discussion on detection techniques was similarly optimistic. Previously, physicists had worried that detection in higher energy fixed target machines would be impossible since reaction products would emerge in a narrow cone. According to Wilson, discussion revealed that such

"fears were not well grounded" considering recent experimental advances, including improved Cerenkov counters and modified bubble chambers, and various schemes for particle separation. (See Cerenkov Counter) In the words of Mozley and Walker, the group subsequently agreed that measurement problems were "not insuperable." In light of the interest in building a scaled up strong focusing proton synchrotron and the optimistic outlook for devising workable detectors, Wilson summarized that the move toward higher energies was "both desirable and feasible." (13)

Wilson's views were shared by McMillan and many other leaders of the physics community. Although researchers had no specific indication, either from theory or cosmic ray data, of exciting results that could be found at higher energies, in 1960 speculation ran high. For example, Oppenheimer thought they probably would discover new unstable, heavy particles, while Geoffrey Chew and Steven Frautschi thought that by achieving energies that greatly exceeded particle masses "the truly simple underlying laws may be revealed." Although opinions varied, in light of the spectacular research success of the 1950s, many top American physicists believed that an accelerator of higher energy would surely produce both new discoveries and elucidation of the puzzles at hand. Moreover, previous experience made them confident that they could obtain funding for the new project, despite its unprecedented expense. This confidence contrasted sharply with the mood in Europe, where CERN physicists hesitated in requesting funding for a new accelerator before

the research potential of the PS had been more thoroughly exploited. (14)

Shortly after the meeting at Rochester, the physics community took an official stand on the desired future course of accelerator development. In October 1960, the Piore Panel, a joint panel of the President's Science Advisory Committee (PSAC) and the GAC was convened. Named for its chairman, Emanuel Piore, director of research at IBM, the Panel included McMillan and other prominent scientists and served as a powerful advisory board to government on high energy physics funding. In mid-December 1960, the Panel issued a report giving the physics community's official funding recommendations. (15)

Convened at a time when the Stanford Linear Acceleator Center (SLAC) proposal was stalled in Congress, the Panel strongly urged that the linear accelerator be approved. The Panel also judged that the high energy accelerator collaboration with the Soviets should continue with due caution for potential problems. When it came to recommendations for the machine itself, the Panel heartily echoed Wilson's sentiments: "It is clear that the need for higher energy can feasibly be met by proton synchrotrons of the alternating gradient type: intensive studies of such accelerators should be made with the expectation of embarking on a program of construction within the next few years. . . ." As in the 1950s, the influence of those interested in building high energy machines was clearly evident in advisory committees. (16)

Unfortunately for MURA, the Panel was less enthusiastic about the daring but unconventional FFAG. The major advantage of the FFAG was that it could produce intense secondary beams, allowing detailed exploration of decay properties and interactions of secondary particles. Although MURA had boasted in its 1958 proposal that a 15 GeV FFAG could produce proton beams that were "at least ten thousand times" more intense than those produced in existing multi-GeV high energy machines, the unexpectedly fine performance of the AGS and PS eroded this advantage. While AGS and PS beams were still about a hundred times less intense than those projected for the FFAG, the Panel noted that by increasing energy instead of intensity, physicists gained "the additional advantage of extending the range of study of all known primary and secondary phenomena and of adding the potential of new phenomena such as undiscovered particles." In view of such considerations, the Panel was unsure whether the program should include "a very high intensity proton accelerator. . . or whether the need can be more usefully met by extension of the energy parameter." After suggesting "early determination of the relative merits of these methods," the Panel judged that to accommodate the next generation of accelerators, the annual high energy physics budget might need to increase by as much as $200 million by 1970. (17)

In an unusual dissenting opinion, Eugene Wigner, an eminent professor at Princeton and long-time promoter of nuclear reactor development, complained that the majority report had exaggerated "the significance of high-energy physics," emphasizing that physicists did

not expect the field to "lead to 'practical results' in the near future." Pointing out that with new accelerator expenses the annual budget for high energy physics might rise as high as $400 million by 1970, Wigner worried that continued "unstinted support" would interfere with other more vital programs, especially by draining necessary manpower. Clearly, the cost of continued accelerator building was beginning to concern scientists with a professional and ideological interest in the development of reactors. (18)

Despite Wigner's misgivings, by the time the Piore Panel issued its December 1960 report, serious planning efforts for a new high energy machine were already underway at Caltech, Berkeley and Brookhaven, where physicists pursued the possibility of collaboration with the Soviets. In the next fourteen months, each institution would submit a proposal to the AEC asking to design a proton synchrotron in the 100 to 1000 GeV range. While the Piore Panel report paved the way for governmental support of such a project, its equivocal stand on the MURA proposal and Wigner's complaints provided early warnings of future debates facing those who promoted the expensive new project.

Summary: How Serious Discussion Began and the Effect on Accelerator Planning

Unlike CERN scientists, who wanted to further exploit the research potential of the PS before mounting plans for a new accelerator, leaders of the American physics community were ready to begin serious planning for the next high energy accelerator by the time

the AGS and PS proved the feasibility of strong focusing. By this time, accelerator experts were confident that they could build a conventional alternating gradient proton synchrotron to energies as high as 1000 GeV. Experience indicated that the tradition of rapid American accelerator development would continue, spurred by technological innovation, ample funding bolstered by the political value of accelerator development, and the self-reinforcing dynamics of the new data sure to be found in abundance with a more powerful machine. The 1960 Piore Panel report officially recommended that the government support planning efforts for a high energy accelerator project, paving the way for further accelerator development. At the same time, it issued an early warning of the pressures to be faced because of MURA and the rising expense of accelerator projects as the new project was being planned and organized.

A Design Proposal Emerges

While Berkeley, Brookhaven, and Caltech all presented design study proposals in the next fourteen months, researchers at Caltech had been working on their design the longest. Whereas planning for a design proposal for a new strong focusing synchrotron began at Berkeley and at Brookhaven after the feasibility of strong focusing had been proven in 1960, the Caltech effort began in the summer of 1959 when Caltech researchers Matthew Sands and Alvin Tollestrup attended a conference sponsored by MURA. According to the conference proceedings, the first week "was devoted to a survey of the accelerator art in order to determine the comparative costs and performance characteristics of various kinds of accelerators. . . ." At this point, before the proven success of the AGS and PS, many felt that colliding beams provided the only practical route to higher energies. Tollestrup remembers that in one workshop MURA researchers, sensing they had the advantage, issued a challenge: design a technically and economically feasible fixed target machine capable of producing the same energy at the point of collision (300 GeV) as that produced by MURA's colliding-beam FFAG accelerator. (19)

Sands, feisty by nature, took up the challenge with relish. In a June 10 paper, published in the conference proceedings, Sands investigated "briefly the technical feasibility and the economic scale of pulsed AG synchrotrons with maximum energies from 100 to 300 Bev." For the 100 GeV machine Sands considered a scaled up version of the Brookhaven AGS with a linear accelerator (or linac) that injected

protons into a synchrotron. To overcome the problems of linac injection and high magnet costs for a more powerful accelerator, he chose a more radical approach for the 300 GeV machine. (20) For this machine, Sands considered a linac that injected protons into a booster synchrotron that in turn cascaded protons into a larger synchrotron with a relatively small magnet aperture. Sands subsequently revealed that the cascading concept had been suggested to him by Wilson several years earlier; others had independently hit upon the idea of cascading beams from one circular machine to another, but the idea had not elicited great interest since such schemes required more beam control than many thought feasible. On the basis of rough calculations, Sands estimated "that the cost of either the 100 or 300 Bev is of the same order as a 10 Bev high-intensity FFAG accelerator." (21)

The conference proceedings cautioned that the machines discussed by Sands and other conference participants had been studied only briefly and "would need further careful engineering studies to provide a significant level of confidence in the accuracy of the performance and cost estimates." Although Caltech had never sponsored the design and development of such a large machine, Sands was determined to pursue his 300 GeV accelerator scheme. When he returned to Pasadena, he continued work on the design. In May 1960, Tollestrup and Robert Walker joined Sands in writing a report for Caltech administrators noting "that Caltech should initiate a project" for designing the next high energy accelerator. In spring and summer the three Caltech researchers

continued design studies and sought advice from accelerator designers at other institutions. (22)

By September 1960, Sands presented a more detailed exposition of his ideas in the Caltech report "A Proton Synchrotron for 300 GeV" which acknowledged helpful discussions with a number of other accelerator experts, including M. Hildred Blewett, Ernest D. Courant and Hartland Snyder from Brookhaven and Kenneth Robinson from the Cambridge Electron Accelerator, in addition to Tollestrup and Walker. The machine's estimated cost, prepared with the help of Hildred Blewett, was $77 million, including preparation of the site, salaries, and accelerator components. (23)

By this time, planning for a new accelerator design proposal had also begun at Berkeley, after some delay. Although Berkeley physicists and engineers had discussed ideas for a future accelerator with energies in the 100-150 GeV range as early as 1955, the laboratory did not submit a proposal for a new machine in the 1950s. Berkeley accelerator theorist David Judd explained in 1960 that some staff members had been thoroughly absorbed in the Bevatron research program while others had been uncertain which of many interesting new design approaches would prove most promising for the next Berkeley machine. (24) Despite Lawrence's tireless efforts in the 1930s and 1940s to build ever larger accelerators, several of Lawrence's colleagues, including Judd and McMillan, remember that he was unenthusiastic about building an accelerator more powerful than the Bevatron for the exploration of elementary particles. In a recent interview, Judd explained that in

conversations with younger colleagues Lawrence often expressed skepticism "about endorsing all these future pie-in-the-sky . . . giant accelerators for basic research alone." More interested in nuclear physics with its abundant practical applications than in high energy physics, Lawrence worried that physicists would fail with an expensive, big high-energy accelerator, discrediting the value of accelerators so that future projects could not get funding. (25)

A change in attitude came in March 1960, two years after Lawrence's death, when CERN Director John Adams visited Berkeley, bringing details of the superb operation of the CERN PS. Now excited about the prospect of building a large strong-focusing synchrotron, McMillan first reviewed the status of previous planning efforts and then convened an accelerator planning committee on August 12, 1960. As explained in a 1961 summary, LRL researchers began by "reviewing the principles of AGS machines" and "examining the design and construction details of the BNL and CERN accelerators." They next defined which features could be extrapolated directly and which would require additional development and extensive re-examination. (26)

The initiation of this painstaking study was the first indication that McMillan, a firm advocate of high energy physics, was willing to mount a design proposal for the next high energy accelerator, a step Lawrence had been reluctant to take. Despite this apparent departure from guidelines established by Lawrence, McMillan's action worked to preserve, rather than alter, the reputation Lawrence labored so hard to establish thirty years earlier. While Lawrence grew less interested in

campaigns for the world's most powerful accelerator, McMillan did not. Although the decision to begin planning for the next Berkeley accelerator echoed the enthusiasm of the young Lawrence, the design effort proceeded with a conservative spirit echoing Lawrence's mood later in life. With a tradition to preserve, Berkeley researchers proceeded with great deliberation and care.

In August 1960, a month before he issued "A Proton Synchrotron for 300 GeV," Sands learned of the newly formed Berkeley accelerator planning group and told Berkeley researcher Lloyd Smith that perhaps they should plan "what, if any, collaboration could be useful" between the two groups. In October, Smith, McMillan and Judd attended a regularly-scheduled information meeting at Caltech. There they encountered a number of researchers from other Southern California universities, including Robert Hulsizer, visiting from the University of Illinois, and Hildred Blewett, visiting from Brookhaven to continue cost estimates. In his meeting notes Smith revealed that the Caltech and Berkeley groups "agreed that as much interchange of ideas should take place as possible without setting up a formal connection, and that we should try to reach agreement on a single proposal for the West Coast." (27)

In the meantime, new accelerator plans were also brewing at Brookhaven. A few days after the August 1960 Rochester meeting, AEC Chairman John A. McCone wrote to Vasily S. Emelyanov asking whether Soviet physicists would be interested in further exploring the prospect of an accelerator collaboration. After McCone received an affirmative

reply, the AEC arranged a meeting at the American Institute of Physics in New York City on September 16. The meeting was attended by a delegation of Soviet physicists, led by Vladimir Veksler from the Joint Institute for Nuclear Research at Dubna, and a delegation of U.S. physicists, led by Brookhaven director Leland Haworth. During the meeting the group agreed that the two nations should sponsor individual national study efforts that could later cooperate. A "comprehensive intergroup discussion" was also planned for the upcoming International Accelerator Conference at Brookhaven in summer 1961. In order to avoid competition with ongoing national efforts, participants agreed that the international accelerator should have an energy over 300 GeV. (28)

A group at Brookhaven was subsequently assembled under the direction of John Blewett (Hildred's husband) to prepare a preliminary design study for the U. S. part of the collaboration. Blewett, an experienced accelerator expert, joined the Brookhaven staff in the laboratory's first year after wartime work on radar and postwar studies in synchrotron radiation at General Electric. After working on the Cosmotron, he helped lead the Brookhaven AGS effort and, with Hildred, spent 6 months helping with plans for the CERN PS. Enthusiastic about the new project, Blewett and his group began making designs for accelerators with energies as high as 1000 GeV. (29)

In the first few months of 1961, individual planning efforts continued at Berkeley, Brookhaven, and Caltech, with communication among all three groups. In addition to the planned discussion at the International Accelerator Conference at Brookhaven, which would focus on

accelerator development, by early 1961 researchers at Berkeley were planning a summer meeting which would further consider experimental and theoretical questions. Sands planned to attend both. (30)

As early as May 1960, Caltech researchers talked of collaborating with others since accelerator designers were in short supply in Pasadena. In January 1961, they accordingly formed a collaboration of several universities, which was christened the Western Accelerator Group (WAG). Despite the sentiments reported by Smith in October 1960, Berkeley did not join WAG. In a March 1961 letter, Sands explained to Blewett, who was clearly eager to obtain advance warning of a Berkeley/WAG collaboration, that they were "still working independently but in close communication with Berkeley." (31)

In April, WAG, which included representatives from Caltech, the University of California, Los Angeles, the University of California, La Jolla and the University of Southern California, submitted a proposal to the AEC for $593,000 to support a fifteen-month study of the design for a new proton synchrotron. They explained that the money would "provide support for the studies now in progress of the feasibility of accelerator concepts and for the initiation of those engineering and architectural studies necessary for arriving at a definitive design of an accelerator and for providing realistic estimates of costs and time schedules." (32) The proposal requested that funds be made available through a supplement to the existing Caltech contract and explained that the project would be directed jointly by Sands and Robert Bacher, a former Los Alamos division leader and AEC commissioner who was now

chairman of the Caltech Physics Department. The proposal also included Sands' original September 1960 report, plus 11 more recent reports which extended and further supported Sands' original ideas for a 300 GeV cascade synchrotron. (33)

In April 1961, the AEC also received a proposal for design funding from Berkeley, submitted as part of the LRL's yearly budget request. The proposal explained that in line with "its traditional interest in high energy physics" the laboratory had kept abreast of technical developments with the two alternating gradient accelerators at CERN and Brookhaven and now felt that "the quickest and easiest way to extend available energies" was to build an expanded alternating gradient synchrotron. LRL proposed making "a detailed study of such a machine in the 100-300 Gev range. The initial study would include feasibility, cost, possible sites, experimental facilities, and organizational considerations." The proposal requested a total of $3 million for a two-year period, which would allow "definitive determination of accelerator and site requirements" so that a formal proposal could be submitted. The budget submission also detailed the necessary design research, including extensive injector studies "directed primarily toward the development of a proton linear accelerator. . . ." (34)

The two proposals confronting the AEC reflected important differences between the two design efforts. In many ways, the WAG proposal was more attractive. It was more formal and complete than its Berkeley counterpart, presented design ideas that had been studied longer and more extensively than Berkeley ideas, and introduced an

injector scheme that was more innovative than that presented by Berkeley. In addition, WAG asked for far less money for the design study and stressed economy in the construction of the machine. LRL, however, had a solid reputation and experience in the construction of large accelerator projects, unlike Caltech or any of the other WAG universities. One of the reasons for the high LRL funding request was that at LRL planned to draw on the talents of its large in-house staff of trained accelerator experts to make a very detailed design and, therefore, included 105 man-years of effort, while WAG estimated only 10 man-years of effort, planning to augment the small nucleus of WAG designers with outside experts who would obtain some support from their home institutions. Another reason for the high LRL total was that the laboratory planned to construct prototypes of major components, including the radiofrequency system (which accelerated the particles) and magnets (which would bend the particle beam so that particle would travel in a circular path). Thus the LRL proposal included $500,000, only $93,000 less than the entire WAG request, just for prototype hardware. The WAG proposal promised an innovative, relatively inexpensive design study centered around a small group of physicists, including the energetic and talented Sands. In contrast, the LRL proposal promised a methodical, relatively thorough design study carried out by a large team of experts, including highly respected veterans of the Bevatron effort, such as Lloyd Smith and Edward Lofgren. (35)

An April LRL memo gives an additional glimpse into the thinking of Berkeley leaders. Two weeks before LRL submitted its proposal to the

AEC, Lofgren summarized his views of the project for McMillan. Lofgren, a stalwart member of the LRL staff, had worked as a graduate student at Berkeley and at Los Alamos on the atomic bomb project during World War II, and after a brief postwar stint at the University of Minnesota, returned to Berkeley to help construct the Bevatron. Now head of an experimental group in high energy physics, he also had responsibility for the Bevatron and advanced accelerator studies. After stating the physics community's need for a new high energy accelerator to continue particle exploration, Lofgren pointed out that the Bevatron would soon no longer be a unique facility since "governments of this and other countries have built up many new large laboratories more or less in the image of LRL at the demand of scientists all over the world." To keep its front-ranking position in high energy physics, LRL needed a new high energy accelerator. Lofgren saw that such an accelerator could be built by Berkeley, Brookhaven, or a new institution. Since choosing a new organization would delay the project by several years, because of time spent gathering the necessary personnel and creating a smooth functioning operation, Lofgren judged that the only reasonable candidates for building the machine were Berkeley and Brookhaven. Lofgren felt that a strong case "in terms of history, demonstrated competence and organization" could be made to the AEC for choosing LRL over Brookhaven so that Berkeley could get the accelerator it needed. (36)

Many of Lofgren's sentiments reflect recurrent Berkeley themes. Just as Lawrence had stressed the laboratory's pre-WWII reputation when

arguing for the most expensive projects of the postwar era, LRL leaders in the 1960s stressed the laboratory's reputation when seeking funds for the most expensive project of the new decade. Also, in line with past experience, LRL physicists tended to see Brookhaven as the laboratory's only important competitor. Although WAG researchers had been working on a design longer and had independently produced a series of technical reports and a well-documented formal proposal, LRL physicists tended to discount them, as in Lofgren's memo, or to speak of the WAG effort as if it were a part of the Berkeley effort. For example, the minutes from an internally circulated accelerator planning meeting held in February 1961 speak of an "international group," led by Brookhaven, and a "California group," which presumably included both WAG and Berkeley. (37)

Both LRL and WAG researchers were aware that the proposed accelerator project would require new procedures because of its size and expense. In his April memo, Lofgren remarked that because of the scope of the new accelerator, "there is no doubt that the accelerator should be a national facility." Similarly, the WAG proposal stated: "An accelerator in the hundred-Bev class would necessarily be a national facility serving the national scientific community." While Brookhaven was established as a regional facility, the idea of a facility created to serve the needs of the entire community of high energy physicists was new. As Lofgren explained in a recent interview, the notion was "in the air" in the physics community at the time, inspired by the realization that physicists could hope to obtain funding for only one such machine. (38)

Although the WAG proposal did not elaborate on the new idea, Lofgren noted that the facility could be made national by organizing an accelerator council composed of representatives from top universities and laboratories with expertise in high energy physics. This council, which should have approximate geographic representation, would report to the director of the Laboratory, acting as his advisory board. In Lofgren's scheme, LRL would still build and operate the accelerator. Since the machine was too large to construct at LRL, Lofgren stressed the importance of finding a site close to Berkeley, acknowledging that LRL physicists might have to go as far as 1000 miles from Berkeley to find a suitable site. (39)

While Lofgren's memo anticipated future management and siting issues, the WAG proposal anticipated a more immediate problem prompted by the size and expense of the new project. The proposal remarked that designs were currently in "a formative and fluid state" that properly encouraged "independent initiative and originality. . . ." Nonetheless, "duplicate <u>detailed</u> design efforts" would not be justified for a single accelerator. In fact, the AEC wondered whether two independent design studies were justified for the expensive project even at this stage. Shortly after receiving the two proposals, the Commission decided to withhold approval of all accelerator design studies, including other smaller design proposals, until new procedures were created for handling the situation. In the wake of the AEC decision, McMillan and Bacher jointly sent a letter to Paul McDaniel, Director of the AEC's Division of Research, in an attempt to make the dual design studies more

palatable to the AEC. The letter explained that a steering group had been established to further coordinate LRL and WAG efforts and pledged "to work toward the objective of a single proposal for the construction of a high energy accelerator to be submitted to the Atomic Energy Commission." (40)

Still uncomfortable with the situation, the AEC asked twenty-three prominent physicists to review the two proposals. As McDaniel explained when asking for Oppenheimer's review, the Commission was sympathetic to the proposed studies, but worried that dual funding "might tend to encourage the establishment of rival claims for an ultimate accelerator." Such action seemed "especially unwarranted in view of the close-geographical proximity of the two groups in question." (41) When AEC representative George Kolstad visited Caltech in late June 1961, he reported that, in general, the reviewers favored continuing with the studies. Anonymous excerpts of the responses revealed, however, that many disapproved of dual studies. In the words of one reviewer: "This country can only afford one 300 BeV accelerator and indeed only one 300 BeV study group." In July McDaniel passed the word to Caltech and LRL through the local AEC office that all design studies "involving a 'national' decision" would require special Commission approval. The expense of the new facility had already prompted new pressures. As the AEC contemplated how to adjudicate an accelerator request of such magnitude, LRL and WAG physicists faced the difficult task of combining two very different design efforts. (42)

In the meantime, summer studies were convening at both Berkeley and Brookhaven. Although there was some overlap between the two meetings on the new accelerator, in general the agendas were complementary, with discussions at Brookhaven centering on accelerator design and discussions at Berkeley centering on experimental techniques for the proposed energy range as well as the expected data and its implication to theory. The Berkeley meeting, which lasted from June to August, provided an in-depth examination of experimental issues considered at the August, 1960 Rochester meeting. This more thorough examination of the experimental utility of the new machine was important, both for building a case to government for funding the machine and for bolstering support within the physics community. Although the group, which included about 30 physicists from various institutions in the U.S. and Western Europe, focused on experimental and theoretical questions, accelerator experts were on hand to advise on the capabilities promised by current designs. Sands, Walker and Tollestrup from Caltech and Hildred and John Blewett from Brookhaven were invited. (43)

In her summary of discussions on experimental issues, Sulamuth Goldhaber (sister-in-law to Brookhaven physicist Maurice Goldhaber) reported that prospects looked promising for producing good quality secondary beams and transporting them to experimental areas. She also noted that detection by current means "such as combinations of Cerenkov counters, seems possible, though awkward, for energies up to 100 Bev, and can certainly be used if better methods are not available." She pointed to a promising new detector, xenon scintillators, which were

undergoing tests at the AGS, and mentioned various particle separator schemes currently under investigation. (See Scintillation Counters) Considering the "inevitable inventions of the next decade," Goldhaber concluded, physicists felt "considerable confidence in the feasibility of significant experiments at much higher energies than presently available." When reporting on the research promise of a higher energy machine, Geoffrey Chew was equally sanguine. He explained that by exploring the new energy range physicists could hope to "discover new symmetries and new conservation laws" in strongly interacting particles. Such exploration could perhaps even uncover "hidden symmetries" in the strong interaction that were obscured in lower energy data. (See Conservation Laws) Such findings would help untangle the confusion in strong interaction theory and determine whether some particles among the flood of new data could be considered really "elementary." Chew also explained that weak interaction theory, which he termed "both incomplete and inconsistent" would also benefit since the new machine would be able to produce a larger quantity of neutrinos than existing accelerators. (44; See Fundamental Interactions)

Brookhaven discussions on accelerator design, which included a report of the Berkeley meeting, began in late July and extended through early September. Included among the roughly two dozen participants were Lofgren, Smith, Judd, several others from LRL and Sands, Tollestrup, and Walker from WAG. Although the meeting also included representatives from several other U.S. and Western European laboratories, Soviet researchers, who had not responded to earlier inquiries about their

progress, did not attend, perhaps in response to the Berlin crisis which broke out in the summer of 1961. (45)

As Blewett reported in September, the mood at the meeting was positive nonetheless, and participants enthusiastically discussed a number of new technical ideas. In line with previously reported WAG results, physicists discussed the use of smaller magnet apertures, which would allow the use of smaller, less-expensive magnets. In addition to cascade injection, as suggested by Sands, researchers also discussed injection with a linear accelerator as well as injection from a ring designed by MURA. Based on a new understanding of orbit theory, Thomas Collins from the Cambridge Electron Accelerator (CEA) proposed leaving empty straight sections between main ring magnets that were much longer than those previously used to allow more room for experimental use. Participants also discussed possible radiofrequency systems, including the use of ferrite-tuned cavities, as suggested by Quentin Kerns at Berkeley, and a scheme for constant-frequency acceleration in short linac-like units, suggested by Kenneth Robinson at CEA. Other innovative ideas included schemes for adding storage rings to the facility as well as an electron synchrotron in the proton synchrotron tunnel. (46; See Synchroton-Acceleration; Storage Rings)

On the basis of the extensive summer study discussions, in August Brookhaven researchers presented a revised version of a previously issued design report. The August report began with a series of papers edited by Luke C. L. Yuan on the research potential of the new machine. In his summary, Yuan presented evidence showing that secondary and

scattered particles would emerge in a very narrow cone if particles were accelerated above 300 GeV. Like Sulamuth Goldhaber, he stressed that current techniques could be extended to higher energies and pointed to numerous promising new ideas for beam separation and detection. (47)

The second part of the report, which was edited by Blewett, explained various design considerations for the new machine and presented a preliminary design for a 1000 GeV alternating gradient proton synchrotron. Although final decisions had not been made about the type of injector or radiofrequency system to be used, Blewett gave rough estimates of some parameters. Due to experience with the AGS, for example, BNL designers had decided to drastically reduce the amount of steel used in magnets, which were less than half the weight per unit length of AGS magnets. Blewett estimated that the proposed 1000 GeV machine would cost $675 million, and by interpolation concluded that similar 700 GeV and 300 GeV machines would cost approximately $500 million and $300 million, respectively, including accelerator components, buildings, site preparation, salaries and a 10% contingency. In his conclusion, Blewett characterized the report as "very brief and qualitative," noting that "a real machine design will emerge only when a group of competent physicists and engineers is assembled for a full-scale attack on the problem." In the midst of exciting design discussion, Brookhaven researchers, who showed no signs of abandoning their accelerator plans, despite the lack of Soviet cooperation, were eager to assume just such an attack. The U.S. now had another vigorous independent design effort for a new high energy machine. (48)

Although no major technical obstacles had emerged, the last four months of 1961 were tense for the three design groups. Support from the 1960 Piore Panel made it likely that a design study would be funded. Nonetheless, the recommendation from this advisory board did not guarantee funding, as shown by the recent trouble SLAC had faced with Congressional approval despite support from the 1958 Piore Panel. The situation was all the more muddy since the AEC had not yet clearly defined its position. It was therefore important for physicists to present a united front to support the project. At the same time, each study group had individual interests to protect. Although researchers felt free to discuss technical issues, such discussions could not solve the most pressing problems. To mediate management disagreements and present a mutually acceptable plan for proceeding with the design study, those planning designs needed a forum for discussion among all three parties. Unfortunately for the anxious planners, they felt pressure from the AEC to postpone planning discussions between groups on the East and West Coast until the AEC had decided how to proceed. (49)

WAG and LRL researchers convened the steering committee, as promised, and meetings were held in September, October, and November, 1961. In the September meeting researchers agreed on technical assignments: Berkeley would focus on linac injectors and the radiofrequency system, WAG would concentrate on synchrotron injectors, and both groups would work on site issues and magnet design. To address more pressing topics, the October and November meetings were devoted almost entirely to the organization of planning efforts. After reviewing

their knowledge of AEC deliberations at the October meeting, McMillan and Bacher guessed that "some sort of national committee and/or national study effort" would be proposed. Researchers also considered various possible LRL/WAG collaboration schemes, agreeing that the working design group should "have a central location with broad provision for national participation. . . ." Discussion at the November meeting focused on the wisdom of instigating discussions with Brookhaven. During the meeting, McMillan phoned an AEC representative and found that the Commission felt a meeting with Brookhaven would be "untimely." According to the meeting minutes, the meeting was subsequently adjourned without setting a new meeting date because researchers felt "that external events would probably dictate the time and purpose of further discussion." (50)

Despite the discouragement of the AEC, only a few days after the WAG/LRL steering committee meeting, Berkeley and Brookhaven researchers made several long telephone calls to discuss current opinions and plans for the next accelerator. After receiving permission from McMillan, Smith called Brookhaven researcher Charles Falk. Falk remarked that he had spoken to Leland Haworth, who had left his post as Brookhaven director several months earlier to become an AEC commissioner. Falk now felt sure that the AEC would announce that "the machine should not be at any existing laboratory and that the site is a wide open national question." After this insight into AEC thinking, which signaled a drastic change from previous practice, the two researchers discussed plans for collaborative design studies. Like LRL and WAG researchers, Brookhaven physicists had discussed the idea of a centralized study.

Although "the prevailing opinion" at Brookhaven was that such a scheme was currently "impractical," they saw four potential locations for such a study: Berkeley, Brookhaven, Caltech, and MURA headquarters at the University of Wisconsin. Neither Falk nor Blewett favored the MURA location and Smith reported that Bacher had expressed doubt that Caltech could provide the necessary facilities. Although each scientist loyally argued that his institution would serve best as the central location, they both agreed to drop the "explosive subject." (51)

Falk had begun the conversation by saying that the Brookhaven group had hoped to arrange a meeting "to exchange views on policy regarding a study, hoping to come to a jointly held position." Nonetheless, as the disagreement between Falk and Smith indicated, achieving a mutually acceptable position was bound to be complicated in light of the long-standing competition and conflicting interests of Berkeley and Brookhaven researchers. Although Falk explained that the new Brookhaven director Maurice Goldhaber had "no strong convictions" about how the design studies were organized, in a later conversation Blewett told Smith that AUI, Brookhaven's management group, was "beginning to get the fever for a big machine near them." Intensified interest for another Brookhaven machine and the new complication that MURA wanted to develop an FFAG injector for the machine made the need for intergroup discussion all the more pressing. Blewett, who according to Smith seemed "genuinely concerned with preventing an east-west civil war," was agreeable to coming to Berkeley for further Brookhaven/Berkeley negotiations, but later called to say that Goldhaber had decided to

postpone such a meeting until they got more information from the AEC. (52)

In the midst of continuing pressures within the physics community, however, a meeting was convened at the University of California at Los Angeles in late December to assemble representatives from BNL, LRL, WAG and MURA to discuss further design study plans. According to notes taken by Berkeley engineer Hayden Gordon, the meeting began with short discussions by Bacher and McMillan emphasizing the importance of campaigning for the new accelerator within government and the physics community. Researchers then turned to a discussion of the objectives and organization of the proposed design study. Participants quickly agreed that the study group should prepare a design and cost estimate adequate for submission of a proposal. Next, Blewett presented an organizational plan for the study and without too much debate participants agreed that there should be a central study group at an existing laboratory led by a single director responsible for organizing and coordinating all research, including contributions from outside groups. Participants also agreed to a plan reminiscent of that presented in Lofgren's April memo. As in Lofgren's plan, the leader of the study group would be advised on policy issues by a committee of accelerator experts with nation-wide representation. In contrast to Lofgren's plan, however, in which the committee reported to the laboratory director, it was now agreed that the committee would report to the AEC. (53)

With this settled, according to Gordon, participants turned to the question of where the design study should be located. Convinced that

they didn't stand a chance against the large and politically powerful groups at Berkeley and Brookhaven and unsure of Caltech support, WAG researchers refrained from making a bid for hosting the study. However, representatives from MURA, Brookhaven, and Berkeley each presented eloquent arguments for why their respective home laboratories should be chosen and intense debate ensued. Although Berkeley and Brookhaven researchers quickly dismissed MURA claims, prolonged debate between BNL and LRL raged. Brookhaven physcists argued that they were the only national experts in strong focusing synchrotrons and could participate only if Brookhaven was the central site since the effort would otherwise detract from AGS development. As Gordon reported, Berkeley representatives contended that only their laboratory possessed the necessary resources to host the study since Brookhaven was involved in such development. The meeting ended with no resolution to the conflicting claims of Brookhaven and Berkeley; as Lofgren had anticipated, eventually the competition narrowed to the traditional rivals in accelerator building, just as it had in the past. (54)

In February 1962, both Berkeley and Brookhaven reinforced their claims by submitting proposals to the AEC asking to be named the center of the design study for a new high energy machine. Berkeley researchers requested funds to produce a plan "for detailed engineering and construction of an AGS accelerator in the energy range of several hundred Bev." They planned in-depth investigation of magnet, radiofrequency, linear injectors and site requirements. After noting that Caltech researchers had agreed to join the Berkeley effort, their proposal outlined the organization for the central study, presenting a plan that matched that accepted at the December meeting, with the minor exception that LRL researchers added a technical steering committee to be drawn from the advisory committee and those working on the design. The proposal then advanced five reasons why LRL should be selected as the center of the study: the long, productive history of the laboratory in blending the talents of engineers and physicists to build major accelerators, the wealth of experience at the laboratory in scientific collaboration, the laboratory's extensive experience in managing large scientific efforts competently and reliably, the availability of shop space and the attractive and easily accessible site of the laboratory, and the availability of a large number of trained accelerator experts. LRL estimated that it could carry out its duties as the central site for the study for $3 million and finish the task in 2 years, echoing the cost and time estimates given in April. The proposal also mentioned that an additional $1 million might be needed by subcenters, but

refrained from requesting such funding until the scale of subcenter activity had been more carefully determined. (55)

The Brookhaven proposal requested that Brookhaven be the center of a national design study for an accelerator in the 300 to 1000 GeV range. It placed less emphasis on engineering design and more emphasis on theoretical accelerator physics issues than its Berkeley counterpart. In addition, it was wider in scope. Expanding on the earlier suggestion that an electron synchrotron could be incorporated in the design, Brookhaven researchers remarked that "it might be possible and useful to attempt acceleration of electrons, deuterons, anti-protons, etc., in the same structure." Researchers also planned to explore schemes for accelerating protons to energies as high as 1000 GeV. Despite these differences, Brookhaven planned to proceed with design studies along much the same path that Berkeley traced. As in the Berkeley proposal, the organization of the study matched that accepted at the December meeting. Within this structure, Brookhaven would coordinate all studies, including outside contributions from such facilities as Caltech and Berkeley, and present a proposal for constructing the machine. Although Brookhaven, fifteen years younger than its West coast rival, did not stress its long history, it evoked Berkeley's other four arguments in its own behalf (experience in scientific collaboration, experience in managing large efforts, availability of on-site resources, and availability of accelerator experts). In addition, the Brookhaven proposal argued that BNL had the largest team of physicists with expertise in strong focusing, that the proximity of the AGS would aid in

tests and measurements, and that the laboratory, with its consortium management, had "developed the broad views and cooperative attitude expected of a National Laboratory." The proposal estimated that the ambitious Brookhaven study would take three years to complete and cost $4 million. (56)

Competing proposals from Berkeley and Brookhaven brought a new wave of concern at the AEC. In February 1962, for the first time, the problem of competing claims for the new machine was discussed between Jerome Wiesner and Glenn Seaborg, the current presidential science advisor and AEC chairman, respectively. As Seaborg's records reveal, Wiesner pointed out that "bad feelings" were brewing at the "large laboratories," and warned that steps needed to be taken "cautiously, but fast" before things got "out of control." Seaborg pointed out that Haworth had been handling the matter for the Commission, and then mentioned a ticklish aspect of the problem for the AEC. Although Seaborg was a chemist, not a high energy physicist, he had previously shared residence at the Berkeley Faculty Club as well as the Nobel Prize with McMillan and had a large number of close associations as a former LRL staff member at Berkeley. In addition, Haworth, who had previously served as one of the leaders of the design team for the Brookhaven AGS and as Brookhaven director, had many close associations at Brookhaven. Wiesner remarked that "the study group should be at Berkeley," adding that "no decision" should be made "on the location of the apparatus." Despite Wiesner's clear opinion about the desired location of the design study, an opinion Seaborg likely shared, the situation required that the

AEC make a well-considered, defensible choice between the two laboratories. Until Mid-1963, Paul McDaniel, Director of the Division of Research, would continue to handle the ticklish situation. Nonetheless, it was already clear that the project that would ultimately claim so much of Seaborg's time was escalating in importance. (57)

McDaniel was a tall, quiet man, trained as a physicist, but skilled as a bureaucrat. On April 19, he sent identical letters to McMillan and Goldhaber and slightly revised versions to 28 other scientists. In the letters McDaniel acknowledged that the AEC had received proposals from Berkeley and Brookhaven. He then inserted a policy statement that echoed what Falk had heard from Haworth:

> With respect to construction and operation of the accelerator. . . it should be noted that the means selected for conducting the study program certainly do not, and cannot, commit the Federal Government to the location or mode of operation of such a facility if it were authorized. We concur, however, in what appears to be the general belief that the scope of the final project, if authorized, would be such that its management necessarily should be separate from the management of any other laboratory. Assuming the acceptance of this principle, the means by which the facility would be operated as a National facility would appear to be a question of method, and well might involve the use of a National organization especially formed for this purpose. (58)

In the remainder of the letter, McDaniel presented ideas for organizing the design studies that the AEC Division of Research was considering for presentation to the Commissioners, asking for comments on these as well as for alternate schemes. He first mentioned the familiar notion of a centralized design effort under the direction of a study leader, noting that "at least 30 to 40 percent" of the work should

be conducted by outside laboratories. He then suggested that a technical committee be formed to advise the design study director on technical issues, noting that an advisory committee might also be needed for policy advice. In closing, McDaniel carefully reminded his correspondents that: "It is clear that a large new accelerator facility for energies of several hundred Bev will receive authorization only if the scientific community is in general agreement on the need for such a facility." (59)

During the next several weeks both McMillan and Goldhaber responded to McDaniel's letter. McMillan reported that he agreed with the general approach to organizing the study, noting that in general it followed the principles discussed at the December meeting and included in both the Brookhaven and Berkeley proposals. He objected, however, to setting the amount of outside participation at 30-40%, insisting that this should be settled later based on the amount of interest shown by other groups. Similarly, he suggested that arrangements for management of the laboratory be left open. Although he acknowledged that the AEC would "always have the power of decision in these matters," he noted that it might be most efficient for an existing laboratory to build and operate the accelerator, at least in the early stages. McMillan also remarked that if Brookhaven were chosen as the location of the study, few Berkeley researchers would be likely to locate there, noting that in that case it was doubtful that much work would be done at Berkeley since "the main part of the accelerator is so much of an integrated whole that

parts of the design can not be split off without serious loss of efficiency." (60)

Although Goldhaber, like McMillan, agreed with the general approach outlined by McDaniel, his response was otherwise quite different. He applauded the idea of having 30 to 40% of the work performed by outside laboratories, proclaiming that Brookhaven "would strongly oppose any suggestions which would tend to reduce this degree of multi-participation." Moreover, despite earlier comments that Brookhaven could not get deeply involved unless the study was located at BNL because of interference with the AGS effort, Goldhaber stated that the laboratory expected "to be deeply involved in the study regardless of its location. . . ." (61)

The contrasting responses to McDaniel's letter provide early evidence for the relatively conservative approach McMillan would consistently take toward management of the new laboratory. Goldhaber was willing, at least when dealing directly with the Commission, to adjust his stand on the design study in line with the new AEC guidelines for siting and management. McMillan was less flexible. He could accept a combined design effort with the relatively small WAG group and a national facility organized along the lines suggested earlier by Lofgren, in which the laboratory director served with the guidance of an advisory committee. However, McMillan balked at accepting changes that would too radically alter the traditional prerogative of a laboratory director to control the construction and operation of an accelerator being designed by his staff.

Goldhaber's approach was influenced by important differences, both between him and McMillan and between Berkeley and Brookhaven. While the new Brookhaven director was also an eminent physicist, unlike McMillan he had never had a particular interest in machine design. Delegating most of the responsibility to Cosmotron/AGS veterans G. Kenneth Green and Blewett, he was much less personally involved with the project than McMillan, a position that probably allowed him to be more open-minded about new AEC views. Brookhaven also differed from Berkeley in important ways. Not only was the Eastern laboratory younger, but perhaps more important, it had consortium management and had been organized to serve outside users. Considering his reputation and influence, McMillan's personal involvement in the project undoubtedly enhanced Berkeley's chances for winning approval to design the machine. His tradition-bound stance on management of the new accelerator was, however, out of step with current AEC notions of what was appropriate for an expensive national facility. This suggested that LRL was in a poor position to boast to the AEC, as Brookhaven had in its proposal, that the laboratory had developed the cooperative attitudes appropriate for a national Laboratory.

An August 1962 AEC summary of the other responses circulated by McDaniel to the Commission reveals that correspondents overwhelmingly supported the idea that the design study be funded and, in general, agreed with the proposed plan for organizing the design study. While several letters suggested that there be two focal points, and a few others favored neutral sites, most cast a vote either for BNL or LRL.

"Of those who expressed a choice between LRL and BNL," the summary revealed "the choice was 6 - 2 in favor of LRL serving as the focal point (not counting the views of Drs. Goldhaber and McMillan on this point)." The summary explained further that LRL was chosen over BNL chiefly because correspondents thought "that BNL should continue to be heavily occupied with getting the AGS into full utilization, while LRL should have a greater strength of engineering talent available for the study program." (62)

After noting that the preferred course of action--establishing the laboratory at a new site under the direction of a geographically balanced management organization--was precluded by the difficulty of obtaining the required lump sum from Congress, McDaniel agreed with the majority of its correspondents, stating that the design study should be conducted at an established laboratory, preferably LRL. McDaniel remarked, however, that if a major laboratory like LRL was given central responsibility for the study, it became especially "important to have made provisions which will insure that the study program have a national character." (63) McDaniel went on to emphasize that such provisions should encourage wide-ranging national participation and also make clear that the location of the national design study would not necessarily dictate the site or management of the facility. McDaniel then recommended that:

> a special High Energy Accelerator Advisory Committee, with a broad National Representation, to consider the broad implications of the study program both initially and throughout the course of the work, and to advise the Commission with

> respect to the appropriate activities of the various laboratories and other groups which may effectively participate in the program." (64)

In contrast to these official pronouncements, Lofgren's notes of an informal meeting that he and McMillan had with McDaniel and his staff on September 26 reveal that the AEC was willing to tolerate a more old-fashioned arrangement. At the meetings, which were held to gather information for the AEC's presentation to the Bureau of the Budget (BOB), McMillan and Lofgren were told that the AEC planned to ask for funding for a "Western Regional" accelerator. According to Lofgren, after some "sparring" over the definition of the term, staff members "did not disagree with our idea that this meant an LRL designed, built, and operated accelerator with safeguards to insure proper use. . . ." This turnabout on the part of McDaniel suggests that Lofgren grossly misinterpreted the discussions, which is unlikely, or that McDaniel was willing to be more flexible in action than on paper. Clearly, McDaniel realized that the AEC needed to formally reserve the right to decide the site of the accelerator. This was standard practice for large construction projects and Wiesner had already indicated the political necessity of enforcing such rules in this case. At the same time, in informal discussions with Berkeley representatives, the careful, taciturn McDaniel shrank from retracting the traditional prerogatives of the accelerator designer. It would be up to Seaborg to take such action, if he chose to do so. (65)

Lofgren's meeting notes, from the September 26, 1962 discussion and a discussion the previous day with Haworth, reveal that Brookhaven was

also mentioned in the AEC request to BOB. In a move reminiscent of arrangements made for constructing the Bevatron and Cosmotron, it had been decided that both laboratories could build machines, one with higher energy than the other. In the current two-step approach, a design study for a machine in the multi-hundred GeV range would be conducted by LRL, and BNL would be allowed to work on the design of a 1000 GeV machine, slated for construction in the long-term future. The FFAG was also included in the budget request, although the AEC admitted that the older MURA proposal would not necessarily survive if only two out of the three large accelerators were funded. (66)

With this deprecation of the MURA proposal and the concession to BNL, Berkeley's major worries were BOB approval and complaints from Caltech, a potential problem since the tentative collaboration between LRL and WAG grew dormant after the December 1961 meeting. In the midst of increasingly intense competition for the design study, WAG researchers began to doubt that they could effectively collaborate in the supposed "Western Regional" effort, feeling they would be hopelessly outnumbered and ignored by the larger Berkeley team. In October, McDaniel wrote to McMillan explaining that funding could be granted immediately after final executive branch approval from the BOB; however, he admitted in the September 26 meeting that disapproval from the powerful Caltech researcher Robert Bacher "could kill the project. . . ." (67)

In the meantime, Washington leaders were worried about the large number and expense of requests for accelerators of various energies and

tensions prompted by the dark horse MURA. Haworth admitted to Lofgren in September that the situation was particularly troublesome because Wiesner had promised at the time the Stanford Linear Accelerator Center was approved that "there would not be another large accelerator for five years." In response to such pressures, Wiesner, with the aid of the AEC, convened another in the series of President's Science Advisory Committee/General Advisory Committee (PSAC/GAC) panels in November to offer advice on future needs in high energy physics. The Panel was named for its chairman, Norman Ramsey of Harvard. A former graduate student of Brookhaven champion Isidor Rabi, Ramsey had worked on both the radar and atomic bomb projects in WWII, aided in the establishment of Brookhaven after the war, and served on a number of advisory boards before joining the GAC. Ramsey was a good choice for chairing the Panel, destined to become one of the most famous of its kind. Well-respected for his scientific capabilities, Ramsey had developed a reputation for being fair-minded and diplomatic. These skills would prove useful as Ramsey, 9 other eminent scientists appointed jointly by PSAC and GAC, and 2 ex officio members from other government agencies investigated the numerous controversial issues at hand. (68)

In early December 1962, as the Ramsey Panel began its deliberations, LRL presented a revised proposal to the AEC. The proposal requested $4,875,000 for a two-year design study for an accelerator in the 100 GeV range, echoing terms presented informally to the AEC in October when the Commission was preparing its budget for BOB. The proposal estimated that a 100 GeV machine would cost $152 million

while a 200 GeV machine would cost $263 million, including accelerator components, architect engineering costs, development costs and 20% technical contingency. The proposal also mentioned that Brookhaven was proceeding with studies for a 1000 GeV accelerator to be approved some time in the future. (69)

While much of the February 1962 proposal had been devoted to management issues, the December proposal focused more on the use of the machine. The proposal explained the research utility of the new machine, along the lines of previous discussions and provided a lengthy discussion of the feasibility of experiments. LRL researchers presented preliminary calculations promising more intense proton beams and "secondary particle beams several orders of magnitude more intense than those in use at present." They also reported optimistically on the prospects for developing adequate beam transport, particle identification, and beam separation techniques for the new machine. (70)

Although the December proposal discussed the organization of the new laboratory, it made no mention of LRL as a study center, unlike the February version. While it mentioned that the laboratory planned to seek guidance from an advisory committee with national representation, it boldly stated that: "LRL would assume full responsibility for design, construction, and operation of the accelerator and facilities." The proposal noted that such a scheme "closely resembles the one followed by Stanford." In contrast to the February report, but in line with McMillan's remarks to McDaniel in May about the infeasibility of splitting off major parts of accelerator design, the December report

made only a passing and tentative reference to outside collaboration, remarking that the laboratory would seek outside assistance "and if people and facilities are available some portions of the study could be carried on away from Berkeley." By the end of 1962, LRL had submitted an accelerator proposal along traditional lines. Moreover, despite pronouncements about policies necessary for a national facility of such expense, the AEC, which had obviously been in close contact with LRL during the preparation of the document, seemed prepared to accept such a proposal. (71)

Before making a decision, however, the AEC transmitted the revised Berkeley proposal to the Ramsey Panel. The Panel had a number of other issues to consider, including Brookhaven's plans for the 1000 GeV machine and for storage rings for the AGS, MURA's FFAG proposal, support for a 10 GeV electron synchrotron at Cornell, various high-intensity meson accelerators, and funding for other important functions such as detector development and university user groups. Panel members had 14 sessions over a 6 month period, interviewing physicists from all over the nation, including MURA, BNL, and LRL representatives who gave reports on their respective proposals, hearing presentations from various government agencies, meeting with BOB, and engaging in constant, vigorous discussion. (72)

The most controversial issue facing the Panel was the relative importance of the high intensity accelerators, including the MURA's FFAG proposal and the meson accelerators. In its final report the Panel first noted that it could not "encourage new high energy accelerator

construction costing millions of dollars unless a useful extension of accelerator parameters is achieved, or a new technology advanced." The Panel further noted that an increase in proton energy was "the single most important energy parameter to be extended." In an explanation of the importance of the high energy frontier, the report repeated the bright prospects for the explication of the strong interaction, echoing Sulamuth Goldhaber and Yuan. In addition, it pointed to the exciting 1962 announcement by Leon Lederman and others that AGS experiments confirmed the existence of two types of neutrinos, in line with theoretical predictions. The "striking success" of such experiments, the report explained "encourages the hope that work with neutrinos at still higher energies will uncover the underlying structure of the weak interactions." In addition to clarification of the weak and strong interactions, the Panel expressed the hope that further high energy research would bring physicists closer to understanding the fundamental relationship of all matter by providing "a clue to the connections among the different kinds of basic force." The Panel admitted that high intensity beams allowed researchers "to study, in detail, interactions of such low probability that they have not yet been observed using present accelerators." Nonetheless, the report mentioned the technological problems associated with high intensity accelerators, in particular the complications caused by high radioactivity levels, and noted the "relatively high cost of high intensity machines." (73)

For these reasons, Panel members felt obliged to recommend only one high intensity machine, the FFAG. In addition, they put the FFAG at a

lower priority than the high energy machines, despite the vociferous pleas of frustrated MURA physicists, who had been working for more than ten years to fulfill the dream of a first-rate Midwestern accelerator. Aware that their recommendation on the MURA proposal would be controversial and perhaps decisive for the continued existence of the group, Panel members spent hours debating over the wording of the paragraphs summarizing their recommendation for MURA. (74)

Deliberations on the Berkeley proposal were also heated. Edwin Goldwasser, a Panel member from the University of Illinois, remembers that Panel members gave strong resistance to the proposal because of LRL's conservative stance on management. Nonetheless, the committee ultimately decided to recommend that LRL be given control of the next high energy accelerator with the idea that the laboratory would not only design, but also build the accelerator. Goldwasser recently explained:

> the fact was that Berkeley had made enormous contributions to the field and had a large number of very talented people. If you want to optimize the success of such a large new venture, you have to get the best people to work on it. It could be hoped that differences in management philosophy could later be resolved. (75)

Ramsey adds two more persuasive arguments that worked in Berkeley's behalf. First, Berkeley had a very strong group which would be kept vital with a new accelerator and second, Panel members were reluctant to see another expensive laboratory built from the ground up. The decision was not universally applauded. Goldwasser remembers that when Rabi, then a PSAC member, heard the news, there were "tears in his eyes. He was concerned that past experience at Berkeley indicated that under the

Berkeley management philosophy the new accelerator would not achieve the essential national participation." (76)

Although the Panel eventually decided in Berkeley's favor, the Berkeley proposal did not escape criticism. In December, Ramsey queried McMillan about the maximum energy of the proposed machine. Although Ramsey went out of his way not to criticize the LRL director, Goldwasser remembers that the Panel would not have been happy with a machine designed to achieve less than 200 GeV. By January 1963 McMillan had agreed to raise the energy range to 150-300 GeV, thus pleasing not only the Panel, but Bacher as well. As Bacher explained to Ramsey, although none in the Caltech group "had any real enthusiasm for a 100 Bev West Coast machine," in light of the increased energy range and further technical discussions, the Caltech group wished to add their "enthusiastic support of the study proposal contained in McMillan's letter." (77)

On April 26, the Ramsey Panel issued its report. Their first recommendation was that the government: "Authorize at the earliest possible date, the construction by the Lawrence Radiation Laboratory, of a high energy proton accelerator at approximately 200 BeV energy." They also recommended that Brookhaven be authorized to construct the proposed AGS storage rings and begin "intensive design studies" for an accelerator in the 600 to 1000 GeV range. All large laboratories, including Berkeley and Brookhaven, "should incorporate an administrative structure with national representation to assure that all proposals for qualified scientists shall be considered on equal footing." The Panel

also recommended that support be given to the Cornell 10 GeV machine and to the construction of electron-positron storage rings at Stanford. Although the Panel members also recommended that the AEC authorize the MURA machine, their support was conditional. In its final form, the Panel's specific recommendation was that the machine should be authorized "without permitting this to delay the steps toward higher energy." (78; See Storage Rings)

In early May, Ramsey met with the AEC to discuss the Panel's findings. According to AEC notes of the meeting, after summarizing the recommendations, Ramsey noted that "the most obvious implication of the program as outlined was that of cost. For instance, the recommended program would result in annual costs in FY 1975 of nearly $600 million, an increase of about $400 million over the FY 1964 level." Discussion then turned to the merits of constructing the MURA accelerator, now estimated at $148 million. Ramsey repeated that the program should continue, as long as the other accelerators were not delayed. No one assumed, however, that such an expensive "conditional" choice would be kept in the current swollen budget. Ramsey later admitted that the Panel's recommendations were really the "kiss of death" for MURA. (79)

In line with the recommendations of McDaniel and the Ramsey Panel and Wiesner's opinion, BOB gave its approval and in April 1963 LRL received permission to begin a detailed design of an accelerator in the 200 GeV range. Concurrently, Brookhaven was authorized to begin studies of a machine in the 600 to 1000 GeV range, along with work on the AGS storage rings. MURA's fate, however, remained unclear. (80)

LRL researchers were triumphant. With McMillan at the helm, the laboratory had defeated its rivals to emerge in sole possession of an approved proposal for the 200 BeV accelerator. (81) True, there had been rumblings that the machine might not be constructed and managed by the laboratory that designed it. However, in light of informal discussions with the AEC and the Ramsey Panel recommendation, it looked as though LRL might have to do little more than consult an advisory committee that had national representation. Otherwise tradition would hold, and Berkeley would again lead the way to higher energy. As in the 1950s, past and current glories had paved the way for a brilliant future. In mid-1963, LRL researchers had every reason to believe this cycle would continue unabated.

Summary: The Emergence of the Berkeley Design Proposal

From the onset, even in corridors of tradition-conscious LRL, it appeared that the new accelerator facility, meant to serve the entire community of high energy physicists, should be different than previous laboratories. By the time of the December 1961 meeting in Los Angeles, those planning design proposals, including Berkeley researchers, agreed that the design study should be conducted at a central site with contributions from outside laboratories and that the design director should operate under the watchful eye of an advisory board with national representation. In his April 1962 letters to McMillan, Goldhaber, and other physicists, McDaniel declared that the accelerator would not necessarily be sited at a location near the design group, adding that

the accelerator might best be managed separately from any existing laboratory.

Despite these signs of change, competition for the proposal proceeded in a manner reminiscent of the previous decade. Although Sands and other WAG researchers were the first to begin serious design efforts on the next strong focusing synchrotron and subsequently produced an innovative, economy-minded proposal, they dropped from competition when it became clear that Berkeley, with its long experience in accelerator development, would dominate West Coast efforts. As was the case with the Bevatron/Cosmotron funding, the AEC, BNL, and LRL eventually agreed to a plan whereby both laboratories could build a new high energy machine, one with higher energy than the other. Moreover, despite earlier proclamations, McDaniel did not contradict Berkeley researchers when they said they intended to design, build, and operate the new machine.

In the meantime, the AEC and the physics community faced the problem of accommodating the expensive program in the already swollen high energy physics budget. This problem was particularly acute because the FFAG was slated for authorization. In light of the unexpectedly high beam intensities of the AGS and PS and the greater research potential of high energy machines, it seemed that the FFAG might be cancelled, a decision sure to embitter already antagonized MURA researchers. In late 1962, the Ramsey Panel was therefore convened to pass judgement on current accelerator plans. Although some Panel members were concerned about Berkeley's management policies, the Panel

gave its stamp of approval to Berkeley to build the machine because of its impressive reputation and the experience and skills of its current staff. When Berkeley received design funding for the new accelerator in April 1963, with the assumption that McMillan would work with an advisory committee to solve management issues, it appeared that for all the talk of the changes necessary for the large new facility, very little would change.

Conclusion: Design Study Arrangements--1960-1963

Serious discussions of the next alternating gradient proton synchrotron began in 1960, nurtured by the confidence and expectations of the previous decade and spurred by the stunning success of the AGS and PS. Although the 1960 Piore Panel report recommendation paved the way for the machine, Wigner's warning about the dangers of unstinted funding and the Panel's equivocal stand on MURA pointed to difficult issues that would have to be addressed as the expensive new accelerator sought funding.

In the next few years, physicists discussed numerous ideas for scaling up proton synchrotron designs amid increased optimism about the research potential of the new machine and raging competition for the design proposal. Although designing a machine in the 200 GeV range posed no major technical difficulties, finding a scheme for planning and organizing the expensive facility was more problematic. Recognizing that changes were necessary since only one such machine would be built, physicists discussed plans for inter-laboratory design collaboration and

the AEC formally reserved the right to select the site at a later date. Despite these signs of change and the warning signals arising from the eroding prospects of MURA, by 1963 Berkeley researchers had gained control of the new accelerator design. At this point they had every reason to believe that LRL would eventually exercise the traditional prerogative of accelerator designers and build and operate the new machine. LRL, with its reputation of past and current excellence, had once again ushered in plans for designing a new, more powerful accelerator.

Notes for Chapter 3

All interviews are located in Fermi National Accelerator Laboratory History Collection, Batavia, Illinois, unless otherwise noted. The following abbreviations are used: Bancroft Library, (Unversity of California, Berkeley, Calfornia); BNL (Brookhaven National Laboratory, Upton, Long Island); DOE Archives, (United States Department of Energy Archives, Germantown, Maryland); FHC, (Fermilab History Collection, Batavia, Illinois); Green Papers, (files of G. Kenneth Green, Brookhaven National Laboratory, Upton, New York); Judd Papers, (files of David L. Judd, LBL); LBL, (Lawrence Berkeley Laboratory, Berkeley, California); LBL Archives, (Lawrence Berkeley Laboratory Archives); Lofgren Papers, (files of Edward J. Lofgren, at Lawrence Berkeley Laboratory); LBJ Library, (Lyndon Baines Johnson Library Archives, Austin, Texas); McMillan Papers, (files of Edwin McMillan, LBL Archives); Salsig Papers, (files of William Salsig, LBL Archives); Seaborg Papers, (files of Glenn T. Seaborg, LBL); Wenzel Papers, (files of William Wenzel, LBL).

1. Quotes from interview with Edwin McMillan, May 16, 1984. Also see Herbert Childs, An American Genius: The Life of Ernest Orlando Lawrence, (New York: E. P. Dutton & Co., Inc., 1968), p. 479 and Lauri Fermi, Atoms in the Family: My Life With Enrico Fermi, (Chicago: The University of California Press, 1954), p. 47.

2. Childs, p. 532.

3. Quotes from Charles Weiner interview with Edwin McMillan, June 1, 1972, Bancroft Library, p. 17. Also see this source pp. 1, 7 and Jaques Cattel Press, ed. American Men and Women of Science, (New York: R. R. Bowker, 1979), p. 108.

4. Quote from John L. Heilbron, Robert W. Seidel, and Bruce R. Wheaton, Lawrence and His Laboratory: Nuclear Science at Berkeley, 1931-1961 (Berkeley: Office for History of Science and Technology, 1981), p. 29. Also see this source pp. 14, 28. M. Stanley Livingston, Particle Accelerators: A Brief History, (Cambridge: Harvard University Press, 1969), pp. 16, 34 and Childs, pp. 313-314.

5. Heilbron, Seidel, Wheaton, pp. 40, 42, 53-60; Jacques Cattel Press, p. 108 and Glenn T. Seaborg, Nuclear Milestones: A Collection of Speeches (San Francisco: W. H. Freeman and Company, San Francisco, 1972), p. 6.

6. Quote from interview with Edwin McMillan, May 16, 1984. Also see G. Hale interview with Owen Chamberlain, June 16, 1976, Bancroft Library, p. 12.

7. McMillan memorandum to L. W. Alvarez, K. M. Crowe, D. A. Glaser, H. S. Gordon, D. L. Judd, E. J. Lofgren, L. Smith, and R. L. Thornton,

August 10, 1960, Lofgren Papers; David L. Judd and Lloyd Smith, "Summary of Accelerator Studies Since June 1960 at LRL," December 26, 1961, unpublished document, McMillan Papers, Box 5; and R. F. Mozley and W. D. Walker, "Conference on Super Energy Accelerators," unpublished document, p. 21, FHC.

8. For an account of the history of Fermilab focusing on political considerations see Anton Jachim, Science Policymaking in the United States and the Batavia Accelerator (Carbondale: Southern Illinois University Press, 1975). A comparison of the history of Fermilab and the Japanese Accelerator Laboratory KEK can be found in Lillian 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: Livingston, M. Stanley, "Early History of the 200-GeV Accelerator," (Batavia: National Accelerator Laboratory, 1968), NAL-12; and Norman F. Ramsey, "History of the Fermilab Accelerator and URA," unpublished manuscript, FHC. The subject is also covered in Theodore J. Lowi and Benjamin Ginsberg, Poliscide, (New York: MacMillan, 1976).

9. Quotes from Vasily S. Emelyanov letter to John McCone, July 18, 1960, in "Relevant Extracts from Exchange of Correspondence between Mr. McCone and Professor Emelyanov, High Energy Physics," FHC. Also see John McCone and Vasily S. Emelyanov, "Memorandum: Cooperation between the United States of America and the Union of Soviet Socialist Republics in the Field of the Utilization of Atomic Energy for Peaceful Purposes," November 24, 1959, pp.3, 30, FHC; L. Jackson Laslett, "Minutes of Meeting to Discuss Possible International Accelerator Project," September 8, 1961, unpublished document, p. 33, FHC; "Piore Panel Report--1960," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), p. 129; John McCone letter to Vasily S. Emelyanov, August 31, 1960, FHC; and Alice L. Buck, A History of the Atomic Energy Commission, (Washington: U. S. Department of Energy, 1983,) DOE/ES/0003/1, p. 4.

10. Lillian Hoddeson interview with Robert Wilson, May 8, 10, 1978, FHC; and "Resume of September 16 Meeting of US and USSR Scientists on Desirability and Feasibility of a New Accelerator of Large and Novel Type," unpublished document, p. 2, FHC.

11. Quotes, respectively, from R. F. Mozley and W. D. Walker, "Conference on Super Energy Accelerators," unpublished document, p. 4 and Robert R. Wilson, "Ultrahigh Energy Accelerators: Summary of a Discussion held at Rochester, N. Y., August 28, 1960," unpublished document, pp. 3, 4-5, 6, FHC.

12. Quotes, respectively, from Keith R. Symon, "Summary of Remarks Regarding Accelerators," enclosure to Keith R. Symon letter to Robert Wilson, September 7, 1960; and Robert R. Wilson, "Ultrahigh Energy

Accelerators: Summary of a Discussion held at Rochester, N. Y., August 28, 1960," unpublished document, p. 6, FHC.

13. Wilson explained that particle separator schemes included: "radiofrequency separations which can be effective up to 20 Gev/c but which will be very expensive; crossed electric and magnetic field devices which can be used to separate particles up to about 10 Gev/c; storage rings which can serve as particle separators by allowing the most unstable particle to decay...." Robert R. Wilson, "Ultrahigh Energy Accelerators: Summary of a Discussion held at Rochester, N. Y., August 28, 1960," unpublished document, p. 12, FHC. Text quotes, respectively, from Robert R. Wilson, "Ultrahigh Energy Accelerators: Summary of a Discussion held at Rochester, N. Y., August 28, 1960," unpublished document, p. 12, FHC; and R. F. Mozley and W. D. Walker, "Conference on Super Energy Accelerators," unpublished document, pp. 18, 21, FHC.

14. Dominique Pestre points out that although accelerator physicists were eager to build a new accelerator, experimentalists at CERN successfully argued against the idea. Participants remember that in the U.S. this difference in opinion between experimentalists and accelerator builders had been evident earlier. For example, in a history of accelerator development at LRL from 1952 to 1960 David Judd noted that plans for a new accelerator were not vigorously pushed during this period because of pressure from experimentalists to exploit the research potential of the Bevatron. This difference in point of view between experimentalists and accelerator builders suggests an intriguing new line of inquiry. How are such differences resolved? Is the case at CERN different because that laboratory receives international funding? David Judd, "The Development from 1952 to 1960 of Planning for A Larger Accelerator at the Lawrence Radiation Laboratory," September 29, 1960, unpublished document; and Dominique Pestre, "La Seconde Generation d'Accelerateurs Pour Le CERN, 1956-1965, Etude Historique d'un Processus de Decision de Gros Equipement en Science Fondamentale," CHS-19, p. 47. (The author wishes to thank Dominique Pestre for this document and Walter Benenson for translation of relevant passages.) Text quote as quoted in Lillian Hoddeson, "Establishing Fermilab in the U.S. and KEK in Japan: Nationalism and Internationalism in High Energy Accelerator Physics During the 1960s," unpublished manuscript, p. 26. Also see Robert R. Wilson, "Ultrahigh Energy Accelerators: Summary of a Discussion held at Rochester, N. Y., August 28, 1960," unpublished document, p. 4 and interviews with Edwin Goldwasser, July 10, 1985 and Laurie Brown, August 28, 1986.

15. The Piore Panel was first convened in 1958 to provide a special advisory board on high energy physics for the executive branch. Advice on high energy physics was also sought by the National Science Foundation, which convened panels in 1954, 1956, and 1958. The 1958 Piore Panel included Piore, McMillan, Hans Bethe, Leland Haworth and Jesse Beams. The 1960 Panel included these members, plus Eugene Wigner and George Lukes. "Piore Panel Report--1960," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on

National Policy and Background Information," pp. 131, 135. See Appendices 4, 5, and 6 of this document for 1954, 1956, 1958 National Science Foundation panels.

16. Quote from "Piore Panel Report--1960," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," p. 127. Also see this source p. 124.

17. Quotes, respectively, from Midwestern University Research Association, "Proposal for a High Energy Research Facility," March 14, 1958, unpublished document, p. 2, Judd Papers; and "Piore Panel Report--1960," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," pp. 125, 127. Also see pp. 128, 130 in the latter source and General Advisory Committee and President's Science Advisory Committee, Report of the Panel on High-Energy Accelerator Physics of the General Advisory Committee to the Atomic Energy Commission and the President's Science Advisory Committee, in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," p. 95 and personal communication from Lawrence W. Jones, July 9, 1987.

18. Quotes from "Commentary by Eugene P. Wigner on the PSAC-GAC Panel Report of December 14, 1960, on High Energy Accelerator Physics," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," p. 132. Also see this source pp. 133-134.

19. Quote from "Introduction," in Midwestern Universities Research Association, "1959 MURA Summer Study on Design and Utilization of High-Energy Accelerator," p. ii, FHC. Also see Lillian 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), p. 14; and interview with Alvin V. Tollestrup and Robert Walker, May 4, 1985.

20. A linac injector for such a machine would be prohibitively expensive. In addition, injection at a reasonable energy (about 1 GeV) would require a larger, more expensive main ring aperture and cause serious problems with main ring field quality at the lower field strength. Private communication, Lloyd Smith, November 13, 1987. Text quote from Matthew Sands, "Ultra High Energy Synchrotrons," in Midwestern Universities Research Association, "1959 MURA Summer Study," MURA Report 465, p. 1, LBL Library, p. 2.

21. Sands gives credit to Marcus Oliphant and T. A. Welton for conceiving cascade schemes. Hoddeson points out that F. Heyn and Lee Teng had also made similar suggestions. Lillian 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), p. 14. Text quote from Matthew Sands, "Ultra High Energy Synchrotrons," in Midwestern Universities Research Association, "1959 MURA Summer Study," MURA Report 465, p. 1, LBL Library. Also see Matthew Sands, "A Proton Synchrotron for 300 GeV," (Pasadena: California Institute of Technology, 1960), CTSL-10. p. 4, FHC; and interviews with Francis Cole, August 29, 1986 and Denis Keefe, Glen Lambertson and Jackson Laslett, December 22, 1986.

22. Quotes, respectively, from "Introduction," in Midwestern Universities Research Association, "1959 MURA Summer Study," MURA Report 465, p. iii, LBL Library; and "The Future of High Energy Physics At Caltech," May 17, 1960, unpublished document. (The author wishes to thank Robert Walker for providing the May 1960 document.) Also see interview with Robert Walker and Alvin Tollestrup, May 4, 1985; private communication, Robert Walker, December 5, 1987; Matthew Sands letter to Hartland Snyder, August 16, 1960; Matthew Sands letter to Wolfgang Panofsky, August 22, 1960, FHC.

23. Matthew Sands, "A Proton Synchrotron for 300 GeV," pp. 1, 16; and Lillian 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), p. 15.

24. Judd explains that researchers wanted a chance to judge the performance of the AGS and PS as a test of strong focusing and further consider the FFAG design, an air core magnet design suggested by Nicolas Christofilos, colliding beam concepts, and a variety of Soviet accelerator speculations. David L. Judd, "The Development from 1952 to 1960 of Planning for a Larger Accelerator," September 29, 1960, unpublished manuscript, pp. 3, 4, Judd Papers.

25. Interviews with Edward Lofgren, May 3, 1984 and wih William Wenzel, May 2, 1984 also reveal that Lawrence was unenthusiastic about building a new high energy machine after the construction of the Bevatron.Quote from interview with David Judd, May 1, 1984. Also see Edward J. Lofgren, "High Energy Accelerator Study," November 23, 1955, unpublished document, Lofgren Papers.

26. Quote from David L. Judd and LLoyd Smith, "Summary of Accelerator Studies Since June 1960 at LRL," p. 1, McMillan Papers, Box 1. Also see David L. Judd, "The Development from 1952 to 1960 of Planning for a Larger Accelerator," September 29, 1960, unpublished manuscript, p. 7, Judd Papers; and Edwin McMillan memorandum to L. W. Alvarez, K. M. Crowe, D. A. Glaser, H. S. Gordon, D. L. Judd, E. J. Lofgren, L. Smith, and R. L. Thornton, August 10, 1960, Lofgren Papers.

27. Quotes, respectively, from Matthew Sands letter to Lloyd Smith, August 29, 1960, FHC and Lloyd Smith, "Notes on a Visit to Cal Tech (Oct. 10-11, 1960)," unpublished document, Lofgren Papers.

28. As mentioned in the minutes of the September 16 meeting, a 70 GeV Soviet synchrotron was under construction at Serpukhov. "Minutes of September 16, 1960," unpublished document, p. 1, FHC. Text quote from Officially-Designated Scientific Group, "Desirability and Feasibility of New Accelerators of Large and Novel Type," Report to U.S. and Soviet Atomic Energy Authorities, no date, FHC. Also see Lloyd Smith letter to Sands, August 24, 1960, FHC; "Resume of September 16 Meeting of US and USSR Scientists on Desirability and Feasibility of a New Accelerator of Large and Novel Type," unpublished manucript, FHC; and R. F. Mozley and W. D. Walker, "Conference on Super Energy Accelerators," unpublished document, p. 1, FHC.

29. John P. Blewett, "Accelerator Design and Construction in the 1950s," in Pions to Quarks; History of Particle Physics in the 1950s eds. Brown, Dresden, and Hoddeson to be published by Cambridge University Press, 1988, pp. 2-3; Luke C. L. Yuan and John P. Blewett, "Experimental Program Requirements for a 300 to 1000-BeV Accelerator," (Brookhaven: Brookhaven National Laboratory, 1961); and M. Stanley Livingston, "Early History of the 200-GeV Accelerator," p. 3.

30. Lloyd Smith letter to Matthew Sands, January 11, 1961, FHC; Sands to Blewett, March 27, 1961, FHC; and "Accelerator Planning Group, Minutes of Feb. 9., 1961 Meeting," February 16, 1961, unpublished document, Lofgren Papers.

31. Quote from Matthew Sands letter to John Blewett, March 27, 1961, FHC. Also see John Blewett letter to Matthew Sands, February 15, 1961, FHC; form letter from Robert Bacher and Matthew Sands, January 12, 1961, FHC; Matthew Sands letter to Robert Bacher, January 30, 1961, FHC; and "The Future of High Energy Physics at Caltech," May 17, 1960, unpublished document. (The author wishes to thank Robert Walker for providing the May 1960 document.)

32. The WAG request included $253,000 requested for technical studies and $340,000 requested for engineering and architectural studies. Text quotes from California Institute of Technology, "A Proposal to the Atomic Energy Commission for the Support of the Accelerator Design-Study Program of the Western Accelerator Group," April, 1961, unpublished document, p. 2. Also see p. 5.

33. When the Los Alamos project began in 1943, Bacher led the Experimental Physics Division, which included Sands and Walker, neither of whom had yet earned a Ph.D., as well as Wilson. Although Wilson had received his Ph.D. only 4 years earlier, in a 1944 reorganization he became leader of the Research Division, which included many of the groups formerly in the Experimental Physics Division. In the meantime, Bacher became head of the Gadget Division, in which McMillan worked. Text references: California Institute of Technology, "A Proposal to the Atomic Energy Commission for the Support of the Accelerator Design-Study Program of the Western Accelerator Group," April, 1961, unpublished document, pp. 2, 5, 12, FHC.

34. LRL had previously requested $200,000 for such studies for that year. The laboratory asked that the original $200,000 be increased to $500,000, and $2,500,000 be granted for the following year. Text quotes from Lawrence Radiation Laboratory, "Extract from LRL FY1963 Budget Submission, submitted 4-21-61," unpublished document, Lofgren Papers.

35. California Institute of Technology, "A Proposal to the Atomic Energy Commission for the Support of the Accelerator Design-Study Program of the Western Accelerator Group," April, 1961, unpublished document, pp. 5, 24, FHC; Lawrence Radiation Laboratory, "Extract from LRL FY1963 Budget Submission, submitted 4-21-61," unpublished document, Lofgren Papers.

36. Quotes from Edward J. Lofgren memorandum to Edwin M. McMillan, April, 6 1961, Lofgren Papers. Also see interviews with Edward Lofgren, April 24, 1984 and October 9, 1987.

37. Quotes from "Accelerator Planning Group, Minutes of Feb. 9., 1961 Meeting," February 16, 1961, unpublished document, Lofgren Papers.

38. Quotes, respectively, from Edward J. Lofgren memorandum to Edwin M. McMillan, April, 6 1961, Lofgren Papers; California Institute of Technology, "A Proposal to the Atomic Energy Commission for the Support of the Accelerator Design-Study Program of the Western Accelerator Group," April, 1961, unpublished document, p. 7, FHC; and interview with Edward Lofgren, May 3, 1984.

39. Edward J. Lofgren memorandum to Edwin M. McMillan, April, 6 1961, Lofgren Papers.

40. Quotes, respectively, from California Institute of Technology, "A Proposal to the Atomic Energy Commission for the Support of the Accelerator Design-Study Program of the Western Accelerator Group," April, 1961, unpublished document p. 7, FHC; Robert F. Bacher and Edwin M. McMillan letter to Paul McDaniel, May 12, 1961, McMillan Papers, Box 5; and A. R. Luedecke memorandum to Paul W. McDaniel and enclosures, May 11, 1961, McMillan Papers, Box 1.

41. Paul McDaniel letter to J. Robert Oppenheimer, May 26, 1961, FHC.

42. Quotes from George Kolstad, Untitled report, June 22-23, 1961, unpublished document, Lofgren Papers. Also see Paul McDaniel memorandum to Ellison Shute, July 3, 1961, McMillan Papers, Box 5.

43. Lloyd Smith letter to Paul McDaniel and enclosures, May 10, 1961, FHC.

44. Quotes, respectively, from S. Goldhaber, "Some Comments Concerning Experimental Questions," and Geoffrey Chew, "Summary of

Theoretical Conclusion from 1961 Berkeley Summer Study," in Lloyd Smith, "Introductory Material for the Report of the Berkeley High Energy Physics Study," December 20, 1961, unpublished document, McMillan Papers, Box 5.

45. Yuan, Luke C. L., and Blewett, John P., "Experimental Program Requirements for a 300 to 1000-BeV Accelerator," (Brookhaven: Brookhaven National Laboratory, 1961); Lillian Hoddeson interview with Francis Cole, March 21, 1978; and Arthur Schlesinger, Jr., A Thousand Days: John F. Kennedy in the White House, (New York: Greenwich House, 1983,) pp. 379-405.

46. John Blewett notes that a ferrite-tuned radiofrequency system had also been used in the Cosmotron. John Blewett, private communication, November 29, 1987. Text references: John P. Blewett, "Remarks on Design Study for a 300-1000 Bev Accelerator," in "Minutes of Meeting to Discuss Possible International Accelerator," September 8, 1961, unpublished document, p. 34, FHC. Also see "Final Newsletter about the Brookhaven Summer Study on 300-1000 BeV Accelerators," July 26, 1961, unpublished document, Green Papers; California Institute of Technology, "A Proposal to the Atomic Energy Commission for the Support of the Accelerator Design-Study Program of the Western Accelerator Group," April, 1961, unpublished document p. 9, FHC; and Lillian Hoddeson interview with Thomas Collins, April 12, 1978.

47. On the issue of the angles at which particles would emerge Yuan reported: "Using kinematic considerations and assuming isotropy of the secondaries in the center-of-mass system, half of the secondary particles produced would be contained in a cone of half-angle 4.5 degrees at 300-Bev and 2.5 degrees at 1000-Bev incident energy." Evidence from accelerators and cosmic rays also indicated "that the transverse momentum of all known kinds for secondaries in the energy range up to 1,000,000 Bev is smaller (of the order of 400 Mev/c) thus indicating a much narrower angle of secondaries produced in the forward cone...." Yuan also explained that there had been "encouraging developments on new techniques for identifying and separating particles at very high energies such as using the relativistic rise characteristics in ionization loss and the small-angle strong interactions." Luke C. L. Yuan and John P. Blewett, "Experimental Program Requirements for a 300 to 1000-BeV Accelerator," (Brookhaven: Brookhaven National Laboratory, 1961).

48. Quote from Luke C. L. Yuan and John P. Blewett, "Experimental Program Requirements for a 300 to 1000-BeV Accelerator," (Brookhaven: Brookhaven National Laboratory, 1961).

49. Edward J. Lofgren, "History of Accelerator Studies at Berkeley," February 22, 1964, unpublished manuscript, p. 2, Lofgren Papers; "Minutes of Accelerator Steering Committee Meetings," November 10, 1961, unpublished document, Lofgren Papers; and "Study Group on

Super High Energy Accelerator, Committee on Experimental Matters," November 22, 1961, unpublished document, Green Papers.

50. Quotes, respectively, from "Minutes of Accelerator Steering Committee Meetings," October 6, 1961, unpublihsed document, Lofgren Papers; and "Minutes of Accelerator Steering Committee Meetings," November 10, 1961, unpublished document, Lofgren Papers. Also see E. M. McMillan, "Minutes of Accelerator Steering Committee Meetings," September 6, 1961, unpublished document, Lofgren Papers.

51. Quotes from Lloyd Smith, "Notes on Phone Conversations with Falk and Blewett, Nov. 14 [1961]," unpublished document, Lofgren Papers. Also see Alice L. Buck, A History of the Atomic Energy Commission, p. 12.

52. Quotes from Lloyd Smith, "Notes on phone conversations with Falk and Blewett, Nov. 14 [1961]," unpublished document, Lofgren Papers.

53. Hayden Gordon, "Minutes of Meeting Held to Discuss the Organization of the Study of a Super-Energy Accelerator," unpublished document, Lofgren Papers; and Brookhaven National Laboratory, "National Design Study for a Super-High-Energy Accelerator," December 21, 1961, unpublished document, FHC.

54. Interview with Alvin Tollestrup and Robert Walker, May 4, 1985; Hayden Gordon, "Minutes of Meeting Held to Discuss the Organization of the Study of a Super-Energy Accelerator," unpublished document, Lofgren Papers; and Edward J. Lofgren, "Notes on a Meeting to Discuss the Organization of a Study of a Super High Energy Accelerator," January 2, 1962, unpublished document, Lofgren Papers; and Brookhaven National Laboratory, "Considerations Relating to Location of the National Study Group at the Brookhaven National Laboratory," December 21, 1961, unpublished document, FHC.

55. Quote from Lawrence Radiation Laboratory, "Proposal to the AEC for a National Design Study of an Ultrahigh Energy Accelerator," unpublished document, Lofgren papers, p. 5-6. Also see this source pp. 9, 10, 13-15, 16.

56. The contrasting approaches to machine design were not new. Blewett points out that when the Bevatron and Cosmotron were being built he noticed that at LRL "machine design was guided mainly by experience and by model studies; not much detailed theoretical analysis had previously been found profitable." John P. Blewett, "Accelerator Design and Construction in the 1950s," in Pions to Quarks; History of Particle Physics in the 1950s eds. Brown, Dresden, and Hoddeson to be published by Cambridge University Press, 1988, p. 10. Text quotes from Brookhaven National Laboratory, "300-1000 BeV National Accelerator Design Study at the Brookhaven National Laboratory," (Brookhaven: Brookhaven National Laboratory, 1961). "Appendix A" in Atomic Energy Commission, "Initiation of Design Studies for a Proton-Synchrotron of Several Hundred BeV,"

August 22, 1962, AEC 1096/1, p. 15, DOE Archives, Secretariat Collection, Box 1474.

57. As Kennedy's science advisor Wiesner was also chairman of the President's Science Advisory Committee, and Director of the Office of Science and Technology (OST) and of Federal Council for Science and Technology (FCST). Although he did not direct the AEC and had no official say in organizing AEC programs, the Commission was a part of the executive branch and worked closely with him since all executive office budgets were approved by the President, whom he advised on scientific matters, before being submitted for Congressional approval. For information on the formation and function of OST and FCST, see James E. Katz, Presidential Politics and Science, (New York: Praeger, 1978) and Thaddeus J. Trenn, America's Golden Bough: The Science Advisory Intertwist, (Cambridge: Oelschlager, Gunn and Hain, 1983). Text quotes from Glenn Seaborg, record of conversation, February 8, 1962, Seaborg Papers. Also see M. Stanley Livingston, Particle Accelerators: A Brief History, p. 72.

58. Form letter dated April 19, 1962 in "Appendix B," Atomic Energy Commission, "Initiation of Design Studies for a Proton-Synchrotron of Several Hundred BeV," August 22, 1962, August 22, 1962, AEC 1096/1, pp. 17-18, DOE Archives, Secretariat Collection, Box 1474.

59. Quotes from form letter dated April 19, 1962 in "Appendix B," Atomic Energy Commission, "Initiation of Design Studies for a Proton-Synchrotron of Several Hundred BeV," August 22, 1962, AEC 1096/1, p. 19, DOE Archives, Secretariat Collection, Box 1474. Also see Paul McDaniel letter to Edwin McMillan, April 19, 1962, Lofgren Papers; and Paul McDaniel letter to Maurice Goldhaber, April 19, 1962, McMillan Papers, Box 1.

60. Quote from Edwin McMillan letter to Paul McDaniel, May 8, 1962, McMillan Papers, Box 1.

61. Quotes from Maurice Goldhaber letter to Paul McDaniel, May 10, 1962, Green Papers.

62. Quotes from "Appendix A" in Atomic Energy Commission, "Initiation of Design Studies for a Proton-Synchrotron of Several Hundred BeV," August 22, 1962, AEC 1096/1, p. 9, DOE Archives, Secretariat Collection, Box 1474. Also see this source p. 8.

63. Quote from "Appendix A" in Atomic Energy Commission, "Initiation of Design Studies for a Proton-Synchrotron of Several Hundred BeV," August 22, 1962, AEC 1096/1, p. 15, DOE Archives, Secretariat Collection, Box 1474. Also see pp. 12-14.

64. "Appendix A" in Atomic Energy Commission, "Initiation of Design Studies for a Proton-Synchrotron of Several Hundred BeV," August

22, 1962, AEC 1096/1, p. 15, DOE Archives, Secretariat Collection, Box 1474.

65. The BOB ruled on all executive branch budgets, including that of the AEC. For more information on the evolution of the AEC budget, see Physics Survey Committee, National Research Council, Physics in Perspective, (Washington D.C: National Academy of Sciences, 1972), pp. 653-659. Text quotes from E. J. Lofgren, "Conference of McMillan, Lofgren, Laslett, Kolstad at AEC Germantown, Sept. 26, 1962," unpublished document, Lofgren Papers. Also see interview with Gerald Tape, November 21, 1986.

66. Edward J. Lofgren, "Conference of McMillan, Lofgren, Laslett, Kolstad at AEC Germantown, Sept. 26, 1962," unpublished document, Lofgren Papers; and Edward J. Lofgren "Conference with Haworth in Washington, " September 25, 1962, unpublished document, Lofgren Papers.

67. Quote from Paul McDaniel letter to E. M. McMillan, October 25, 1962, McMillan Papers, Box 1. Also see Edward J. Lofgren, "Conference of McMillan, Lofgren, Laslett, Kolstad at AEC Germantown, Sept. 26, 1962," unpublished document, Lofgren Papers and interview with Alvin Tollestrup and Robert Walker, May 4, 1985.

68. Others on the Panel were: Philip H. Abelson, Owen Chamberlain, Murray Gell-Mann, Edwin Goldwasser, Tsung D. Lee, Wolfgang K. H. Panofsky, Edward M. Purcell, Frederick Seitz, and John H. Williams. Ex Officio members were Randal M. Robertson from the NSF who represented the Technical Committee on High Energy Physics of the Federal Council for Science and Technology and David Z. Robinson, from the Office of Science and Technology. General Advisory Committee and President's Science Advisory Committee, Report of the Panel on High-Energy Accelerator Physics of the General Advisory Committee to the Atomic Energy Commission and the President's Science Advisory Committee, in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), pp. 85, 104. Text quote from Edward Lofgren, "Conference with Haworth in Washington, September 25, 1962, unpublished document, Lofgren Papers. Also see Jaques Cattel Press, ed. American Men and Women of Science, p. 4097; and Kevles, The Physicists: The History of a Scientific Community in Modern America, (New York: Alfred A. Knopf, 1978), p. 368.

69. Lawrence Radiation Laboratory, "Proposed Program for an Alternating Gradient Synchrotron in the 100 BeV Range," December 8, 1962, unpublished document, pp. I-2, IV-3, V-8; Edwin McMillan record of telex to Paul McDaniel, October 2, 1962, Lofgren Papers; and Edwin McMillan telex to Paul McDaniel, October 2, 1962, Lofgren Papers.

70. Quote from Lawrence Radiation Laboratory, "Proposed Program for an Alternating Gradient Synchrotron in the 100 BeV Range," December

8, 1962, unpublished document, p. II-7. Also see this source pp. II-4-II-12.

71. Quotes from Lawrence Radiation Laboratory, "Proposed Program for an Alternating Gradient Synchrotron in the 100 BeV Range," December 8, 1962, unpublished document, p. III-3.

72. General Advisory Committee and President's Science Advisory Committee, Report of the Panel on High-Energy Accelerator Physics of the General Advisory Committee to the Atomic Energy Commission and the President's Science Advisory Committee, in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), p. 106; Lillian Hoddeson interview with Norman Ramsey, December 19, 1978, FHC.

73. Quotes from General Advisory Committee and President's Science Advisory Committee, Report of the Panel on High-Energy Accelerator Physics of the General Advisory Committee to the Atomic Energy Commission and the President's Science Advisory Committee, in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), pp. 94, 89, 90, 95. Also see Lillian Hoddeson interview with Norman Ramsey, December 19, 1978, p. 43; interviews with Edwin Goldwasser, July 10, 1985, p. 4 and Owen Chamberlain, April 24, 1984; and Gordan Danby, Jean-Marc Gaillard, Dino Goulianos, Leon Lederman, Nariman Mistry, Melvin Schwartz, and Jack Steinberger, "Observation of High-Energy Neutrino Reactions and the Existence of Two Kinds of Neutrinos," Physical Review Letters, 9 (1962), p. 36.

74. Goldwasser remembers that Panel members could not concur on the exact wording of the phrase expressing their conditional recommendation of MURA. To accommodate conflicting opinions, both proposed wordings were used. In the section "Discussion of Some Principal Recommendations" the Panel recommended that the AEC should authorize the FFAG "provided this is not expected to delay significantly the authorization of the steps toward higher energy...." In contrast, the section "Specific Recommendations" suggests that the AEC recommend the accelerator "without permitting this to delay the steps toward higher energy." Edwin Goldwasser letter to the author, July 23, 1985. Text references: Lillian Hoddeson interview with Norman Ramsey, December 19, 1978, p. 79, and interview with Edwin Goldwasser, July 10, 1985.

75. Quote from interview with Edwin Goldwasser, July 10, 1985.

76. Quote from interview with Edwin Goldwasser, July 10, 1985, p. 3. Also see Lillian Hoddeson interviews with Norman Ramsey, December 19, 1978 and January 22, 1980.

77. Quote from Robert E. Bacher letter to Norman Ramsey, January 8, 1963, Lofgren Papers. Also see Norman Ramsey, Letter to E. M.

McMillan, December 19, 1962, Lofgren Papers; and interview with Goldwasser July 10, 1985.

78. Wilson had plans to increase the energy of his 10 GeV electron synchrotron and later achieved 12 GeV with the Cornell machine. Text quotes from General Advisory Committee and President's Science Advisory Committee, Report of the Panel on High-Energy Accelerator Physics of the General Advisory Committee to the Atomic Energy Commission and the President's Science Advisory Committee, in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), pp. 103-104. Date of report given on copy of report in LBL Archives, Box 5.

79. Fiscal Year 1975 (FY 1975) ran from July 1, 1974 to June 30, 1975, while fiscal year (FY 1964) ran from July 1, 1963 to June 30, 1964. Text quotes from Atomic Energy Commission, "Summary Notes of Briefing on the Report of the GAC-PSAC Panel of High Energy Accelerator Physics," May 9, 1963, p. 4, unpublished document, Seaborg Papers; and Lillian Hoddeson interview with Norman Ramsey, January 22, 1980. Also see General Advisory Committee and President's Science Advisory Committee, Report of the Panel on High-Energy Accelerator Physics of the General Advisory Committee to the Atomic Energy Commission and the President's Science Advisory Committee, in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), p. 108.

80. Brookhaven researchers eventually got permission from the AEC to increase beam intensity at the AGS rather than build storage rings. Maurice Goldhaber letter to Paul McDaniel, October 10, 1963, Directors Office Files, Brookhaven National Laboratory and interview with John Blewett, November 17, 1986. Text references: "Design, Location, and Construction of the 200 BeV Accelerator," Chapter 7 in Richard Hewlett, The Atomic Energ Commission During the Administration of President Lyndon B. Johnson, Volume I, Part I, p. 139 and Paul McDaniel letter to Maurice Goldhaber, April 2, 1963, Green Papers.

81. At this time billion electron volts was commonly abbreviated "BeV." Since the accelerator is widely known by this name, this abbreviation is used here when referring to the machine. All other energy units are expressed in the more modern term, GeV. See Electron Volt.

Chapter 4: New Arrangements for Managing and Siting the Accelerator--1963-1965

Introduction

On December 7, 1963 Kermit Gordon, Director of the Bureau of the Budget wrote an acerbic memorandum to President Lyndon Baines Johnson to argue against authorization of the MURA accelerator. As part of his argument Gordon complained: "High energy physics is already by far the costliest and most lavishly supported field of basic science." He also referred to "the searching questions currently being raised about the levels of research and development (i.e., big science), in which high energy physics is sometimes singled out as an example of over-emphasis. . . ." While Gordon's claim that high energy physics was the most costly field of basic science is debatable, there is no doubt that by 1963 the sub-field faced criticism from those who questioned the relative merit of "big science," a term coined by physicist Alvin Weinberg to emphasize the expense and large scale of modern research. At a time when huge new accelerator projects were being planned at Berkeley, Brookhaven, and MURA, accelerator funding was a very visible portion of a rapidly expanding budget for research which lacked abundant practical applications. (1)

As the AEC struggled to cope with funding pressures prompted by the increased expense of accelerator projects, the physics community continued to grapple with other issues arising from the increased scope of the next large accelerator. In September 1964, as Berkeley

physicists labored to design a larger alternating gradient synchrotron, Edwin Goldwasser, former Ramsey Panel member and physicist at the University of Illinois, wrote a letter to McMillan which expressed the opinion of a growing number of high energy physicists:

> I believe that the motivation for construction of new accelerators is very different from what it was when the bevatron was built. During Ernest Lawrence's day a tremendous evolution occurred from his first cyclotron to his last. The organization which operated these facilities evolved along with the hardware and instrumentation. We have now advanced one more generation to a point at which national motivation expressed essentially unanimously by the entire scientific community is required in order to obtain the government support that is needed .... I do not believe that an advisory committee is an adequate instrument through which such confidence will be generated. I believe that the actual control must be in the hands of an organization similar, perhaps, to AUI, but national in its scope. (2)

Although the sentiments expressed by Gordon and Goldwasser were not new, such feelings had never before been expressed so forcefully or with such anticipation that traditional practice would change.

When LRL received funding for the 200 BeV design project in mid-1963, McMillan and other Berkeley researchers were confident that the laboratory would also construct the accelerator at a California site and then manage the accelerator facility. In the next year and a half, however, the federal budget tightened, the MURA proposal was defeated, young, active experimentalists began agitating for outside user rights, and conflicting views about the relative value of basic and applied research evident in the 1940s remerged. Despite LRL attempts to plan the accelerator along traditional lines, by early 1965 a new organization had been formed and plans for managing and siting the new

accelerator had slipped from LRL control. To understand how mutually reinforcing pressures and resulting social tensions changed plans for the new accelerator, it is necessary to consider two questions. First, what was the atmosphere both inside and outside the physics community after the AEC decision to authorize funding for BNL and LRL and how did this atmosphere affect the 200 BeV project? Second, how did new plans for managing and siting the accelerator evolve?

Design Authorization and the 200 BeV Project

McMillan wasted no time in fulfilling his obligation to arrange an advisory committee for the 200 BeV project. On May 10, 1963 he sent McDaniel a suggested list of participants, explaining that all 10 candidates had "agreed to serve as such a committee, and have been informed that their appointment is subject to AEC approval." As agreed, the list included candidates from various parts of the United States: 4 were from the East, 4 were from the West, and 2 were from the Midwest. McDaniel replied immediately, assuring McMillan that the AEC would likely approve the list. He also suggested that McMillan draft "a formal description of the duties and responsibilities" of the committee after conferring with his long-time colleague Wolfgang Panofsky, director of SLAC, who had recently established such a committee. The proposed committee included Richard H. Dalitz from the University of Chicago, William B. Fretter from the University of California, Berkeley, Murray Gell-Mann, from Caltech, Maurice Goldhaber from BNL, G. Kenneth Green from BNL, Vernon W. Hughes from Yale, Leon Lederman from Columbia,

Panofsky from Stanford, and Herbert York from the University of California at San Diego. William F. Fry from the University of Wisconsin was designated as chairman. (3)

After obtaining suggestions from Panofsky, McMillan dutifully submitted the requested advisory committee charter in July 1963. In line with previous Berkeley suggestions, he proposed that the 10-member committee would have national representation, be nominated by the LRL director and approved by the AEC, and report to the LRL Director. The committee would also "set its own agenda" and have the right to "consider all technical, scientific, and administrative matters connected with the high energy accelerator design study." In the next several months McMillan's charter was distributed to the committee, the AEC approved committee members, and members formally accepted. All the candidates suggested by McMillan eventually served on the committee, with the exception of Dalitz, who planned to be out of the country and therefore was replaced by Lee C. Teng of Argonne. (4)

While these arrangements were being made for the advisory committee, Berkeley researchers were also enthusiastically organizing the 200 BeV project. Shortly after design authorization Lofgren was officially named director of the study and Smith his assistant. As promised, a large amount of the laboratory's considerable talent and manpower was channeled into the project. To handle magnet lattice calculations and other theoretical considerations, Lofgren drew from Berkeley's stable of accelerator theorists, which included Lloyd Smith, Alper Garren, and L. Jackson Laslett. He assigned other specialists to

the design of various components: magnet designers included Charles G. Dols and R. A. Kilpatrick; magnet power supply designers included Fritz Grutter and H. W. Vogel; injection designers included H. P. Hernandez, William A. S. Lamb, and R. B. Meuser; radiofrequency designers included Quentin Kerns and Jack Peterson; control designers included Edward C. Hartwig; accelerator system designers included Glen Lambertson and mechanical systems designers included William Salsig. In addition, radiation problems were investigated by several researchers, including W. S. Gilbert and Ralph Thomas, experimental use was investigated by Berkeley experimentalists, including Denis Keefe, George Trilling, and William A. Wenzel, and details involving the accelerator plant and site were investigated by a variety of specialists, including Donald G. Eagling, W. Popenuck, and H. A. Wollenberg. Primary responsibility for estimates and schedules fell to W. W. Chupp. (5)

From the onset, the average effort equalled 55 full-time workers, including the physicists and engineers involved in the design, as well as programmers, draftsmen and other support personnel. Ultimately, more than 130 people contributed to the project. Although 8 scientists from other institutions participated in the project while on leave, including a Brookhaven and a CERN physicist, Caltech researchers did not join the project. By the end of 1963 the design team produced 9 new technical reports; by the first months of 1965 they would produce 70 additional reports covering every aspect of the design. (6)

The design of the main ring magnets was a particularly crucial consideration, since 600-700 magnets would be needed. A computer code

was used to calculate the fields of various configurations of steel and conductor. Although both "H" shaped magnets were considered, Berkeley researchers finally focused attention on the more conventional "C" shaped magnets. As planned, prototype magnet models were constructed. Another pressing issue was the choice of injector. Although Berkeley researchers initially favored linac injection, as studies progressed the problems of linac injection appeared increasingly formidable and injection with a booster appeared more attractive. The intense Berkeley design effort was aided by a series of meetings on accelerator design, begun in January 1964 to bring together accelerator experts from LRL, Brookhaven and CERN, where planning had started for a 300 GeV strong focusing synchrotron. (7)

To provide the promised detailed proposal for construction of the machine, Lofgren planned to organize the results of the in-depth study into a massive design report. When design study editor Francis Cole began to compile results for the report, the conservative bent that had characterized early design studies was evident. Faced with rising tensions in the physics community and government and aware that LRL had been chosen for the design study because of their experience and expertise, team members felt that approval to continue the expensive project hinged on providing a design report worthy of the laboratory's fine reputation. As Keefe recently explained, they felt they had to produce a conservative, highly reliable design and cost estimate because their "feet were being held to the fire." (8)

In January 1964, when the Berkeley 200 BeV project had been underway for about 8 months, Lofgren met with McDaniel and other AEC Division of Research staff members to discuss the status of design efforts. According to Lofgren's meeting notes, after he presented the current schedule, which showed that LRL would produce the proposal in October, 1965, they discussed arrangements for obtaining architect-engineering services and guidelines for preparing the final proposal and cost estimates. The next topic of conversation was siting plans. In line with previous informal discussion, but in contrast to formal statements, the AEC Division of Research staff was willing to facilitate Berkeley efforts to make arrangements for a California site for the accelerator. Lofgren mentioned that they had a site in mind, Camp Parks, a 3,636-acre site in the Livermore-Amador Valley, 37 miles from San Francisco. AEC staff member Phillip McGee agreed to "make a private and informal inquiry" to see if the site was already earmarked for national defense purposes. In the meantime, McGee and McDaniel agreed, it was time to start "formal steps regarding site selection." The AEC staff advised that Berkeley researchers go through the proper channels, pointing out that there were AEC standard procedures for the process, but admitted that "some simplification of the procedures may be worked out . . . because of the nature of" the LRL project. Lofgren happily noted that it appeared they would "get a very high degree of cooperation from the Research Division Staff." (9)

In early 1964, McMillan had every reason to feel proud about the progress of the 200 BeV design study. The advisory committee was being

organized, as promised, an enormous design effort had been launched, and a cooperative relationship had been established between the AEC staff and the design group. Despite these optimistic signs, pressures were building both in the physics community and in government that would soon threaten Berkeley prospects.

As McMillan understood, there was a growing sensitivity within the physics community about how the accelerator would be used. The President's Science Advisory Committee (PSAC) convened a committee of high energy accelerator users, chaired by Myron L. Good of the University of Wisconsin, to review the Panel's findings. An official summary of committee comments reveals that the group endorsed the Ramsey Panel recommendations but noted "the need for emphasizing the national character of new facilities," stressing that "access should be available not only to the accelerator, but to on-site support activities, and that outside groups should have a voice and responsibility in certain aspects of laboratory management." (10)

Leon Lederman, who served on both McMillan's adisory committee and the Good Panel, was particularly eager to reinforce and extend panel recommendations. Lederman, the current director of Fermilab, was then a lively 42-year old physicist from Columbia University, well-known for his participation in a number of important experiments, including the 1962 AGS experiment that confirmed the existence of two neutrinos. In June 1963, Lederman wrote an informal report for a 1963 Brookhaven summer study entitled "The Truly National Laboratory (TNL)," using the initials TNL as a pun on BNL, which he felt was not functioning as a

truly national facility. Lederman noted that cooperation and enthusiasm for the new facility would only be assured "when it is clear that the new facilities are accessible as a right to any physicist bearing a competitively acceptable proposal." The ideal laboratory he described had complete on-site facilities for outside users, resources for facilitating individual experiments, an accessible and pleasant site, scheduling and advisory committees to assure fairness in the allocation of beamtime and free communication between management and users, and a strong laboratory director "responsible to a governing body of wide national representation." Lederman also suggested in the report that a committee be formed to ensure that TNL characteristics would prevail as Berkeley and Brookhaven pursued their design studies. (11)

Lederman's battle cry for outside user rights was part of a general trend spelling trouble for LRL. Like Lederman, many young, active experimentalists were worried about access to the next large accelerator, which would be the only one of its kind. Since career prospects often hinged on access to the most powerful accelerator, equitable access for outside users was inevitably an important issue. Although some, like Lederman, complained that Brookhaven did not offer fair access to outside users, for many experimentalists, the likelihood of LRL control of the next high energy accelerator prompted particular concern. While Brookhaven had been established to serve a wide range of users, LRL had not. Berkeley's approach to outside user access reached back to the laboratory's beginnings in the 1930s. Lawrence built the laboratory with private funding, and although he always accepted

requests from researchers of other institutions who wanted to do experiments on LRL machines, such requests were granted only on an invitational basis. When federal funding began after World War II, the practice continued, much to the annoyance of many outside the Berkeley circle. In addition, participants agree that "anti-Berkeley" feelings were exaggerated by long-term animosities based on East-West Coast rivalries. (12)

Although McMillan acknowledged the growing pressure for equitable access, he diplomatically but firmly downplayed the issue, despite the fact that he was in contact with all advisory committee members, including Lederman. In his July letter to McDaniel, for example, he proposed that the advisory committee might "also consider means by which facilities which may be constructed on the basis of our design study . . . will be made available nationally, on an equitable basis, to all qualified experimenters, " but added the proviso that such steps should be taken "while safeguarding the quality and productivity of the program." Lederman's strong advocacy of outside user rights and McMillan's insistence upon qualifying such rights was an early warning of the conflict that would arise between the LRL director and his advisory committee. (13)

As tensions rose within the physics community, MURA was causing further headaches for the AEC staff. After the 1960 Piore Panel remarked that unexpectedly high beam intensities at the AGS and PS made the need for high intensity machine questionable, the AEC was flooded with impassioned pleas for the long-planned ambitious FFAG accelerator.

MURA researchers were able to elicit spirited support from a number of corners. It was relatively easy to gather the backing of Midwestern university presidents, who had nothing to lose and quite a bit to gain if a prestigious facility were built in their region. (14)

The group also had considerable support within the physics community. MURA argued convincingly that the Midwest deserved a first-class machine and the group's innovative design work inspired admiration among accelerator specialists. In line with the opinions of the Ramsey Panel, which included two important Midwestern physicists, most physicists preferred to have all three machines. While many approved of the high intensity machine, most physicists, including MURA supporters, sided with the Piore and Ramsey Panels. All agreed that the extension of the high energy parameter, which would allow the discovery of new particles, was more scientifically important than extending the high intensity parameter. After all, MURA's original aim had been to make a novel contribution that would provide entry into the AEC's budget, an orientation that encouraged creativity while setting the group apart from the mainstream. (15)

When faced with the Ramsey Panel's conditional approval and the AEC's reluctance to make a final decision, MURA physicists threw themselves into a desperate, eleventh hour attempt to save their accelerator project. Particularly active in the effort were Elvis Stahr, an elegant man who spoke with a slight Southern accent, and Bernard Waldman, the usually soft-spoken, congenial man who got angry with McMillan at Dubna. Stahr, then the president of Indiana

University, served as MURA president and Waldman was MURA's staff director. The Berkeley and Brookhaven projects were the other large accelerator projects considered by the AEC that year, and both had been given authorization in April. As Waldman recently explained: "For a long time I had felt Midwesterners were not getting a fair shake. By 1963 I wondered if the Midwest would be a permanent step-child." (16)

Such sentiments were taken up with gusto by Midwestern politicians. A 1962 economic recession inspired congressmen to renew traditional concern with the geographic distribution of research funding. Many Midwestern representatives were particularly inflamed over the issue. For example, Minnesota's Democratic Senator Hubert H. Humphrey accused in a 1964 hearing that the federal government was "making a vast intellectual wasteland out of America by having the R & D contracts concentrated as they are in limited geographical areas." Midwestern politicians quickly saw the particular advantages of championing the FFAG. Since the accelerator would cost nearly $150 million, it was big enough to warrant political attention. By winning the project for his constituents, a congressmen could not only promise new jobs, but also greater prestige and increased industrial opportunities for his district. Thanks to the effort of Stahr, in the summer of 1963 President Kennedy received a steady stream of form letters signed by Midwestern congressmen stating that MURA "represents only a crucial first step in restoring the Midwest to its rightful place--along with the East and West Coasts--as a major center for educational research." Similar letters were also sent to BOB and the AEC. As Waldman remembers: "We

believed that our efforts had been successful and that Kennedy would put us in the budget." (17)

Despite this optimism, MURA soon received another jolt. In July the GAC met to consider a number of issues. As in the 1950s, this prestigious organization regularly advised the AEC on policy matters, including accelerator requests. By this time the sages of the physics community were willing to oppose MURA more bluntly than even before. Although the current committee included John Williams, who had served on MURA's first board of directors, the GAC admitted that the majority of its members "felt there is not adequate justification at this time for approval of the MURA accelerator construction." To justify their decision, GAC members pointed to the high cost of the machine and the fact that it would not extend the high energy frontier. (18)

This damning decision, in light of budget cuts and increased political support for MURA, placed the AEC in an awkward position. It was time for AEC chairman Glenn Seaborg, McMillan's long-time associate, to become more directly involved in accelerator planning. Seaborg, a tall gangly man who served as Chancellor of the University of California at Berkeley just before coming to the AEC, strongly believed in the power of negotiation. Against the backdrop of the continuing ill-will between Argonne and MURA, he was already exploring schemes with Midwestern physicists that would provide a machine for MURA and conciliate internal Midwestern tensions. Discussion topics included schemes for incorporating the MURA design into future plans to upgrade the AGS and plans for building MURA near Argonne, perhaps with the same

management. About the time the GAC made its decision in July, some Midwestern physicists resurrected the suggestions that MURA could host a design study for a high energy machine. (19)

As discussion continued on the various contingency plans, MURA's prospects grew dimmer. In mid-November a reconvened Ramsey Panel reaffirmed its stand on the relative merit of high energy and high intensity. Later that month, President Kennedy, a supporter in the eyes of MURA researchers, was assassinated. In early December BOB Director Kermit Gordon cast the Bureau's vote on MURA. In a memorandum to the new President, Lyndon Johnson, Gordon recommended that MURA not be authorized due to lack of "resounding scientific support" for the machine, potential adverse affects on funding for the 200 BeV machine, Argonne's newly funded Zero Gradient Synchrotron, and "current fiscal stringency . . . ." Later in December, just a month before the budget would be presented to Congress, the AEC finally made its decision. In a memorandum to Johnson, Seaborg explained that although the Commission still favored authorization of MURA, the project was not to interfere with high energy proposals and could not be placed "in as high a relative priority as those items which it was asked to identify in its letter of December 13, 1963 to the BOB for possible deletion from" the current budget request. (20)

On December 20, Johnson met with Wiesner, Seaborg, Gordon and other BOB representatives, MURA supporters, including Stahr and Waldman, and several Midwestern congressmen, including Humphrey. After listening to several of them list the advantages of building MURA, Johnson read a

statement listing its disadvantages. Unbeknownst to MURA representatives, Wiesner, who was soon to return to MIT, had prepared the statement ahead of time at the President's insistence. After a vague remark about the difficulty of funding MURA, Johnson dismissed the group. As Waldman remembers: "I felt dazed. I hadn't been sure we would win, but I never thought it would end like this." (21)

Seaborg recorded in his diary that Johnson called him several hours after the meeting, acknowledging that they were "going to be in serious trouble in Minnesota." Johnson therefore asked Seaborg to be prepared to offer something to congressmen likely to be upset by the decision to kill MURA, especially Humphrey. In fact, Johnson's negative appraisal of MURA was destined to send waves of dismay through both Congress and the physics community. The day after the meeting, Stahr wrote an anguished letter asking that Humphrey urge Johnson to reconsider the value of the MURA proposal. The letter complained, in part, that the statement read by Johnson "omitted some highly relevant matters, and its prejudice showed on its face. . . . I don't know who wrote it, but it sounded exactly as if it might have been prepared by someone from New England or the West Coast. . . ." An angry Humphrey quickly wrote to Johnson, enclosing Stahr's letter. (22)

By late January 1964, when the President's budget was submitted to Congress with no mention of the FFAG, MURA's fate was sealed. For the first time, a major accelerator project was cancelled. By this time the physics community had mended fences the best it could. To quell the long-term animosities in the Midwest, intense negotiations were

conducted among MURA, University of Chicago and Argonne representatives, Seaborg and his staff, and other important leaders of the physics community, including Ramsey and Frederick Seitz, president of the National Academy of Sciences. MURA representatives had received abundant reassurance that both Wiesner, with his New England affiliations, and Seaborg, with his West Coast affiliations, had supported the project and that the offensive statement had been written at Johnson's insistence. It was finally agreed that steps would be taken to include more participation at Argonne from the complete community of Midwestern physicists and consequently the University of Chicago joined other Midwestern universities to form Argonne Universities Association, a consortium responsible for formulating general policy at Argonne. In addition, arrangements were made for MURA physicists to join the Brookhaven effort to design a 1000 GeV machine. In an effort to mend political fences, these plans were explained to Humphrey in a letter from Johnson and at a January 17 meeting with Wiesner, Seaborg, and Seitz. On January 20 the AEC issued a press release officially announcing MURA's fate and future plans. (23)

According to Seaborg's notes of the meeting with Humphrey, after the plans for MURA had been discussed, Seaborg asked whether "the 200 Bev accelerator would go ahead at Berkeley as planned, and Wiesner, especially, said that that would be his interpretation, and the others seemed to agree." Seaborg's records further reveal that despite his earlier support of a MURA collaboration with Berkeley, the Chairman ultimately argued against the idea. As he told MURA representatives on

January 14, such a plan was contrary to the recommendations of the Ramsey Panel and would eventually lead to "a clash over the final decision." This is hard to take at face value since he agreed to a MURA/Brookhaven collaboration which could be denounced by the same arguments. Seaborg further explained to Goldwasser a few days later that Seitz had suggested that MURA collaborate with Brookhaven rather than Berkeley and Wiesner had endorsed the plan. "Wiesner's reason was that the West Coast accelerator was farther along, and Seitz said MURA works better with Brookhaven." While this reasoning makes more sense, these conversations leave the possibility open that Seaborg was willing to make some effort to protect the interests of his long-time Berkeley colleagues as arrangements were being made for MURA. At the same time, as Chairman of the AEC, Seaborg's constituency reached beyond LRL and he could not professionally afford to continue to protect Berkeley once the situation became sticky. As the February 1962 conversation with Wiesner indicated, from the outset he was sensitive about his LRL affiliation, and although Haworth had recently resigned his post as AEC commissioner to head the National Science Foundation, the situation remained doubly ticklish because Haworth had been replaced by Gerald Tape, another Brookhaven physicist. Thus, Stahr's inflammatory accusation of executive branch favoritism towards the East and West Coasts did not bode well for a future protective attitude on the part of the AEC toward Berkeley or Brookhaven. (24)

At just this point, Seaborg was confronted with evidence of a change in attitude towards the new accelerator among leaders of the

physics community in the midst of continuing internal tension. Seaborg's records reveal that as the final painful arrangements were being made for MURA on January 17, he spoke with both Ramsey and University of Chicago President George Beadle. In contrast to his Panel's earlier recommendation, Ramsey now wondered whether the location of the next large accelerator "should be fixed . . . ." Similarly, Beadle related that at a policy meeting convened to discuss the MURA problem many physicists felt "the next generation machine ought to be national, and the site ought to be kept open." After the death of MURA, many Midwestern physicists felt it would be blatantly unfair to build the next accelerator on the West Coast. Since the expensive accelerator needed the unamimous support of the physics community, leaders of the physics community knew that such feelings would have to be appeased as plans proceeded for the new accelerator. (25)

As MURA's fate was being settled, arrangements for the 200 BeV accelerator were also taking on a new complexion for government leaders. As Seaborg told Goldwasser on January 23, BOB now recognized that two accelerators were "on the horizon and that we have a national program." The problem, he explained, was that there were "two accelerators and three sites." Seaborg warned that it might "wind up as one accelerator, with an order to everybody to get together, if the scientists fail to come to agreement." The dissatisfaction of Midwestern scientists and politicans, made it difficult for Seaborg to justify funding for accelerators on the East and West Coasts. In his conversation with Goldwasser, Seaborg explicitly stated, for the first time, that "the

site of the 200 Bev accelerator . . . is still undetermined." In a later conversation with Beadle he remarked that "the Stanford accelerator is the last of its kind. The new ones will be of the consortium management type." As the Chairman's change in attitude indicates, Berkeley's prospects for managing and building the new accelerator at a California site were perceptibly dimmer in early 1964 than they had been a mere six months before. (26)

Summary: Design Authorization and the Effect on the 200 BeV Project

After the AEC decision to authorize the Berkeley and Brookhaven design studies, the atmosphere both inside and outside the physics community eroded Berkeley's prospects for managing and building the accelerator. Inside the physics community, the battle cry for outside user rights, already apparent by mid-1963, presaged trouble for McMillan in arranging for management of the facility. In addition, as MURA went through its death throes in the last six months of 1963, a number of factors conspired to link the project to the East and West Coast design studies, throwing into jeopardy Berkeley's prospects for building the machine. MURA was able to gain support from Midwestern politicians by pointing to the inequitable geographical distribution of research funds, and since MURA's final competitors were in fact Berkeley and Brookhaven, this issue came into even sharper focus when MURA was defeated. It was particularly easy to accuse Berkeley of unfair privilege because of its reputation and because arrangements had been made for MURA researchers to collaborate with Brookhaven researchers instead of those at Berkeley.

In addition, attention was more squarely focused on LRL than on BNL because the Berkeley machine was the next to be built. Thus, by early 1964, in the midst of continuing turmoil within the physics community and the budding realization that the 200 BeV machine might not survive budget cuts if such divisiveness continued, important leaders, including Ramsey and Seaborg, acknowledged that the site of the new accelerator should be kept open and that it should be managed by a national corporation.

Management and Siting Plans Evolve

In February 1964, a month after MURA's fate had been announced, LRL staff members met with the 200 BeV advisory committee. Further meetings were held that spring. While waiting for the committee's response, McMillan encountered further evidence of the change in mood about management issues, a change accelerated by the death of MURA. Inspired by a February letter from AUI president Edward Reynolds summarizing Brookhaven high energy physics plans, Milton White, director of the Princeton-Pennsylvania Accelerator, widely disseminated a letter in April addressed "To Friends of Fundamental Particle Physics." White proclaimed that "realistic proponents of high energy machines . . . must strive for a realistic goal which takes full cognizance of the political and economic realities of the present era." White suggested that a "new, contracting, managing group . . . be set up to run the laboratory for the benefit of all scientists everywhere." Asking "that all regional, parochial interests be swept aside" for the sake of unanimous support, White proposed that the new laboratory "be located in some central, but on the average convenient, area not previously associated with high energy physics." (27)

On April 19 McMillan responded to White, who had apparently sent him a personalized version of the generally distributed letter. After acidly remarking that such proposals "had perhaps better remain buried in the archives of groups like the Good Panel," McMillan declared that an accelerator was not "an industrial plant, to be located where economics dictate and operated by hired hands from the region and

managers exported from the home office." He argued that those designing, constructing, and operating a large accelerator "should have a sense of identification and continuity that is hard to establish in a new group. . . ." Stressing that scientists work best in a multi-disciplinary environment close to an established university, McMillan also insisted that it would be time-consuming and difficult to form a nation-wide management group. On a more personal note, McMillan said he resented "the implication that the interest of Berkeley is regional and parochial." He explained that LRL researchers were only "seeking to continue the line of development started many years ago by Professor Lawrence," a goal they felt to be compatible with the desire to create a national facility. McMillan was also "distressed by the bald assertion" that Berkeley plans were "doomed to failure," fearing that this unfortunate opinion, which many had already expressed to him, would gain credence from repetition. He concluded that "the best way for 'friends of fundamental particle physics' to promote the development of high energy facilities" was to "support fully the recommendations of the Ramsey Panel. . . ." (28)

Ironically, a few weeks after evoking Ramsey Panel recommendations, McMillan received a warning from Ramsey based on other Panel findings. In a May 13 letter written at the request of Lederman, Ramsey remarked that he personally didn't "care as to the form of the laboratory administration provided it is fair and efficient, with favorable arrangements for visiting scientists from universities deprived of the opportunity of having their own accelerators." However, he explained,

during Ramsey Panel discussions he had discovered "that a large number of physicists believe that various formal organizational requirements must be met before they can support a unique facility laboratory." In his opinion, such views had arisen because of the troubled relations between Midwestern physicists and Argonne and because "a number of institutions and physicists are--rightly or wrongly--jealous of the University of California. . . ." While such opinions had been implied by the Good Panel and Lederman's "Truly National Facility" and more overtly stated by White and others, McMillan had never before been confronted so directly. In view of the current mood, Ramsey further urged that LRL "make a reasonable number of concessions to assure those who might become opponents of the accelerator that the laboratory will be fairly administered without too much of the advantage going to a single institution." (29)

Upon receiving a May 26 letter from William Fry, McMillan discovered the magnitude of the concessions being requested by his advisory committee. Fry first noted the committee's "satisfaction with the technical progress and . . . high confidence in the capabilities of the LRL group to produce a successful design." After this vote of confidence, Fry presented a management scheme reminiscent of that discussed by the AEC in 1962 but diametrically opposed to LRL plans. The committee proposed that McMillan initiate plans for the University of California to form "a Corporation of a national group of major universities active in the field of high energy physics." This corporation, which would be led by a Board of Trustees with national

representation, would sponsor the proposal to construct the accelerator "based on the results provided by the current LRL design study." Although "the talents and experience" of LRL scientists and engineers would be "utilized fully" during the design and construction of the accelerator, as the facility approached "the operating and research phase, its management should become independent of LRL." While LRL would hire senior scientific and technical staff members, permanent appointments would be made only with the approval of the Corporation, which would eventually appoint a director "responsible to the Corporation for the program of the laboratory." (30)

Despite Ramsey's urging to make concessions, McMillan vehemently opposed the suggested scheme. As Fry remarked in a letter to committee member Lee Teng, after meeting with McMillan it became "clear" that LRL would "not accept a National Board for the construction stage. . . ." In an attempt to iron out a mutually acceptable plan, Fry subsequently made plans for a fall meeting between LRL management and the committee. (31)

As tensions rose over arrangements for managing the new facility, other tensions were erupting that would affect the project. At just this juncture, interest in science and technology funding was intensifying in Washington. Budgets were larger than ever, and some had already begun to question the social value of research and the role of scientists in policymaking. In September 1963, for example, political reporter Meg Greenfield remarked: "As presiders over the national purse, are the scientists speaking in the interest of science . . . government or . . . their own institutions? Is their policy advice . . . offered

in furtherance of national objectives--or agency objectives--or their own objectives?" A month after Greenfield's statement Congressman Albert P. Thomas, chairman of the House appropriation subcommittee in charge of independent science agencies proclaimed: "We hear that research money is running out of the ears of grant recipients . . . . It's time to take a look." By late 1963 congressional investigations were indeed being formed and by the mid-1960s various aspects of science and technology funding were under close scrutiny by a number of congressional committees. (32)

Although high energy physics was not the only target of scrutiny, the field received quite a bit of scrutiny. For one thing, Congressional attention had already been drawn by the MURA controversy. As indicated by Humphrey's remarks at the 1963 hearing, even before its defeat, MURA was seen as a cause célèbre in the fight for equitable geographic distribution of research funding. As the issue grew more heated with increased overall congressional interest in science and technology funding, MURA's fate only added fuel to this political fire. Although Johnson insisted in his conciliatory January letter to Humphrey that the Midwest already received its fair share of funding, an argument that was later vigorously defended by Seaborg and others, the President also stated that he fully shared the "strong desire to support the development of centers of scientific strength in the Midwest. . . ." In response to continuing congressional agitation about perceived inequities, a number of steps were taken. In 1965 Johnson issued a memorandum entitled "The Strengthening of Academic Capabilities for

Science Throughout the Country," which advocated allocating funding to increase "the number of institutions capable of performing research of high quality." In addition, as reported in a 1966 congressional hearing, the NSF was directed to develop "a more equitable distribution of . . . funds . . . to avoid the concentration of such activities in any geographic area." Although this issue reached far beyond consideration of high energy physics funding, the Berkeley 200 BeV project was particularly vulnerable to criticism by those protesting geographic funding inequities since it received funding at the time when MURA was cancelled. Thus the funding distribution debate formed an important part of the political background as arrangements were being made for the new project. (33)

While MURA's death also caused a great deal of controversy within the scientific community, the terms of debate were different there than in Congress. As elected officials with a geographically based constituency, congressmen saw the defeat of MURA as a flagrant example of unequal distribution of federal monies, a long-standing complaint extending back to the debates surrounding the establishment of the AEC and NSF. (See Chapter 1) Within this context, a central issue was how to weigh merit and geographic distribution in the setting of funding priorities. In contrast, although some Midwestern high energy physicists were terribly angry that the imbalance in funding allocations left them without a convenient, first-class accelerator facility, few advocated the dilution of meritocracy, one of the great pillars of scientific tradition. Within the physics community, the complaints of

Midwestern physicists were congruent with the movement for outside user rights. Within this context, the central issue was how to allocate resources to deserving researchers when the number of first-class facilities was strictly limited. Despite these differences, the combined pressure from Midwestern politicians and physicists formed a potent push for a Midwestern accelerator. (34)

In addition to the issue of geographic distribution of funding, other issues conspired to draw attention to high energy physics in general and accelerator funding in particular. As Figure 1 shows, at just this time the proportion of basic research funding was rising relative to overall federal expenditures. Whereas basic research consumed just over .1% of federal expenditures in the late 1950s, by 1965 it commanded about .25%. High energy physics, which unlike other subfields received almost all its funding (90%) for the Atomic Energy Commission, commanded a very noticeable portion of the basic research budget for that agency. Moreover, since accelerator projects required large chunks of construction funds, they formed an especially visible portion of the high energy physics budget. (35)

Such considerations were not lost on the Joint Committee for Atomic Energy (JCAE), the powerful congressional overseer of the AEC and traditional champion of atomic energy programs, including accelerator projects. In the annual hearings on the AEC budget held in early March 1964, Chester Holifield, JCAE chairman and Representative from California, pointed out that the high energy physics budget had jumped from $53 million in 1960 to $135 million in 1964. He noted that the AEC

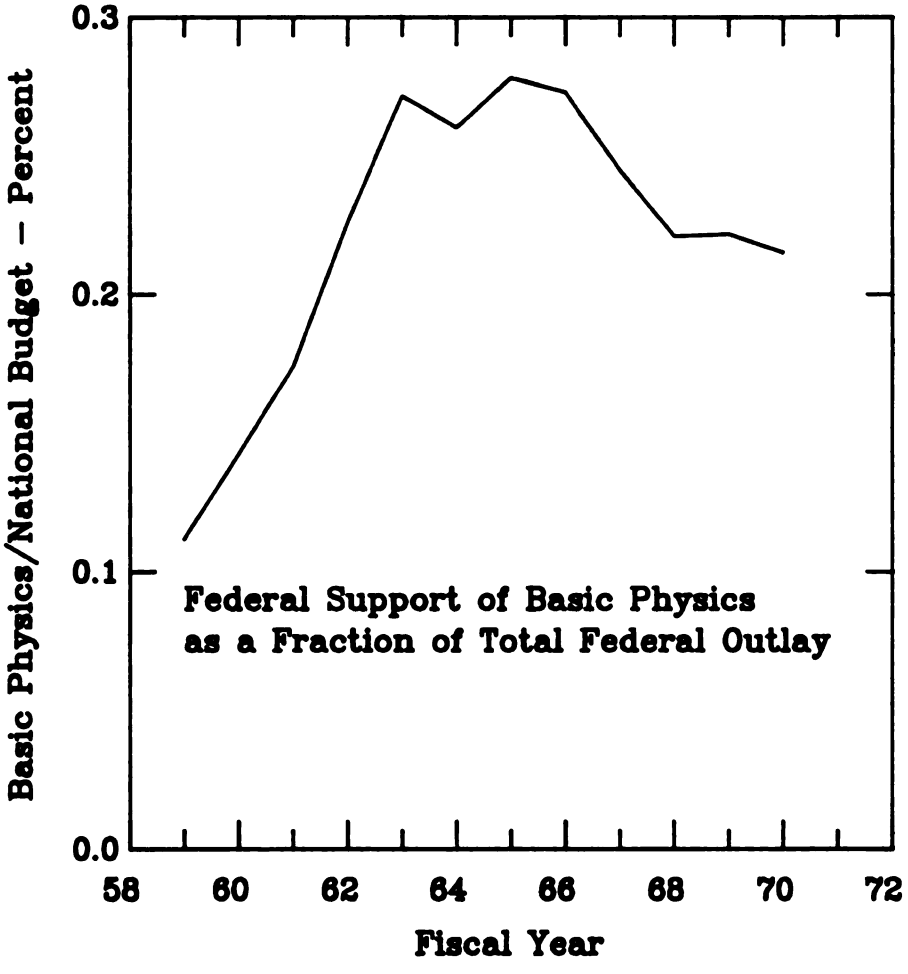

Figure 1.--Federal obligation for basic physics research as a fraction of the total federal budget. Data taken from Physics Survey Committee, National Research Council, Physics in Perspective (Washington, D.C.: National Academy of Sciences, 1972), p. 648.

shouldered about 90% of the financial burden of the subfield, but reminded the executive branch officers that it was "sometimes hard to sustain in the Congress, the appropriations for basic research: it is much harder than it is for applied research and development." Considering this fact of life, it concerned him that "the overall budget of the AEC stays about the same, but the high-energy part of it is increasing. . . ." Conceding that it was "perfectly natural" for scientists to have a limitless supply of ideas and ambition for exploring them, he nonetheless worried that in accommodating such researchers, decisionmakers were "squeezing to death many fields of science." "Very frankly," he concluded, "the Congress is becoming alarmed . . . ." (36)

After this warning, Holifield began to quiz Princeton chemist Donald Hornig, the new president's science advisor, on proposed accelerator projects. Later the JCAE chairman pointed out that the MURA accelerator had been cut from the executive branch budget because of a tight budget, noting that millions were being spent on the design of the 200 BeV accelerator. He queried: "What assurance does the executive branch provide that this planned 200-billion-electron-volt accelerator will not also be scrapped in time?" To successfully champion AEC budget requests, Holifield insisted, the JCAE needed a defensible national policy for high energy physics which had explicit presidential approval. The policy, which should take into account long-term projected costs for such expensive projects as the proposed 200 and 1000 GeV accelerators, would not act as "a rigid plan," but as "a general plan with a forecast

of expenditures. . . ." Holifield added that in light of the current high cost of accelerator projects it would be necessary to reassess periodically the scientific utility of proposed projects, "what has happened in science in the meantime . . . and what the fiscal situation of the Nation is." (37)

By the end of March, Hornig submitted a 3-page policy statement to John O. Pastore, JCAE vice-chairman and Senator from Rhode Island. Hornig stated that high energy physics was making rapid progress in the understanding of fundamental physical laws and noted that "it has been true historically that in the long run these understandings have had a very great impact on science and technology and on all mankind." Thus, he argued, it was "in the national interest to continue vigorous exploration" of the field. He therefore urged that government support a two-step advance in energy, with the second accelerator providing energies of 1000 GeV in about 20 years. Hornig also wrote to Johnson, enclosing the letter to Pastore and presenting proposed time schedules and expenditures. Hornig estimated that the 200 BeV project would be up for authorization in fiscal year 1967 (July 1966 to June 1967), cost about $280 million, and be completed by June 1973. Although figures for the 800-1000 GeV machine were more speculative, he guessed the larger accelerator would be up for authorization in fiscal year 1968 (July 1967 to June 1968), cost about $800 million, and be completed by June 1976. To provide a more complete national policy statement on high energy physics, the AEC subsequently made plans to compile a much longer report to be ready in time for JCAE hearings the next spring. (38)

By the time of the March 1964 JCAE hearing, complaint about the rising cost of high energy physics and its effect on other fields was also being raised within the physics community. At the hearing Holifield quoted statements by Eugene Wigner. As a member of the 1960 Piore Panel, Wigner's primary complaint had been that high energy physics was draining an undue amount of scientific manpower. He now noted that escalating high energy physics funding was more serious. "As a result of the economy measures" he explained, "there is a serious shortage of funds in others areas, and even low-energy nuclear physics suffers heavily." Wigner was not the only one worried about the situation. Alvin Weinberg, Director of Oak Ridge National Laboratory, and like Wigner, a long-time proponent of the development of nuclear reactors, had been issuing warnings about the social implications of the cost of big science for several years. With the threat of budget cuts, his criticism of high energy physics sharpened. In a 1963 Minerva article he presented a method for assessing the relative value of various scientific programs, remarking that if one considered his assessment criteria, "relevance to the sciences in which it is embedded, relevance to human affairs, and relevance to technology--high-energy physics rates poorly." (39)

High energy physicists quickly mobilized to counter Weinberg's assessment. White organized a special session at the annual American Physical Society (APS) meeting in April 1964. At the roundtable discussion that concluded the session, Weinberg faced NAS President Seitz, who chaired the meeting, four high energy physicists, and George

E. Pake, a physicist who described himself as "sympathetic" to the field. While many panel members cited problems caused by the growing size and expense of high energy physics research, all but Weinberg stoutly proclaimed the superiority of the field. For example, C. N. Yang from the prestigious Institute for Advanced Studies spoke of the "nobility" of the field's scientific aim while Goldwasser called the field "the essence of pure science." (40)

In June, Weinberg encountered further public resistance to his ideas when Physics Today published a rebuttal of Weinberg's article by the eminent Victor Weisskopf, then Director of CERN. While Weisskopf endorsed Weinberg's assessment criteria, he argued that high energy physics fared well when judged by such a standard. After agreeing that the field held little "technological merit," he insisted that its value stemmed mainly "from the fact that this field is basic and relevant for al [sic] sciences and therefore touches questions which all thinking human beings are deeply interested in." In response to Weisskopf and high energy enthusiasts at the APS panel Weinberg constantly reiterated his basic contention. As he explained at the APS meeting: "Science which commands great public support must be justified on grounds that originate outside the particular branch of science demanding support: it must rate high in social, technological, or scientific merit, preferably in all three." (41)

As Weinberg's words rang in the air, leaders of the physics community worried about the rising level of dissension. Within the subfield of high energy physics the unhappiness of Midwestern physicists

joined growing agitation for outside user rights. Although Wigner and Weinberg's criticism of the value of high energy physics remained a minority opinion among physicists, their arguments created further divisiveness within the overall physics community and reinforced Congressional skepticism about the value of basic research at a time when budgets were being cut. At the April APS meeting Seitz sagely noted the ill effect of geographic competition within the physics community in the midst of such tensions. "One is reminded" he mused, "of the way in which the competition among the nations of Western Europe in the last century has had the effect of decreasing the collective strength of all." High energy physicists had particularly strong reasons for concern. In light of the multimillion dollar price tags of new accelerator projects, they could not hope to secure continued funding without support from the overall physics community, and such a task would be impossible without internal unity. As Seitz concluded: "Wisdom would seem to indicate that the family of high-energy physicists must somehow learn to resolve its differences and speak with a unified voice." (42)

By this time yet another panel was considering the future direction of high energy physics. In spring 1964, the National Research Council of the NAS convened a Physics Survey Committee. This committee, which was headed by Pake, was asked "to identify the most significant research problem in various subfields of physics, as a basis for estimating levels of support necessary to assure the balanced development of the field as a whole over the next 5 to 10 years." Pake subsequently

organized 9 panels, including a panel on high energy physics under the direction of Robert Walker, former WAG researcher. (43)

In early November 1964, just as negotiations between LRL and its advisory committee were heating up, the Panel released its conclusions. Like the Good Panel, the Walker Panel endorsed the findings of the Ramsey Panel. However, the new Panel went much farther than its predecessors in defining recommendations for the organization of the next accelerator facility. After noting that the Panel was not "the appropriate body" for designing an organizational plan, Panel members nonetheless enumerated four features which characterized the national organization they had in mind. Three of the features did not seriously contradict LRL plans: (1) the Panel advocated that the laboratory have a written charter to define its "national character," (2) a strong director independent from the control of those wishing to use the accelerator, and (3) a committee with national representation to approve experiments. In contrast to McMillan's views, however, the Panel stated: "The body which decides the major policies of the laboratory and to which the director is responsible should be national in representation . . . ." After noting that LRL and the Advisory Committee were currently discussing the problems of the new laboratory, Panel members expressed their hope that a solution would be found "within the framework" of the organizational features they had described. In the wake of increasing tensions both inside and outside the physics community and this new call for nontraditional management, all eyes in the high energy physics community turned to Berkeley. (44)

By this time intense negotiations had been underway between McMillan and the 200 BeV Advisory Committee for over a month. In late September, the Committee had gathered for a two-day meeting in Berkeley and amid numerous attempts to find a mutually acceptable plan, the Advisory Committee devised a new management scheme. This scheme, which was presented in draft form to McMillan on October 7 and subsequently distributed to various LRL staff members, included the formation of a national management corporation similar to the one suggested in May. In the October version, however, the corporation would join with the Regents of the University of California in a "Joint Venture," which would supervise the design and construction of the new accelerator. Although the University of California was allowed a vote, along with the AEC, in the selection of the new laboratory Director, the laboratory would "become the sole responsibility of the National Corporation" after construction of the machine. (45)

In late October 1964, just before the Walker Panel assembled for its final meeting, 13 members of the 200 BeV design team responded to the proposed scheme in a memo sent to Lofgren. Although the designers supported "the idea that national access and national participation in scientific policy be guaranteed to the fullest," they strongly objected to the notion of a Joint Venture. Due to the potential for "serious problems" between the management partners and the "danger of interdepartmental disputes, inefficiency and low morale" among the staff, designers felt the plan "would seriously hamper the speed and quality of the project." The designers also complained that the new

version forced an ill-advised distinction between machine builders and users. As design team member Glen Lambertson recently recalled, the team was horrified by the unprecedented plan of building the complicated machine and turning it over at some arbitrary point to another group since accelerators often require an unpredictable amount of finetuning. As Lambertson also admitted, part of the problem was that designers had joined the effort expecting the traditional prerogatives of machine builders. After receiving the memo, Lofgren passed it along with his endorsement to Advisory Committee member William Fretter. One participant remembers that the growing tension joined a more general feeling of anger and disquiet: as tempers at LRL rose, the first major demonstrations of the newly formed Free Speech Movement erupted at the adjoining University of California campus. (46)

After a meeting on October 29, Fry forwarded the unanimous report of the Advisory Committee to McMillan, wishing him and the design team future success. McMillan was incensed to discover that the report, a slightly edited version of the one distributed on October 7, still proposed the Joint Venture scheme. On November 2 he wrote to Panofsky, his long-time colleague and Advisory Committee member, complaining that he was "deeply upset" by the Advisory Committee's apparent refusal to consider design team complaints. LRL leaders were upset, however, by more than just this perceived breach of etiquette. In a November 3 letter to Advisory Committee members, Fry remarked that both McMillan and Lofgren "were very unhappy with the report," adding that it was now "clear that neither of them want a National Committee to administer the

laboratory, which clearly is in opposition to our views." After additional minor editing, a final report was delivered to McMillan on November 11, 1964. (47)

With the Advisory Committee report in hand, McMillan had the next move. Although the Committee's plan for national corporation management was responsive to outside user pressures and the need to gain unanimous support from the physics community, McMillan faced a variety of pressures to resist the nontraditional management scheme. For one thing, he faced the expectations of his staff. While William Wenzel, a young LRL experimentalist who had served on both the Good and Walker Panels, wrote urging McMillan to take an active part in organizing national corporation management, the design team members had already made their resistance plain. In addition, McMillan faced an even greater potential for resistance to any nontraditional arrangement that would affect the use of the machine from Berkeley's star team of active inside users, who were just now struggling to adjust to outside user representation in the scheduling committee at the Bevatron. Even more important, in line with his own long-standing views, McMillan saw the push for non-LRL management as an unworkable idea prompted by jealousy and opportunism. At this point, the pressing desire to continue Lawrence's tradition was a great burden. (48)

On November 23, 1964 McMillan wrote to McDaniel, remarking diplomatically that the Advisory Committee report was in general "a very thoughtful document." He further agreed that a national management corporation "offers a valid mechanism for assuring national

representation, and in the present context probably the best way." Despite this important concession, he urged that the matter of turning over eventual operation of the machine to the corporation "be kept open for further discussion." Since this had been the crux of disagreement between McMillan and the Committee all along, it was clear that arrangements for managing the new facility were still unsettled. (49)

Although McMillan mentioned to McDaniel that steps were being taken to establish a national corporation, management plans was soon commandeered by Seitz, who was determined to see the rapid formation of a new management organization to gain support among physicists for the new facility. He conferred with Seaborg and Tape to get AEC opinions and also approached AUI, but Brookhaven's management organization decided not to take on the project. Empowered by his considerable influence as NAS President, Seitz took the decisive step in late November: he called an informal NAS meeting and plans were made for inviting 25 university presidents to a January meeting to cast future plans for large accelerator projects. (50)

On January 17, 1965 the meeting was held as planned. Seitz, Hornig, and Seaborg all addressed the assembled university presidents, explaining the pressing need for a national organization to manage the 200 BeV facility and providing information on the current status of the project. In the course of vigorous discussion, it was proposed that a new nationally based organization be formed using AUI as a model. The initial goal of the organization would be to administer the 200 BeV accelerator, although other projects might be assumed later. The

university presidents would act as overseers of the new laboratory, while a group of administrators and senior scientists would formulate the policies governing its operation. Although participants agreed that Berkeley should be allowed to continue design work on the accelerator, the new management organization would take charge of site selection. The meeting of university presidents marked a turning point in plans for the new accelerator: while ideas for management and site selection would be refined and amended in the next year, from this point Berkeley researchers had no assurance that they would be allowed to manage and build the machine at the site of their choice. (51)

Summary: How New Management and Siting Plans Evolved

Berkeley's already eroding prospects for building and managing the accelerator declined throughout 1964. Pressure for establishing a national organization for managing the machine increased steadily within the physics community, beginning in the spring with Milton White's fervent letter to the high energy physics community and culminating with the Advisory Committee's insistence the following winter that a national corporation assume eventual control of the accelerator facility. In the meantime, in the aftermath of MURA, the unhappiness of Midwestern politicians and scientists, although differently motivated, combined to form a potent push for a Midwestern accelerator. This pressure joined with rising concern, both inside and outside the physics community, about the rising proportion of basic research funding and continued agitation for outside user rights. Although Berkeley's reputation, and

the tradition that reinforced it, had been important in gaining the Ramsey Panel recommendation and AEC design funding in 1963, by the next year these characteristics had become liabilities. Faced with the difficulty of convincing his staff to accept unpleasant changes and his own strong desire to continue the line of LRL's glorious past, McMillan refused to plan actively for non-LRL management. This stand, in light of the pressures inside and outside the physics community and concern that the accelerator would not be funded without general physics community support, prompted Seitz to form an organization to assume control of both management and siting plans for the new accelerator.

Conclusion: New Arrangements for Managing and Siting the Accelerator--1963-1965

Despite Berkeley's hopes that the new accelerator would be built and managed along traditional lines, in 1964 a series of mutually reinforcing pressures and resulting social tensions changed plans for managing and siting the new accelerator. Due to the tightening budget, the emergence of expensive Berkeley and Brookhaven proposals, and the lack of enthusiastic support from the physics community, the MURA proposal was cut from the budget. The MURA defeat intensified two complaints which had their roots in the previous two decades. In line with the push for geographic distribution of research funding in late 1940s, Midwestern politicians campaigned against the inequitable geographical distribution of research funding, taking on MURA as a cause célèbre. At the same time, the wrath of Midwestern scientists, which

had been building since the 1950s, exploded. Once again, the Midwest had been passed over in favor of already prominent institutions. Although Midwestern scientists argued for fair funding distribution on the basis of meritocracy, while Midwestern politicians argued for the evenly geographic distribution of funding, the combined force of both groups produced considerable political pressure for a Midwestern accelerator, undermining Berkeley's plans to build the 200 BeV accelerator in California.

Berkeley's plans were further undermined by new tensions arising from the unprecedented expense of the machine. Because of its high cost, physicists realized that only one such accelerator could be built. This realization, in light of Berkeley's reputation for favoring inside users, prompted the advisory committee for the new accelerator to advocate management by a corporation with nation-wide representation. Although this plan, which would abolish the traditional prerogative of accelerator designers to manage the machine, had strong support from outside user advocates, it was vehemently opposed by LRL leaders.

The impetus for national corporation management was further fueled by concern for the funding prospects of the expensive new machine. Conflicting views about the relative value of basic and applied research, previously masked by ample funding, now emerged. Both Congressmen and physicists active in reactor development decried the rising proportion of basic research funding. Leaders of the physics community were concerned that such complaints would slow the healthy flow of physics funding and make it difficult to obtain the large,

noticeable funding allocations necessary to design and construct the accelerator. In addition, such complaint threatened to further splinter the physics community, which was already burdened by the unhappiness of Midwestern physicists.

When LRL and its advisory committee were unable to agree on a management scheme for the new laboratory, NAS President Frederick Seitz spearheaded efforts to create a new organization with national representation to assume responsibility for the accelerator after it was designed. This step, which also led to the opening of the site competition, was responsive to pressures both inside and outside the physics community. The new organization kept hopes alive for a Midwestern accelerator, provided assurance for outside user advocates, and promised to unify the high energy physics community and optimize the chances for continued funding. Although it was modeled after Brookhaven's management organization, the new organization played a new role. AUI had been formed before the first large accelerator proposal was mounted at Brookhaven, and thus Brookhaven accelerator designers, like those at Berkeley, had built and managed the accelerators they had designed. In contrast, the new organization threatened to usurp the traditional prerogatives of Berkeley accelerator designers. The cost of the 200 BeV, the tightening budget, and resulting social tensions had forged new plans. For the first time, a group of accelerator designers had no assurance that they would be allowed to manage and build a machine at the site of their choice.

Notes for Chapter 4

All interviews are located in Fermi National Accelerator Laboratory History Collection, Batavia, Illinois, unless otherwise noted. The following abbreviations are used: Bancroft Library, (Unversity of California, Berkeley, Calfornia); BNL (Brookhaven National Laboratory, Upton, Long Island); DOE Archives, (United States Department of Energy Archives, Germantown, Maryland); FHC, (Fermilab History Collection, Batavia, Illinois); Green Papers, (files G. Kenneth Green, Brookhaven National Laboratory, Upton, New York); Judd Papers, (files of David L. Judd, LBL); LBL, (Lawrence Berkeley Laboratory, Berkeley, California); LBL Archives, (Lawrence Berkeley Laboratory Archives, LBL); Lofgren Papers, (files of Edward J. Lofgren, at LBL); LBJ Library, (Lyndon Baines Johnson Library Archives, Austin, Texas); McMillan Papers, (files of Edwin McMillan, LBL 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); Wenzel Papers, (files of William Wenzel, LBL).

1. A JCAE document prepared from NSF figures presented at the March 1965 AEC authorization hearings revealed that when total federal support for basic research from June 1963 to July 1964 was considered "the life sciences, earth sciences, and astronomy [had] more expenditures than high-energy physics." U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, (Washington D.C.: GPO, 1965), p. 232. Text quote from Kermit Gordon memorandum to President Lyndon Baines Johnson, December 7, 1963, Seaborg Papers. Also see Alvin Weinberg, Reflections on Big Science, (Cambridge: MIT Press, 1967).

2. Edwin Goldwasser letter to Edwin McMillan, September 9, 1964, Lofgren Papers.

3. Other regions, such as the South, did not have large numbers of high energy physicists, and therefore there was no strong push for representatives from these regions. Text references: Edwin McMillan letter to Paul McDaniel, May 10, 1963, McMillan Papers, Box 5; and Paul McDaniel letter to Edwin McMillan, May 14, 1963, DOE Archives, Secretariat, Box 1425.

4. Quotes from Edwin McMillan letter to Paul McDaniel, July 24, 1963, McMillan Papers, Box 5. Also see W. K. H. Panofsky letter to Edwin McMillan, June 10, 1963 and form letter from Edwin McMillan, July 25, 1963, McMillan Papers, Box 5; G. Kenneth Green letter to Edwin McMillan, September 16, 1963, Green Papers, Box I; G. Kenneth Green letter to Edwin McMillan, November 27, 1963, Green Papers, Box I; William Fry memorandum to "Members of the Advisory Committee of the Berkeley High Energy Accelerator Study," October 6, 1964, Green Papers, Box I; and Edwin McMillan letter to Frederick Seitz, December 16, 1964, Green Papers, Box I.

5. Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume II, (Berkeley: Lawrence Radiation Laboratory, 1965) UCRL-16000, pp. B1-B4 and private communication, Edward Lofgren, March 22, 1988.

6. Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume II, pp. B1-B4.

7. Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume II, pp. C1-C6; Brookhaven National Laboratory, "LRL-CERN-BNL Coordination Session on Super-Energy Accelerators, January 13-15, 1964," unpublished document, Green Papers, Box II; Livingston, M. Stanley, "Early History of the 200-GeV Accelerator," (Batavia: National Accelerator Laboratory, 1968), NAL-12. p. 7; and "Berkeley Study for a Proton Synchrotron in the 150-300 GeV Range," Lawrence Radiation Laboratory, August 9, 1963, UCRL-10869; personal communication, Lloyd Smith, November 13, 1987. (The author wishes to thank Lloyd Smith for the August 1963 document.)

8. Quote from interview with Denis Keefe, Glen Lambertson, and L. Jackson Laslett, December 22, 1986.

9. Quotes from Edward Lofgren, "Notes of a Conversation with Paul McDaniel, Wallenmeyer, Severiens, Phil McGee, and Fitcher, a New Member Regarding the Accelerator Study. Germantown, January 16, 1964," January 17, 1964, unpublished document, Lofgren Papers. Also see State of California, "Camp Parks, Sierra Foothills," unpublished document, p. 1, Salsig Papers.

10. Others on the committee were Martin L. Perl, Frank S. Crawford, William A. Wenzel, George E. Masek, Alvin V. Tollestrup, Melvin Schwartz, Leon M. Lederman, Aihud Pevsner, James Cronin, and Louis S. Osborne. "National Policy for High Energy Physics Program," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), p. 52. Text quotes from "Attachment C: Summary of Comments Solicited by the Office of Science and Technology from a Group of Users of High Energy Accelerators," in U. S. Congress, Joint Committee on Atomic Energy, Congress of the United States, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965,) p. 121. Also see interview with William Wenzel, May 2, 1984.

11. Quotes, respectively, from Leon Lederman, "The Truly National Laboratory," in "1963 Super-High-Energy Summer Study," Brookhaven National Laboratory, AADD-6, pp. 9 and 10. Also see this report p. 46 and Lillian 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), p. 45.

12. Interviews with Edward Lofgren May 3, 1984, Leon Lederman, July 20, 1984, and William Wenzel, May 2, 1984; Lillian Hoddeson interview with Frederick Seitz, February 7, 1980, FHC.

13. Quotes from Edwin McMillan letter to Paul McDaniel, July 24, 1963, McMillan Papers, Box 5.

14. Edward Lofgren, "Conference with Haworth in Washington," October 1, 1962, unpublished document, Lofgren Papers.

15. The two Midwesterners on the Ramsey Panel were Goldwasser and John Williams. Goldwasser and Frederick Mills, who were MURA supporters, and Bernard Waldman, MURA's staff director, all admit that while they supported the FFAG, they felt that exploring high energies was more important than exploring high intensities. Interviews with Edwin Goldwasser, July 10, 1985, Frederick Mills, July 13, 1984, and Bernard Waldman, February 4, 1984. Also see Hoddeson interview with Norman Ramsey, December 19, 1978; interview with Dave Judd, May 1, 1984, personal communication from Lawrence W. Jones, July 9, 1987.

16. Quotes from interview with Bernard Waldman, February 4, 1984.

17. Quotes, respectively, as quoted in U.S. Congress, Subcommittee of the Select Committee on Small Business, "The Role and Effect of Technology in the Nation's Economy," 88th Congress, First Session, (Washington D.C.: GPO, 1963) p. 19; Daniel Greenberg, The Politics of Pure Science: An Inquiry into the Relationship between Science and Government in the United States, (New American Library: New York, 1967), p. 250; and interview with Bernard Waldman, February 4, 1984. Also see pp. 245-246. Also see interview with Gerald Tape, November 26, 1986.

18. Quote from Manson Benedict letter to Glenn Seaborg, July 24, 1963, p. 7, Seaborg Papers. Also see Midwestern University Research Association, "By-Laws of Midwestern Universities Research Association," February 7, 1956, unpublished document, Judd Papers.

19. Glenn Seaborg, record of conversation, July 17, 1963; Glenn Seaborg, record of conversation, July 22, 1963; Leonard Greenbaum, A Special Interest: The Atomic Energy Commission, Argonne National Laboratory, and the Midwestern Universities, (Ann Arbor: The University of Michigan Press, 1967), pp. 155-156; and interview with Glenn Seaborg, February 16, 1984 and Gerald Tape, November 21, 1986. See Greenbaum for more details of the interaction among MURA, Argonne, the University of Chicago, and the AEC.

20. Quotes, respectively, from Kermit Gordon memorandum to Lyndon Johnson, December 7, 1963, Seaborg Papers; Glenn Seaborg memorandum to Lyndon Johnson, December 14, 1963, Seaborg Papers; Glenn and Seaborg, record of conversation, January 17, 1964, Seaborg Papers.

21. Quote from interview with Bernard Waldman, February 4, 1983. Also see Glenn Seaborg, diary, TS, December 20, 1963.

22. Quotes, respectively, from Glenn Seaborg, diary, TS, December 20, 1963, Seaborg Papers and Elvis Stahr letter to Hubert Humphrey, December 21, 1963, Atomic Energy, Box Ex AT/AT 2, White House Central Files, LBJ Library. Also see Lyndon Baines Johnson letter to Hubert Humphrey, January 16, 1964, Secretariat, DOE Archives, Box 1424.

23. Some, like Greenbaum, believe that the rejection of the MURA machine forced the Midwestern physics community to come to terms with its internal discordances. Despite its failure, the MURA organization, in this way, "became a vehicle for reform." Greenbaum, p. 57. Text references: Glenn Seaborg, record of meeting, January 14, 1963, Glenn Seaborg, record of conversation, January 17, 1964, Glenn Seaborg, record of conversation, January 17, 1964, Glenn Seaborg record of conversation, January 23, 1964, Glenn Seaborg, record of meeting with Hubert Humphrey, January 17, 1964, Seaborg Papers; Lyndon Baines Johnson letter to Hubert Humphrey, January 16, 1964, Secretariat, DOE Archives, Box 1424; interview with Bernard Waldman, February 4, 1983; Greenberg, pp. 263-264 and Anton Jachim, Science Policy Making in the United States and the Batavia Accelerator, (Carbondale: Southern Illinois University Press, 1971), pp. 82-99.

24. Although Brookhaven designers remember that their relations with MURA researchers had been friendly, the collaboration between Brookhaven and MURA never got off the ground, like previous plans for a LRL/WAG collaboration. Private communication, John Blewett, November 29, 1987. Text quotes, respectively, from Glenn Seaborg, record of meeting, January 17, 1964, Glenn Seaborg, record of meeting, January 14, 1964, Glenn Seaborg, record of conversation, January 23, 1964, Seaborg Papers. Also see Alice L. Buck, A History of the Atomic Energy Commission, (Washington: U. S. Department of Energy, 1983,) DOE/ES/0003/1, p. 12.

25. Quotes, respectively, from Glenn Seaborg, record of conversation, January 17, 1964 and Glenn Seaborg, record of conversation, January 17, 1964, Seaborg Papers.

26. Quotes, respectively, from Glenn Seaborg, record of conversation, January 23, 1964 and Glenn Seaborg, record of conversation, March 2, 1964, Seaborg Papers. Also see Edward Lofgren, "Outline of Presentation to Accelerator Study Advisory Committee," February 22, 1964, unpublished document, Lofgren Papers; and Edwin McMillan letter to Paul McDaniel, April 27, 1964, Secretariat, DOE Archives, Box C478.

27. According to BNL records, Reynolds set a copy of "Report of the High Energy Study Group" to 180 members of the scientific community. The report discussed a number of topics, including BNL plans for converting the AGS to higher intensity, the 600-1000 GeV machine, and a meson factory. Edward Reynolds form letter, February 21, 1964,

Brookhaven National Laboratory, Director's Office Files, Box 9. Text quotes from Milton White letter "To Friends of Fundamental Particle Physics," April 13, 1964, Lofgren Papers.

28. Quotes from Edwin McMillan letter to Milton White, April 20, 1964, Lofgren Papers.

29. Quotes from Norman Ramsey letter to Edwin McMillan, May 13, 1964, Green Papers, Box I.

30. Quotes from William Fry letter to Edwin McMillan, May 26, 1964, McMillan Papers, Box 5.

31. Quote from Jack [William] Fry letter to Lee [Teng], undated, Green papers, Box I.

32. Committees which investigated geographical distribution of funding included a House subcommittee headed by Emilio Daddario and a House select committee headed by Carl Elliot, both established in 1963, a Senate committee headed by Jospeh S. Clark convened in 1965 and a Senate subcommittee headed by Fred R. Harris, established in 1966. See U.S. Congress, Subcommittee of the Select Committee on Small Business, "The Role and Effect of Technology in the Nation's Economy," 88th Congress, First Session, (Washington D.C.: GPO, 1963); U.S. Congress, "Government and Science: Hearings Before the Subcommittee on Science, Research, and Development of Committee on Science and Astronautics," 88th Congress, Second Session (Washington D.C.: GPO, 1964); U.S. Congress, Subcommittee on Science, Research and Development of the Committee on Science and Astronautics, "Government and Science: Distribution of Federal Research Funds," 88th Congress, Second Session (Washington D.C.: GPO, 1964); U. S. Congress, Subcommittee on Employment and Manpower of the Committee on Labor and Public Welfare, "Impact of Federal Research and Development Policies on Scientific and Technical Manpower," 89th Congress, First Session (Washington D.C.: GPO, 1965); and U.S. Congress, Subcommittee on Government Research of the Committee on Government Operations," Hearings, "Equitable Distribution of R&D Funds by Government Agencies," 89th Congress, Second Session, (Washington D.C.: GPO, 1966). For an analysis of these discussions, see Anton Jachim, Science Policy Making in the United States and the Batavia Accelerator, pp. 82-99. For further background on congressional interest in science and technology funding and other hearings see Michael D. Reagan, Science and the Federal Patron, (New York: Oxford University Press, 1969); Donald R. Fleming, "The Big Money and High Politics of Science," Atlantic Monthly, August, 1965; and Kevles, pp. 413-414. Text quotes as quoted in Daniel J. Kevles, The Physicists: The History of a Scientific Community in Modern America, (Alfred Knopf: New York, 1978), pp. 395 and 395-396.

33. MURA's defeat and its affect on the political background for the 200 BeV has been noted by many writers, including Greenberg in The Politics of Pure Science, Anton J. Jachim, Science Policy Making in the

United States and the Batavia Accelerator, and Theodore J. Lowi and Benjamin Ginsberg, Poliscide, (New York: MacMillan, 1976). All three writers, however, ignore the major contribution that outside user tensions played in setting this background. Text quotes, respectively, from Lyndon Johnson letter to Hubert Humphrey, January 16, 1964, Secretariat, DOE Archives, Box 1424; Lyndon Johnson,"The Strengthening of Academic Capabilities for Science Throughout the Country," in U.S. Congress, Subcommittee on Government Research of the Committee on Government Operations, "Equitable Distribution of R&D Funds by Government Agencies," 89th Congress, Second Session, (Washington D.C.: GPO, 1966), p. 5, and as quoted in this souce, p. 1. Also see Glenn Seaborg record of conversation, January 17, 1964, Seaborg Papers; Reagan, pp. 145-148; and Fleming, pp. 304-305.

34. In line with ignoring the importance of outside user tensions, Jachim, Greenberg, and Lowi obscure the distinction between the concerns of Midwestern physicists and those of Midwestern politicians.

35. Physics Survey Committee, National Research Council, Physics in Perspective, (National Academy of Sciences: Washington D.C, 1972), pp. 645, 646, 648.

36. During this period an AEC budget typically went through several steps. After BoB input the budget was sent to the President. Once approved, the AEC budget was incorporated into the presidential budget, which was presented to Congress. The JCAE then held hearings so that the AEC staff and experts could present their views on proposed AEC expenditures. After incorporating their revisions, the JCAE members took the AEC budget to separate appropriation hearings in the Senate and House of Representatives. Private communication, Gerald Tape, November 4, 1987. For a general description of the budget cycle for federal agencies, see Physics Survey Committee, National Research Council, Physics in Perspective, p. 655. Text quotes from U.S. Congress, Joint Committee on Atomic Energy, "Atomic Energy Commission Authorization, FY 1965," 88th Congress, Second Session, (Washington D.C.: GPO, 1964), p. 1487.

37. Quotes, respectively, from U. S. Congress, Joint Committee on Atomic Energy, "AEC Authorization, FY 1965," 88th Congress, Second Session, pp. 1508 and 1499.

38. Quotes, respectively, from Donald Hornig letter to Senator John O. Pastore, March 27, 1964, White House Central Files, Box AT 1; and Donald Hornig memorandum to Lyndon Baines Johnson, March 27, 1964, Hornig Papers, Box 1, LBJ Library.

39. Another vocal critic of high energy physics was chemist Philip Abelson, who discovered neptunium with McMillan and later became editor of Science. Although he had served on the Ramsey Panel and endorsed its final recommendations, Abelson later criticized the 200 BeV and decried the level high energy physics funding. In a January 1966 Saturday Review

article, for example, he proclaimed: "Never, in the history of science have so many fine minds been supported on such a grand scale, and worked so diligently, and returned so little to society for its patronage." Fellow Ramsey Panel member Edwin Goldwasser recently remarked: "We thought we'd educated him in the course of the meetings .... but we were wrong." Quotes, respectively, as quoted in Kevles, p. 417, and interview with Edwin Goldwasser, July 10, 1985. Weinberg's article was first published in the Winter 1963 edition of Minerva. A detailed explanation of his assessment criteria and other musings on large-scale research can be found in Reflections on Big Science, (Cambridge: MIT Press, 1967). Text quote in U. S. Congress, Joint Committee on Atomic Energy "AEC Authorization, FY 1965," 88th Congress, Second Session, (Washington D.C.: GPO, 1964), p. 1500.

40. Other panel members were Val Fitch, Owen Chamberlain, and Maurice Goldhaber. "Appendix 19" in U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, p. 754. Text quotes, respectively, from "Appedix 17," in U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, pp. 756, 752, and 753.

41. Quotes, respectively, from "Appendix 17," and "Appendix 19," in U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, pp. 744 and 754.

42. Quotes from "Appendix 18," in U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, p. 751.

43. Other Panel members included Rodney Cool, Sidney D. Drell, Val Fitch, Roger Hildebrand, F. E. Low, H. K. Ticho, William D. Walker, and William A. Wenzel. Appendix C, "National Policy for High Energy Physics Program," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965,) p. 55. Text quote as quoted in Atomic Energy Commission, "Policy for National Action in the Field of High Energy Physics," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), p. 54.

44. Quotes, respectively, from Report of the Panel on Elementary Particle Physics to the Physics Survey Committee of the National Academy of Science, Draft, November 8, 1964, pp. 5-20 and 5-21, FHC. Also see this source p. 1-1.

45. Quotes from unsigned draft to Edwin McMillan, October 7, 1964, McMillan Papers, Box 5.

46. The Free Speech Movement (FSM) was formed in late September 1964 to protest regulations restricting campus political activity. After a series of sit-ins and negotiations between FSM leaders, including Mario Savio, and University of California President Clark Kerr, Sproul Hall was occupied in late November. As a result of the demonstrations, 800 demonstrators were arrested, the largest such arrest to that date in California history. Milton Viorst, Fire in the Streets: America in the 1960's, (New York: Simon and Schuster, 1979), pp. 288-296. Text quotes, respectively, from Frank T. Cole, Charles G. Dols, Fritz Gruetter, Edward C. Hartwig, Denis Keefe, Quinten Kerns, William A. S. Lamb, Glen R. Lambertson, L. Jackson Laslett, Jack M. Peterson, William M. Salsig, Lloyd Smith, and George H. Trilling memorandum to Edward Lofgren, October 26, 1964, McMillan Papers, Box 5; and interview with Denis Keefe, Glen Lambertson, and Jackson Laslett, December 22, 1986. Also see Edward Lofgren letter to William B. Fretter, October 26, 1964, Lofgren Papers; and William Wenzel letter to M. Stanley Livingston, June 14, 1968, Wenzel Papers.

47. Quotes, respectively, from Edwin McMillan letter to Wolfgang Panofsky, November 2, 1964, McMillan Papers, Box 5; and William Fry letter to G. Kenneth Green, November 3, 1964, Green Papers, Box I. Also see William Fry letter and enclosures to Edwin McMillan, October 30, 1964 and Edwin McMillan to Paul McDaniel, November 23, 1964, Lofgren Papers.

48. William Wenzel letter to Edwin McMillan, November 6, 1964, Wenzel Papers; and interviews with Edwin McMillan, May 16, 984, Edward Lofgren May 3, 1984, Leon Lederman, July 20, 1984, and William Wenzel, May 2, 1984.

49. Quotes from Edwin McMillan letter to Paul McDaniel, November 23, 1964, McMillan Papers, Box 5.

50. McMillan mentioned in a December 16 letter to Seitz that Clark Kerr, University of California President, had planned to make arrangements with other university presidents to form the national corporation, but that the AEC stopped him. Edwin McMillan letter to Frederick Seitz, December 16, 1964. Text references: Lillian Hoddeson interview with Frederick Seitz, February 2, 1980; and interviews with Gerald Tape, November 21, 1986 and Kenneth Pitzer, May 17, 1984.

51. W. B. Fowler, "Meeting at National Academy of Sciences, January 17, 1965, Summary of Notes Taken by Theodore P. Wright," April 13, 1965, FHC; and Frederick Seitz, "National Academy of Sciences Meeting of University Presidents, January 17, 1965," in U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, pp. 8-9.

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THE FIRST "TRULY NATIONAL LABORATORY": THE BIRTH OF FERMILAB

Volume II

By
Catherine Westfall

A DISSERTATION

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Chapter 5: Siting and Funding the Accelerator--1965-1967

Introduction

Throughout the country in 1965, at conference social hours and at office parties, physicists heard Arthur Roberts' latest song as they drank coffee from styrofoam cups and munched cookies. Roberts, an experimentalist at Argonne with a knack for musical satire, played his own piano accompaniment as he sang "The 200 BeV Accelerator." (1)

The AEC's new competition
Has brought forth a wondrous refrain
A new song has captured the heart of the nation
From California to Maine.
Hear Kansas City, Detroit and Decatur
Proclaiming their grace and their civilized charm
The two hund· ·d BEV accelerator
Gives cultu. ·. shot in the arm

Oh--Particle physics you've stolen our heart
Come all and bid for this prize.
With three hundred million to spend for a start
See how it lights up the stars in our eyes.
For the beauties of Euclid our hearts may not yearn
That hard gemlike flame in our eyes may not burn
But for that kind of money we're willing to learn
So research forever we cry.

Chicago's trees make her parks a sensation,
In Seattle the sunshine's the thing,
Texas reads only the "Times" and "The Nation,"
Mississippi loves M. Luther King.
Ballet's all the rage in Fort Dodge and Topeka,
In Hanford desire for Chagall is acute.
Los Angeles' John Birch Society's weaker,
And string quartets flourish in Butte.

Oh--Particle physics you've stolen our heart;
Now you will uplift our souls.
The two hundred BeVer will change us forever,
Transforming our tastes and advancing our goals--
We will turn from Bonanza and Gunsmoke away

Out library budgets will triple today
For three hundred million whatever you say.
So research forever we cry.

We will reach a consensus on what should be done,
With every group happy except perhaps one--
The men who can build it and make the thing run.
So research forever--
We'll part from you never
Till death do us sever--
You wives just gonna love West Texas--
We cry.

Many physicists, like Roberts, saw the 200 BeV site competition that raged throughout 1965 and 1966 as a bizarre spectacle of opportunistic boosterism. Although non-fiscal arrangements for accelerator projects had traditionally been handled within the urbane confines of the physics community, during this period physicists found that arrangements for a multi-hundred million dollar project could attract considerable public and political attention. This change was only one of the many to be faced at the time. Since 1960, the social climate had changed radically, both inside and outside the physics community. Accelerator plans no longer proceeded within the comfortable parameters of tradition in the midst of a propitious funding environment. During this period, the success of the 200 BeV proposal proved that a multi-hundred million dollar accelerator project could be funded within the new economic and political climate. At the same time, new precedents were set for siting and managing a major accelerator laboratory. By 1967, the groundwork had been laid for a novel laboratory along the lines of the "Truly National Laboratory" described by Leon Lederman in 1963.

To understand this pivotal period it is necessary to consider three questions. First, how did physicists and government leaders begin efforts to marshall support for the machine in early 1965 and how was the site selection process established? Second, how was the Berkeley design received in mid-1965 and how were finalist sites chosen? Third, how were the final arrangements made for siting and funding of the new accelerator and how was the first director chosen?

Marshalling Support and the Site Selection Process

As 1965 began, Seaborg and Seitz knew it was time to mount an intensified campaign for the 200 BeV accelerator. After the Berkeley group finished the preliminary design, which was due in July, the project would need its first large funding allocation. Due to the size of the investment, the decision to grant this allocation would signal a federal committment to complete the project. In the last round of authorization hearings in 1964, the JCAE had emphasized the difficulties of obtaining Congressional approval due to the rising costs of high energy physics research, and complaints from Weinberg, Wigner and other physicists worsened the prospects for funding an expensive project.

The AEC also worried about obtaining support from its executive branch partners, the White House and BOB. By 1965, the influence of scientists was dwindling in the executive branch. After Sputnik, a concerted effort was made to construct a formal science advisory system in government. As public policy analyst James E. Katz explains: "Between 1958 and 1963 a science advisory network was built up which not only

constituted a bureaucracy, with all its attendant problems, but which robbed the White House science officers of some of their legitimating rationale." Whereas the scientific community had enjoyed close contact before 1963 with the president through the science advisor and PSAC, with the Johnson administration this closeness became, in the words of public policy analyst Thaddeus J. Trenn, "diffused through close contact with the bureaucracy and formal institutionalism." This trend was strengthened by a lack of rapport between Johnson and Hornig. As the science advisor later explained, although Johnson listened to him and responded when he identified major issues, "there has never been any easiness about it." Moreover, some perceived that Johnson was generally less sympathetic to basic science than Kennedy had been. To obtain support for the expensive accelerator project from the President, BOB, Congress and the general public, both the physics community and the AEC mounted aggressive campaigns. (2)

Although physicists had marshalled support for high energy physics and the new accelerator projects within their own community since 1960, increased efforts were made in 1965 to gain wider support. For example, The Nature of Matter: Purposes of High Energy Physics, a collection of essays edited by Luke Yuan, was published in January 1965. As Yuan explained in the preface, planning for the document emerged from Brookhaven efforts to design the 600-1000 GeV accelerator. While pursuing these studies, Brookhaven physicists sensed "misunderstanding of the objectives of high energy physics," and decided that a clearer

explanation was "urgently needed among high energy physicists, the scientific community as a whole, and the general public." (3)

The volume began with a foreword by Oppenheimer, who presented an eloquent reworking of traditional accelerator justifications that would be repeatedly echoed in the next few years. In the 1950s, physicists had pointed to the practical benefit of technological spinoffs from accelerator development. Now that accelerators were much more expensive and specialized, Oppenheimer specifically delineated types of spinoffs and stressed the benefit to other sciences rather than to technology. He observed that "the techniques of ultra high energy physics, experimental, observational, computational, mathematical, will prove of the greatest value throughout science, and indeed often in technology." Oppenheimer also found a way to evoke the famous contributions of nuclear physics without making unrealistic promises, noting that it was "not impossible" that physicists would produce "an unanticipated discovery of profound importance to technology and to human welfare." However, Oppenheimer placed greatest stress on a new theme. Conjuring the exciting and prestige-laden discoveries of the past decade and a half, he emphasized the great cultural value of high energy physics research, insisting that successes in the struggle to understand the nature of matter had not only enriched science and technology, but had lighted "the whole of man's life." (4)

Oppenheimer's views were reinforced and amplified by essays written by 28 well-known theoretical physicists. For example when explaining the purpose of the field, Julian Schwinger proclaimed: "The world view of

the physicist sets the style of the technology and the culture of the society, and gives direction to future progress." To stress the importance of further accelerator development, many contributors described recent accomplishments and current challenges. For example, several essays pointed to the inspiring success of the SU(3) classification scheme developed independently by Murray Gell-Mann and Yuval Ne'eman in 1961. The scheme, which brought order to the confusion of particles by organizing strongly interacting particles into families of 8 and 10, gained general acceptance after it successfully predicted the mass and existence of a new particle, omega minus, found at the Brookhaven AGS in 1964. As T. D. Lee explained, an accelerator "in the range of 200 to 1000 BeV" would "certainly be crucial" in exploring the "detailed dynamics of this strong SU(3) symmetrical interaction. . . ." (5)

While these sentiments were being transmitted to a general audience, the AEC was sending a similar message to government leaders. On January 25, 1965, Seaborg sent President Johnson the "Policy for National Action in the Field of High Energy Physics," the full-length AEC report written to augment Hornig's spring 1964 policy statement. Like Yuan's collection of essays, the report was boldly complimentary and optimistic about the future success of the field. It characterized American high energy physics as "an exciting and vigorous field of basic research which ranks high among our most prominent scientific undertakings." The report also noted that the recent bounty of particle discoveries might "mark the beginning of one of those intensely

productive and exhilarating eras in which apparently uncorrelated facts accumulated over many years are tied together, and science makes a 'quantum leap' forward . . . ." The report then described recent advances, the research potential of extending the high energy frontier, and the potential for technological spinoffs, along the lines of previous discussions. (6)

Guidelines for long-range planning for federal support of the field were consonant with this high appraisal. The report insisted that it was "in the national interest to support vigorous advancement of high energy physics. . . ." To aid in this important goal, the AEC was "appropriately serving as the executive agent" in shouldering the majority of financial responsibility for the field. Instead of gauging funding on AEC applied science programs, the "level and character of support for high energy physics should be determined and periodically reassessed in the context of the overall national science program, . . . advances and promises of advances in the field itself, and the then existing fiscal situation." The report added that the nation's high energy physics program currently required "increased financial support, especially for the provision of advanced accelerators and equipment." Specific plans included improvements to several existing accelerators, construction of the 200 BeV accelerator, and continued planning for the 600 to 1000 GeV machine. To support this ambitious plan, the Commission projected a steady increase in the national high energy physics program, estimating that funding in 1970 should be almost twice that for 1965. (7)

The report was an aggressive assertion of AEC prerogatives. While it incorporated the JCAE suggestion that the high energy physics budget be assessed periodically to take changing economic conditions into account, provisions for major funding increases and for setting a funding standard independent of applied projects ignored Congressional concern about the rising proportion of basic research funding. On January 26 Johnson sent the report to the JCAE with his vote of confidence, noting that it provided "a useful guideline for decision-making in the development of high energy physics." (8)

One factor that probably favored presidential support for increased AEC high energy physics funding was the relationship between Johnson and Seaborg, which was close, according to Seaborg. As Seaborg explained in a recent interview: "He liked me . . . I had his confidence." In addition, as the policy report proved, Seaborg was willing to lobby strongly for high energy physics funding, even though his own scientific specialty was transuranic chemistry. His strong advocacy likely stemmed in part from his early Berkeley contacts. Probably more important, however, was that Seaborg, the first scientist to head the AEC, saw promotion of basic research as an important part of his mission as AEC chairman. (9)

Seaborg knew that BOB would be an adversary due to the expense of the 200 BeV and the tightening budget. The AEC position would have been even stronger had Johnson transmitted the policy report with a stronger endorsement. Nonetheless, as policy analyst W. Henry Lambright points out, "each presidential interaction with Congress over the 200 BeV was

reducing BOB's leeway." This left Congress, and the powerful JCAE, as the next major stumbling block. (10)

On March 2, 3, 4, and 5 a subcommittee of the JCAE held hearings on the status of high energy physics research, providing an opportunity for the physics community and the AEC to publicly express opinions and giving both groups the chance to gauge the mood of the powerful congressional committee. Dozens of physicists made presentations on overall research objectives, recent accomplishments, and the status of research programs at various laboratories in the US, Europe, and the USSR. For example, Gell-Mann explained the utility of SU(3) and amused his audience with the explanation that the term "quarks" had been taken from James Joyce's Finnegan's Wake. Lofgren spoke on the status of the 200 BeV design study, showing diagrams of a four-stage machine that included a Cockcroft-Walton preaccelerator, a linear accelerator, a booster synchrotron, and a larger synchrotron. In addition to incorporating Sands' idea of a booster synchrotron, the design also incorporated the long straight sections proposed by Tom Collins. Lofgren did not present a detailed cost breakdown, which would be included in the upcoming design report, but when quizzed on the cost of the machine, he remarked that "construction cost will go a little higher and the operating cost a little lower" than the $280 million construction cost mentioned in the AEC's policy statement. The Congressmen did not object to this admission, and, in general, seemed ready to accept the broad outlines of the Berkeley design. (11)

Although the atmosphere of the rest of hearings was similarly friendly, several issues sparked heated disagreement. Predictably, a roundtable discussion led by Seitz on the relation of high energy physics to other sciences, which included both Wigner and Yuan, displayed sharply divergent views on the social value of the field. Although they let panel members do most of the talking, JCAE members sided several times with those arguing the scientific merits of high energy physics. (12)

However, the Joint Committee took a more combative stand with high energy physics proponents when discussing plans for the new accelerator laboratories. Although Committee members seemed prepared to accept the "Policy for National Action in the Field of High Energy Physics," which they did not discuss aside from a little quibbling on the strength of the presidential recommendation, they reacted strongly to management plans. (13)

The largest source of contention was the NAS management organization being planned by the university presidents gathered by Seitz. At the beginning of the hearing, the Committee questioned whether the university presidents in the overseeing board were competent to make technical decisions, whether the physicists in the policy-making board were competent to tackle large-scale management issues, and whether the new organization would dilute the AEC's control of the project. When Leland Haworth, now NSF director, explained that the organization was much like the one organized after World War II to manage Brookhaven, Representative Hosmer from California interrupted, remarking that

Haworth "ought to know" that "this is not the situation in World War II; this is 1965." Considering the expense of the 200 BeV, it was "incumbent" upon the JCAE "to insure that the management and organizational setup is of the type that is designed and calculated to produce the highest return for the taxpayers' dollar." Hosmer added that he simply did not "believe that a group of university presidents or a group of high-energy nuclear physicists are perhaps the best people in the world . . . to determine the final management setup." (14)

At this point, LRL researchers had reason to hope that JCAE resistance to the new NAS management organization would work to renew plans for an LRL-managed machine in California. However, as the hearing continued, Hornig and AEC staff members continued to press for national corporation management and gently stressed that LRL was out of step with this goal.

In a panel discussion on management requirements for future laboratories, JCAE members pressed Seaborg to explain how the 200 BeV facility would be managed differently from the other large laboratories, which were already called national facilities. Seaborg reiterated that they planned a management scheme much like that at Brookhaven, and when questioned, admitted that LRL was "less of a national laboratory than the other laboratories." This negative message about Berkeley was underlined when McDaniel presented a table showing that inside users at LRL consumed 65% of accelerator time, compared with 20% and 35% at Argonne and Brookhaven, respectively. When Tape reported on the results of the Berkeley Advisory Committee in a later discussion, he refrained

from mentioning LRL resistance, but again stressed the need for management by a national corporation. Hosmer first responded by suggesting the use of a management consultant for the new laboratory, but when Tape, Haworth, and Hornig pointed to the management experience of the AEC and insisted that the new laboratory would allow the full participation of the best physicists throughout the country, JCAE members let the management issue drop. (15)

The JCAE was even more adamant about siting the new accelerator than about management plans. Although the broad outlines of the Berkeley 200 BeV design were accepted without complaint, a discordant note was hit in Lofgren's presentation after he remarked that many aspects of the 200 BeV design, such as foundations, and water and power availability, were "very sensitive" to the specific site chosen, and the Berkeley group had therefore used the Camp Parks site in California as an example "to accomplish definite design studies." Representative Hosmer immediately asked whether Lofgren was trying to "sell" them the California site. This was a sensitive issue since a Denver group led by eminent scientist Edward U. Condon had prepared a statement in February advocating a Boulder-Denver area site for the new accelerator, and by early March the AEC had also received recommendations for sites in Hanford, Washington and St. Louis, Missouri, in addition to the Camp Parks site. Hosmer decried such attempts "to jump the gun . . . and start a big national fight about where this thing is going to be." He warned that if the offenders did not stop, he would "do everything possible . . . to block the possibility of their ever getting this

machine. . . ." Since several contenders for the site had already emerged and tempers were already inflamed over the regional distribution of research funds, there was no easy way to avoid a site competition, but the JCAE clearly wanted the AEC to take firm charge of the politically volatile contest. Thus, when summarizing the hearings, Representative Melvin Price from Illinois remarked that in the near future he expected "the AEC will be prepared to tell us what they have already done in respect to site selection and what they plan to do." (16)

In light of the generally positive JCAE response to the AEC policy statement and the affirmative stance taken on the social value of the field, it was likely that the Committee would perform its traditional role and champion the new accelerator project, assuming JCAE concerns were addressed. However, the lack of JCAE enthusiasm about the new management organization created a ticklish problem for Seaborg and Seitz. They were reluctant to disband the organization because it seemed a viable vehicle for proving to Congressmen and physicists alike that the new accelerator facility would be truly national in character. In addition, such an organization was desperately needed to marshall support within the physics community, and although dissenting opinions on the value of high energy physics had not eroded Joint Committee support at the March hearings, leaders knew that they had little chance of ultimate success for the expensive project without unified physics community support.

The Committee's stand on site selection also caused headaches. At the January 1965 meeting of university presidents, participants discussed the possibility that the new organization would both manage the laboratory and select a site. Tape explained that the AEC wanted to avoid taking sole responsibility for site selection. By letting the NAS group assume the task of mediating the site selection contest, the AEC could avoid the sort of accusations suffered in the aftermath of MURA's defeat. The NAS management organization was in a better position than the AEC to bear the inevitable sore feelings of losers because of the dual prestige of the NAS and the group of university presidents, and because of the national representation of the group. As the March hearings made clear, however, JCAE members wanted the AEC, not an outside group, to handle this politically volatile issue. After all, the AEC was more likely to make a politically responsive decision than an outside group and the JCAE had more leverage if the AEC retained control. Seaborg, Seitz and other leaders were faced with the challenge of addressing the conflicting concerns of all those involved when making further arrangements for the new laboratory. (17)

For the next several months, a working committee directed by J. C. Warner, president of the Carnegie Institute of Technology, explored procedures for forming the new management organization. Articles of incorporation for the Universities Research Association, Inc. were filed on June 21, 1965, and a fall board of trustees meeting was planned. (18)

In the meantime, Seitz and Seaborg removed site selection from the list of duties for the new management organization, and an alternate plan for site selection was quickly drafted. Before the March hearings ended, Seaborg wrote to Seitz proposing that the Academy "study the site selection problem in considerable detail using facilities, information and personnel that the AEC and its contractors will make available. . . ." Seaborg identified "general site criteria," including "suitable geology," "availability of power and water," "sufficient acreage," "proximity to a major jet airport" and "cultural center that includes both a large university and a well-developed research and development base," "ability to mobilize the necessary staff," and "regional cost variations." Seaborg noted that he expected the study to conclude with "appropriate recommendations to the Commission for the best locations or location for this national facility on the basis of the criteria outlined above together with such additional criteria that the NAS may believe to be important." (19)

In the next month Seaborg and Seitz built the mechanism for site selection. On April 6, Seitz presented Seaborg with an outline of the proposed NAS effort. The Academy would appoint a committee of "distinguished scientists" who would agree on site criteria, identify whether specialized panels were needed, study site-related material and make site visits, and present a site recommendation report. At Seaborg's direction, the AEC had already prepared a more specific set of site criteria, including a more detailed description of desirable geographical and environmental features, which was forwarded to the

Joint Committee. By mid-April the AEC had formed a Task Group with representatives from the Divisions of Research and Construction to gather preliminary site information and screen all incoming site proposals for minimum site requirements before they were referred to the NAS. By this time the NAS had also recruited several site selection committee members, including the chairman, Emanuel Piore. On April 20, AEC and NAS representatives signed a contract to formalize the arrangement, which specified March 31, 1966 as the deadline for the site evaluation report. An April 28, 1965 AEC press release announced the AEC-NAS agreement, presented the more specific site criteria and preliminary design information, and requested that additional site proposals be submitted to the AEC by June 15, 1965. (20)

Summary: Marshalling Support in Early 1965 and Establishing the Site Selection Process

In early 1965 aggressive steps were taken both by the physics community and the AEC to marshall support for the 200 BeV accelerator. As revealed in The Nature of Matter: Purposes of High Energy Physics, when appealing to the general public, the physics community called on the substantial capital of exciting research discoveries to mount their strongest argument. The AEC used a similar approach when addressing government leaders in "Policy for National Action in the Field of High Energy Physics," boldly presenting a plan for increased support for the field. When Johnson gave the report his vote of confidence and the JCAE

seemed willing to accept both the policy report and preliminary Berkeley design, funding prospects for the new accelerator looked good.

However, March hearings on the status of high energy physics revealed that the JCAE, the physics community, and the AEC had conflicting concerns that directly affected ongoing arrangements for managing and siting the new accelerator. While pressure for national corporation management had grown in the physics community, the JCAE was unenthusiastic about a management organization planned by university presidents and the NAS. In addition, while the JCAE was insistent that the AEC control the site competition, the Commission was not eager to be the sole arbiter in the emotion-laden contest. To address these conflicting concerns, leaders of the AEC and the physics community took a series of decisive steps after the March hearings. In response to the physics community, arrangements for the new management organization continued. At the same time, Seaborg and Seitz arranged a joint AEC-NAS effort to relieve the management organization from site selection duties. The AEC-NAS arrangement preserved overall AEC authority, in response to JCAE concerns, but protected the AEC from the repercussions of becoming the sole arbiter of the competition. By mid-1965 plans for national corporation management had been well launched and the course for site selection had been set. In addition to these continuing efforts, Seaborg and Seitz also had to continue lobbying for the first major funding allocation which would allow the new laboratory to come to life.

The Berkeley Design and the Selection of Finalist Sites

In June 1965, as site proposals flooded into the AEC, the Berkeley design group proudly presented, as promised, its preliminary design in two, thick blue-covered volumes. Explaining that it was "not possible to optimize the design of all components of such a large accelerator without consideration of the site of the facility," the report considered the design at two sites, Camps Parks and a site in the Sierra Nevada foothills. The first volume contained detailed descriptions of accelerator components, the buildings in which they would be housed, experimental areas, and support facilities. These descriptions included a summary of the technical problems encountered in the design and an explanation of the chosen solutions. For example, the report contained a definitive discussion of problems arising from magnet misalignment. The second volume contained engineering drawings, figures, and charts to support the text. (21)

In addition to the design, the report covered a number of other topics. It explored possible experimental use of the machine, including descriptions of schemes for transporting, separating, and detecting secondary beams. In a section describing possible future expansion, the report also presented ideas for increasing intensity by an order of magnitude, accelerating antiprotons, and a later addition of storage rings. (See Storage Rings) The report also included expanded site criteria fashioned along the lines of those distributed by the AEC. (22)

As promised, the report also contained detailed cost estimates, along with a construction schedule. LRL researchers estimated that the annual budget in the first year of operation would be $58.65 million, including wages, supplies, major procurement items such as power, equipment and construction, accelerator improvements, and engineering and shop costs. The estimate for construction of the accelerator facility was $287.8 million, including accelerator components and related apparatus and buildings, and engineering and architectural fees, with a 25% contingency for accelerator and technical components and a 15% contingency for more conventional construction. This estimate was based on the Camp Parks site, although the report also compared costs for the two sites, concluding that an accelerator built on the Sierra Nevada site, which did not require deep-pile foundation, would cost almost $13 million dollars less. The report stressed that the estimated construction cost was close to the $280 million estimate given in the 1965 AEC "Policy for National Action in the Field of High Energy Physics." It also explained that $19.65 million had been added to cover "possible escalation in the total project cost" over time. In addition, $40.5 million had been added to provide "research equipment sufficient to provide for exploitation" of the accelerator, including a bubble chamber and a computer. With these additions, the total LRL request was $348 million. Berkeley researchers estimated that research could begin 7 years after construction of the project was approved, adding that 11 years would probably elapse between approval and full operation of all facilities. (23)

The machine itself was a strong-focusing synchrotron capable of accelerating protons to 200 GeV with an intensity of 30 trillion protons per pulse. As the report explained, this intensity was "an order of magnitude higher than those . . . available at lower energy." As Edward Lofgren, leader of the LRL design team, explained at the March hearings, the machine had four stages. As explained in the design report, protons were first "generated in an ion source and accelerated to an energy of 750 keV in a Cockcroft-Walton preinjector." Next they were "accelerated from 750 keV to 200 MeV in an Alvarez-type linear accelerator." After reaching 200 MeV, the beam was "injected into a rapid-cycling injector synchrotron," and accelerated to 8 GeV. At 8 GeV, seven successive beam pulses were "transferred to the main synchrotron in 1/3 second and stored in orbits around its circumference." The magnetic field of this main ring was held constant during injection so that 8 GeV protons would circulate "at constant radius until the circumference . . . filled." After filling, all protons were accelerated to 200 GeV. (See Figure 2) (24)

In addition to the machine, the design included multiple experimental areas. Two external beam areas were planned, as well as an internal-target facility, allowing "a total of 12 or more simultaneous operating experiments." In addition to two bubble chambers that would "operate nearly continuously," LRL researchers envisioned that about "40 major experiments with other detectors" would be completed each year. (25)

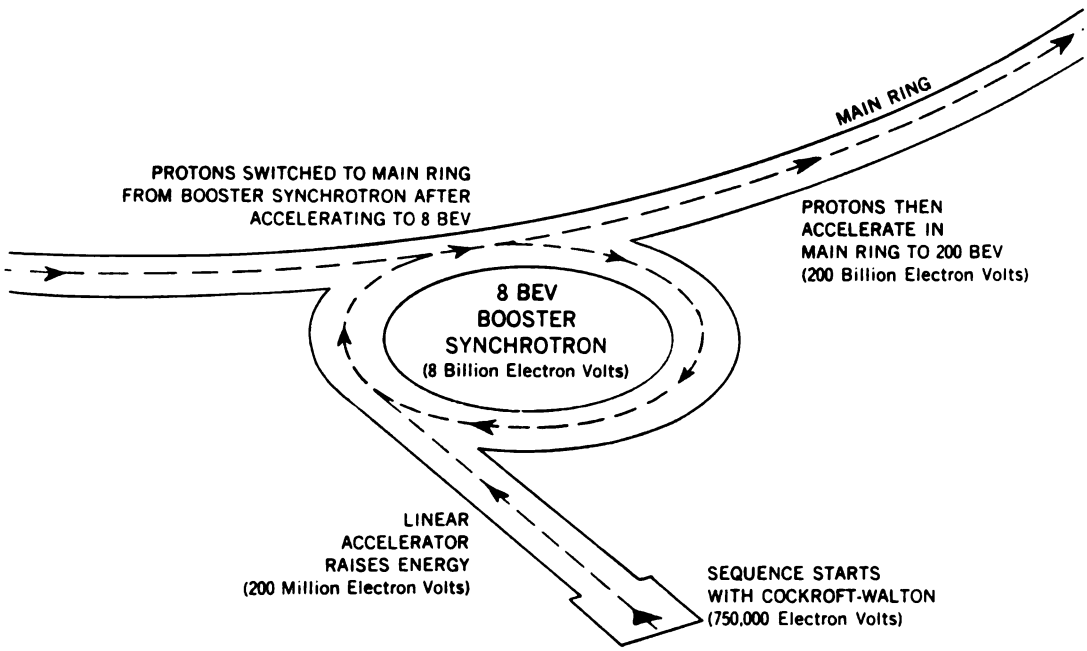


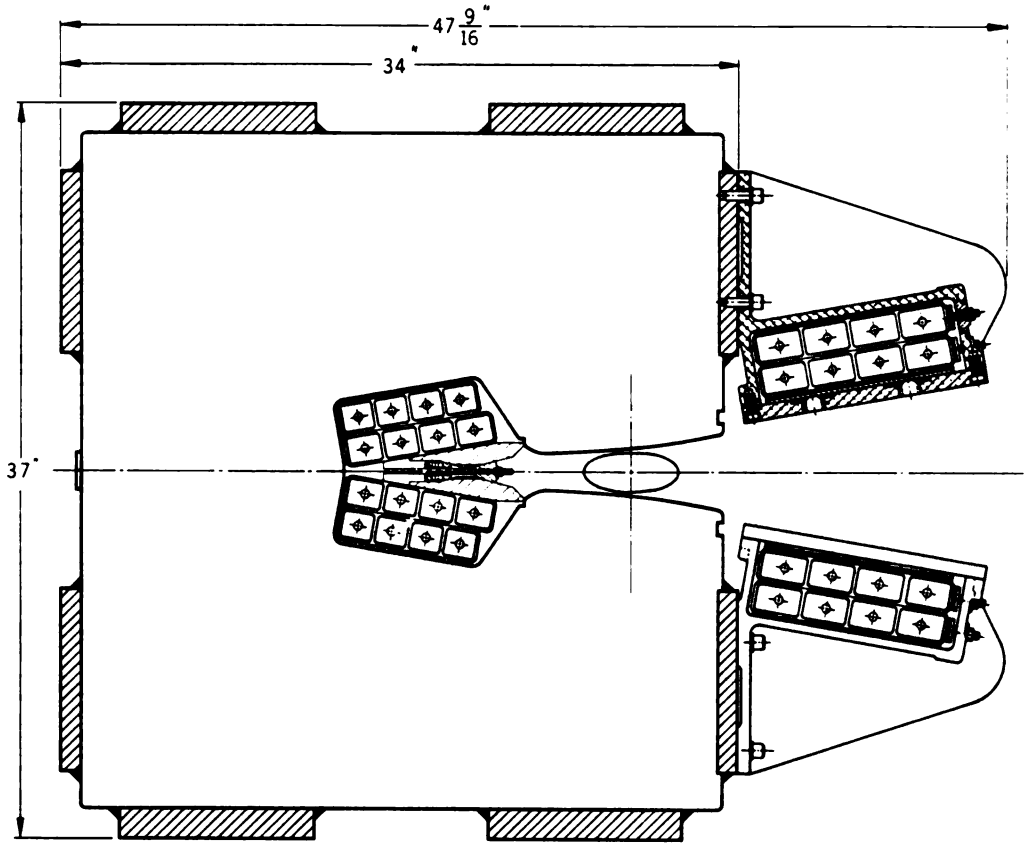
Figure 2.--Sequence of acceleration for the 200 BeV accelerator. Protons are first accelerated in the Cockroft-Walton, injected into the linear accelerator for further acceleration, injected into the booster synchrotron for even further acceleration, and finally injected into the main ring, where they reach the design energy of the accelerator. From University of California, "200 BeV Accelerator and the California Site," (Berkeley: University of California), p. 11.

The report identified several important new technical features in the accelerator facility, including the booster synchrotron and the use of long straight sections. The report also stressed that unlike previous accelerators, which used simple internal targets, the new accelerator would employ techniques that allowed researchers to rely on extracted beams for experiments. The use of extracted beams offered a number of advantages in detection, radiation safety, and in the operation of the machine. The report also stressed that the facility would "be a national facility . . . open to all on the basis of the scientific merit of the experiments proposed." In line with this claim, LRL researchers proclaimed that "approximately 70% of the experimental program" would be conducted "by visitors and 30% by the resident staff." (26)

Although the idea of an outside-user dominated facility opposed LRL tradition, the design report otherwise reflected a vision of the facility congruent with Berkeley's glorious past. For example, in line with LRL's tradition of providing superior resources, Berkeley plans provided ample experimental accommodations. The proposed experimental areas were extensive, the request for experimental equipment was generous, and plans for intensity increases and antiprotons beams further enhanced experimental opportunities. The design report also faithfully reflected the conservative Berkeley spirit, evident since the early 1960s. As planned in 1963, Berkeley researchers produced a highly reliable design and cost estimate. The report shows that LRL physicists took particular pains to scrupulously avoid an underestimation of cost.

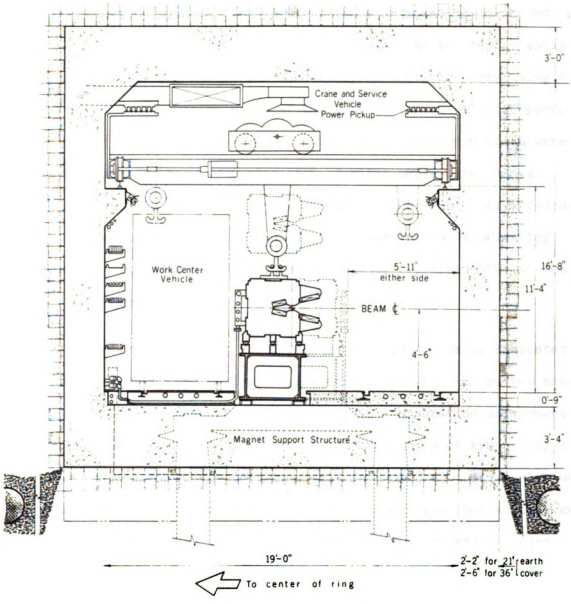
This was no accident: Lofgren remembers that as the design report was being compiled in early 1965, Paul McDaniel of the AEC stressed the importance of avoiding overruns because such problems had recently been encountered with reactor projects. Thus, the contingency allowance of 15% for conventional construction and 25% for accelerator construction was noticeably larger than the 10% contingency used in the 1961 preliminary Brookhaven design, and Berkeley researchers included an additional allowance for cost escalation over time. In addition, although construction costs for the Sierra Nevada site were slightly lower, the overall cost estimate was based on the Camp Parks site. (27)

This relatively conservative style was also evident in various parts of the design. For example, Berkeley designers considered using an H-shaped magnet instead of the C-shaped magnet used in other large strong-focusing synchrotrons, noting that the more compact "H" magnet provided "reductions in magnet, power supply, magnet enclosure, and foundation costs." However, they ultimately decided in favor of the "C" magnet, stressing that it was "a proven configuration," and allowed easier access to the vacuum chamber for future modifications and repairs. (See Figure 3) As the choice of the more costly "C" magnets illustrates, the Berkeley designers were inclined to spend a bit more money to increase the likelihood of continuous, reliable performance. This tendency was also clearly evident in the substantial magnet mooring used to assure stability at the compressible-soil Camp Parks site and provisions for locating elaborate cranes in magnet enclosures to expedite repairs. (See Figure 4) (28)



Primary Ring Magnet Elements
Gradient Magnet Cross Section

Figure 3.--C-shaped magnet designed by Berkeley team. From Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Vol. II (Berkeley: Lawrence Radiation Laboratory, 1965), UCRL-16000, p. V-6.



Main-Synchrotron Enclosure Cross Section

Figure 4.--Magnet, tunnel, and magnet support structure, as designed by Berkeley team for the Camp Parks site. Note subatantial foundation used to assure stability. From Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Vol. II (Berkeley: Lawrence Radiation Laboratory, 1965), UCRL-16000, p. VI-11.

The Berkeley design report sent a clear message to the AEC: LRL researchers were prepared to build the reliable, full-service accelerator facility they felt the physics community deserved, a machine worthy of the laboratory's fine reputation. As the report aptly summarized, the design was "basically conservative, remaining reasonably close to the current state of the art, to insure reliable performance, rapid construction, and predictable costs." Although costs were predictable, they were also high, for the ample experimental accommodations, liberal cost estimating, and conservative features of the design all worked to increase the accelerator's price tag. However, as Berkeley designer Denis Keefe recently explained: "We produced the design we thought the AEC wanted to see." (29)

After presenting its design report, LRL formally requested that the next phase of the project be introduced into the AEC budget process. By summer 1965, after the push for national corporation management at the March hearings and the subsequent incorporation of the Universities Research Association Inc. (then called URAI), LRL researchers were willing to cooperate with a national corporation that would eventually assume responsibility for operating the new accelerator. Assuming that further funding allocations would begin immediately and that a site would be chosen by the end of the year, LRL researchers were poised to proceed with the final design and construction of the accelerator. However, numerous complications, including spirited criticism of the Berkeley design, intervened to destroy such expectations. (30)

After hearing about the design from a talk given in Frascati, Italy, Robert Wilson leapt into action. Wilson, organizer of the 1960 New York planning meeting on accelerators and director of an accelerator laboratory at Cornell, produced a series of critical letters and reports that circulated widely throughout the physics community and the AEC in the fall and winter of 1965. The criticism began with a September 1965 letter to McMillan. Although the details were "most professionally worked out," Wilson remarked, the design was "much too conservative," and "lacking in imagination." In addition, it had been made "without enough regard for economic factors." Wilson noted that "as someone who helped disassemble radios for parts in the old days at the Radiation Laboratory, he was "offended" by the $348 million Berkeley cost estimate, which he characterized as "ridiculously high." Wilson worried that the high estimate would both kill the 200 BeV project and endanger the future of the 600-1000 GeV machine. (31)

In an attached paper "Some Proton Synchrotrons, 100-1000 GeV," Wilson proposed a rough alternate design of three synchrotrons and a linac, based on his experience with the Cornell electron synchrotron. The paper urged that all components be kept "simple and understandable," and featured magnets that were small, and therefore less expensive. In contrast to the Berkeley design with its easily accessible "C" magnets and cranes, the paper showed "H" magnets and 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." Similar spartan guidelines were applied to the design of experimental

facilities and research equipment. Wilson insisted, in fact, that a minimal investment in such accommodations was actually an advantage for experimenters. Noting that research interests and techniques change rapidly, he concluded that presenting experimenters with "expensive facilities" before they had a chance to use the machine "may tend to paralyze better developments later on." In line with its emphasis on economy, the September report estimated that the components of a 200 GeV accelerator would cost $70 million, and with research equipment and shielding, the machine would cost "less than 100 million dollars." For $200-300 million, the price range Berkeley designers estimated for the 200 BeV machine, Wilson claimed he could build the components of a 1000 GeV machine and experimental areas. The report also stressed that the project could be completed in about 3 years, instead of the 7 years specified in the Berkeley report. (32)

Wilson was not the only physicist to present an alternative to the Berkeley design. In an October 1965 paper, Samuel Devons from Columbia advocated using the Brookhaven AGS as a booster for a higher energy accelerator. After the addition of an extra magnet power supply, approximately every third pulse of the AGS "would be injected into a larger strong-focusing magnet ring and accelerated to between 120-150 Bev." With such a scheme, the AGS could still operate as a stand-alone accelerator. Moreover, the physics community would get a multi-hundred GeV machine for only $100 million, instead of the $280 million allocated for the 200 BeV in the AEC policy report. Devons suggested that additional money could be saved by eliminating other, less important

projects, such as efforts to increase intensities at the AGS and the ZGS. With all these savings, "prompt and vigorous development of a design for an accelerator" over 500 GeV could begin within current budget limitations. (33)

Many physicists were somewhat taken aback, particularly by the bluntess of Wilson's attack; LRL physicists were furious. It was annoying to be challenged about the management and site of the accelerator, but infuriating, considering their high reputation as accelerator builders, to have their design challenged by those outside the clique of machine builders at LRL and BNL. Wilson's objections were especially upsetting because they were so strong and so unexpected. After all, in the 1930s Wilson had been one of Lawrence's "boys," along with McMillan. When Wilson travelled to Berkeley to discuss design ideas, he met with a chilly reception from leaders of the Berkeley design team that would typify his interaction with them for many years to come. Physicists subsequently debated whether the course of the project should be diverted along the lines of suggestions made by Wilson and Devons and the AEC made plans to review the design. In the meantime, site selection created further complications. (34)

On June 5 and 6, 1965, about a week before the AEC deadline for site proposals, the NAS site committee met in Washington to plan their attack. From the beginning, committee members appreciated some of the difficulties inherent in their task. Although some site selection problems would be "highly technical and engineering in character," other issues, such as the social environment "immediately surrounding the

laboratory" were "less well defined." The committee later decided that the AEC should provide a list of the sites that failed initial screening along with an explanation of the basis of exclusion. After receiving AEC decision criteria and site information, the committee planned to develop its own guidelines. (35)

Within a few days, the AEC Task Group led by W. E. Hughes had devised two sets of criteria. The first set was the "go-no go criteria," required by any proposal for referral to the NAS. This set of criteria included minimum acreage of 3,000 acres, specified power capability, access to residential communities with a commute of one hour or less, and adequate access to highway, airport, and freight transportation. The second set of criteria were those which seemed to call for "trade-offs between the technical and other factors" to obtain "the best over-all efficiencies and economies. . . ." This set of criteria rated sites according to economic factors, such as locally dependent construction costs, and "the intangibles associated with environment . . . and land," such as proximity to high-powered graduate programs in the physical sciences and land configuration. Hughes admitted that the Task Group "expected that possibly more than 90 percent of the proposals submitted" would meet the minimum requirements of the first set of criteria. Clearly, site selection would require much more than simply disqualifying obviously unsuitable sites. (36)

Within a month, site selection had become even more complicated. The AEC announced on July 9 that the Commission received 126 proposals recommending 200 sites in 46 states. (See Appendix A) A group of

Midwestern physicists spearheaded a proposal for Madison, Wisconsin, the site of the former MURA headquarters. In the wake of speculation that the 600-1000 GeV machine might never be built because of the tightening budget, Brookhaven submitted a proposal. Proposals were also submitted for several AEC facilities, including reactor sites at Hanford, Washington and Oak Ridge, Tennessee, a move that gained the vigorous support of AEC Commissioner James Ramey. Since many felt the accelerator would bring prestige and economic benefit to its community, a large number of proposals were also submitted by citizen groups with no affiliation to the high energy physics community or to the AEC. (37)

The unexpected volume of proposals meant more work for the AEC and NAS site selection groups. In addition, the vigorous response increased the political importance of the contest. Less than a week after the AEC announcement, JCAE member Senator Clinton Anderson wrote Seaborg demanding clarification on the roles of the NAS site selection committee and the URAI in site selection and reiterating the JCAE demand that the AEC retain ultimate responsibility for site selection. After receiving explanations from Seaborg and Seitz, the JCAE refrained from complaining further about the AEC-NAS arrangement, but the episode gave evidence of the increasing political sensitivity of site selection. With more groups vying for the site, more politicians were drawn into the contest, and as site selection attracted more attention, the AEC became more vulnerable to criticism. (38)

By the end of July, the AEC had screened all proposals according to the established criteria and were ready to transmit a list of 34 sites

to the NAS. However, the political climate made Seaborg anxious. As he explained to Hornig, the list deleted "some applications from politically powerful parts of the country," including Arkansas, Iowa, and Montana, and he worried "about White House reaction regarding the . . . losers." Taking a tack they would consistently pursue throughout the site selection process, Seaborg and Hornig sent feelers to gauge the White House reaction before proceeding further. A month later Seaborg sent letters to both Seitz and Johnson presenting a list of 34 sites and five alternates. (See Appendix B) In his letter to Johnson, Seaborg emphasized that he would "continue to keep" Johnson "informed regarding the progress being made in this site selection." (39)

Despite these precautions, Seaborg soon faced trouble from the White House. On September 1, Marvin Watson, a close aide to Johnson, called to tell Seaborg and Tape that a group of White House "political animals" had unanimously agreed that announcement of the sites should be delayed to avoid jeopardizing "passage of the President's legislative program." A few days later, on a flight to Austin, Johnson encountered an enraged fellow Texan, Congressman J. J. Pickle. Although three Texas sites appeared on the list (Houston, Dallas-Fort Worth, and San Antonio), the Austin site had been deleted. In Pickle's presence, Johnson instructed his aide Horace Busby to pursue the matter with Seaborg. As Busby later explained to the Chairman, Johnson understood that the site had been set aside because of its inability to provide adequate power, and this touched a particularly "sensitive spot" for the President "because it has been his life's work to promote and foster the

lower Colorado River power down in that area." Busby therefore recommended "in the strongest terms" that Seaborg contact Johnson. "Quite frankly," he remarked, the AEC just could not "successfully argue to the President that the city of Austin could be eliminated at this stage," although this "did not necessarily mean that Austin has to get the project ultimately. . . ." (40)

Seaborg immediately notified Seitz that more sites would probably be added to the list for consideration by the NAS and issued a press release explaining there would be a delay in the announcement of the NAS list. On September 10, two days after hearing from Watson, Seaborg and Tape met with Joseph Califano, a member of Johnson's inner White House circle, to present a new plan of action. As Califano's summary to Johnson explained, they agreed that the NAS should "examine about 85 sites, rather than the 35 it had been examining." The new list included seven Texas sites, including Austin. Noting that the Commissioners were "currently subject to considerable political heat in attempts to find out what communities are being studied and what communities have been eliminated," Califano suggested that they be permitted to proceed with the list of 85 sites "since they can justify the elimination of every community beyond the 85 on the list as patently inadequate as a site for the accelerator." As Califano also pointed out, the arrangement with the NAS offered another advantage. "After releasing the list," he explained, "they can refuse to answer any additional queries until the report from the National Academy of Sciences is received." On September 13 Califano told Seaborg that Johnson had approved their plan, and the

Chairman sent the list of 85 sites to Seitz and issued a public announcement of the list on September 15. (See Appendix C) (41)

In the words of a White House Public Information officer, the announcement attracted "considerable attention" nationwide. As Seaborg lamented in his diary, the public immediately learned of the original short list. NAS Site Committee member Herbert Longenecker, who had apparently not been informed of the change in plans, announced that the NAS committee was considering 30-35 sites. Congressional reaction to the announcement of the list was particularly strong. Seaborg's office was soon besieged with complaints and requests for information. In addition, JCAE member Craig Hosmer made an angry public statement. In line with his statements at the March hearings, the Representative was annoyed that the site selection contest had become so large and heated. He charged that the AEC had "bungled" siting arrangements, which were now "mired in a mammoth pork barrel." (42)

A skeptical tone was also heard outside of Congress. When summarizing information obtained from UCLA Chancellor Franklin Murphy on the first URAI Council of Presidents meeting in November, Vice Chancellor Carl York remarked that the Council had "agreed to support the site committee and the AEC in their selection to reduce political tension." York then made an accusation that would echo through the physics community for many years to come: "However, it is perfectly clear" he explained, "that the final decision on the site will be made by Lyndon Baines Johnson." (43)

Despite the pointed criticism levied by Hosmer, York and others that fall, Seaborg had carved a relatively strong position, both for the AEC and for the 200 BeV project. Without support from Johnson, the project could not continue; thus, the original list of sites had to be enlarged. By changing the list, Seaborg left himself open to the accusation, which was true, that politicians had been allowed to influence the siting selection process. However, the AEC gained an optimally defensible position. As Califano had explained to Johnson, the longer list eliminated only those sites which were clearly inadequate, and thus it was an easier list for the AEC to defend. In addition, as he recently admitted, Seaborg calculated that a larger number of site competitors would help funding prospects. With more congressmen lobbying for specific sites, congressional support for funding was bound to increase. Moreover, an intense site competition worked to divert attention from the issue of whether the 200 BeV should be funded, to the issue of where it should be built. By keeping in close touch with the White House, encouraging the continuation of good relations with Congress, and letting the NAS take responsibility for the sticky task of evaluating the 85 sites with minimum requirements, the AEC could operate from a position of considerable strength in its efforts to arrange siting and funding for the 200 BeV project. (44)

After the transmittal of the list of 85 sites to the NAS, Seaborg and Hornig carefully plotted how they would next proceed with site selection. First they asked Califano for further "guidance." After checking with Johnson, Califano agreed that that the NAS should choose

the best 3-10 sites from the list of 85. Seaborg's task was to "develop a comprehensive system to evaluate and analyze in depth the remaining 3-10 site locations. . . ." Califano stressed "the need for the AEC to make the final choice in the best interests of the country objectively, rationally and supported by clearly understandable reasons." Seaborg responded by noting that the AEC could handle a careful review, which would take 3-6 months, within the current budget. In the next several months, Seaborg was also careful to keep the White House informed of NAS progress, including the number and location of completed site visits. (45)

While procedures were being cleared with the White House, Seaborg and Hornig also took steps to ease relations with congressmen. Because of the large amount of congressional interest in the last announcement of sites, plans were made to send letters individually to Congressmen to announce further site selection progress. Particular pains were taken to keep congressmen informed of plans for site visits in their districts. Seaborg also graciously received delegations advocating individual sites, although, as he told representatives from Arkansas and Pennsylvania in October, at this point the AEC could only refer all information to the NAS. Seaborg also faithfully discharged his duty to keep the JCAE informed of AEC activities, as he was legally required to do. (46)

Although the AEC benefitted when the list of sites was lengthened to 85, this move only increased the burden of the NAS Site Selection Committee. In addition to assuming full responsibility for judging all

sites with minimum requirements, the Committee also faced a substantially increased workload. When transmitting the list to the NAS in September, Seaborg assured Seitz that the Commission was willing to extend the deadline and increase the budget to compensate for the greater workload. He added that "the Commission and staff" would be "happy to assist the Committee in any way that they" could. This offer proved welcome, especially since Seaborg soon pressed for visits to every site in the wake of the accusations of unfairness and increased congressional interest that arose after the announcement of the 85-site list. It was subsequently agreed that the bulk of site visits would be handled by AEC-organized site visitation teams, which would include at least one high energy physicist per team. To obtain extra advice, the committee also formed two panels of specialists. A Foundation Requirements Panel evaluated the geological suitability of sites, and a Panel of Accelerator Scientists gave advice on other physical attributes affecting construction, such as power, climate, and water. (47)

Great effort was taken to make this initial narrowing of sites as thorough and rigorous as possible. After each site visit by the AEC team a report was compiled and forwarded to the NAS committee, and, in most cases, committee members also interviewed team captains. NAS committee members also made a number of site visits "to get firsthand impressions" and "to sharpen their own insight about the factors important in site selection," as they later explained. Shortly after receiving the first list of sites in early September, the Committee began visiting sites, and by the end of the month, Chicago, Wisconsin,

and Denver sites had been visited. Committee member Goldwasser remembers that committee members eventually visited about 25 sites. Throughout this process site criteria were amended and refined. In November committee chairman Piore polled members of the physics community to get advice on site criteria and the AEC presented a new set of criteria for consideration. (See Appendix D) (48)

As deliberations continued and the NAS Site Evaluation Committee made preparations for its March report, further tensions were building in response to increasing budget pressure and the continued debate over the 200 BeV design.

Burdened with the war in Vietnam and burgeoning Great Society programs, Johnson began to stress austerity in mid-1965. Throughout the year the economic situation worsened. By December, White House advisors warned that unless spending drastically declined, a hefty tax increase would be needed to avoid inflation, a step Johnson was loath to take. In line with the push for frugality, a BOB representative insisted in August that the AEC avoid making a funding commitment to the 200 BeV in the next fiscal year (July 1966 to June 1967) because of the "rough" budget sessions currently being held with the President. In October, Califano discussed the issue with Johnson, and the President readily agreed that funding should be deferred, except for a few million dollars to support the AEC effort to choose the final site. (49)

In the wake of increasing fiscal pressure, the BOB also pushed the AEC to pare down their expensive accelerator project. Thus, in December 1965 the Commission reluctantly asked LRL to make a cost estimate based

on an accelerator with reduced initial intensity and less extensive experimental facilities. The LRL design group subsequently presented two ideas for a reduced scope accelerator. (50)

As explained in a December 13 report, in this exercise LRL researchers "tried to avoid changes that would jeopardize the reliable operation of the accelerator." The report then presented an injector capable of producing one tenth of the intensity promised in the original report. LRL researchers subsequently devised an alternate scheme with radiofrequency and magnet power supply systems with a reduced repetition rate. Since this reduced repetition rate would lower the voltage requirements on both systems, money would be saved initially on supplies. The overall design was not affected, however, and the systems could later be replaced to provide the original capabilities promised in the June 1965 Design Report. In both schemes, the number of experimental stations and experimental equipment was substantially reduced from the original design. As later announced by the AEC, projected cost reductions for the accelerator were $60 million for the first scheme and $56 million for the second scheme. Both schemes reduced the cost of experimental facilities by $28 million. (51)

As Berkeley researchers were struggling to make quick revisions to their design, concern about the project was growing at the AEC. For example, in his notes for a December 12 meeting, Seaborg reports that he explained to URAI Trustees that "the President had not yet approved the project." Although their immediate problem was to get funding to continue design work for the next fiscal year (July 1966 to June 1967),

it was also necessary "to do everything correctly in order to successfully obtain approval of the machine through all stages of the approval process, and in view of all the political pressures." The problem of finding funding for an expensive project at a time when budgets were shrinking was exacerbated by growing criticism of the Berkeley design. In addition to continued heated debate within the physics community, Devons had written to the JCAE, drawing congressional attention to the dispute and prompting Chester Holifield, Chairman of the JCAE, to demand that the AEC formally review the issue. (52)

At this juncture, Seaborg and Seitz had reason to feel proud that they had successfully nurtured the formation of the URAI. By this time the new management organization had elected a Board of Trustees, including prominent scientists and other distinguished academics from throughout the country, and formally offered its services to the AEC and the JCAE as the contracting agency responsible for constructing and operating the new accelerator. Although the 200 BeV project was not ready for construction, in light of BOB pressure to cut costs, continued divisiveness among physicists, and JCAE irritation, it could well use a group of well-respected professors prepared to rally support for the machine within the physics and academic communities. As Seaborg emphasized at the December 12 meeting, they had to have "unanimity among scientists and university administrators" if they hoped "to get the machine." (See Appendix E) (53)

By December, both Seitz and Seaborg were critical of attempts to divert the course of the 200 BeV project. In a December 14 letter to

Ramsey, who was now chairman of the URAI Scientific Subcommittee, Seitz caustically remarked that Wilson, another member of the Subcommittee, "may soon appreciate that the framework within which the big machine will proceed will probably be quite different from that surrounding the Cornell machine." In a December 20 letter to Ramsey, Seitz further concluded that "it would be foolhardy to introduce any radical new proposals at this time." At the December 12 meeting with the URAI Trustees, Seaborg levied similar criticism, which applied not only to the Wilson and Devons proposals, but also to the reductions the AEC had asked LRL to consider. As explained in his notes, in his "personal view," attempts "to change to another energy and/or intensity of beam would be a mistake and might jeopardize getting anything because the balance was so precarious." Despite these personal views, in response to JCAE pressure and discontent with the physics community, the AEC and the URAI took careful steps to thoroughly discuss and evaluate design criticism to see whether it was time to divert the course of the 200 BeV project. (54)

In January 1966, two meetings were held to discuss the criticisms of Wilson and Devons. Brookhaven's management organization, AUI, hosted a meeting at the Biltmore Hotel in New York on January 15 which was attended by the URAI Scientific Subcommittee, AEC representatives, and about 150 high energy physicists. On January 24, the AEC hosted a similar, smaller meeting in Washington. (55)

Wilson later characterized the New York meeting as a "sort of advertised rumble," meant to provide "a real thrashing out of ideas."

Wilson and Devons argued that costs needed to be cut and promoted their individual schemes while members of the LRL Design team argued that a national accelerator facility of this size required a conservative, reliable design. In the ensuing debate, the core issue was not the relative technical merits of competing design ideas. Instead, researchers debated whether it was desirable to scale-down the price of the Berkeley 200 BeV design, either by reducing capability by taking an intermediate step or by greater risk-taking, to free money for obtaining the highest possible energy as soon as possible. Although many physicists had been interested in the broad outlines of Wilson's ideas, Wilson had fewer supporters when his alternate design was more thoroughly discussed. Wilson remembers that after his presentation, "nobody was impressed" with his ideas. The Scientific Subcommittee report to the URAI similarly noted that there "seemed to be no widespread strong support" for either the Wilson or Devons proposals. (56)

The Washington AEC meeting held on January 24 produced similar results. The meeting began with a number of presentations, including talks by McMillan, Lofgren, Devons, and Wilson. According to URAI Scientific Subcommittee notes, participants found they could not agree on the amount of cost savings to be gained by using the AGS as an injector, as suggested by Devons. In addition, Wilson remembers that the response to his ideas was "generally negative." Scientific subcommittee notes further report that although participants could not agree on the optimal design of the accelerator, they did agree that the next

accelerator should have an energy of 200 GeV. Henry Smyth, the current chairman of the URAI Board of Trustees, went further, expressing his "personal conclusion" that none of the modifications, including that of reduced scope, seemed to "justify a re-opening of the whole question of the 200 BeV accelerator." He thought "it would be a shame to lose the momentum acquired" so far, and noted that "suggestions as have merit" could simply "be taken into account in the process of developing and planning the accelerator." (57)

On the heels of the AEC meeting, both the URAI and the AEC took an official stand on the design. On January 26, Smyth wrote to Seaborg, explaining that the URAI Board of Trustees had asked him to relay the news that they were in "general agreement" with his concluding statement at the January 24, 1966 meeting. A February review by an AEC staffer firmly deprecated the design challenges, characterizing the Wilson and Devons proposals as shortcuts that neglected "the overall needs of the national program" and reflected "a distinctly minority opinion" among physicists. The report noted that Devons' suggestion had "a number of drawbacks," in particular tying up "the world's highest energy and most exciting facility in high energy physics . . . with all of the accompanying limitations on flexibility." While the report admitted that Wilson had suggested "a number of interesting design ideas," it noted that he was "willing to have a more marginal design, take greater risks and work out difficulties as they arise," an approach that did not "seem appropriate for a several hundred million dollar national facility. . . ." In contrast to these criticisms, the AEC judged that the LRL reduced

scope schemes maintained a "balance between safeguards and risks" and were thus "still appropriate for a national accelerator facility." (58)

Although the AEC preferred to proceed with the original Berkeley design, as indicated by Seaborg's comments in December, BOB pressure made it necessary to retain reduced scope as an option. On February 16 Seaborg sent the report to Holifield with his endorsement. After these official pronouncements, the design debate subsided. Without official support for diverting the 200 BeV project along the lines of new design ideas, those critical of the Berkeley design simply waited, knowing from past experience that the design would be open to further revision when plans for actual construction began. (59)

In the meantime, the site selection process ground on. When they began examining site data in their search for finalist sites in late 1965, NAS Site Evaluation Committee members soon realized that their decision would hinge on intangible factors. Although the Foundation Requirements Panel urged that the accelerator be built on a site with minimal risk of geological instability, this requirement did not discount a large number of sites. Another tangible factor to consider was cost, but committee members quickly decided that it was more important to choose a site where a productive laboratory could grow than to choose a site where a laboratory could be built and operated cheaply. As the Site Committee later argued in its final report "the ultimate cost of the project depends less on the physical features of the site than on the competence and ingenuity of the physicists and engineers who will design the facility and be responsible for its operation." The

January 1966 report of the Panel of Accelerator Scientists reinforced this opinion. Since "application of ingenuity and money can overcome most physical disadvantages of a particular site," they argued that "a prime consideration is the attractiveness of the site and its environment for the permanent staff, and that this consideration should be not be subordinated to the physical ones." As in design discussions, non-technical factors dominated siting considerations. (60)

With this established, the Committee found it relatively easy to narrow the field to a few choice sites, which included, among others, sites in California and at Brookhaven, and the first sites visited--two sites in the Chicago area and sites in Denver, Colorado and Madison, Wisconsin. As Piore explained in a November meeting, due to the expense of in-depth site evaluation, which he estimated to be as much as $1 million, the Committee was concentrating on these choice sites. According to meeting notes taken by McDaniel, when questioned by Seaborg, Piore admitted they were not "using the point system at all," and only applied ratings "to check their judgments and conclusions reached by other means." Some AEC Commissioners were disappointed that so few AEC facilities were included among the choice candidates. In addition, because of their desire for an easily defensible site, they were concerned that so many proposals were being dismissed so quickly. (61)

Although committee members resisted pressure to include more AEC sites, they were also concerned about potential adverse reaction to the

list of finalist sites. As Goldwasser explained in an October letter to Piore:

> It is foolish to think that our only job is to come up with the best possible site. Politics are involved in this decision and in its acceptance by the Commission, by Congress, by the President, by the community of scientists and by the public. Unless it is quite universally accepted and supported our exercise will have been a fruitless one. (62)

Because of such concerns, charts were ultimately made listing both AEC and NAS committee tallies of various sites. In addition, although Committee members seriously considered choosing one from the list of choice sites, they ultimately decided to include several sites with top qualifications, which were scattered throughout the country. As Goldwasser remembers, committee members felt such a selection allowed the AEC to consider "political realities" when choosing the final site. (63)

As planned, the Site Committee made recommendations for finalist sites in a March 1966 report. When explaining the basis of recommendations, the report noted that the Committee determined whether "a given site had suitable physical properties," then "assigned paramount importance to the considerations that affect the recruiting of personnel for the national accelerator laboratory, and the participation of the nation's high energy physicists." The report later explained that although none of the sites was "ideal," the Committee found several sites which "in general," satisfied all important requirements. Brookhaven and one of the California sites, the Sierra Nevada Site, made the list, although the Camp Parks Site was discounted because the Panel

of Foundation Requirements judged that the site could be affected by earthquakes. The list also included sites in Ann Arbor, Michigan; Denver, Colorado; Madison, Wisconsin; and two sites in Illinois, South Barrington or Weston. (64)

When the AEC received the NAS recommendation, staff members followed a pre-arranged, carefully orchestrated procedure to announce the NAS list of finalist sites. First the White House was notified. As Seaborg explained to Hornig, he agreed to a short delay after being informed of problems in "getting votes on the rent subsidy bill." Then, shortly before the public announcement, congressmen were notified. Finally, on March 30, 1966, a press release announced the finalist sites. (65)

After the announcement, Seaborg's office was barraged with angry calls as congressmen and business leaders pressured him to reconsider a favored site. Additional pressure to reevaluate sites came from Senator Edward Long of Missouri, who complained on the Senate floor on May 9 that "the Academy has picked six seriously inadequate sites." However, the Chairman steadfastly insisted that selection of finalist sites had been made on the basis of the site criteria and would not be reconsidered. With this settled, the last agonizing phase of site selection was about to begin. Although the Denver site was not associated with past tensions, the site contest included sites sponsored by the two traditional rivals--LRL and BNL--and several Midwestern sites at a time when many felt that the region was long overdue for a major accelerator. It was now up to the AEC to make the final decision. (66)

Summary: The Berkeley Design and the Selection of Finalist Sites

Although a good start had been made in the first months of 1965 in arranging management, siting, and funding for the 200 BeV accelerator, in the next year complications arose with both the design of the accelerator and with the siting contest. In both cases, non-technical issues prompted controversy. When the LRL design team presented plans for a reliable, full-service accelerator costing $348 million, Robert Wilson and Samuel Devons presented alternate design ideas. In the ensuing debate, the core issue was not the relative technical merits of competing design ideas. Instead, physicists debated whether it was desirable to scale-down the price of the Berkeley 200 BeV design, either by reducing capability by taking an intermediate step, as suggested by Devons, or by greater risk-taking, as suggested by Wilson. The advantage, in either case, was that money would be freed for obtaining the highest possible energy as soon as possible.

While physicists debated the wisdom of diverting the course of the project to obtain this benefit, site selection also became more difficult. The $250 million project attracted considerable attention from the general public and from politicians, including President Johnson. As a result, far more proposals were submitted than expected, requiring the evaluation of 200 sites. In addition, although the AEC tried immediately to skim about 35 sites, Johnson intervened, insisting that an Austin site be restored to the list. To satisfy Johnson, extricate the AEC from the uncomfortable position of eliminating sites with minimal requirements, and increase the base of congressional

support, the AEC subsequently forwarded 85 sites to the NAS committee. While this move strengthened the position of the AEC and increased funding prospects for the accelerator, it placed a considerable burden on the committee. In addition to the increased workload caused by the large number of sites, the committee simultaneously faced judging all sites with minimum requirements and the growing realization that site selection would hinge on intangible factors. As with management and design plans, cost and resulting pressures made nontechnical issues dominant in the site contest.

In the meantime, economic pressures steadily increased, in response to heavy spending for Vietnam and Great Society programs. Despite earlier optimism, by the end of 1965 Seaborg was worried, not only about the prospects for obtaining funding in the next fiscal year, but also about obtaining ultimate approval of the project. Such worry was only exacerbated by the continued design debate, which had attracted congressional attention. At this point, promoters of the 200 BeV project were fortunate that Seitz and Seaborg had nurtured the formation of URAI, now a functioning organization with an elected Board of Trustees. Together the URAI and the AEC worked to air design complaints and rally continued support for the machine. Indeed, after a flurry of meetings in January 1966, design debate subsided. In March, after countless tally sheets, site visits, and the advice of specialized panels, the NAS Site Selection Committee chose the finalist sites: the sites sponsored by LRL and Brookhaven; Ann Arbor, Michigan; Denver, Colorado; Madison, Wisconsin; and two sites in Illinois, South Barrington or Weston. By

April 1966 the AEC faced the challenge of choosing one site from the several qualified, ardent contenders while guiding the project through the process of funding approval, a process now delicate in light of increasing economic pressures.

Final Siting and Funding and Selection of the First Director

When faced with the task of guiding the 200 BeV project throughout the last, sensitive stages of site selection and funding, Seaborg and Hornig were careful to check with the White House for further "guidance." The record shows no strong reaction from Johnson at this juncture. However, BOB, the project's biggest foe since 1965, took action, realizing that the AEC planned to complete the final selection in 3 to 6 months, that the first large funding request would follow the announcement of the final site, and that the federal budget was tight. On April 22, 1966 BOB Director Charles Schultze wrote to White House Aide Califano stating that it was "clearly necessary that the Bureau of the Budget review the bases for the AEC's choice," since operating and construction costs would vary from site to site. After Califano agreed, Schultze wrote to Seaborg, mentioning that he "assumed" the Commission intended "to look closely into the relative construction and operating costs at the various alternative sites." In addition, Schultze insisted that the AEC provide a study showing the cost differences in construction and operation arising from a reduction in intensity. Such considerations were "especially timely," he noted "in view of the President's concern about construction outlays." (67)

Despite the rising budget pressures, Seaborg took a firm stand against attempts to pare down the expense of the 200 BeV project. Although he noted in his May 12 reply to Schultze that relative operating and construction costs would be considered, in line with NAS Site Evaluation Committee sentiments he stressed that a "critical

determinant of the ultimate total cost of the project . . . is the quality and capability of the scientists and engineers that can be attracted to the laboratory. . . ." He also noted the exciting neutrino experiments that would be facilitated by higher intensity beams and explained that it would be much more expensive to extend experimental facilities after construction than to build them as part of the original project. "Evaluation of the differences in benefits against the differences in costs," he concluded, led the AEC to the conviction that the original scope of the June 1965 Berkeley design study was "the most appropriate for the national program." (68)

Although the AEC and the BOB would continue to debate reduced scope, by mid-June 1966 Seaborg had ironed out a plan for White House and BOB participation in site selection. As Seaborg's notes from a June 13 meeting with Califano indicate, they agreed that Seaborg would "discuss prospective sites with Charlie Schultze at a stage just before the Commissioners have made their final decision, but when the possible identity of the site is becoming known." Seaborg would then "get in touch with . . . Califano to get a feeling for the timing of the announcement and any possible White House Reaction to the choice." In this way, Seaborg could keep both BOB and the White House informed and give each a chance to argue against the AEC choice before a public announcement was made, these being necessary moves to prevent potentially disastrous objections. At the same time, the AEC was free from direct intervention while the decision was being made. This arrangement pleased Seaborg and the other Commissioners because it gave

them the power to make the decision themselves and allowed them to deny, in good conscience, the accusation that politicians were dictating the choice of the finalist site. (69)

While these steps were being taken, the AEC also quickly made arrangements to evaluate finalist sites. Plans were made for site visits by AEC staff members, with at least one Commissioner included in each visit. Commissioner James Ramey eventually visited all the sites, while Seaborg and Tape visited the Weston, Denver, Madison, Ann Arbor and the California sites, and John Palfrey visited Brookhaven. Site visits gave the proponents of individual sites the opportunity to plead their case directly to the Commission, and gave Commissioners the chance to gain first-hand information. During this period, site proponents provided supplementary information, and the Commission asked for additional information. For example, local utility companies were asked for projected power costs and the governors of the six states sponsoring sites were asked to confirm and clarify all site-related commitments made by local groups and politicians. On the basis of this information, the AEC staff prepared detailed reports to aid the Commissioners in their decision making. Great care was taken to make the evaluation as objective as possible. For example, the AEC staff devised three separate methods for tabulating construction costs to ensure the reliability of results, and both the AEC staff and the Commissioners considered ways to refine decision criteria. (70)

In the midst of the AEC's deliberations, promotion efforts for individual sites increased to a near-frenzy. As the Long Island

Association of Commerce and Industry announced on April 15, their research "verified that all the competing sites have, as of this date . . . launched a major political program." The Long Island group planned meetings with local politicians and with education, labor, and business leaders to lobby support for the project. Similar efforts abounded at other sites. For example, as explained in a May press release, after a 13-day campaign in Illinois, a citizens group proudly presented Illinois Governor Otto Kerner "with 6,727 signatures of land owners and residents in support" of the Illinois site. Kerner himself had already heartily endorsed efforts to make Illinois the new home for the accelerator, which he called the "greatest scientific prize in this century." As the first site proposed, Denver had the benefit of more than a year of campaigning by the Proton Accelerator Committee of the University of Colorado, which was aided by the Colorado Division of Commerce and Development and endorsed by John Love, Governor of Colorado. All members of the California delegation to the House of Representatives, with the exception of JCAE members who reserved judgement, signed a letter to Seaborg insisting that "on the merits" the California site was "the obvious and only possible choice." The Madison site was warmly endorsed by Wisconsin Governor Warren Knowles, by the Wisconsin State Chamber of Commerce, and by the Madison Federation of Labor. Michigan Governor George Romney declared in a letter to Seaborg that his state could "match or excel others in any requirement that may be considered." (71)

Although every finalist site was promoted by local politicians, an AEC tally of congressional inquiries, compiled when the AEC began its

final evaluation, indicated that the Illinois site had particularly strong congressional support, with 27 interested congressmen. The New York and Michigan sites came in a close second and third, with 20 and 19 interested congressmen, respectively, and California, Wisconsin, and Colorado trailed behind, with 13, 13, and 7 interested congressmen, respectively. Interested congressmen in Illinois, Wisconsin, and Michigan, unlike those favoring other finalist sites, lobbied for their region, as well as for individual sites, further amplifying the strength of Midwestern support. (72)

Despite the dramatic campaign efforts, the AEC also faced those who protested against building the accelerator at a local site. While complaints were heard from residents about many sites, the strongest complaint ironically came from Illinois, despite the state's strong support for the project. On April 5, just as the AEC was beginning its final evaluation, the State of Illinois withdrew the South Barrington site from competition, noting strong community opposition to building the accelerator in the affluent Chicago suburb. Science reported that South Barrington residents feared that the influx of physicists would "disturb the moral fiber of the community." In contrast, residents of the village of Weston actively campaigned for the project, proclaiming that it would bring prosperity to their community. However, farmers close to the Weston site complained vehemently, and a petition with 114 signatures opposing the use of the site was sent to the AEC. As one irate farmer explained in a letter to Johnson, local farmers considered

it "dastardly" to place such a facility on "some of the richest farming soil in the world." (73)

As public furor about the finalist sites increased, the mood within the physics community became more tense. Although such decisions had traditionally been made within the confines of the physics community, the site contest was now a hotly contested fight for community prestige and economic benefit that extended far beyond the physics community. Many physicists found the public spectacle distasteful and the loss of control disturbing, especially since the stakes were so high for physicists. Because there would only be one such facility, the future of American high energy physics hinged on the success of the new laboratory, and as indicated by the comments of the AEC Site Evaluation Committee and the AEC, many felt that the site of the facility would be an important factor in its success. In addition, physicists lobbying for individual sites were battling for the best interests of their regions, their institutions, and their own careers. LRL physicists, who entered the contest determined to extend the laboratory's tradition of accelerator building, had a particularly strong sense that the future success of LRL hinged on winning the new machine.

Tape, who acted as the liaison between the Commission and the physics community, also remembers that as the site competition continued, the Commission found itself at odds with some elements of the physics community. In particular, he was disturbed when the design groups at LRL and BNL acted as though the accelerator really could not be built except at Brookhaven or the LRL-sponsored site because of

unmatched expertise and proven competence of the BNL and LRL design teams. Despite the push for outside user rights by young active experimentalists, whose interests were best served by a new facility unaffiliated with either established laboratory, many physicists agreed with this view. For example, Wilson, who certainly had nothing to gain from such a decision, later admitted that he felt at the time that the traditional prerogative of the accelerator builder should be honored and the California site chosen, and if that were impossible, then the experts at Brookhaven should have been chosen. (74)

As Tape remembers, the Commissioners saw the matter differently. In their view, the decision to have a site competition could not be defended if the location of the design team was the determining factor. As Tape explains, how could they mount an expensive, time-consuming site selection process "and then turn around and say the machine goes to Berkeley, or Brookhaven because of proven competence," a fact well known at the onset? Thus, it would have been difficult to defend the choice of either Brookhaven or the California site unless an extremely convincing case could have been made without reference to the expertise of local design groups, which considerably weakened the chances of both laboratories. Physicists at both laboratories sensed this, but in light of the growing list of disappointments suffered by Berkeley physicists and the importance they placed on winning the machine, LRL physicists were particularly upset. In fact, McMillan felt that the AEC had betrayed them in response to those who were jealous of their past success. For example, McMillan remembers feeling that the AEC was

determined not to give them anything, to the delight of enemies who gloated, "poor old Berkeley, here they used to be on the top of the heap, now they're nobody." Tape concludes: "Old patterns had to change, and it was a painful time for all of us." (75)

In the meantime, the AEC was faced with additional problems. Although the Commission had hoped to make a final site decision as early as July 1966, in late June, Seaborg received a letter from Clarence Mitchell, director of the Washington Bureau of the NAACP. Mitchell complained that Illinois had not passed legislation to enforce open occupancy laws and had a history of housing discrimination. Any hope for an imminent decision vanished as the AEC mounted an extensive campaign to investigate civil-rights compliance at finalist sites. By the end of July, proposers were asked to provide assurances from local governments, labor unions, real estate associations, and citizen groups that minorities would not face discrimination in the communities surrounding the proposed sites. At the same time, the Commission asked for judgments on the six communities from the Equal Employment Opportunity Commission, the Community Relations Service, Department of Justice, the President's Committee on Equal Opportunity in Housing, the Commission on Civil Rights, the Civil Service Commission, and the Office of Federal Contracts Compliance, Department of Labor. In addition, in September governors of the six states containing finalists sites received copies of an summary of civil-rights objectives written by William Taylor, director of the Civil Rights Commission. At this time they were also told that the AEC had hired consultants to further investigate power

capabilities. As Seaborg explained to Schultze in July, due to "some problems regarding civil rights," the Commission might need "a number of months" to reach a final decision. (76)

Aware that the emotions accompanying the site competition were only getting stronger with time, URAI officials worked hard to ease tensions and focus attention on future plans as the site competition dragged on through 1966. After the first meeting between the URAI and LRL on January, 15, 1966, Board of Trustees member Norman Ramsey talked with McMillan for hours to hear the LRL director's concerns and help facilitate future arrangements between LRL and URAI. Amid the heated debate over individual sites, the organization was careful to remain nonpartisan, constantly reminding physicists of the necessity for unified support for the machine. As Ramsey remembers, despite these efforts, the Board worried that "losing groups would make a big uproar," to get the site transferred, and instead "get the project killed." To avoid this catastrophe, Ramsey, who was elected URAI president in October, began a fall tour of meetings with user groups from various laboratories to stress that the final AEC choice would be non-negotiable and that the machine would be lost unless the physics community supported whichever site was chosen. In November, when the organization officially assumed its current name, "URA," lists had been compiled of possible leaders of the new laboratory. At this time, amid plans for a meeting to show general support for the project to coincide with the announcement of the final site, Ramsey also made preparations for the

URA's official request to assume management resposibilities, which the AEC wanted as soon as the site was announced. (77)

In November 1966, after eight months of deliberation, the Commissioners were ready to make the final site decision. By this time, the AEC staff had summarized site information, including staff studies of operation and construction costs, into a working paper that was placed into the hands of the Commissioners just before the final decision was announced. Although this document did not give detailed information about the status of civil rights, such information had been summarized for Commissioners in a document issued in August. While the Commissioners may have discounted this evidence and made the decision on some other basis, or might have let others influence the decision, as is often claimed, these documents provide a good summary of the information available for judging sites according to the official criteria. (78)

The November summary, which was organized according to the categories given in the November 1965 site criteria, showed many similarities among the six sites. (See Table 1) When land suitability was judged, all sites had sufficient acreage, could be turned over to the AEC at no cost, and had adequate subsurface and surface soils to support the proposed accelerator. All the sites, except the Denver site and the California site, required the acquisition of some farming land, and the California site was currently used for grazing. In addition, all sites were within a hour's commute to communities of reasonable size, all sites had adequate power and water supplies for the accelerator, and

Table 1.--November 1966 AEC table comparing cost estimates of six finalist sites for the 200 BeV accelerator.*

Site	Project Basis (in thousands)		Project Basis Plus Excluded Costs (in thousands)		Staff Engineering Judgment re Location and Cost Estimate Range
	Min.	Max.	Min.	Max.	
Ann Arbor	251,200	297,100	253,200	299,100	Closer to Max.
Brookhaven	263,900	285,300	264,100	285,800	Avg.
Chicago	246,500	293,400	247,400	294,400	Closer to Min.
Denver	237,700	243,800	239,800	247,900	Avg.
Madison	243,100	248,300	244,900	250,100	Avg.
Sierra	239,810	246,330	240,410	247,330	Closer to Max.

*AEC Commissioners had this table, along with other summary information on the six sites, when they made their final decision. Data from John A. Erlewine memorandum to Glenn Seaborg, Wilifred Johnson, Samuel Nabrit, James Ramey, and Gerald Tape with enclosure "200 BeV Summary," November 21, 1966, Seaborg Collection, Box 139, DOE Archives.

annual operating costs for all sites ranged from $63.719 million for Ann Arbor to $65.045 million for Brookhaven, a difference of only 2%. (79)

Despite these similarities, differences were apparent. The Weston and Brookhaven sites showed less promise for future expandability than the other sites, and the Madison site had elevation differentials that exceeded the 100-foot limit set by the criteria. Construction cost comparisons, which estimated costs for building the machine described in the original 1965 Berkeley design, showed that if all costs were considered, the accelerator would be most expensive to build at Ann Arbor, with Brookhaven a close second, and least expensive to build at Denver, with the California site a close second. When accessibility for accelerator users was measured, Weston and Ann Arbor topped the list, with an estimated average travel time of 3.5 hours for all groups and accessibility in one day for 77% of users in major cities. The California site had by far the poorest rating in this category, with an average travel time of almost 7 hours and only 23% accessibility. Denver also ranked poorly, with an average travel time of almost 5 hours and only 23% accessibility. (80)

The August 31 summary of the status of civil rights at the six sites showed that all sites were similar in many ways. As the report explained: "All six site states have fair employment practices laws with enforcement provisions." In addition, the Equal Employment Opportunity Commission considered "the employment attitude at all sites 'generally progressive.'" The AEC staff felt that discrimination was unlikely at community facilites, such as hospitals and stores, in the communities

surrounding all sites. Nonetheless, the report admitted that Illinois had "no fair housing laws," and generally "adverse comments" had been received about two site areas: "the Ann Arbor NAACP expressed the opinion that Negroes suffer discrimination in that area; civil-rights leaders in DuPage County express doubt that assurace of nondiscrimination would be honored." (81)

Evaluating such information posed a challenge for Commissioners. Although some site information, for example construction costs and estimated travel times, could be easily ranked, the Commissioners had to find a way to weigh the various factors. Since major complaint from politicians or physicists could kill the project, they were also under considerable pressure to produce a site that could be defended to both physicists and politicans as the "best" site. Geographical considerations aside, many politicans, like the BOB, would surely see Denver as the most attractive site because the accelerator could be built there with the least expense. However, the NAS Site Evaluation Committee had judged, and the AEC had concurred, that construction costs were not as important as factors that would influence staff recruitment and use of the facility. The most straightforward measure of the use of the facility was accessibility to outside users. If this factor was given top priority, the Ann Arbor and Weston sites would top the list. Discounting regional preferences, such a measure would likely please active, young experimentalists, but would eliminate the California and Brookhaven sites, which clearly scored highest in the potential to recruit staff members because of the large staffs already assembled at

BNL and LRL. As already explained, the Commissioners would have had a hard time defending the choice of either Brookhaven or the California site, using staff availability as the prime consideration, and both sites got poor ratings in other important measurements: Brookhaven was the second most expensive site and the California site was the least accessible.

Commissioners also had to consider that the Denver site, which was an inactive bombing range, was the only site that could be acquired without disrupting farm or grazing land; that the Madison site had elevation differential problems; and that the Weston and Brookhaven sites had limited expandability. Civil-rights information provided an additional complication. Although Weston was one of the most attractive sites over all, since it fell mid-range in the ranking of construction costs and was one of the most accessible sites, it ranked poorly in civil rights.

According to Seaborg's records, on November 29 the Commissioners narrowed the list of sites to Weston and Madison. After making sure that BOB and the White House knew a decision was imminent on December 7, the Commission selected the Weston site. On December 16 Seaborg wrote a letter to Johnson and issued a press release announcing the site. In the next few days phone calls were made to break the news to JCAE members, Governor Kerner and other Illinois officials, and McMillan. (82)

Almost immediately, the decision was criticized. On December 19, NAACP official Clarence Mitchell wrote to tell Seaborg that the decision was a "considerable disappointment" to him because of the lack of fair-housing legislation in Illinois and asked for "specific assurances" as to efforts to "prevent racial discrimination in housing." A December 20 New York Times article cynically wondered whether Weston was "picked because it was most suitable to the purpose or primarily to give a politically important area its 'fair share' of the spoils?" The newspaper asked for a detailed explanation of the bases for the choice, as did New York Senator Jacob Javits, who sent the JCAE a list of 29 questions, including the demand that the AEC justify the dismissal of Brookhaven in light of the cost savings to be gained by using the Brookhaven AGS as an injector. According to Seaborg's diary, LRL physicists also questioned the "fairness" of the decision in private conversations with the Chairman. As David Judd explained in a February 1967 letter to Seaborg, LRL physicists had felt for some time that the AEC did not understand LRL's "problems and concerns," and in "this atmosphere, the site decision came as a crushing blow." Although LRL did not make a public complaint, McMillan refused Seaborg's request to publicly endorse the site. As Judd explained, to Berkeley ears, the request sounded like "Give gladly your last drop of blood as you die!" (83)

According to Seaborg's diary, when JCAE member Holifield quizzed him on the basis of the choice the day the site was announced, Seaborg explained that they "added up the positive factors of each site and it

seemed that Chicago came out ahead." In a January 18, 1967 report, "Basis for the Selection of the Chicago (Weston) Site for Location of the 200 BeV Laboratory" the AEC defended its choice of the Weston site, emphasizing its accessibility, which would "assure that all available talents can be readily and speedily brought to bear on its design and use even though many contributing scientists may never be actual members of the Laboratory's staff." To avoid emphasizing that the site was not the least expensive of the candidates, the report noted that the cost of the Weston site was "the least of the earth-base sites," which also included Ann Arbor and Brookhaven, "and may be quite comparable with those of the rock-base sites (Denver, Madison and Sierra)." It also noted that they expected "less than 5% differential," between the Weston site and the rock-base sites, including Denver, Madison, and the California site, "which is well within the range of accuracy of the estimates at this stage." The Commission downplayed civil-rights problems in Illinois, acknowledging "differing views" about the status of housing nondiscrimination and integration in public schools in the communities surrounding Weston. On a positive note, the Commission noted that it had "obtained assurances of nondiscrimination and equal opportunity from local government entities, labor unions, business, and industry, lending institutions, professional groups," in addition to assurances that great efforts would be made to "prevent or offset discrimination . . . ." (84)

As the Commission noted the 1967 JACE hearings when answering Javit's questions, all cost savings to be gained by use of the

Brookhaven site, including the use of the AGS as an injector, were "to a large extent offset by the higher construction costs prevailing in the BNL area" as compared with other sites. "Furthermore," the Commission added, "use of the AGS as an injector would lead to a significantly different and decreased national high energy physics program than would prevail with two separate machines." (85)

In recent interviews, Commissioners Seaborg, Tape, Ramey, and Nabrit confirm the general outlines of these official justifications. Commissioners remember that they were concerned about Illinois civil-rights problems, but ultimately decided that Weston's advantages more than offset this disadvantage. Although cost comparisons were taken into account, the Commissioners did not seriously consider the use of the AGS as an injector as a factor in site selection. In the end, accessibility was considered the most crucial characteristic, and Weston's easy accessibility clinched the decision for the Illinois site. Although the Commissioners acknowledge that, because of previous Midwestern tensions, they were pleased to find that a Midwestern site was superior, they all insist that they would not have chosen a Midwestern site unless they sincerely believed it possessed superior qualities. (86)

Long before the winning site was announced, some doubted that the Commission would make the final decision. In November 1965, more than a year before the decision was made, UCLA Vice Chancellor Carl York predicted that Johnson would determine the siting decision. Aware that this skepticism was in the air, Seaborg began denying such accusations

the day the decision was announced. As his diary entry for December 16, 1967 records, in his conversation with JCAE member Holifield the Chairman stated "definitely" that the Commissioners "were not influenced by the White House." Seaborg continues to insist that the Commission chose Weston based on the site criteria and received no pressure or interference in final site selection from the Johnson or anyone else. This assertion was recently confirmed in interviews with Tape, James Ramey, and Samuel Nabrit, the other surviving Commissioners who served during this period. (87)

Despite continued assertions, many feel that the Commission did not make the decision. A few months after the decision was announced, Newsweek repeated the more specific rumor that "Senate GOP Leader Everett Dirksen . . . might be brought around" to support a fair-housing bill pushed by Johnson "by the prospect of a $375 million nuclear accelerator to be built back home in Illinois." When the bill came to a vote the next year, Dirksen indeed switched swiftly from vehement opposition to support of the bill, which was passed by Congress in April, 1968. In Poliscide, a 1976 treatise decrying the acquisition of land for the accelerator, Theodore J. Lowi and Benjamin Ginsberg reported that Illinois Senator Paul Douglas noted in an interview that he "got a solid promise with something of an escape clause" that the accelerator would be located in Illinois." The idea that Johnson was willing to promote a Midwestern site was particularly believable in light of his well-publicized January 1964 letter to Hubert Humphrey after the defeat of MURA, which stated his "strong desire to support the

development of centers of scientific strength in the Midwest . . . ." (See Chapter 4) (88)

With this evidence, the suggestion that the final site was chosen by Johnson as part of a political deal proved extremely persuasive within the physics community. Most physicists accept the interpretation that Johnson made a political deal with Dirksen, and many are vehement in their belief. This view has been reinforced by statements made by government officials. Lowi and Ginsburg quote an anonymous "respectable source in the Johnson administration" who says "he is certain that the president told the AEC . . . to make sure the facility ended up within 50 miles of Chicago." In Presidential Management of Science and Technology: The Johnson Presidency, a perceptive 1985 account of executive branch decisionmaking, Henry Lambright repeated the assertion that Johnson made the decision, quoting David Robinson, then with the Office of Science and Technology. (89)

The strongest piece of evidence supporting the idea that Johnson made the decision is the testimony of Robinson, who presumably had no motive for lying and was in a position to observe Johnson's involvement in the decisionmaking process. However, Robinson could have been misled. For example, as Tape has suggested, it would have been easy to interpret AEC efforts to keep the White House informed as evidence that the President made the final decision, especially since Johnson did intervene in the decision of which sites would go to the NAS Site Selection Committee in 1965. (90)

Douglas' statement does not provide convincing evidence that Johnson made the decision. As the Senator admits, Johnson did not make an absolute commitment. In addition, by the the time the decision was made, Douglas had lost his Senate seat. Johnson may well have dangled an open-ended promise that an Illinois site would be chosen in order to extract a political favor from Douglas. This does not prove, however, that Johnson fulfilled his promise, or ever intended to do so. It seems particularly unlikely that the President would had felt compelled to honor such a promise after Douglas lost power. (91)

Similarly, Dirksen's sudden change of heart on the fair-housing bill fails to provide convincing evidence of presidential intervention in the siting decision. Although the shift in alliance strongly suggests that the Senator made a political deal with Johnson, this does not necessarily mean that Johnson used site selection as a bargaining tool. Even if he did, this does not prove that Johnson actually fulfilled his part of the bargain.

It is also easy to see how the Douglas-Johnson and Dirksen-Johnson stories gained currency. Considering the deep disappointment felt by losers and the fact that the final choice was responsive to previously expressed Midwest demands, it is not surprising that the Commission was accused of making the choice because of political pressures. Since the prediction that Johnson would make the decision was already in the air, it is also not surprising that Dirksen's sudden change of heart was interpreted as evidence that Johnson had, in fact, made the decision. This story, like the

Douglas-Johnson story, was all the easier to accept because of the natural tendency to attribute important decisions to powerful leaders. (92)

Although these factors help explain why people believe that Johnson made the decision, including the Dirksen-Johnson variant of the story, they do not explain the vehement support for the Dirksen-Johnson story and its wide acceptance within the physics community. This strength of conviction suggests that this particular story holds special appeal for physicists. The story does seem to offer an interpretation that would have been believable, attractive, and useful to physicists, considering the pressures of the time.

Because of the scope of the 200 BeV project, the physics community was faced with a number of changes prompted by inside forces, including reassessment of the rights of outside users and the prerogatives of accelerator builders. At the same time, physicists faced changes prompted by outside forces, most notably, a loss of control in project planning due to the increased public and political attention attracted by the expense of the project. The most vivid display of this change came with the site selection process. Although the nationwide site selection contest grew from the pressure to protect the rights of outside users within the physics community, with more than $200 million at stake, it soon became an important contest for citizen groups and politicians. It was easy for physicists to believe that the final site was determined by a political deal because they tended to see the entire site selection process as a vulgar, public display that was beyond their

control, and therefore vulnerable to corruption. In addition, the Dirksen-Johnson story, set against the backdrop of this view, presented an interpretation of the final site selection that was both attractive and useful. If site selection was no more than a public spectacle settled by a political deal between two powerful politicians, then physicists could ignore the role of internal forces in forging the uncomfortable new arrangement and prevent internal blood-letting. Moreover, community-wide acceptance of this interpretation could help unite physicists at a time when unity was desirable, both to bolster stability within the physics community and to help secure funding prospects.

Although the idea that Johnson made the decision is both compelling and believable, the written record, from official AEC files and Johnson's Presidential files, as well Seaborg's records, substantiates the claim made by Commissioners that they seriously considered the available site information and made the decision themselves. These records trace a coherent sequence of information gathering by AEC staff members, followed by decisionmaking meetings by the Commission. When questioned about this sequence, each Commissioner provided details consistent with the written record and with the testimony of other Commissioners. In addition, the decision itself seems reasonable, considering the information available to Commissioners at the time of the decision. The record does show that Johnson was informed of the status of decisionmaking efforts from the onset and was given the opportunity to intervene two times, just before the sites were submitted

to the NAS in the summer of 1965 and just before the final site was announced in December 1966. Although Johnson clearly intervened in 1965, there is no evidence that he did so in 1966.

Other evidence argues against Johnson's involvement. Those who have studied Johnson's presidency agree that by late 1966 Johnson was terribly preoccupied by Vietnam. As biographer Doris Kearns points out, "as the months went by, consumed more and more by the war in Vietnam, Johnson saw the heads of his domestic agencies less and less. Gradually, an atmosphere developed in which the domestic Cabinet members stopped asking for private meetings, assuming the President was too busy to concern himself with their business, as if their business were no longer his." (93)

No evidence exists that Dirksen was deeply involved in this issue. Ramey insists that the Senator showed relatively little interest in the 200 BeV, and Wilson remembers that when he was introduced to Dirksen in 1966 the Senator rudely noted: "I don't care whether we have that project . . . or not." In line with this lack of enthusiasm, the March 1966 record of congressional interest complied by the AEC staff listed Dirksen, but did not judge him as having a "strong" interest in the project. Douglas also did not receive such a rating. It is possible that the Commissioners are mistaken and Johnson indeed chose Weston, prompted by a deal with Dirksen, Douglas, or some other political consideration, without leaving a traceable written record, or that the written record was destroyed. However, in light of the evidence, such a scenario seems unlikely. (94)

On December 19, amid the clamor following the site announcement, the URA and the AEC met to discuss further arrangements for the new laboratory. The first topic on the minds of URA representatives was the choice of a leader for the new laboratory. As noted in an AEC summary of the meeting, URA representatives announced that they had decided to ask Lofgren to be Director of URA Design Studies. Although they contacted Wolfgang Panofsky, current SLAC director, he had expressed no interest in the directorship. Even though the AEC did not announce its formal approval of the offer to Lofgren at this point, Tape remarked that Lofgren was "the outstanding candidate," noting that he "would influence the attitudes of those at Berkeley who had performed the original design study and would enable AEC to capitalize on what they had done." (95)

As indicated in the meeting notes, Commissioners were particularly concerned about the upcoming congressional hearings which would determine JCAE support for the first large allotment of funding for the project. Because of the tensions within the physics community and the expected "thorough questioning" at the hearings, Seaborg spoke apprehensively of "pockets of resistance to the choice of the Weston site," and URA representatives pledged to help prevent opposition within "the educational and scientific community." When questioned about reduced scope, Seaborg announced that the AEC had reluctantly decided to request about $240 million for the project, sensing that that "it was necessary that the future not be mortgaged too heavily." As other AEC records show, despite Seaborg's valiant arguments against reduced scope in May, his attitude changed as budgetary pressures increased throughout

1966. By the end of the year, Seaborg had concluded that a price limitation was necessary to assure executive branch support for the machine. This assessment was certainly sensible: at that very moment the Johnson administration was preparing an emergency request for $12.3 billion, including $4.467 billion in new appropriations, to meet the unexpected, escalating costs of the Vietnam war, which showed no signs of abating. (96)

In the next few weeks the URA submitted a proposal formally offering to assume "responsibility for the final design and construction of the 200 BEV accelerator and for the development and operation of the laboratory," and received a temporary contract from the AEC. During this period the URA also offered the job of design director to Lofgren. However, in a January 12, 1967 letter, Lofgren declined the position. Lofgren explained that he was dissatisfied with the description of his duties, feeling that "the directorial position should not be limited to responsibility for design and construction of the accelerator facility." In addition, he felt unsure of the Weston site and unconvinced that he could assemble the necessary staff "and develop an organization having the enthusiasm and spirit needed to make the project a distinguished success." After some deliberation the URA decided to offer Wilson the position of Laboratory Director, this time making it clear that the position included overall responsibility for the laboratory. (97)

Wilson was a strong candidate for a number of reasons. Although he had never built one of the large proton synchrotrons, he was a well-respected physicist who had directed the laboratory at Cornell for

twenty years. In fact, he had just finished building an electron synchrotron at Cornell, which had come in on budget and ahead of schedule. The URA needed a strong, energetic physicist of stature to lead the new laboratory, especially considering the site. Although Weston might eventually appeal to outside users because of its accessibility, the flat farmland held little appeal for the designers who would live there. In addition, Wilson was willing to seriously consider the job, an important point since, as Ramsey explains, URA leaders worried that the project would suffer if another candidate turned them down. Because of budgetary pressures, Wilson's stress on economy was now, ironically, a strong point, although earlier URA leaders had been annoyed by Wilson's economizing schemes, which seemed in 1965 to threaten the course of the project. (98)

Before making the offer official, Ramsey carefully gauged reaction to the decision, both in the AEC and in the physics community. At first, the Commissioners were unenthusiastic, remembering with irritation Wilson's spirited design criticism in 1965, which in their opinion had threatened the future of the project. In particular, the Commissioners questioned whether the physics community, and LRL in particular, would accept Wilson's appointment. Nonetheless, the Commission was eventually persuaded to endorse Wilson, and, as Ramsey remembers, most physicists outside the Berkeley circle favored the appointment. (99)

To no one's surprise, however, many LRL physicists were enraged at the choice. As Judd explained in February, Wilson "was perhaps the

worst" choice for director for "securing the whole-hearted help and cooperation of the LRL team" because of his public attacks on the LRL staff's "judgement and competence. . . ." By this time LRL complaints about the site had also become more vigorous. LRL physicists insisted that the accelerator would be prohibitively expensive to build at Weston, and as Judd reported, none of them "could conceive of moving our lives and families to the dismal Chicago area . . . ." According to Seaborg's diary, JCAE members Holifield and Hosmer, both from California, sided with the unhappy Berkeley physicists. (100)

While the AEC worried about relations with the JCAE on the eve of authorization hearings, the URA faced the uncomfortable task of forming a team that necessarily included the hurt and angry LRL design group, Wilson, and URA leaders. As Ramsey explained in a recent interview, he visited Berkeley in late January to find that the "URA was vigorously criticized for even going along" with the decision to built the accelerator at Weston. As LRL physicist L. Jackson Laslett recently explained, "a substantive vexing issue" was whether the URA could effectively lead the project, in light of budget reductions, "or whether the project would lead to substantial cost overruns before satisfactory completion, with consequent long-term damage" to the national program in high energy physics. Wilson visited the group in early February to discuss LRL criticism of the site, and as he explained in a 1967 tape recording, LRL physicists "were completely and ostentatiously negative and unfriendly." Members of the LRL design team remember being annoyed at Wilson's attitude, feeling that he was unwilling to listen to their

ideas. Despite the mood of his audience, Wilson pluckily insisted that he saw no reason why the machine couldn't be built at Weston and explained some of his ideas. When he later asked who would be willing to accept a job at the new facility, only one or two hands tentatively rose in the sea of angry faces. (101)

In the meantime, on February 8 the Commissioners met with California Representatives Holifield and Hosmer. According to Seaborg's record of the meeting, Holifield remarked that Wilson was the "author of a very questionable letter . . . ." The Commissioners took great pains to explain Wilson's qualifications as a "very imaginative scientist," and stressed his attention to "cost and management details" at Cornell. Although Seaborg mentioned the meeting between Wilson and the LRL designers, optimistically noting that misunderstandings might soon be resolved, Hosmer pointed out that LRL designers would probably not join the project. At the end of the meeting, both Holifield and Hosmer stressed the importance of finding a good team to build the new laboratory. (102)

As it happened, at just this juncture several LRL physicists were on their way to Washington to testify at the authorization hearings. Seizing the opportunity, Seaborg organized a series of meetings to resolve the multiple tensions surrounding Wilson's appointment. At a decisive meeting on February 14, Lofgren came armed with a report prepared by the LRL staff which assessed the estimated additional cost for building at the Weston site. According to LRL notes, an AEC representative noted that the Commission expected only a $5 million cost

difference between the California and Weston sites. Lofgren disagreed, announcing that LRL estimated the project would cost $53 million more at the new site. Ramsey interceded to stress the now-familiar theme 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 based on detailed data from the site. With debate thus temporarily assuaged, the terms of the transition could be negotiated. Wilson entered the meeting and gave his consent to a list of tasks ensuring the continued involvement of the LRL design group in the new design effort. By the end of the meeting a workable, if not amiable, arrangement had been negotiated so that work on the accelerator could proceed. (103)

Later that day, Seaborg, Tape, and Ramsey took Wilson to meet Holifield, Hosmer, and their fellow JCAE member Melvin Price, an Illinois Congressman. By this time, Ramsey had formally offered the directorship to Wilson. During the meeting, the JCAE members quizzed Wilson on his management philosophy. Although Wilson remembers feeling nervous, Tape felt that by the end of the meeting Wilson had "made a hit with them." Seaborg's diary similarly notes that the Congressmen "seemed to like Wilson," adding that they later gave their approval to Wilson's appointment. (104)

In between these negotiations, the AEC faced the JCAE in annual authorization hearings. The accelerator was a top issue. The Joint Committee spent three days discussing the accelerator and directed a subcommittee to deliberate for two more days. The JCAE demanded that the

AEC justify the choice of Weston, the decision to reduce the accelerator's scope, and the role of URA. On the basis of the AEC's defense, the JCAE would decide whether to support funding for the continuation of the project.

Unsurprisingly, both California and New York congressmen grilled the Commission on the Weston site while Midwestern congressmen staunchly defended the choice. As part of this inquiry, Senator Jacob Javits from New York entered 29 questions about the site criteria in the record. He remarked further that Weston was only "a cornfield" while Brookhaven was "a great working, effective laboratory," and questioned the fairness of the site selection. At this point the JCAE Chairman vehemently defended the AEC, noting it was his "unequivocal . . . irrefutable understanding that the White House had nothing at all to do with the selection of the site." (105)

The AEC was also asked to defend the choice of Weston on the basis of other considerations, the most important being cost differentials between the sites and the question of civil rights. One session was spent on a point-by-point analysis of the site selection process, with the AEC constantly emphasizing that decisions had been made on the basis of the site criteria. (See Table 2) When Lofgren and McMillan appeared before the subcommittee, they refrained from mentioning their cost figures. Although neither man expressed enthusiasm about the project, both stressed LRL's willingness to cooperate in the new effort and limited negative comments to vague warnings about the necessity of proceeding with caution with new ideas, such as a scheme for expanding

Table 2.--February 1967 AEC table comparing cost estimates of six finalist sites for the 200 BeV accelerator.*

Site	Rock Sites			Earth Sites		
	Sierra	Denver	Madison	Weston	Ann Arbor	Brookhaven
	(thousands)	(thousands)	(thousands)	(thousands)	(thousands)	(thousands)
Direct construction cost all rock sites	$230,800	$230,800	$230,800	$249,700	$240,700	$240,700
Site related items:						
Earthwork[1]	12,200	11,400	9,200	3,900	3,900	4,000
Soil foundation[2]	0	0	500	(2,500)	32,000	3,500
Climate[3]	0	2,300	5,500	4,400	4,400	4,500
Seismic	2,000	0	0	0	0	0
Cost index[4]	0	(3,000)	0	3,500	4,000	23,300
Most probable direct construction cost	245,000	241,500	246,000	250,000	285,000	278,000
Most probable direct[5] construction cost, including variables excluded from the DUSAF project estimate	246,000	243,500	248,000	251,000	287,000	278,500

[1]Earthwork (which includes excavation and backfill)

[2]Soil-foundation (which includes bearing capacity; magnet foundation and ground water provisions).

[3]Climate (which includes construction scheduling and efficiency; facilities heating; storm drainage; cooling system; wind, snow and frost).

[4]Adjustments for local construction cost variations based on the Dow Dodge index applied to that portion of direct construction costs which are subject to local cost variations.

[5]Variables included are: site access (roads and railroads), water supply, sewer outfall.

*This table was presented for the public record as part of hearings before the Joint Committee on Atomic Energy. Note that the totals in Table 2 approximately correspond to those that would be obtained by assuming "average," "minimum," or "maximum" values (as shown in the last column) of the range given in the "Project Basis Plus Excluded Costs" column. Data from U.S. Congress, Joint Committee on Atomic Energy, Hearings. 90th Congress, First Session (Washington, DC: GPO, 1967), p. 45.

the machine. This restraint testifies both to the success of behind-the-scenes meetings with the AEC and the URA and to Berkeley's innate conservatism. Holding to tradition even in the face of crushing defeat, LRL scientists chose to voice their arguments within the scientific community. (106)

In contrast to the restrained atmosphere of these discussions, emotionalism ran high in civil-rights testimony. For example, Mitchell of the NAACP decried the choice of Weston, claiming that it was an "example of how human dignity and fairplay get short shrift from some top government agencies." Edward Ruthledge from the National Committee Against Discrimination in Housing went farther, demanding that the JCAE "withhold final approval of the Weston site pending specific positive demonstration that Illinois and Chicago will in fact plan, enact, and carry through an across-the-board program to open wide the doors of equal opportunity for minority families." Although the JCAE did not seem enthusiastic about a delay, AEC officials shuddered. If the JCAE endorsed Ruthledge's plan, the 200 BeV project would undoubtedly be deadlocked for some time, since the Illinois legislature had consistently balked at numerous open-housing bills, despite Governor Kerner's efforts to push them through. (107)

In addition to the site issue, considerable attention was given to the proposed reduction in scope and the role of the URA. John Pastore, recently appointed JCAE chairman and Senator from Rhode Island, grilled Seaborg on reduced scope, wanting to know what prompted this change in plans. Although Seaborg tried to sidestep the issue, he eventually

admitted that the impetus was budgetary pressure from BOB, not the consideration that a reduced scope machine would serve as well as the original design. JCAE members were clearly annoyed that BOB had intervened. As Hosmer later remarked: "I am not at all certain we are warranted in authorizing a nickel to proceed on this bitterly disappointing scaled down and shaken poor man's Bev accelerator." (108)

Into this debate stepped JCAE member William Bates of Massachusetts who pointed out that the European research facility CERN had announced plans to build a 300 GeV accelerator and wondered why they were "settling on a second-rate . . . machine when the European countries [were] planning a first-class facility." Why not think in terms of increased capability? Bates' question gave the AEC the perfect opportunity to tout the idea of building an expandable machine. Although LRL designers had considered ideas for expandibility since 1964 and were about to publish a well-honed expandability scheme, the idea now became associated with Wilson, since the Commissioners were stressing his willingness to apply innovative ideas within a budget. Discussion of the expandable machine gave the AEC a way to bridge the gap between JCAE expectations and BOB restrictions in the hearings. With an expandable machine, the JCAE could feel they were getting the accelerator they originally promoted and the AEC could still keep within the budget set by BOB. (109)

The introduction of the expandable machine idea to the JCAE, and its association with Wilson, had several consequences. For one thing, JCAE support of the new idea in the hearings allowed the idea to be

officially incorporated into plans for the project, despite the fact that such a midstream change was usually forbidden. In addition, its association with Wilson helped bolster the position of the URA. During the hearings, the JCAE members reiterated their skepticism about the need for the organization. The most pointed questions came from California congressmen who were still disgruntled that LRL had lost the machine. Although resigned to the fact that the management and siting issues could not be reconsidered, the Californians still needed soothing. The attractive combined offer of an expandable machine and an energetic, economy-conscious director provided a much needed balm. Since anxiety over leadership of the new facility underlaid much of the opposition to the URA, this soothing also worked to the benefit of the URA, which emerged from the hearings relatively unscathed. (110)

The association of Wilson with the important concept of an expandable machine also increased the AEC's desire to work with the URA to recruit Wilson, who had not yet formally accepted the directorship. Wilson was in a strong bargaining position, and he knew it. Before accepting the directorship, he obtained a salary that required the maximum compensation available from the AEC plus a yearly contribution from the URA. In addition, he insisted that the AEC agree to a list of concessions: the laboratory would have a "long range plan" to assure "excellence of the architecture," approval authority would be given to a local AEC office to avoid red tape, and the AEC would give its pledge to support construction authorization in 1969. When Wilson accepted the directorship on March 1, 1967, he was full of ideas and enthusiasm for

establishing an attractive, first-class, research institution at the greatest possible speed. As everyone waited for the congressional verdict on funds for the 200 BeV, Wilson vigorously planned for the new laboratory, powered by the hope that the project would receive its first large funding allotment. (111)

In April 1967, the JCAE subcommittee passed its judgment on plans for the 200 BeV. The subcommittee report applauded the idea of an expandable machine, denounced reduced scope, and rather reluctantly accepted the continued existence of URA, which still had only a temporary contract. Although the Joint Committee insisted that outside users already had reasonable access to other AEC accelerators, they bowed to the desire to ensure access "more as a matter of right than sufferance." In June, the full Committee made its funding recommendation to Congress. The majority report suggested that $7 million of the $10 million requested be authorized for the new project. However, JCAE Chairman John Pastore submitted a dissenting opinion which suggested that the project be deferred. Pastore questioned whether the 200 BeV was really the AEC's top choice machine, presumably because of the heated debate within the physics community, and insisted that at a time when the budget was swollen with high priority items, there was "no urgent, compelling public need demanding an immediate beginning on the construction of the 200 BeV accelerator." Pastore's most vigorous complaint, however, was that of "the six finalists among the considered sites, the AEC chose a location in the only State of the six which does not have open housing legislation." Pointing out that open housing

legislation had been defeated in the Illinois Senate just days after money was appropriated for the 200 BeV project, Pastore angrily insisted: "In the name of advancing science and technology, we should not be guilty of retreating from our boasted principles of equity, equality, humanity." (112)

After reaching the Senate, the funding bill was vigorously debated. In the midst of this debate, a New York Times editorial angrily articulated the mood of many Americans in 1967, the year that race riots erupted in Newark and Detroit and anti-war protesters marched on the Pentagon. "The nation is engaged in a bloody war in Vietnam; the streets of its cities are swept by riots born of anger over racial and economic inequities," the editorial cried. "It is a distortion of national priorities to commit many millions now to this interesting but unnecessary scientific luxury." Such skepticism about the relative social value of science was destined to grow. Despite complaints, however, Congress eventually passed the funding bill over Pastore's objections. On July 26, 1967, Johnson signed the bill and the first large funding allotment for the 200 BeV was secured. (113)

The 200 BeV project was alive, but in precarious health. The URA had been officially granted the right to begin managing the project, but was given no promise for the future. Wilson had been given just enough design funds to squeak by for the next year, and he had to build a full scope accelerator for just $250 million. In light of the budgetary and social pressures of the time, future funding battles seemed certain.

Although many challenges remained, it was a time of great rejoicing, for the physics community had its accelerator.

Summary: Final Siting and Funding Arrangements and Selection of the First Director

The AEC faced many battles before siting and funding arrangements could be concluded. As the Commission struggled to gather additional site information, the contest attracted considerable attention from politicians and citizen groups. Although the Commission had hoped the site might be chosen as early as July, by summer Clarence Mitchell of the NAACP complained of the lack of open housing legislation in Illinois, prompting new investigations into the status of civil rights in all states containing finalist sites. While site selection ground on, budgetary pressures increased along with escalating Vietnam costs. By late 1966, as the Commission prepared to announce the finalist site, Seaborg reluctantly gave in to BOB pressure for a $250 million budget.

On December 16, 1966, the AEC announced that the accelerator would be built in Weston, Illinois. The Commission was immediately besieged with opposition. LRL physicists refused to endorse the site and Mitchell decried the choice because of the lack of fair housing in Illinois. Ironically, in light of NAACP complaints, others claimed that the site was chosen as a result of a political deal President Johnson made with Senator Everett Dirksen, who suddenly changed from vehement opposition to support of an open-housing bill. Although such accusations continue, close inspection of the written record and other evidence suggests that

Commissioners sincerely considered the official site criteria and chose the site themselves. While Commissioners were undoubtedly pleased to choose an Illinois site because of previous Midwestern complaints, the decision apparently hinged on the site's accessibility to outside users.

As soon as the site was chosen, URA leaders negotiated a temporary contract with the AEC and chose leaders for the new laboratory. URA's task was complicated when Edward Lofgren refused to direct the new design team after the Weston site was chosen. When URA asked Robert Wilson to assume the directorship, LRL fury reached a peak. Still angry at Wilson's 1965 criticism, LRL physicists felt initially that they could not work with Wilson. In addition, they were convinced that the accelerator would be prohibitively expensive to built at Weston. Chester Holifield and Craig Hosmer, the two California JCAE members, took up the cause of the despairing LRL group, creating further problems for the AEC on the eve of authorization hearings.

Despite these many difficulties, the 200 BeV project survived intact. Although they retained serious doubts about the site, LRL physicists agreed to continue with the project, reserving further judgment until an updated design and thorough cost estimate could be obtained. In the meantime, the AEC presented arguments at authorization hearings for the choice of Weston, reduced scope, and the URA. While JCAE members accepted Weston and the URA without too much difficulty, the decision to request a reduced scope machine, which was prompted by BOB pressure, upset them. By promoting the idea of an expandable machine and stressing Wilson's willingness to stay within the budget,

however, the AEC was able to bridge the gap between JCAE expectations and BOB restrictions. As the funding bill was debated in Congress, the project suffered continued complaints about the status of civil rights in Illinois and the expense of the project in the midst of urban poverty and the Vietnam war. Nonetheless, the funding bill passed through Congress and was signed by Johnson in the summer of 1967. By this time, Wilson was already hard at work on plans for the new laboratory.

Conclusion: Siting and Funding the Accelerator--1965-1967

From 1965 to 1967, tradition-breaking management and siting plans for the new accelerator were completed. During this period, the new management organization, the Universities Research Association, Inc., evolved into a functioning organization ready to oversee management of the new laboratory. At the same time, the AEC held a site selection contest and chose a site.

As the the AEC and the NAS Site Selection Committee made siting and funding arrangements for the new accelerator from 1965 to 1967, pressures prompted by the expensive project intensified and the economic and social climate shifted. Outside the physics community, political pressure for the new accelerator reached new heights. While the $148 million MURA proposal attracted considerable public and political attention, causing headaches for the AEC, site selection for the more expensive 200 BeV attracted even more attention, prompting complications for the AEC and a threatening loss of control for physicists. At the same time, economic pressure worsened in the wake of the escalating

costs of the Vietnam war. This pressure, along with emerging questions about the social value of basic research, created an economic and social climate much less favorable to accelerator funding than that enjoyed by physicists in the previous decade. Funding difficulties for the new accelerator were compounded by tensions within the physics community, where divisiveness spurred by the MURA defeat was exacerbated by the site competition and disagreements about the 1965 Berkeley design.

To offset these difficulties, the project enjoyed a number of benefits, which drew largely from the considerable capital of scientific interest and prestige accrued by high energy physics research. The accomplishments of the field were praised directly in promotion efforts by the AEC and the physics community. In addition, the prestige and interest inspired by the subfield bolstered the project's prospects in more subtle ways. The project benefited from URA efforts to quell site and design disagreements within the physics community, efforts that relied heavily on the often-repeated admonition that without unanimous support the community would not get the machine, an admonition that only worked because large numbers of physicists strongly wanted the new research tool. The project also benefited from the strong advocacy of Seaborg, whose support grew largely from a commitment to sponsoring basic research, support which was undoubtedly intensified due to the high prestige of the field. The great public interest in the site selection contest also stemmed from public acceptance of the main tenets of those promoting high energy physics: the prestige of the field and the promise that practical benefit would accompany further investment in

accelerators. Although the fervor of the battle complicated the task of the Commissioners and increased tension within the physics community, it also enhanced funding prospects by widening political support for the project and turning attention from the issue of whether the project should be funded to the issue of where it should be sited.

Aided by the accrued capital of prestige and interest in high energy physics, in 1967 the AEC was able to secure the project's first large funding allocation, an allocation which signaled a federal commitment to complete the project. As this success proved, the US government was willing to continue the postwar tradition of building ever-larger accelerators, even though the economic and social climate had become much less favorable for science funding.

As funding was secured, the groundwork was laid for an accelerator laboratory quite different from the major accelerator laboratories of the previous decade. The pressures of the 1960s had forged an organization poised to build a novel laboratory along the lines of the "Truly National Laboratory" described by Leon Lederman in 1963. As Lederman had proposed in 1963, the laboratory director reported to a governing body with national representation, a governing body committed to ensuring the rights of outside users. In addition, as Lederman suggested in 1963, and as subsequent political pressures dictated, those choosing the site gave preference to facilitating outside user participation, rather than accomodating the traditional prerogative of accelerator designers to build an accelerator at the site of their choice.

The formation of the URA, the site selection contest, and the decision to build the accelerator in Illinois rather than at a site sponsored by Berkeley or Brookhaven, redefined the methods developed in the 1950s for planning and organizing major accelerator laboratories. Instead of building an accelerator project at Berkeley with the hopes of building another later at Brookhaven, an arrangement which would have reinforced patterns for outside user participation formed when the US supported multiple major accelerators, plans were laid for a single, centrally located major accelerator laboratory organized to serve outside users.

Besides laying the groundwork for the organization of the new laboratory, the pressures of the planning years also influenced the development of the new laboratory in other ways. The most important influence came with the choice of Robert Wilson as the laboratory's first director. In light of the tightening budget and the commitment to build an expandable machine for $250 million, in 1967 the URA chose, and the AEC supported, the budget-conscious Wilson as the laboratory's first director after Lofgren's refusal to continue with the project when the Weston site was chosen. Thus, Wilson, with his reputation for risk-taking and cost-cutting, was given the responsibility of establishing the new laboratory and directing the design and construction of the accelerator, tasks that would be complicated both by the legacy of past problems and by increasing social and economic pressures.

Notes for Chapter 5

All interviews are located in Fermi National Accelerator Laboratory History Collection, Batavia, Illinois, unless otherwise noted. The following abbreviations are used: Bancroft Library, (Unversity of California, Berkeley, Calfornia); Blewett Papers, (files of John Blewett, New York City, New York); BNL (Brookhaven National Laboratory, Upton, Long Island); DOE Archives, (United States Department of Energy Archives, Germantown, Maryland); FHC, (Fermilab History Collection, Batavia, Illinois); Green Papers, (files G. Kenneth Green, Brookhaven National Laboratory, Upton, New York); LBL, (Lawrence Berkeley Laboratory, Berkeley, California); LBL Archives, (Lawrence Berkeley Laboratory Archives); Lofgren Papers, (files of Edward J. Lofgren, at LBL); LBJ Library, (Lyndon Baines Johnson Library Archives, Austin, Texas); 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); Wenzel Papers, (files of William Wenzel, LBL).

1. Arthur Roberts, "The 200 BeV Accelerator," unpublished song, FHC.

2. Quotes, respectively, from James E. Katz, Presidential Politics and Science, (New York: Praeger, 1978), p. 38; Thaddeus J. Trenn, America's Golden Bough: The Science Advisory Intertwist, (Cambridge: Oelschlager, Gunn and Hain, 1983), p. 51; and as quoted in W. Henry Lambright, Presidential Management of Science and Technology: The Johnson Presidency, (Austin: University of Texas Press, 1985), p. 185. Also see Katz, pp. 34-35 and 45.

3. Quotes from Luke C. L. Yuan, ed., Nature of Matter: Purposes of High Energy Physics, (Springfield: Clearinghouse for Federal Scientific and Technical Information, 1965), p. 1.

4. Quotes from J. Robert Oppenheimer, "Foreword," in Luke C. L. Yuan, Nature of Matter: Purposes of High Energy Physics, (Springfield: Clearinghouse for Federal Scientific and Technical Information, 1965), p. 5.

5. Borrowing a term from Buddhism, Gell-Mann labelled SU(3) "The Eightfold Way" since many strongly interacting particles are grouped into sets of eight. As James S. Trefil explains, SU(3) is "a technical mathematical term describing the properties" of such groupings. Trefil notes that SU(3) "bears the same logical relation to elementary particles as the periodic table does to chemical elements." As with the periodic table, scientists did not understand at first why the classification device worked, but found it useful in explaining and

predicting results. The development of SU(3) led to the idea that particles are composed of quarks. James S. Trefil, From Atoms to Quarks: An Introduction to the Strange World of Particle Physics, (New York: Charles Schribner's Sons, 1980), p. 134. For a good detailed explanation of the development of SU(3) and the concept of quarks from the point of view of a theoretical physicist, see Abraham Pais, Inward Bound: Of Matter and Forces in the Physical World, (New York: Oxford University Press, 1986) pp. 552-569. For a fascinating explanation of SU(3) and the development of quark theory from a sociological point of view, see Andrew Pickering, Constructing Quarks: A Sociological History of Particle Physics, (Chicago: The University of Chicago Press, 1984). Text quotes, respectively, from Julian Schwinger, "The Future of Fundamental Physics," in Yuan ed. p. 23 and p. 51. Also see this source pp. 33, 55, 59, and 75 and Pickering, pp. 57-60.

6. Quotes, respectively, from Atomic Energy Commission, "Policy for National Action in the Field of High Energy Physics," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), pp. 9 and 13. Also see this source pp. 10-19.

7. Quotes from Atomic Energy Commission, "Policy for National Action in the Field of High Energy Physics," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," p. 20. Also see this source pp. 41-42, 49.

8. Quote from Lyndon Johnson letter to Chester Holifield, January 26, 1965, in "Correspondence Relating to National Policy on the High Energy Physics Program," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," pp. 1-2. Also see H. Orlans, Contracting for Atoms, (Washington, D.C.: The Brookings Institution, 1967), p. 225.

9. Seaborg explains that his appointment came after he worked with McGeorge Bundy in The Panel on Basic Research and Graduate Education, a 1959 PSAC panel. At this time Seaborg also got to know Wiesner, who was then a MIT professor and regular PSAC member. The panel's report, "Scientific Progress, the Universities, and the Federal Government," which came to be called the Seaborg Report, strongly argued for increased funding for science education and basic research. Seaborg feels that Bundy and Wiesner persuaded Kennedy to appoint him AEC chairman largely because they were impressed with the Seaborg Report and his performance on the panel. President's Science Advisory Committee, "Scientific Progress, the Universities, and the Federal Government," Washington D.C.: GPO, 1960; and interview with Glenn Seaborg, February 16, 1984. Text quote from interview with Glenn Seaborg, February 16, 1984.

10. Quote from Lambright, p. 60.

11. Quote from U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, (Washington D.C.: GPO, 1965), p. 69. Also see this source pp. 1, 29-40, 43-44, 61-77 and Atomic Energy Commission, "Policy for Natiional Action in the Field of High Energy Physics," in U.S. Congress, Joint Committee on Atomic Energy, "High Energy Physics Program: Report on National Policy and Background Information," (Washington: GPO, 1965), p. 48.

12. U.S. Congress, Joint Committee on Atomic Energy, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, pp. 204-237.

13. U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, pp. 204-237.

14. Quotes from U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, p. 12. Also see this source pp. 5, 6, 11, 23.

15. Quote from U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, p. 377. Also see this source pp. 378, 382-383, 388-389.

16. Quotes, respectively, from U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 89th Congress, First Session, pp. 66, 67, and 391. Also see Proton Accelerator Committee of the University of Colorado, "Advantages of the Boulder-Denver Area for the 200-300 BeV Proton Accelerator," unpublished document, FHC and Glenn Seaborg letter to Frederick Seitz, March 2, 1965, Secretariat, DOE Archives, Box 1425.

17. Tape announced at the January 17 meeting of university presidents that the Commission wished "to stay out of any argument with respect to the site selection." W. B. Fowler, "Meeting at National Academy of Sciences, January 17, 1965, Summary of Notes Taken by Theodore P. Wright," April 19, 1965, unpublished document, p. 4, FHC.

18. Glenn Seaborg letter to Frederick Seitz, March 2, 1965, Secretariat, DOE Archives, Box 1425; Hoddeson interview with Frederick Seitz, February 7, 1980; and Leonard L. Bacon, "Minutes of First Meeting of Board of Trustees of Universities Research Association, Inc.," September 16, 1965, unpublished document, Lofgren Papers.

19. Quotes from Glenn Seaborg letter to Frederick Seitz, March 2, 1965, Secretariat, DOE Archives, Box 1425.

20. Quote from Enclosure to Frederick Seitz letter to Glenn Seaborg, April 6, 1965, Secretariat, DOE Archives, Box 1425. Also see Gerald Tape letter to Chester Holifield, April 1, 1976 and Paul McDaniel memorandum to Members of Field Offices and Directors of Divisions, Headquarters, April 13, 1965, Secretariat, DOE Archives, Box 1425; Frederick Seitz letter to Theodore P. Wright, April 13, 1965, Green Papers, Box III; Joseph L. Smith, contract with Frederick Seitz, contract no. AT(49-8)-2783, Secretariat, DOE Archives, Box 1425; and Atomic Energy Commission, "AEC-NAS Enter Agreement on Evaluating Sites for A Proposed New National Accelerator Laboratory," April 28, 1965, press release, Seaborg Papers.

21. Quote from Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, (Lawrence Radiation Laboratory, 1965), UCRL-16000, p. I-1. Also see this source I-3, IV-10-IV-31, and Volume II and private communication, Francis Cole, February 2, 1988.

22. Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, pp. XIII-1, XIII-13-XIII-33; XIV-1-XIV-11; XVIII-1-XVIII-12.

23. Quotes, respectively, from Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, pp. XVI-2 and XVI-3. Also see pp. I-7-I-8; XVI-1, XVI-3, XVI-18, XVI-12, XVII-9, XVI-16.

24. Quotes, respectively, from Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, pp. I-1 and I-5.

25. Quote from Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, p. I-7.

26. As the report explains, extracted beams "make it possible to use secondary particles produced at very small angles in a target; they are more flexible and efficient; they lead to less buildup of radioactivity in the accelerator enclosure." Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, p. I-6. Text quotes, respectively, from this source pp. I-6 and I-7.

27. Luke C. L. Yuan and John P. Blewett, "Experimental Program Requirements for a 300 to 1000-BeV Accelerator," (Brookhaven: Brookhaven National Laboratory, 1961), p. 148; Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, pp. XVI-1 to XVI-3; and Edward Lofgren, private communication, February 16, 1988.

28. Quotes, respectively, from Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, pp. V-7, XVI-1; and interview with Denis Keefe, Glen Lambertson, and Jackson Laslett, December 22, 1986. Also see Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, p. XVI-I; and interview with Francis Cole, August 29, 1985, p. 5.

29. Quotes, respectively, from Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, p. XVI-1; and interview with Denis Keefe, Glen Lambertson, and Jackson Laslett, December 22, 1986.

30. Lawrence Radiation Laboratory, "Construction Project Data Sheet, Schedule 44," Extract from FY1967 Budget Submission, June 1, 1965, Lofgren Papers; Edward Lofgren, "Proposal for the Construction of a 200 BeV Accelerator," Draft, August 10, 1965, Lofgren Papers; Edward Lofgren, "Notes for Briefing for URA Trustees Subcommittee Meeting," March 3, 1966, unpublished document, Lofgren Papers.

31. Quotes from Robert Wilson letter to Edwin McMillan, September 27, 1965, FHC.

32. Quotes from Robert Wilson, "Some Proton Synchrotrons, 100-1000 GeV," September 22, 1965, unpublished manuscript, FHC. Also see Lillian Hoddeson interview with Robert Wilson, May 8, 10, 1978.

33. Quotes from Samuel Devons, "Comments on the High Energy Physics Program in the United States," October 1965, unpublished manuscript, Lofgren Papers.

34. Lillian Hoddeson interview with Robert Wilson, May 8, 10, 1978; interviews with Edward Lofgren, April 24, 1984, and John Blewett, November 17, 1986; and Lillian Hoddeson interview with Norman Ramsey, February 26 and 27, 1980.

35. Committee members were Robert Bacher from Caltech, Harvey Brooks from Harvard, Val Fitch from Princeton, William Fretter from the University of California, Berkeley, William Fry, University of Wisconsin, Madison, John William Gardner, Carnegie Corporation, Edwin Goldwasser, University of Illinois, G. Kenneth Green, Brookhaven, Crawford Greenewalt, du Pont, Herbert Longenecker from Tulane University, Emanuel Piore from IBM and Kenneth Reed from the NAS. Gardner later withdrew from the Committee when he was appointed Secretary of the Department of Health, Education and Welfare. Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, unpublished document, FHC. Also see "Site Selection Committee, Advisory to the Atomic Energy Commission," June 5, 6, 1965, unpublished document, Green Papers, Box V.

36. The AEC Task Group rated proposals according to a point system. As Hughes explained: "In scoring a total of 1000 points for these criteria, 300 points are applied to the criteria under the main heading for land, 250 points are applied to the criteria under the main heading of utilities, and 450 points are applied to the criteria under the main heading for environment." He further explains, "The distribution of points between these three headings was made by first dividing the total of 1000 points between the economic aspects (495) and the intangibles associated with environment (450) and land (55)." W. E.

Hughes memorandum to Paul W. Mcdaniel, June 8, 1965, Secretariat, DOE Archives, Box 1425. Text quotes also from this source.

37. Atomic Energy Commission, "Wide Distribution Shown in AEC List of Proposals for 200 BeV Accelerator," July 9, 1965, press release, Seaborg Papers; interviews with Francis Cole, July 13, 1984, John Blewett, November 17, 1986, and James T. Ramey, November 21, 1986; Paul W. McDaniel letter to Maurice Goldhaber, May 2, 1965, Green Papers, Box III; Theodore Wright letter to Paul McDaniel, June 10, 1965, Green Papers, Box III; and James T. Ramey memorandum to Glenn Seaborg, John Palfrey, Gerald Tape, R. E. Hollingsworth, Spofford English, and Paul McDaniel, July 23, 1965, Seaborg Papers.

38. Clinton Anderson letter to Glenn Seaborg, July 15, 1965, Seaborg Papers; Frederick Seitz letter to Clinton Anderson, July 22, 1965, Seaborg Papers; and Glenn Seaborg letter to Clinton Anderson, July 24, 1965, Seaborg Papers.

39. Quotes, respectively, from Glenn Seaborg, Diary TS, July 28, 1965 and Glenn Seaborg letter to Lyndon Baines Johnson, August 28, 1965, Seaborg Papers. Also see Glenn Seaborg letter to Frederick Seitz, August 24, 1965, Seaborg Papers.

40. Quotes, respectively, from Glenn Seaborg, Diary, TS, September 1, 1965 and Glenn Seaborg, record of conversation, September 8, 1965, Seaborg Papers. Also see Horace Busby memorandum to Marvin Watson, September 8, 1965, Box Ex FG 262, White House Central Files, LBJ Library; and Lambright, pp. 95, 124.

41. Quotes from Joseph Califano memorandum to Lyndon Johnson, September 10, 1965, Ex FG 202 Box 262, White House Central Files, LBJ Library. Also see Dwight A. Ink letter to William Moyers, September 8, 1965, Ex FG 202 Box 262, White House Central Files, LBJ Library; Glenn Seaborg, Diary, TS, September 13, 1965, Seaborg Papers; Glenn Seaborg letter to Frederick Seitz, September 13, 1965, Seaborg Papers; Atomic Energy Commission, "AEC Asks NAS to Evaluate 85 Site Proposals for 200-BeV Accelerator," September 15, 1965, press release, Seaborg Papers; Lambright, p. 13.

42. Quotes as quoted in Duncan Clark letter to Hayes Redmon, September 17, 1965, EX/FG/RS/PR/8 Box 6, White House Central Files, LBJ Library Also see Glenn Seaborg, Diary, TS, September 15, 1965; and Donald Hornig letter to Joseph Califano, October 19, 1965, FG 202, White House Central Files.

43. Quotes from Carl M. York memorandum to Professors David Saxon and Harold Ticho, November 8, 1965, Lofgren Papers.

44. Interview with Glenn Seaborg, December 16, 1984.

45. Quotes, respectively, from Joseph Califano memorandum to Lyndon Johnson, October 22, 1965, Ex FG 202 Box 262, White House Central Files; and Joseph Califano memorandum for the record, October 25, 1965, Ex FG 202 Box 262, White House Central Files, LBJ Library. Also see Glenn Seaborg letter to Lyndon Johnson, October 12, 1965, Agency Report, Confidential File; and John V. Vinciguerra letter to Marvin Watson, November 4, 1965, FG 202, White House Central Files, LBJ Library.

46. Glenn Seaborg letter to Lyndon Johnson, October 12, 1965, Agency Report, Confidential File, LBJ Library; Donald Hornig letter to Joseph Califano, October 19, 1965, FG 202, White House Central Files; and "Procedures to be Used In Arranging Site Visits," no date, unpublished document, Green Papers, Box V; and interviews with Gerald Tape, November 21, 1986 and James Ramey, November 21, 1986.

47. The Foundation Requirements Panel included Ernest Courant from BNL, Ernest Dobrovolny from the U.S. Geological Survey, Frederick Mills from MURA, Jack Oliver from Lamont Geological Observatory, and Ralph Peck from the University of Illinois. The Panel of Accelerator Scientists included John Blewett from BNL, Thomas Collins from CEA, Denis Keefe from LRL, B. J. Krause from Argonne, Aaron Lemonick from Princeton-Penn Accelerator, Edward Lofgren from LRL, John Martin from Oak Ridge, Boyce McDaniel from Cornell, Frederick Mills from MURA, Richard Neal from SLAC, Louis Rosen from Los Alamos, William Salsig from LRL, James Sanford from BNL, Julius Spiro from BNL, and R. E. Taylor from SLAC, Lee Teng from Argonne, William Walker from BNL, and C. Kenneth Green from BNL. Appendices A and B in Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, unpublished document, FHC. Text quotes from Glenn Seaborg letter to Frederick Seitz, September 13, 1965, Seaborg Papers. Also see Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, pp. 41-42, FHC; G. Kenneth Green letter to Frederick Seitz, draft, no date, Green Papers, Box V; and interviews with Gerald Tape, November 21, 1986 and Edwin Goldwasser July 10, 1985.

48. Quote from Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, unpublished document, p. 43, FHC. Also see Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, unpublished document, p. 41, FHC; K. Reed memorandum for the record, "Report of Visit to the Wisconsin Site," September 13, 1965, Green Papers, Box V; C. K. Reed memorandum for the record, "Report of Visit to Chicago Sites," September 13, 1965, Green Papers, Box V; C. K. Reed memorandum for the record, "Report of Visit to Denver Site," September 30, 1965, Green Papers, Box V; Paul McDaniel "Notes on NAS Site Evaluation Committee on November 22, 1965," unpublished document, Seaborg Papers; Emanuel Piore letter to Edward Lofgren, November 1, 1965, Lofgren Papers; Emanuel Piore letter to Frederick Mills, November 1, 1965, Mills Papers; and interview with Edwin Goldwasser July 10, 1985.

49. Quote from Glenn Seaborg, record of conversation, 27 August 1965, Seaborg Papers. Also see Joseph Califano memorandum for Lyndon Johnson, October 22, 1965, Ex FG 202 Box 262, White House Central Files; and Doris Kearns, Lyndon Johnson and the American Dream, (New York: Harper and Row, 1976), p. 296.

50. Although some have suggested that the push for reduced scope resulted, at least in part, from design criticism, Seaborg later told the JCAE that the motivation was strictly budgetary. Melvin Price letter to John
Pastore, April 19, 1967, in U.S. Congress, Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967), p. v. Text reference: Edward J. Lofgren, "On the Costs of an Accelerator With Reduced Initial Capabilities," December 13, 1965, unpublished document, Lofgren Papers; and Edward J. Lofgren letter to Paul McDaniel, December 14, 1965, Lofgren Papers.

51. Quote from Edward J. Lofgren, "On the Costs of an Accelerator With Reduced Initial Capabilities," December 13, 1965, unpublished document, Lofgren Papers. Also see Edward J. Lofgren letter to Paul McDaniel, December 14, 1965, Lofgren Papers; Atomic Energy Commission, "Review of Program Plans for Higher Energy and Higher Intensity in Accelerator Facilities," February 11, 1966, unpublished document, pp. 23-24, FHC; and Lloyd Smith, private communication, February 24, 1988.

52. Quotes from Glenn Seaborg, "Meeting of Board of Trustees, Universities Research Association, Inc.," December 12, 1965, unpublished document, Seaborg Papers.

53. As a November 30, 1965 URAI press release explained, management of the organization would "be in the hands of a Board of Trustees elected at a meeting of the Council of Presidents at the [National] Academy [of Sciences] on November 7." Universities Research Association, November 30 1965, press release, Lofgren Papers. See Appendix 5 for a list of URAI Trustees and Universities, 1965-1966. Text quote: Glenn Seaborg, "Meeting of Board of Trustees, Universities Research Association, Inc.," December 12, 1965, unpublished document, Seaborg Papers. Also see Frederick Seitz letter to Norman Ramsey, December 14, 1965, and Chester Holifield; and "The Proposed 200 BeV Accelerator," remarks on the floor of the House of Representatives, January 25, 1965, unpublished document, Seaborg Collection, Box 138, DOE Archives.

54. Quotes, respectively, from Frederick Seitz letter to Norman Ramsey, December 14, 1965, FHC; Frederick Seitz letter to Norman Ramsey, December 30, 1965, FHC; and Glenn Seaborg, "Meeting of Board of Trustees, Universities Research Association, Inc.," December 12, 1965, unpublished document, Seaborg Papers.

55. Atomic Energy Commission, "Review of Program Plans for Higher Energy and Higher Intensity in Accelerator Facilities," February 11, 1966, unpublished document, p. 2, FHC; Wolfgang Panofsky letter to Paul McDaniel, January 5, 1966, FHC; and Edwin Goldwasser letter to Paul McDaniel, January 18, 1966, FHC.

56. Quotes, respectively, from Lillian Hoddeson interview with Robert Wilson, May 8, 10, 1978 and "Report to Board of Trustees by the Scientific Subcommittee," January 25, 1966, unpublished document, FHC. Also see Atomic Energy Commission, "Review of Program Plans for Higher Energy and Higher Intensity in Accelerator Facilities," February 11, 1966, unpublished document, p. 2, FHC.

57. Quotes, respectively, from Lillian Hoddeson interview with Robert Wilson, May 8, 10, 1978; and "Report to Board of Trustees by the Scientific Subcommittee," January 25, 1966, unpublished document, p. 5, FHC. Also see "Meeting on High Energy Physics, Agenda," January 24, 1966, unpublished document, p. 6, FHC.

58. Wilson was a member of the Board of Trustees when the Board endorsed Smyth's conclusions. He remembers that he again summarized his views for the Board at their January 25 meeting, but finally agreed that there was no widespread support for alternate design schemes. Hoddeson interview with Wilson, May 8, 10, 1978. Text quotes, respectively, from Atomic Energy Commission, "Review of Program Plans for Higher Energy and Higher Intensity in Accelerator Facilities," February 11, 1966, unpublished document, pp. 6, 8, 6, 7, 24, FHC. Also see Henry D. Smyth letter to Glenn Seaborg, January 26, 1966, FHC.

59. Glenn Seaborg letter to Chester Holifield, February 16, 1966, FHC.

60. Quotes, respectively, from Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, unpublished document, p. 7, FHC; and Panel of Accelerator Scientists, "Report of the Panel of Accelerator Scientists," January 25, 1966, unpublished document, p. 1, Mills Papers. Also see Appendix A in Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, unpublished document, FHC; Paul McDaniel "Notes on NAS Site Evaluation Committee on November 22, 1965," unpublished document, Seaborg Papers; and interview with Edwin Goldwasser July 10, 1985.

61. Quote from Paul McDaniel "Notes on NAS Site Evaluation Committee on November 22, 1965," unpublished document, Seaborg Papers. Also see interview with Edwin Goldwasser July 10, 1985.

62. Goldwasser letter to Emanuel Piore October 13, 1965, Green Papers, Box V.

63. Quote from interview with Edwin Goldwasser, July 10, 1986. Also see "Evaluations," unpublished document, Green Papers, Box V.

64. Quotes, respectively, from Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, unpublished document, pp. 7, 23. FHC. Also see this source pp. 23-39 and Appendix A, p. 11.

65. Quote from Seaborg record of conversation, March 29, 1965, Seaborg Papers. Also see Joseph Califano memorandum for Lyndon Johnson, March 14, 1966, White House Central Files, Ex Fg 202, Box 262, LBJ Library; Paul McDaniel memorandum for Glenn Seaborg, John Palfrey, James Ramey, and Gerald Tape, December 2, 1965, Secretariat, Box 1423, DOE Archives; and Atomic Energy Commission, "AEC to Select 200 BeV Accelerator Site from Among Six Locations Recommended by NAS," March 30, 1966, press release, Seaborg Papers.

66. Quote from excerpt of the Congressional record, appended to John Burke memorandum to Glenn Seaborg, James Ramey, Gerald Tape, John Palfrey, Seaborg Collection, Box 138, DOE Archives. Also see Glenn Seaborg, record of conversation, March 31, 1966, Seaborg; and Glenn Seaborg record of conversation, March 31, 1966, Seaborg Papers.

67. Quotes, respectively, from Joseph Califano memorandum to Lyndon Johnson, March 14, 1966, White House Central Files, Ex FG 202, Box 262, LBJ Library; Charles Schultze memorandum to Joseph Califano, April 22, 1966, White House Central Files, Ex FG 202, Box 262, LBJ Library; and Charles Schultze letter to Glenn Seaborg, April 23, 1966, Seaborg Collection, Box 140, DOE Archives.

68. Quotes from Glenn Seaborg letter to Charles Schultze, May 12, 1966, Seaborg Collection, Box 138, DOE Archives.

69. Quote from Glenn Seaborg, record of meeting, June 13, 1966, Seaborg Papers. Also see interviews with Gerald Tape, November 21, 1986 and James Ramey, November 21, 1986.

70. Atomic Energy Commission, "The Atomic Energy Commission During the Administration of Lyndon B. Johnson," Volume I, Part I, Chapter 6, unpublished manuscript, p. 145, Seaborg Papers; Atomic Energy Commission, "Summary Notes of Briefing on Progress Report on 200 BeV Site Analyses," May 10, 1966, unpublished document, Seaborg Papers; "Supplementary Information on the Suitability of the Brookhaven National Laboratory as a Site for the 200 BeV Accelerator, May 4, 1966, unpublished document, Blewett Papers; Glenn Seaborg letter to Edmund Brown, August 4, 1966, Salsig Papers; and Gerald Tape, private communication, February 4, 1988.

71. Quotes, respectively, from Long Island Association of Commerce & Industry, "Action Program for the 200 BeV Accelerator," April 15, 1966, unpublished document, Green Papers, Box III; Illinois Information

Service, May 31, 1966, press release, FHC; Illinois Information Service, April 1, 1966, press release, FHC; California Members of the House to Glenn Seaborg, June 24, 1966, Secretariat, Box 7741, DOE Archives; and as quoted by John Erlewine, "Summary of Proposers' Written and Oral Commitments Re the 200 BeV Accelerator Project," July 25, 1966, unpublished document, Secretariat, Box 7741, DOE Archives. Also see Proton Accelerator Committee of the University of Colorado, "Advantages of the Boulder-Denver Area for the 200-300 BeV Proton Accelerator," unpublished report, FHC; Warren Knowles letter to Paul McDaniel, June 10, 1965, Mills Papers; Frank Hausheer letter to President Frederick Harrington, June 10, 1965, Mills Papers; and Marvin Brickson letter to Frederick Harrington, May 26, 1965, Mills Papers.

72. Anton Jachim explains that concerted efforts to lobby for a Midwest site for the 200 BeV had already begun by the time of the September 1965 Midwest Governors' Conference. Jachim's gives further details of Midwest and Illinois efforts, pp. 117-129. Anton Jachim, Science Policymaking in the United States and the Batavia Accelerator, (Carbondale: Southern Illinois University Press, 1975). Text reference: John Erlewine, "Summary of Proposers' Written and Oral Commitments Re the 200 BeV Accelerator Project," July 25, 1966, unpublished document, Secretariat, Box 7741, DOE Archives.

73. The situation at Weston was complicated by the fact that Weston was a partially completed real estate development project in financial trouble. Some, like William Wohl who wrote to the AEC in April 1966, claimed that villagers simply wanted to sell the land on their part of the site as "a way out of their financial difficulties." Others, like Weston mayor Arthur Theriault, explained that villagers began lobbying for the site in an effort to get "some industry to Weston," thinking they could be "neighbors to the facility." The plight of those opposing the land acquisition is dramatized in Theodore J. Lowi and Benjamin Ginsberg, Poliscide, (New York: MacMillan, 1976), which emerges as a diatribe against eminent domain. Quotes, respectively, from William Wohl letter to the Atomic Energy Commission, April 28, 1966, Seaborg Collection, Box 138, DOE Archives and Arthur Theriault, draft of an untitled speech given February 16, 1969. Also see Karyl Louwenaar, "The Story of the Village of Weston: Notes Taken in Interview with Kenneth Reeling," August 18,1969, unpublished manuscript, FHC. Text quotes, respectively, as quoted in Daniel Kevles, The Physicists: The History of a Scientific Community in Modern America (New York: Alfred A. Knopf, 1978) p. 419; and Thomas B. Husband letter to Lyndon Johnson, May 5, 1966, Seaborg Collection, Box 138, DOE Archives. Also see Spofford English letter to John Conway, June 9, 1966, Seaborg Collection, Box 138, DOE Archives; and Jack McManus letter to Lyndon Johnson, July 5, 1966, Secretariat, Box 7741, DOE Archives.

74. Interview with Gerald Tape, November 21, 1986, and Lillian Hoddeson interview with Wilson, May 12, 1978.

75. Quotes, respectively, from interviews with Gerald Tape November 21, 1986 and Edwin McMillan May 16, 1984. Also see interviews with Edward Lofgren May 3, 1984, David Judd, May 1, 1984, and John Blewett, November 17, 1986.

76. Quote from Glenn Seaborg, record of conversation, July 13, 1966, Seaborg Papers. Also see Henry Traynor memorandum to Glenn Seaborg, James Ramey, Gerald Tape, July 29, 1966, Secretariat, Box 7741, DOE Archives; Glenn Seaborg, Diary, TS, September 15, 1966, Seaborg Papers; and Jachim, p. 127.

77. Quote from Lillian Hoddeson interview with Norman Ramsey, February 26, 27, 1980. Also see Edwin McMillan memorandum to Emilo Segre, January 19, 1965, Lofgren Papers; Universities Research Association, Inc., October 11, 1966, press release, Lofgren Papers; C. M. York memorandum to F. D. Murphy, November 7, 1966, Lofgren Papers; and Norman Ramsey letter to URA Board of Trustees, November 18, 1966, FHC.

78. Glenn Seaborg record of conversation, November 28, 1966, Seaborg Papers; John Erlewine memorandum to Glenn Seaborg, Wilifred Johnson, Samuel Nabrit, James Ramey, and Gerald Tape with enclosure "200 BeV Summary," November 21, 1966, Seaborg Collection, Box 139, DOE Archives; and Harry Traynor memorandum to Glenn Seaborg, Wilifred Johnson, Samuel Nabrit, James Ramey and Gerald Tape, August 31, 1966, Seaborg Collection, Box 131, DOE Archives.

79. John Erlewine memorandum to Glenn Seaborg, Wilifred Johnson, Samuel Nabrit, James Ramey, and Gerald Tape with enclosure "200 BeV Summary," November 21, 1966, Seaborg Collection, Box 139, DOE Archives; and Appendix D1 in Site Evaluation Committee, "The Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, unpublished document, FHC.

80. John Erlewine memorandum to Glenn Seaborg, Wilifred Johnson, Samuel Nabrit, James Ramey, and Gerald Tape with enclosure "200 BeV Summary," November 21, 1966, Seaborg Collection, Box 139, DOE Archives.

81. Quotes from Harry Traynor memorandum to Glenn Seaborg, Wilifred Johnson, Samuel Nabrit, James Ramey and Gerald Tape, August 31, 1966, Seaborg Collection, Box 131, DOE Archives.

82. Seaborg record of conversation, December 7, 1966, Seaborg Papers; Glenn Seaborg, record of conversation, December 15, 1966, Seaborg Papers; Glenn Seaborg, Diary, TS, November 29, 1966, Seaborg Papers; Glenn Seaborg, Diary, TS, December 16, 1966, Seaborg Papers; Glenn Seaborg, record of conversation, December 17, 1966, Seaborg Papers; Glenn Seaborg, record of conversation, December 20, 1966, Seaborg Papers; and Atomic Energy Commission, "AEC Selects Site for 200-BeV Accelerator," December 16, 1966, press release, Seaborg Papers.

83. Quotes, respectively, Clarence Mitchell letter to Glenn Seaborg, December 19, 1966, Seaborg Papers; "Weston's Flawed Windfall," New York Times, December 20, 1967, FHC; Glenn Seaborg, Diary, TS, January 19, 1967, Seaborg Papers; and David Judd letter to Glenn Seaborg, February 10, 1967, McMillan Papers, Box 2. (The author wishes to thank Robert Seidel for providing the February 10, 1967 document.) Also see John Conway letter to R. E. Hollingsworth, March 17, 1967 in U.S. Congress, Joint Committee on Atomic Energy, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967), p. 44.

84. Quotes, respectively, from Glenn Seaborg, Diary, TS, December 16, 1966, Seaborg Papers; and Atomic Energy Commission, "Basis for the Selection of the Chicago (Weston) Site For Location of the 200 BeV Accelerator Laboratory," press release, January 18, 1967, FHC.

85. Quotes from U.S. Congress, Joint Committee on Atomic Energy, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967), p. 47.

86. Interviews with Glenn Seaborg, February 4, 1983 and February 16, 1984; Gerald Tape, November 21, 1986; James Ramey, November 21, 1986; and Samuel Nabrit, October 6, 1987.

87. John Palfrey, the fifth Commissioner serving at the time, is deceased. Text quotes from Glenn Seaborg, Diary, TS, December 16, 1966, Seaborg Papers.

88. Quotes, respectively, from "The President: Something Borrowed," Newsweek, February 27, 1967, p. 28; as quoted in Lowi and Ginsberg, p. 101; and Lyndon Johnson letter to Hubert Humphrey, January 16, 1964, Secretariat, DOE Archives, Box 1424. Also see interviews with Gerald Tape, November 21, 1986, James Ramey, November 21, 1986, and Samuel Nabrit, October 6, 1987; "A Switch in Time," Newsweek, March 11, 1968, p. 38; "It May Be A Bell Tolling For Me," Newsweek, April 22, 1967, p. 23.

89. The view that the decision was made by Johnson as part of a political deal emerged in almost all interviews with physicists, with the exception of Gerald Tape, the one physicist on the Commission. Text quote from Theodore J. Lowi and Benjamin Ginsberg, p. 79. Also see Lambright, p. 62.

90. Interview with Gerald Tape, November 4, 1987.

91. Lowi and Ginsberg, p. 102.

92. Interview with James Ramey, November 21, 1986.

93. Quote from Kearns, pp. 293-294.

94. Quotes, respectively, from Lillian Hoddeson interview with Robert Wilson, January 12, 1978; and Atomic Energy Commission, "Congressional Interest on 200 BeV Site Locations," March 18, 1966, unpublished document, Seaborg Collection, Box 138, DOE Archives. Also see interview with James Ramey, November 21, 1986.

95. Quotes from W. B. McCool, "Summary of Notes of Meeting with Representatives of Universities Research Association," December 19, 1966, unpublished document, Seaborg Papers.

96. Quotes from W. B. McCool, "Summary of Notes of Meeting with Representatives of Universities Research Association," December 19, 1966, unpublished document, Seaborg Papers. Also see Glenn Seaborg Diary, TS, July 20, 1966; Charles Schultze memorandum to Lyndon Johnson, August 10, 1966, White House Central Files, Box FI4, LBJ Library; Glenn Seaborg, record of conversation, December 7, 1966, Seaborg Papers; and Louis Heren, No Hail, No Farewell, (New York: Harper and Row Publishers, 1970), p. 139.

97. Quotes, respectively, from Universities Research Association, "Proposal for Continuing Studies for a 200 BEV Accelerator Facility," December 23, 1966, unpublished document, FHC; and Edward Lofgren letter to Norman Ramsey, January 12, 1967, Lofgren Papers. Also see Norman Ramsey, "Universities Research Association, Inc.: History and Issues," January 17, 1973, unpublished manuscript, FHC; Norman Ramsey letter to Edward Lofgren, December 30, 1966, Lofgren Papers; Universities Research Association, "Board of Trustees, Minutes of Meeting, January 15, 1967," unpublished document, FHC; Lillian Hoddeson interview with Norman Ramsey, February 26 and 27, 1980; and interview with Edwin Goldwasser July 10, 1985.

98. Lillian Hoddeson interview with Norman Ramsey, February 26 and 27, 1980; interview with Edwin Goldwasser, July 10, 1985; and Jaques Cattel Press, ed. American Men and Women of Science, (New York: R. R. Bowker, 1979), p. 661.

99. Glenn Seaborg, Diary, TS, January 16, 1967, Seaborg Papers; and Lillian Hoddeson interview with Norman Ramsey, February 26 and 27, 1980.

100. Quotes from David Judd letter to Glenn Seaborg, February 10, 1967, McMillan Papers, Box 5. Also see Glenn Seaborg, Diary, TS, February 11, 1967, Seaborg Papers.

101. Quotes, respectively, from Lillian Hoddeson interview with Norman Ramsey, February 26 and 27, 1980; L. Jackson Laslett, private communication, February 22, 1988; Robert Wilson, tape recording, "1967 Berkeley Meeting," FHC; and interview with Denis Keefe, Glen Lambertson, and Jackson Laslett, December 22, 1986. Also see Lillian Hoddeson interview Robert Wilson, May 12, 1978.

102. Quotes from Glenn Seaborg, record of meeting, February 8, 1967, Seaborg Papers.

103. Glenn Seaborg, Diary, TS accounts, February 9, 1967, February 11, 1967, and February 14, 1967, Seaborg Papers; D. Keefe, "Report on Meeting Between LRL Personnel and the Atomic Energy Commission," February 14, 1967, unpublished document, McMillan Papers, Box 2; and 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," unpublished manuscript, Salsig Files.

104. Quotes, respectively, from interview with Tape, November 21, 1987 and Glenn Seaborg, Diary, TS, February 14, 1967, Seaborg Papers. Also see Norman Ramsey letter to Robert Wilson, February 6, 1967, FHC; and Robert Wilson, private communication, February 1, 1988.

105. Quote from U.S. Congress, Joint Committee on Atomic Energy, United States Congress, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967), pp. 37 and 71. Also see this source pp. 28-29, 35-77, 78-81.

106. U.S. Congress, Joint Committee on Atomic Energy, United States Congress, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967) pp. 22-394.

107. Quotes, respectively, from U.S. Congress, Joint Committee on Atomic Energy, United States Congress, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967), pp. 97 and 108. For a description of Kerner's efforts, see Jachim, p. 128.

108. Quote from U.S. Congress, Joint Committee on Atomic Energy, United States Congress, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967), p. 93. Also see this source pp. v, 31-32.

109. Lofgren remembers that he first got the idea of designing an expandable machine during a 12-day hospital visit for back problems in March 1964. Although this idea was taken up by LRL designers, no one seriously considered implementing expandability ideas to the 200 BeV design until later, when the AEC requested plans for a reduced scope accelerator in late 1965. Several expandability ideas were proposed, including the idea of initial reduction of the repetition rate for the radiofrequency and magnet systems, which would later be replaced, as mentioned in the February 1966 AEC report. As Lloyd Smith explains, the best-developed Berkeley idea for an expandable machine "was to build a tunnel appropriate for the final design energy but to leave out a substantial fraction of the bending magnets and operate first at a lower energy." Quote from Lloyd Smith, private communication, February 24, 1988. Also see Edward Lofgren, private communication, February 16, 1988; and Al A. Garren, Glen Lambertson, Edward Lofgren, and Lloyd Smith, "Extendible-Energy Synchrotron," Nuclear Instruments and

Methods," 54, 223 (1967), which was submitted on April 14, 1967. (The author wishes to thank Lloyd Smith for providing the 1967 document.) Text quote from U.S. Congress, Joint Committee on Atomic Energy, United States Congress, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967), p. 24. Also see this source pp. 25-28.

110. Norman Ramsey interview with Lillian Hoddeson, 26 and 27 February, 1980; and U.S. Congress, Joint Committee on Atomic Energy, United States Congress, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1967), pp. 280-327.

111. Quote from Robert Wilson letter to Glenn Seaborg, February 28, 1967, FHC. Also see Norman Ramsey letter to Robert Wilson, February 27, 1967, FHC; Robert Wilson letter to Norman Ramsey, March 2, 1967, FHC; and Lillian Hoddeson interview with Norman Ramsey, February 26, 27, 1980.

112. The URA was not granted a permanent contract until January 22, 1968. E. J. Bloch letter to John Pastore, January 28, 1968, FHC. Text quotes from U.S. Congress, Joint Committee on Atomic Energy, Hearings, 90th Congress, First Session, (Washington D.C.: GPO, 1967) p. viii; and from U.S. Congress, Joint Committee on Atomic Energy, "Atomic Energy Commision Authorizing Appropriations, FY 1968," 90th Congress, First Session, (Washington D.C.: GPO, 1967) pp. 57 and 58. Also see this document pp. 36, 59.

113. Quotes from "Vs. Scientific Luxury," New York Times, July 16, 1967, editorial. Also see Mary Beth Norton, David Katzman, Paul Escot, Howard Chudacoff, Thomas Paterson, and William Tuttle, eds. A People and a Nation: A History of the United States, Volume II, (Dallas: Houghton Mifflin Company, 1982), p. 953; Atomic Energy Commission, "The Atomic Energy Commission During the Administration of Lyndon B. Johnson," Volume I, Part I, Chapter 6, unpublished manuscript, p. 146, Seaborg Papers; and Lyndon Johnson letter to Glenn Seaborg, July 26, 1967, Seaborg Collection, Box 170, DOE Archives.

Chapter 6: Establishing the Laboratory--1967-1972

Introduction

When offering the directorship to Robert Wilson, Norman Ramsey made it clear that the URA wanted a strong director. According to Ramsey, the Board of Trustees considered "the Laboratory Director" to be "the key person in the entire undertaking." The tradition of a strong director reached back to the days of Ernest Lawrence and the first large American accelerator laboratory. Although physicists disagreed about other issues when planning the new accelerator laboratory, they consistently agreed that the facility needed a strong director with the authority to dictate the course of the laboratory. For example, provisions were made for a powerful director in the December 1961 meeting held at UCLA to discuss design plans at BNL, LRL, WAG and MURA, as explained in Chapter 3. In addition, as explained in Chapter 4, Leon Lederman spoke of a strong director for his "Truly National Laboratory in 1963, and the 1964 Walker panel specifically urged that the director have "strong powers." The URA was formed in early 1965 to play the role of an overseer, responsible for appointing the director, policymaking, and general efforts to promote the best interests of the machine. Once formed, the organization decided to avoid direct involvement in the administration of the laboratory. As Ramsey explained to the JCAE at the 1967 hearings, URA leaders intended to "work through" the director instead of "spending our time in a Washington office second-guessing him." (1)

As a result of these arrangements, much of the new laboratory was shaped by Wilson's background, personality, and values. Wilson was born in 1914 in Frontier, Wyoming. His parents separated when he was small, and he spent most of his childhood under the care of his father, who trained as a civil engineer, but eventually became a Wyoming Senator. While growing up, Wilson spent periods of time on ranches, learning to ride and throw a lariat by the time he was a teenager. With the help of an uncle, he also learned to use a blacksmith shop to make and repair tools. As he recently noted, through such experiences he gained "confidence that with your own hands you could build large contraptions and make them work." Wilson further explained that local traditions encouraged him to view fighting as manly, helped him to develop a sense of independence, and instilled in him "a kind of reverence for nature, and a desire to identify with it." In addition to such influences, which stemmed primarily from male role models, Wilson remembers the strong influence of his maternal grandmother, who sponsored the relocation of several black families from Kentucky to Wyoming. As Wilson explains, she exposed him to "the tradition of a responsibility to blacks." (2)

After enrolling in the University of California at Berkeley and meeting Lawrence, Wilson began work at the early Radiation Laboratory. Research accomplishment came quickly. After completing his Bachelor of Science degree in 1936, Wilson began studying proton structure with experiments that measured the scattering when a proton beam hits a proton target, a line of research he would continue until interrupted by World War II. As a graduate student Wilson also devised a theory of

cyclotron focusing, which was later expanded by McMillan. Lawrence credited both young men for the resulting focusing improvements, which expedited further cyclotron development in Berkeley. (3)

Wilson received his Ph.D. in 1940 and began working as an instructor at Princeton, collaborating with Enrico Fermi on early measurements of the neutron absorption of uranium, measurements important to the development of the first nuclear reactor and plutonium production for the atomic bomb. When the project to design the bomb began, Wilson moved to Los Alamos, where he first directed the cyclotron group and later served as head of the Experimental Physics Division. During this period Wilson distinguished himself by devising several clever experiments to measure the nuclear properties of uranium and plutonium, including crucial measurements of the delay time of fission neutrons. After the war Wilson accepted a professorship at Harvard, where he designed a cyclotron. In 1947 he went to Cornell to head the Laboratory of Nuclear Studies and directed the design and construction of a series of electron synchrotrons. (4)

While building his reputation as a scientist and accelerator builder, Wilson also gained a reputation for his strong personality. Even as a young scientist, Wilson was willing to defy those with greater experience when it came to promoting a new idea. For example, while still a graduate student, he continued the successful development of a sliding vacuum seal to aid beam measurements necessary for work on cyclotron theory, despite the criticism of the famous British physicist,

Marcus Oliphant, who insisted that such a scheme had already failed at the Cavendish Laboratory. (5)

Wilson was also known for his impulsiveness and his urge to exploit existing circumstances for optimum benefit, traits aptly portrayed in an anecdote well-known to his Los Alamos colleagues. In the midst of the intense time pressure of the bomb project, Wilson became impatient because of a long wait in line for the project's lone barber. When he returned to his office, he impulsively dashed out an order for a barber's chair to express his annoyance. When the chair arrived months later, Wilson did not remember at first why it had been ordered. Before long, however, he recruited a physicist with hair-cutting talent and an informal alternate barber service was indeed instituted. In addition, Wilson soon found an additional use for the chair--when not being used for haircuts, it provided a particularly comfortable spot for Wilson to mull over his many decisions and worries as an administrator. (6)

Due to his impulsiveness and desire to achieve ultimate benefit, Wilson also gained the reputation for taking risks as an accelerator builder. This tendency gained considerable attention in the physics community when Wilson was planning the design of a 1 GeV electron synchrotron at Cornell in the early 1950s. After hearing of the discovery of strong focusing in 1952, Wilson decided to take a chance and redesign the machine. The resulting design incorporated the untried principle of strong focusing and featured magnets much smaller than originally planned. After solving some magnet construction problems, the Cornell machine operated in 1954, becoming the world's first

strong-focusing accelerator. (See Chapter 2) Wilson's reputation for creating daring designs that provided maximum capability for minimal cost was further bolstered in 1965 when he presented an innovative alternate design as part of his vigorous criticism of the Berkeley design. As the varied reactions to Wilson's design ideas illustrate, his ideas and strong personality were capable of inspiring both high admiration and great annoyance. (See Chapter 5) (7)

In addition to his background and strong personality, Wilson brought a distinctive set of values to the new accelerator project. From the beginning, even before his formal acceptance of the directorship, Wilson called the new facility the "National Accelerator Laboratory" (NAL), a name that reflected his idealistic vision of what the laboratory could become. Like Leon Lederman's "Truly National Laboratory," NAL would be committed to serving the needs of the entire community of high energy physicists. As he explained to the NAL staff in the course of construction, to fulfill this committment, the laboratory had to be more than just a machine surrounded by buildings. It also had to be "a state of mind" a place "where discoveries can be made." He went on to explain that such a laboratory required an atmosphere of mutual respect among workers, an atmosphere conducive to adventure, free communication, and creativity. Wilson, who was a sculptor as well as a physicist, put a special emphasis on physical beauty. As Wilson explained to his staff, a beautiful environment was important for the laboratory, not only because such an environment would encourage contentment among workers, but also because "you have to value other

things in order to value research." For Wilson, the urge for beauty underlie the search for scientific truth, and this search had spiritual overtones. Thus, in a 1968 lecture he compared accelerators with cathedrals, explaining that both "were technological constructions of great beauty and significance that expressed the aspirations and spirituality of their age." (8)

In line with this view of the cultural role of accelerators, Wilson felt that the laboratory should be built to enhance its surroundings. In addition to providing a scientific showpiece for the nation, Wilson first dreamed of surrounding the laboratory with an ideal community in which beauty, good will, and physics would thrive, an idea which was inspired by the sense of community at Los Alamos. Although Wilson quickly decided that such ideas were impractical, he continued to have a grand vision of the laboratory which incorporated his humanistic and aesthetic ideals. (9)

In many ways the laboratory which emerged in 1972 reflected the pressures and tensions of the years in which it was planned. The tightening funding environment in 1967 encouraged the URA to choose, and the AEC to support, the budget-conscious Wilson as the laboratory's first director when Edward Lofgren declined the position after the Weston site was chosen. (See Chapter 5) Since the URA made provisions for a strong director, Wilson's background, personality, and values had a strong influence on how NAL was established and how the new accelerator was designed and constructed. Because of continuing economic pressures and his innate enthusiasm for producing maximum capability

within strict budget limitations, Wilson firmly embraced the commitment to build a 200 GeV expandable machine for $243 million. In light of the push for outside user rights in the planning years, Wilson was also obliged to create a "Truly National Laboratory" in which all users were assured fair access to the accelerator. The development of the laboratory was also shaped by tensions that arose during the site selection contest, such as complaints from civil-rights leaders and those forced to vacate land.

When facing the task ahead, Wilson was aided by the URA and by other NAL staff members, including Edwin Goldwasser, whom Wilson recruited as Deputy Director in spring 1967. The new laboratory also had support for continued funding from the AEC and from the JCAE, since both groups had sponsored initial funding and now had a political stake in the success of the project. Support from these traditional champions of accelerator funding, along with support from the physics community, was important in securing the necessary funding allocations to complete the project. (10)

To gain a sense of the laboratory christened in 1972, it is necessary to consider three questions. First, how was the laboratory funded as it was being established, and how did funding allocations affect the laboratory and the physics community? Second, how were the non-technical aspects of the laboratory established: what steps were taken to secure the rights of outside users and build a beautiful laboratory and how did Wilson and Goldwasser react to civil rights and land acquisition problems? Third, what was Wilson's design for the

accelerator and did this design signal a change in the construction of accelerator projects?

Design and Construction Funding

The years 1967 to 1972 were a time of great social unrest in the United States. In 1968 both Robert Kennedy and Martin Luther King, Jr. were assassinated, race riots broke out in 168 cities across the nation, and protest against the Vietnam war escalated. At the close of 1972, the year of the Watergate break-in, the war had not ended, despite Henry Kissinger's declaration in October that "peace is at hand." (11)

During this turbulent period, questioning of the social value of science and technology intensified. In 1969, after the triumph of the first manned space flight to the moon, pollsters found that Americans considered the space race to be of less importance than poverty or air pollution. As concern for the environment grew, the safety of nuclear power was called into question, and the fear rose that radioactive material from reactors would be stolen to make bombs. Often, science was blamed for all technological perversions. Historian Theodore Roszak complained that science had provided "the image of nature that invited the rape [of the environment] and . . . the sensibility that has licensed it." In line with such sentiments, a 1971 poll revealed that only 37% of those questioned chose a "very favorable" rating to describe their confidence in scientists. (12)

Since the AEC and the JCAE were the official overseers of the nation's nuclear energy program, both groups came under increasing fire as public concern about the safety of nuclear power grew. In 1971 a particularly vehement condemnation of the AEC and JCAE came from nuclear power critic H. Peter Metzer, who characterized the Commission as "a fanatically defensive protectionist clique of tenured bureaucrats." Metzer accused the AEC, which had responsibility for the regulation as well as the research and development of nuclear energy, of ignoring radiation safety issues in its zeal to promote the energy source. According to Metzer, the Joint Committee members were also to blame because they had increasingly failed to maintain their proper congressional watchdog role so that the AEC and the JCAE "changed from healthy adversaries into pals. . . ." (13)

Such criticism, which would force the reorganization of AEC responsibilities into two new organizations and the dissolution of the JCAE in the mid-1970s, had considerably eroded the political power of both groups by the late 1960s. Because of their weakened position and public skepticism about the value of science and technology, the JCAE and AEC found it difficult to champion funding for any part of the nuclear energy program. As JCAE member Craig Hosmer noted in a 1968 Physics Today article, "the cozy relationship between the physical sciences and the federal treasury is on the rocks." (14)

Hosmer's prediction proved accurate. 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." High energy physics funding did not escape this trend. As a prominent panel of high energy physicists pointed out in 1968, funding that year was 40% below that predicted in the 1965 AEC report "Policy for National Action in the Field of High Energy Physics." (See Figure 5) As Figure 6 shows, the field continued to be funded at a much lower level than expected. Instead of the regular increases shown in the 1965 report, funding levels for high energy physics from 1970 to 1984 remained relatively constant, with notable decreases in the mid-1970s and early 1980s. (15)

The trend in physics funding made it difficult to secure the expected allocations to fund the design and construction of NAL. As Ramsey explained at a December 1968 meeting, in that year NAL requested $75 million, but only $25 million was included in Johnson's budget. Although the JCAE approved this amount, Congress subsequently cut the allocation to only $7.1 million. After further extensive debate, $14.74 million was eventually allocated. For the next two years the laboratory continued to receive smaller allocations than requested. Although formal approval for the entire budget was obtained in July 1969, as Wilson had requested, in early 1971 he reported to the JCAE that NAL had received only $150 million of their $243 million construction budget. By June 1972, the target date for reaching 200 GeV, Congress had provided only $192.407 million for design and construction. (16)

Big projects often face funding delays, and there was little worry at NAL at this point that funding would be terminated due to the federal investment already made. Nonetheless, Wilson felt that the slow funding

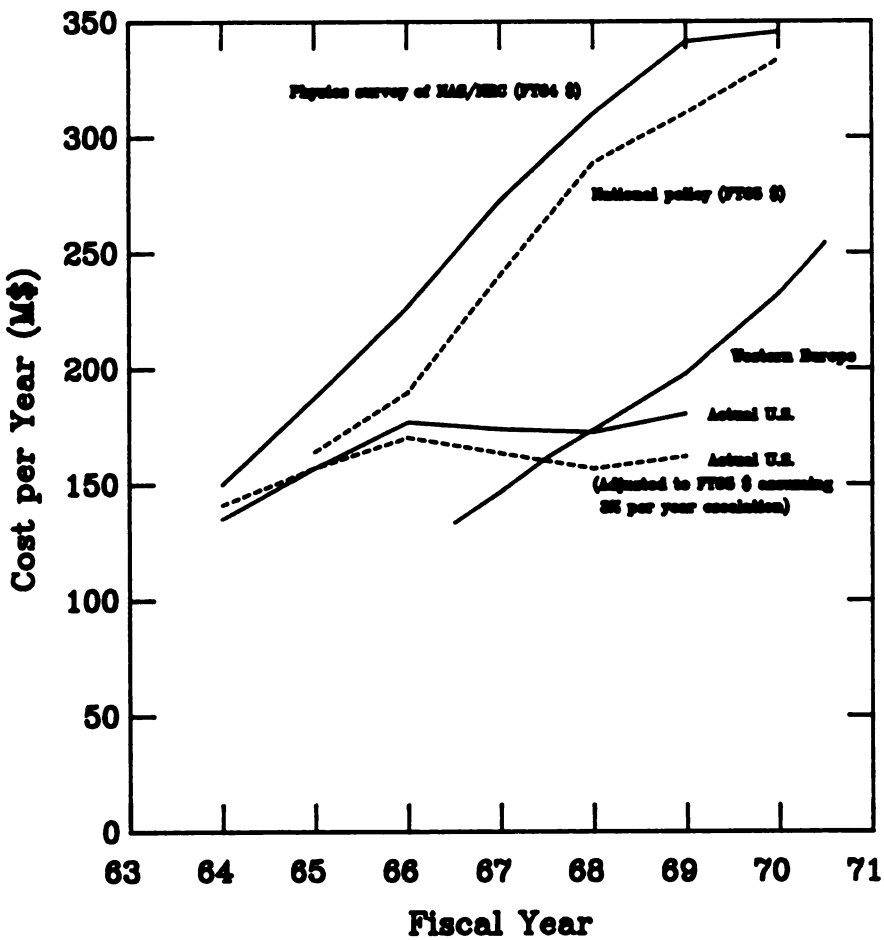


Figure 5.--High energy physics projected funding and actual funding as promised in 1968. Data from High Energy Physics Advisory Panel, "The Status of High Energy Physics Today," January 1968, p. 38.

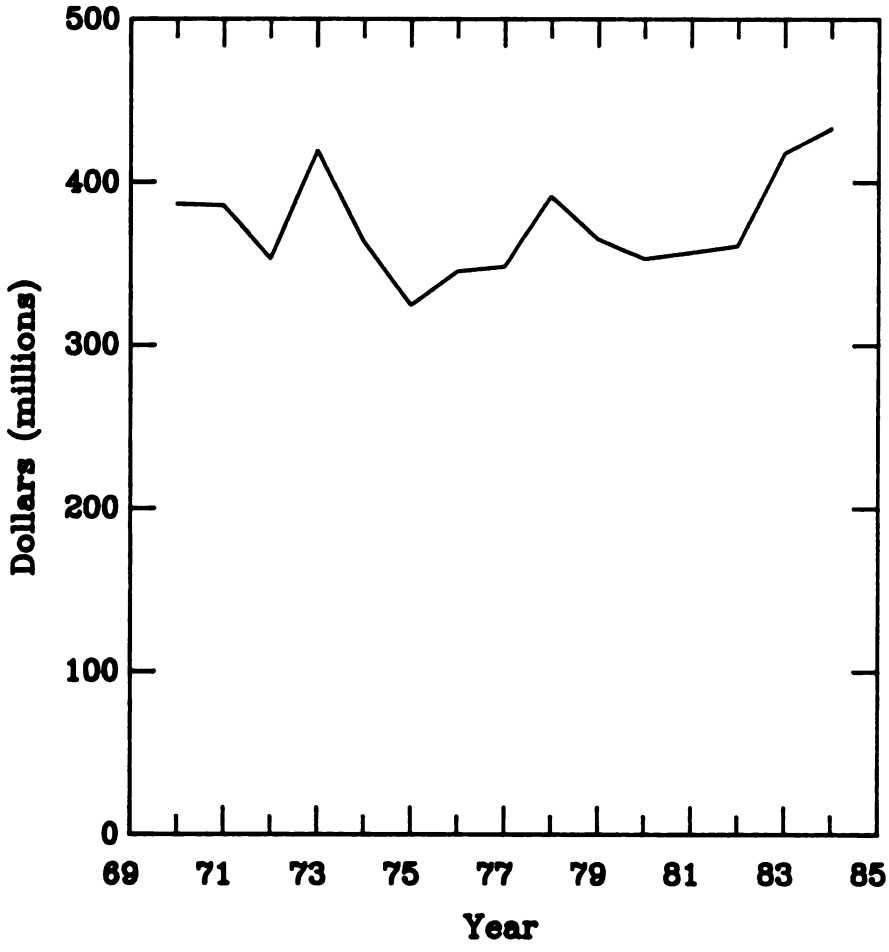


Figure 6.--High energy physics funding for fiscal years 1969 to 1984. Funding is given in fiscal year 1983 dollars using inflation factor chosen by Physics Survey Committee. Construction funds are not included. Please note that the Department of Energy took over high energy physics funding (and most other functions) of the short-lived Energy Research and Development Administration, which assumed the non-regulatory functions of the Atomic Energy Commission when it was dissolved in 1975. Data from Physics Survey Committee, Physics Through the 1990s: An Overview (Washington, D.C.: National Academy Press, 1986), p. 125.

rate threatened the success of the project. As he recently explained, constructing the project in small pieces was not economical and risked grave staff morale problems. "I figured the only way I could accomplish such a task was to gather a group of people, get them excited about the task, and then say 'Let's Go'." Such concern prompted Wilson to submit a letter of resignation to Ramsey after hearing that NAL would only receive $7.1 million in 1968. Wilson explains that instead of accepting the letter, Ramsey worked with AEC officials and Wilson to fashion a workable budget, a task that became easier when the allocation was raised. (17) As funding delays continued, Wilson adjusted to the inconvenience, and in keeping with past performance, found ways to make the most of the situation. As he explained to the JCAE in the 1971 authorization hearing, NAL responded to the low rate of funding by:

> doing only the most urgent construction, by making sure that no contingencies arise, by keeping the number of employees to an absolute minimum, by exploring technical innovations . . . to keep down operating costs, and by scrounging used and old-fashioned equipment to bolster our meager equipment funds." (18)

Recent interviews reveal that those working on the accelerator were barely aware that NAL leaders were facing such problems. As Quentin Kerns, head of the radiofrequency group, recently explained: "We were always too busy to worry much, and besides, we trusted Wilson." (19)

Although morale at NAL did not suffer because of the slow funding rate, it was soon clear that continued funding for the laboratory, which had been planned with the expectation of a much higher AEC budget, would require sacrifices from the rest of the physics community. Seaborg's

records reveal that in 1969, at just the time when NAL needed a major portion of its construction funding, newly elected President Richard Nixon made severe cuts in the AEC budget. BOB first suggested that a new meson facility slated for Los Alamos be struck from the budget. However, as Seaborg pointed out to the current presidential science advisor, Lee DuBridge, the Los Alamos accelerator was a "must" because if it were cut, JCAE member Clinton Anderson, a Senator from New Mexico, "would make a career out of stopping the 200 BeV." After continued agonizing negotiations in the executive branch, the ax fell. The Princeton-Pennsylvania Accelerator, which had only been operating at full design energy since 1963, was terminated and a third of the budget for the Cambridge Electron Accelerator was cut. As an AEC representative later explained, the Commission had decided to make these cuts instead of making an even reduction of all programs because "you want to maintain the viability of your most frontier laboratories." (20)

In contrast to the vigorous complaints raised in the early 1960s when the proportion of basic research rose relative to overall federal research expenditures, a survey of Physics Today reveals few complaints about the sacrifices made to see NAL to completion. One exception to the general mood of support for the machine was a complaint from Princeton high energy physicist David Bartlett about cuts to the accelerators at Princeton and Cambridge, which he voiced at an April 1970 APS meeting. Nonetheless, Victor Weisskopf summed up the views of many. As reported in Physics Today, Weisskopf noted that "particle physicists could consider themselves relatively fortunate" because of continued funding

for NAL "although money for the new machine means that other programs must be cut." (21)

In the midst of ardent support from many top physicists, including Ramsey, NAL enjoyed considerable general support within the physics community. Apart from an accusation at the 1970 meeting about the "overglamorizing" of high energy physics, little complaint was heard about cuts to other subfields. This is particularly impressive considering the disproportionate cuts during this period to budgets already much lower than the high energy physics budget and the relatively enormous costs of NAL. For example, in early 1971, when NAL still had $93 million coming from its $243 million construction budget, the AEC cut the low energy physics operating budget by $2.3 million, more than half the cut for high energy physics, although the total low energy physics budget was less than one-quarter that of high energy physics. In addition, the total estimated low energy physics operating budget for 1972 was less than half the anticipated $60 million operating budget for the new accelerator. (22)

Summary: Design and Construction Funding

Against the backdrop of social unrest and growing public skepticism about the value of science and technology and the faltering political power of the JCAE and AEC, the rapid growth of physics funding halted abruptly. In this funding environment, money for NAL was allocated at a slower rate than expected. Although morale at NAL did not suffer because of funding problems, the physics community as a whole had to

make sacrifices to ensure the continued funding of the project, which had been planned with the expectation of a much higher AEC budget. In the midst of pro-NAL campaigning by Norman Ramsey and others, the physics community bore, with impressive stoicism, the necessary sacrifices, which included major cuts to some programs and the termination of the Princeton-Pennsylvania Accelerator. In contrast to the debate in the mid-1960s about the relative value of basic and applied research, little complaint emerged in the 1970s about the desirability of assigning top funding priority to high energy physics and the NAL accelerator, the subfield's hope for the future. With the help of the JCAE and AEC, campaigning by Ramsey and other top physicists, and his own ability to make the most of a difficult situation, Wilson was able to continue design and construction efforts at full speed.

Establishing the Non-Technical Aspects of the Laboratory

At Cornell, Wilson had not dealt extensively with outside users; the small Cornell accelerators had always been operated primarily for the benefit of Cornell researchers. However, after discussion with Goldwasser and URA officials, Wilson agreed that it was important to immediately solicit the views of potential outside users, even though many years would pass before they would have a machine to use. Afterall, as Wilson recently noted, NAL leaders intended to make outside users "the whole focus" of the laboratory, in keeping with the spirit of Lederman's "Truly National Laboratory." Thus, in April 1967 a meeting

for potential users was held at Argonne National Laboratory, in Illinois. As Ramsey explained in his letter of invitation, in addition to status reports on construction plans and an address by Illinois Governor Kerner, the meeting would feature discussion of plans for establishing "the avenues of interaction and communication between the Laboratory and the members of the high energy physics community." The attendance was impressive--over 400 scientists registered for the meeting. According to a subsequent URA press release, "there was general agreement on the desirability of some form of Users Group." An organization committee was therefore established and by the end of the year another meeting had been called for potential users. Thereafter, the NAL users group met once a year. As the users group was being organized, outside users received further confirmation of their role in the new laboratory--in early 1968 the laboratory formally announced that only 25% of NAL research would be conducted by NAL physicists, a clear signal that outside users would dominate the research program. (23)

Annual summer studies provided another official avenue of communication between NAL leaders and potential users. In 1968, with a revised version of the design for the new accelerator in hand, Wilson and Goldwasser began the tradition of annual summer studies. As the announcement for the first meeting explained, although a new design had been developed, "plans will be subjected to continuing review, and they will be revised and updated in accordance with the development of physics ideas on the one hand and technology on the other." Wilson and Goldwasser were particularly interested in eliciting the participation

of top-rank experimentalists and theorists in the project, both to ensure that plans for experimental areas and detectors were consonant with evolving research objectives and to garner support from the community's most influential members. To encourage optimum participation, Wilson and Goldwasser accepted an offer made by the Physics Division of the Aspen Institute to conduct a joint NAL/Aspen Institute program at Aspen, Colorado. With the extra incentive of Aspen's beautiful mountain scenery, NAL leaders were able to consistently recruit large numbers of participants, despite the fact that most were required to pay for lodging and meal expenses from their own research budgets. (24)

Goldwasser recently explained that NAL leaders received a "tremendous amount of useful input" from the users meetings and summer studies. Laboratory records clearly show the influence of these meetings on evolving plans. For example, after lively discussion at the 1968 summer study on the desirability of continued bubble chamber studies, users recommended that the laboratory build a large liquid-hydrogen bubble chamber. As a result, NAL leaders actively pursued plans for obtaining bubble chambers, including a proposal to build a 25-foot bubble chamber in collaboration with Brookhaven. Although funding could not be obtained for this project, a 15-foot bubble chamber was eventually built by NAL. Users also played a crucial role in the design of experimental areas. As revealed in NAL progress reports, when work on experimental facilities began in late 1969, the NAL staff started with summer study recommendations. Subsequently, the evolving experimental

area design was presented to users for comment. J. Richie Orr, who directed construction of meson research facilities, recently explained that proposals and prominent individual experimenters also influenced the design. Experimental areas were also reviewed by the Program Committee, an advisory group convened to review proposals for experiments and give other advice on the experimental program. (25)

By this time other major laboratories had appointed program committees to review proposals. Plans for a program committee, like plans for the users group, began early at NAL: Wilson appointed the Program Committee in December 1969 and the first call for proposals was made in March 1970, more than two years before the deadline for a 200 GeV beam. This not only gave experimenters time to prepare experiments, but also provided additional assurance that user needs were being considered as the laboratory was built. To further bolster user confidence in the laboratory, Wilson made sure that the original Committee was impressive: the 9-member group included three Nobel laureates, Murray Gell-Mann, Owen Chamberlain, and Tsung Dao Lee. (26)

Although physicists would design and build the accelerator, an architect-engineering group was needed to design and construct the buildings to house accelerator components and other conventional structures. For the sake of efficiency, in early May 1967 Wilson decided to retain DUSAF, the consortium of architect-engineers the Berkeley group used to make cost estimates for conventional construction. However, because of the importance beauty played in his vision of the laboratory, Wilson played a dominant role in decisions affecting the

appearance of the facility. Wilson and Goldwasser both remember that DUSAF members were first surprised and somewhat dismayed by Wilson's strong interest in their work, especially since this interest sometimes led to temper tantrums during which Wilson tore up unacceptable design plans and threw them on the floor. According to Wilson, in time he nonetheless enjoyed good rapport with DUSAF representatives. "Eventually," he explains, "they came to love my displays of temper because they showed that I cared." (27)

Although Wilson kept a tight rein on architect-engineering costs, as he did on all costs for the laboratory, his desire to create a beautiful laboratory sometimes overrode his urge to economize. For example, although reworking of engineering designs tends to increase construction costs, some plans were revised in line with Wilson's standard of beauty. For example, DUSAF representative George Laposky reported in a 1969 Inland Architect article that DUSAF rerouted a road to avoid 250 year-old twin-oak trees. "In other places," he added, "the pavements will bypass clumps of trees or small depressions. The roads will be less efficient, but more harmonious." To ensure that the laboratory had a harmonious color scheme, Wilson brought Angela Gonzales, an artist from Cornell. As Goldwasser explains, although Wilson's taste dominated plans, all painting had to be approved by Gonzales, who served as Wilson's "aesthetic watchdog." In line with Wilson's flair for achieving maximum benefit from a given situation, sometimes ways were found to simultaneously achieve beauty and solve a construction problem. For example, Goldwasser remembers that when the

excavation necessary for the pump storage reservoir for the laboratory's water supply created tons of earth, the earth was used to create a large hill to help break the flat Illinois landscape. (28)

Perhaps the most vivid expression of Wilson's vision of the laboratory is the design of the central office building, or "high-rise," which clearly reflected both his humanistic and aesthetic ideals. When Wilson first began making plans for the laboratory, he seized upon the idea of a distinctive central building in the form of a high-rise. As the 1969 Inland Architect article explained, Wilson wanted the structure to have "significant architecture," to serve as "a magnet telling people: 'Something important is happening here.'" As Wilson recently explained, he also felt that a high-rise would play a crucial role in creating a laboratory in which discoveries could be made. By providing a panoramic view of the Illinois landscape, the elevated structure would give physicists a beautiful working environment, an environment conducive to creativity. In addition, the structure would centralize the staff, encouraging increased communication and a spirit of camaraderie. Goldwasser remembers that he and Wilson even considered methods for making the elevators slower to further encourage chance encounters between staff members that might not otherwise share ideas. To provide an attractive environment inside the building and further encourage spontaneous communication and camaraderie, Wilson suggested building an atrium on the ground floor of the building, an idea inspired by the Ford Foundation Building in New York. (29)

The high-rise idea sparked complaint, from both physicists and the URA Board of Trustees, who pointed out that it would be cheaper to build a cluster of smaller buildings. Nonetheless, Wilson proceeded with design plans. Goldwasser remembers that the Director first held a contest to judge various variations of the high-rise design and let the senior staff choose the winner. After this bow to democracy, however, Wilson worked closely with one of the DUSAF architects and a new design emerged. Although funding delays prevented quick construction of the building, today the high-rise contains 15 floors of offices in the up-sweep of an A-frame building which overlook a central open area with an atrium and cafeteria. (See Figure 7) A recent interview with members of the Berkeley design team gave evidence of how the high-rise has served both as an expression of beauty and as a unifying symbol for those who work there. Although Wilson's design ideas still evoke annoyance after twenty years, Berkeley researchers gave accolades to the high-rise. As Glen Lambertson explains: "The high-rise is one thing that really works--it expresses the style of the laboratory." (30)

Although he had played no role in site selection, Wilson was extremely disturbed by the complaints of civil- rights leaders about the choice of the Illinois site, in line with his humanistic ideals and early sensitivity to the injustice suffered by black people. Goldwasser, who was also a long-time supporter of civil rights, was similarly affected. Before accepting their positions, both NAL leaders decided that building an organization in Illinois that was responsive to the rights of minorities would actually help to promote civil rights.

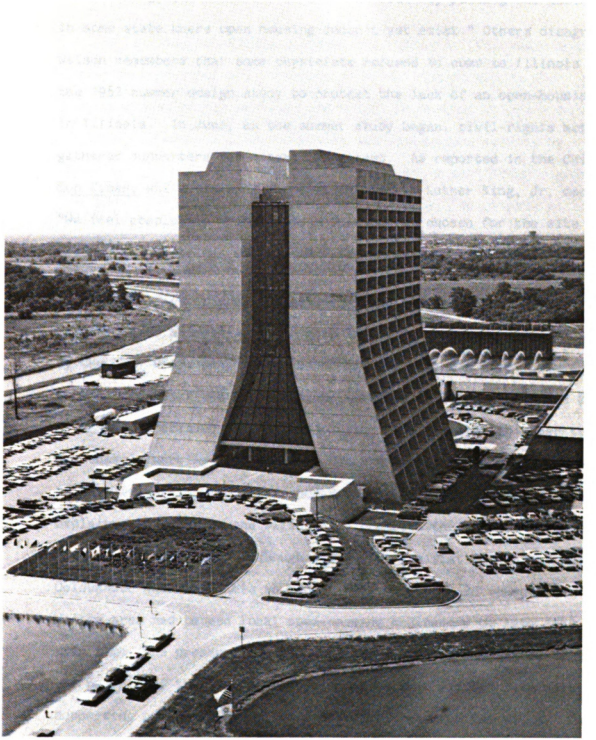


Figure 7.--High-rise office building at Fermi National Accelerator Laboratory.

As Wilson recently remarked, "my argument was, if you want to promote open housing, you will have more of an effect by putting the laboratory in some state where open housing doesn't yet exist." Others disagreed. Wilson remembers that some physicists refused to come to Illinois for the 1967 summer design study to protest the lack of an open-housing law in Illinois. In June, as the summer study began, civil-rights activists gathered supporters for a tent-in protest. As reported in the Chicago Sun Times, while visiting the tent-in, Martin Luther King, Jr. declared: "We feel absolutely that Weston should not be chosen for the site because the state has not passed a fair-housing bill." (31)

In the face of such criticism, Wilson and Goldwasser were determined to champion civil rights in Illinois, a goal already shared by the AEC, which sought assurances of civil- rights compliance from local governments and organizations when Weston was chosen. (See Chapter 5) Seaborg continued to push for civil-rights legislation after the site was chosen, working with Illinois Governor Kerner to promote the passage of both state-wide and local open-housing legislation. As Goldwasser explained in 1969, he and Wilson also promoted the cause at numerous town council meetings. Although state-wide legislation was not passed, Goldwasser was later able to report that more than 30 communities in the Weston area had passed local open-housing ordinances by late 1969. By this time, the pressure for statewide open-housing had abated because of the passage of the 1968 fair-housing bill (that Dirksen belatedly supported, prompting rumors of a political deal for the site). This

bill, which was scheduled to go into affect in 1970, covered 80% of the nationwide private-housing market. (32)

In addition to efforts aimed at the community, NAL leaders also worked hard to serve the interests of minorities at NAL as the laboratory was being organized and built. Because of Wilson's other pressing responsibilities, this task was implemented largely by Goldwasser, who was aided by Kennard Williams, a civil-rights activist recruited to manage an aggressive affirmative-action program at the laboratory. Wilson and Goldwasser kicked off their laboratory-wide civil-rights campaign by issuing the "Policy Statement on Human Rights" on March 15, 1968. The document announced that NAL would "seek the achievement of its scientific goals within a framework of equal employment opportunity and of a deep dedication to the fundamental tenets of human rights and dignity." In the document NAL leaders also promised to "create conditions for special opportunities" for the disadvantaged "by establishing aggressive employment practices and by instituting special educational and apprentice programs." (33)

By 1969 Goldwasser was able to report considerable progress. After numerous visits to the worst slums of inner- city Chicago, Williams had found recruits for several training programs, including a training session for 100 young "hard-core unemployed," who received training from a local labor union to operate earth-moving machinery. Although it proved difficult to recruit black engineers and physicists because so few were available, Goldwasser reported that 20% of non-professional laboratory employees were black. To help enlarge the future supply of

black professionals, the laboratory initiated a program designed to give career guidance to black students from Chicago schools to help them prepare for jobs in the physical sciences and engineering. NAL civil-rights efforts were impressive enough to reverse Clarence Mitchell's stand. Although the NAACP official opposed construction at the Weston site in 1967 authorization hearings, in mid-1968 he wrote a strong letter of support for NAL when the laboratory funding allocation was cut. (34)

Complaint about civil rights was not the only problem Wilson and Goldwasser inherited. Although Kerner promised at the April 1967 Argonne meeting that Illinois would "be ready to move ahead immediately," to secure land for the site as soon as legislation passed, NAL leaders expected some difficulty. Before construction of the laboratory could begin, residents from the village of Weston and surrounding farms had to be vacated, a process bound to be unpleasant. The situation was particularly frustrating because land acquisition problems directly affected the progress of the new laboratory, yet NAL leaders had very little influence in state-controlled land acquisition procedures. (35)

While waiting for Illinois to procure the promised land, NAL leaders rented office space in an office building in the west Chicago suburb of Oak Brook. Here, with the help of numerous visiting accelerator specialists and his growing staff, Wilson directed an effort to refine the Berkeley design in summer 1967. By late 1967, however, the staff had grown and some groups were ready, or nearly ready, to begin

construction of prototype components. In addition, DUSAF needed to begin work soon to keep to Wilson's demanding schedule. (36)

To facilitate research and development of the linear accelerator, the most straightforward and well-defined portion of the design, Wilson made special arrangements with the savings and loan company that owned much of Weston so that the linac group could move into three on-site houses in late 1967 and construction could begin on a new building. Since no AEC construction funds were yet available, the URA agreed to finance the construction project, with the understanding that NAL would later refund the money. Despite this stop-gap measure, it was apparent that the laboratory would soon need to take possession of its new home. Unfortunately for the harried NAL staff members, initial procedures for assessing and acquiring the land did not begin until fall 1967, when Kerner appointed committees to acquire the land and select professional property appraisers. (37)

By spring 1968, new problems arose. Although NAL leaders expected resistance from farmers, who fought to keep the AEC from choosing Weston, they expected little trouble from Weston villagers, who had lobbied for the site. (See Chapter 5) Unfortunately for Wilson and Goldwasser, Weston villagers soon changed their tune. Although they expected the site boundary to fall just west of Weston, when site boundaries were officially delineated, Weston lay inside the boundary. Although Weston residents tried to obtain land so that their village could be relocated near the boundaries of the new laboratory, by May proposals for annexing the necessary land had failed and relocation

seemed unlikely. On May 15, 1968, a newspaper in the Weston area reported that the Weston village board had written to Wilbur Mills, chairman of the Ways and Means Committee of the House of Representatives, to complain about land acquisition procedures. According to the article, copies of the letter were sent to several Illinois congressmen, JCAE chairman John Pastore, and President Johnson. Clearly, land acquisition would be more complicated than expected. (38)

Despite complaints, land acquisition efforts continued. Although procedures for obtaining farm property moved slowly, the Chicago Tribune announced on June 30, 1968 that Weston villagers had agreed to dissolve their community and relinquish all property by October 1. Accordingly, Wilson and Goldwasser made plans to move all employees to the new site by this date. By this time the village land was assessed and Illinois was ready to pay off landowners. In mid-August the Wheaton Daily Journal reported that the last three homeowners, including Weston mayor Arthur Theriault, had signed agreements for selling their property. Despite these arrangements, some village families made no plans to leave, determined to force eviction proceedings. (39)

With his characteristic urge to reap maximum benefit from available resources, Wilson decided to use village structures, which included about 100 houses, for office and storage space, reasoning that construction costs would be saved and that the buildings could later be used for outside user accommodations. While waiting for the completion of formal acquisition procedures, Wilson also made arrangements to rent

more village houses so that the main ring modelling and other groups could move on site. (40)

Although Wilson's arrangements were clever, they provided some challenges for the NAL staff. As village houses were vacated they had to be cleaned, refurbished, and converted for laboratory use. NAL physicist Francis Cole, who headed village construction and acted as NAL liaison to the Weston village board, recently explained that some houses were quite dirty and in poor condition. In addition, although by October 1968 all NAL staff members had moved to the village and construction had begun on additional buildings, some Weston families remained until the end of the year. Amid the noise and chaos of the budding laboratory, huge trucks lumbered past yards where small children played. (41)

Happily, no major accident occurred, although the awkward situation led to incidents that are amusing, at least in retrospect. Cole remembers that on one hectic day Wilson received word that an irate resident wanted him to arrange for disposal of her garbage. At the 1968 users group meeting Wilson gleefully recalled the day he impulsively decided to make a systematic status check of all projects and accidentally barged in on a family innocently eating dinner. Although relations were understandably strained at times, Theriault later publicly expressed appreciation to Cole "for making this transition a little easier" and Wilson thanked Theriault for his help "during what must have been a very trying period . . . ." (42)

Although Weston problems were well in hand by late 1968, land acquisition efforts for the rest of the property were not proceeding smoothly. According to local newspaper accounts, in October several farm owners complained to state representatives that the designated land was being appraised below its true market value and asked the State of Ilinois to recall acquisition negotiators. Although Illinois had established "quicktake" provisions, whereby property would be immediately handed over to the state for transfer to the AEC, even if the price was disputed, and the price negotiated later, several landowners went to court over the issue. Although quicktake was upheld in court, the protracted negotiations created an awkward situation for NAL leaders since they had already organized a groundbreaking ceremony based on promises that all lands in the site would officially be transmitted to NAL by the end of 1968. Even though construction had begun on the new site, officials were leery of breaking ground in a public ceremony without clear title to the land. As the date loomed closer, AEC officials frantically made arrangements to get the title for at least a small portion of the site. Ramsey remembers that they received the necessary title with just 24-hours to spare, allowing the ceremony to be held on December 1, as planned. (43)

When 1969 began without the transfer of title of the remaining property, Wilson was furious. As he explained to Seaborg: "There are certain pieces of property which we do not yet have which we need urgently for construction . . . ." In addition, the issue consumed what Wilson considered an unreasonable amount of his time. After intercession

by the AEC and Illinois Senator Charles Percy, Illinois acquired all land for the site by March 31, 1969. On April 10, 1969, Illinois formally transferred title for the whole site to the AEC. (44)

Nonetheless, some farmers remained on the land, and by summer, the presence of several barns and houses interfered with surveying efforts and Wilson threatened to take legal action to ensure that two farms would be vacated. Although all residents had left by the end of 1969, NAL staff members had to contend with cattle grazing on the site as late as December 1970. Reminiscent of the experience at Weston, forced eviction prompted unusual requests. In July 1969, for example, AEC Area Manager Kennedy Brooks received a letter from an 11 year-old girl begging him to make arrangements for three cats which had regretfully been abandoned in a barn loft. Brooks dutifully promised that they would do their best to find the creatures good homes. Wilson was relieved when the protracted land acquisition was finally over. After all, the process, which had taken over two years, had been largely out of his control even though it affected the progress of the laboratory. Nonetheless, his sensitivity to the rolling Illinois farmland prompted him to later arrange a small museum of farm equipment, so that the original use of the land would not be forgotten. (45)

Summary: Establishing the Non-Technical Aspects of the Laboratory

Wilson faced a number of challenges when establishing the non-technical aspects of the laboratory. In light of the pressure for outside user rights, it was crucial to assure potential outside users that the laboratory would serve their needs. Although Wilson had not been a champion of outside user rights, at the urging of Goldwasser and URA officials he took numerous steps to involve outside users in NAL planning and bolster user confidence in the new laboratory. By 1968 the laboratory was formally committed to devote 75% of the research program to outside users, by 1969 annual users meetings and summer studies were underway, and by 1970 a prestigious Program Committee was judging experimental proposals.

Wilson was also determined to create a beautiful laboratory that would encourage creativity and mutual respect among workers, in line with his vision of what the laboratory could become. Wilson therefore assumed a dominant role in all decisions affecting the appearance of the laboratory, including landscaping. Perhaps the most vivid expression of Wilson's humanistic and aesthetic ideals is the design of the central office building, or "high-rise." Even the most ardent critics of the laboratory agree that the high-rise provides a panoramic view of the Illinois countryside, creates a central meeting area for the staff, and supplies a symbol that helps define and a unify the laboratory.

Although Wilson had not been involved in site selection, as NAL Director he faced civil-rights complaints, a legacy from the bitter site

selection contest. Because of his humanistic ideals, which Goldwasser shared, such complaints were particularly distressing. With the help of Seaborg, Wilson and Goldwasser campaigned for the passage of both state-wide and local open-housing legislation. Civil-rights efforts within the laboratory, which were spearheaded by Goldwasser, included an aggressive minority-recruitment program and various training programs. Although it proved difficult to recruit black engineers and physicists because so few were available, NAL civil-rights efforts were impressive enough to convince NAACP official Clarence Mitchell to switch from opposition to support for the laboratory.

In the case of land acquisition, Wilson inherited ready-made problems which he had little power to assuage. Although Governor Kerner promised in April 1967 that land would be available quickly, Illinois first had to evict residents from the village of Weston and surrounding farms, a process that proved unpleasant and time-consuming. Although Wilson had little control over state-controlled land acquisition procedures, delays could destroy his ambitious construction schedule. As a temporary solution, Wilson obtained permission in late 1967 from the savings and loan company that owned much of Weston and some NAL groups were allowed to move to the site and start construction before the land was officially acquired. When the last NAL groups moved to the village on October 1, 1968, however, staff members still faced the challenge of sharing quarters with several village families. Although the last villagers vacated the site in the next few months, at this point farmers lodged legal complaints against land acquisition

procedures. The resulting delays forced Wilson and other NAL leaders to frantically make special arrangements for the official ground-breaking ceremony, which had been planned with the expectation that the laboratory would hold a clear title to the land by late 1968. After fervent complaint from Wilson, the entire site was finally acquired in April 1969.

Design and Construction

When Wilson agreed to direct the design and construction of the new machine he assumed a formidable task. Many physicists questioned whether a viable 200 GeV accelerator could be built within the available $243 million budget, and although there was general support for the idea of an expandable machine, it was even more unlikely that an expandable machine could be built for such a sum. For example, the High Energy Physics Advisory Panel, a prestigious AEC panel led by Victor Weisskopf, studied the evolving plans for the accelerator and concluded "there would be real danger that a rigid upper financial limit might either make the construction" of a 200 GeV machine "impossible or might restrict the experimental exploitation below an acceptable minimum." The panel also worried that such a limit might cause cost overruns that would impugn the reputation of the design team. Because of his spirited criticism of the 1965 Berkeley design, Wilson also faced the animosity of the LRL design team. In addition, because of his lack of experience in building large proton synchrotrons and his reputation for risk-taking, he faced the skepticism of other top accelerator experts. (46)

The task was also complicated by the extent of the revisions to be made to the Berkeley design. The design would have been reconsidered at this point in any event, to take individual site factors into consideration and refine other design considerations. However, considering the already apparent conflicts between the LRL and Wilson approach to accelerator design and the fact that a new team would

construct the machine, it was clear that this revision would be particularly extensive. Another complication was the short time schedule. To optimize the chance for getting formal approval for the full budget in 1969, Wilson was determined to present costs and schedules to the AEC in time for the new budget. This meant that the "Schedule 44," the official request to the AEC, had to be ready by mid-October. Thus, at a time when many accelerator experts were skeptical of the project, Wilson had to assemble a team to produce a design for a $243 million expandable 200 GeV machine in a matter of months. (47)

Despite these complications, Wilson dove into preparations for the summer design study with characteristic vigor and enthusiasm. Sensitive to the difficulties he might encounter in gathering a team to design the machine, Wilson chose an attractive office building in the west Chicago suburb of Oak Brook, which offered an appealing vista of Illinois farmland. A large number of experts were invited, including members of the LRL design team. Wilson recently explained that he wanted all the expert advice he could get and also saw the meeting as a chance for NAL leaders and prospective employees to see whether they could work well together. To encourage attendance, the URA also offered extra pay so that participants would make more money by coming to Weston than by staying at their home institutions. Recruitment efforts for the summer study were clearly a success. Accelerator designers came from all over the country, including Berkeley and Brookhaven. Although the length of visits varied, about 25 participants were present at a time. (48)

Many would have considered the working conditions incommodious for such a crash design effort. In the first weeks the Oak Brook quarters were unfinished and totally open -- no walls, offices, furniture, nothing but an expanse of tile floor reaching to the windows with their panoramic view of Illinois farmland ten floors below. DUSAF and Brobeck and Associates, an accelerator consulting firm, were also housed at Oak Brook, as was an AEC Office assigned to the project under the leadership of Lawrence Mohr. Design discussions on various parts of the accelerator proceeded simultaneously, with separate cost estimations by the engineering groups and on-the-fly coordination by Wilson. In the beginning, summer study participants also shared their quarters with telephone crews, workmen installing walls, and according to one observer, "people from other offices in the building who came to stare in amazement at the chaos . . . ." Thanks to the AEC, desks, typewriters, chalkboards and pencils, along with a complete library, a librarian and a secretary, soon arrived from MURA, which was being dismantled in Madison. Wilson loved the communal environment and lack of partitions, later declaring "communication was made by simply raising one's voice and will never again be so direct or so good." (49)

Wilson later explained that he approached the design much as he approaches a sculpture. From the beginning he had a rough idea of an accelerator capable of much higher energy, but as various ideas were suggested he tried to keep his image of the machine fluid so that innovative ideas could be incorporated. He was also intent upon keeping a balanced view of the whole machine in his mind. "Most professionals,"

he explained "divide the work into parts and then sit down and do a professional job on each part." This 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 could only be created by developing each part to enhance the development of every other part. In line with his holistic approach to the design, Wilson also insisted that designers not lose sight of the ultimate purpose of the machine. To ensure that physics considerations were not forgotten, Wilson encouraged active experimentalists to help with the design. In addition, he convinced Robert Serber, a prominent theoretician, to give periodic physics lectures during the design study, a service he had also performed in the midst of machine design at Berkeley in the late 1940s. (50)

Wilson's concern for balance also influenced the way he approached other aspects of the design study. In his opinion, as laboratory director he needed to keep a good balance between the interests of the physicists, who would use the accelerator, and the interests of the federal government, who would pay for the accelerator. If the machine could not be used for exciting physics experiments, physicists wouldn't use it. At the same time, if the machine was too expensive, the government would refuse to pay for it. Wilson feels this approach to accelerator design resembles that of his former teacher, Ernest Lawrence. As Wilson explains, "Lawrence designed machines based on the amount of money he had available . . . . Once he figured what he wanted to build, he expected little difficulties, and then refused to back down

from his goal, ever." For Wilson, Lawrence's lesson was that the accelerator builder should strive for optimum physics capability and minimal cost. Ironically, this lesson, which incorporated Wilson's characteristic desire to achieve optimum benefit from available resources, led him to take risks unacceptable to Edwin McMillan, the physicist Lawrence chose to succeed him. While the Berkeley team concentrated on producing a conservative design that would be sure to work right away, Wilson insisted that the Oak Brook designers take risks, figuring that whatever failed could be fixed later. In his opinion, if the accelerator worked immediately, it had been overdesigned. (51)

Although Wilson saw the design effort as an adventure, some at the summer study were dismayed at his design style, which they saw as flamboyant and risky. A particularly vivid reflection of Wilson's style could be seen in the design of the main ring magnets. In line with his 1965 suggestions, Wilson promoted the design of compact, H-shaped magnets. Instead of the combined-function magnets shown in the Berkeley design, Wilson decided to use separate magnets for bending and focusing, an idea Gordon Danby brought to Oak Brook. Although separated function magnets had never before been used in a proton synchrotron, they promised more flexibility in accelerator operation and allowed more efficient beam focusing. In addition, while Berkeley schemes for an expandable machine hinged on the later addition of magnets, by using separated function magnets, Wilson was able to design magnets that could go to energies as high as 500 GeV with the later addition of power

supplies. The resulting design took the risk of placing magnet coils much closer to the beam than in previous designs. (52)

The design was cleverly innovative: in addition to the advantages of separated function magnets, the compact magnets required smaller tunnels and less steel and thus saved money. In addition, Wilson's scheme for expansion could be implemented without the lengthy downtime required by Berkeley schemes. Nonetheless, critics felt that the risk of using untried features, compact magnets, and magnet coils close to the beam was unacceptable considering the cost and importance of the project. Some physicists also criticized the economical, but inconvenient, main ring tunnel, which was small and lacked cranes or airconditioning, and the decision to omit caissons as foundations for the main ring magnets. (53)

In addition to concern over Wilson's design style, some summer study participants objected to Wilson's leadership style, which they saw as impulsive and domineering. For example, some members of the Berkeley design team who were helping with magnet calculations were annoyed when Wilson chose the radius of main ring. Designers 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 1000 meters, exactly, explaining that a round number would be easier for everyone to remember. As Wilson recently explained: "I didn't have time to sit around and talk about things forever . . . . I had to listen for awhile and make the decision, or else the rest of the design would have been delayed." (54)

Despite such complaints, Wilson's style of leadership and accelerator design also won a large number of ardent supporters. For example, Quentin Kerns, who had helped develop the radiofrequency system in the Berkeley design, remembers "being terribly impressed with Wilson. After meeting him I felt he was a good leader about to build a great accelerator and I wanted to help him." Other accelerator specialists agreed. Although Kerns and Frank Cole were among the few members of the LRL team to join the new laboratory, Wilson recruited several accelerator specialists from Brookhaven, including Alfred Maschke and Arie van Steenbergen. Wilson's recruitment efforts were also aided by the fact that MURA had been disbanded and funding was running low at the Cambridge Electron Accelerator (CEA) and at the Princeton-Pennsylvania Accelerator (PPA). Thus, Wilson was able to recruit a number of physicists from these institutions, including CEA's director M. Stanley Livingston, an eminent scientist who built the first cyclotron under the direction of Lawrence and helped invent strong focusing; CEA staff member Thomas Collins; PPA staff members Miguel Awschalom and Frank Shoemaker; and MURA staff member Donald Young. (55)

As plans proceeded, it became evident that along with the enthusiasm and excitement, tension was mounting. Although Wilson managed to push through his ideas, an undercurrent of disagreement grew stronger. When a cardboard model of the main ring tunnel appeared holding a wooden magnet, some participants snickered at its size, although they waited until Wilson was out of earshot. As Wilson prepared drafts of the Schedule 44, some judged his ambitious budget and

schedules to be unrealistic, arguing that Wilson had been unsuccessful in his attempts to redesign the machine in only a few months. (56)

Such dissension came to a head in September, when Wilson delivered a paper describing the NAL design at the VI International Conference on High Energy Accelerators in Massachusetts. Wilson outlined plans, including provisions for compact magnets and limited foundations, and announced that he hoped to have the machine accelerating protons to 200 GeV by June 1972. Although he had been able to cow resistance while on home turf, this meeting for accelerator experts provided a prime forum for his detractors. Ramsey remembers that he hardly had a chance to listen to any of the talks because prominent physicists, from Berkeley as well as many other institutions, constantly called him aside to complain about Wilson's design. The community's top accelerator professionals "very vigorously opposed" the effort, going so far as to call it "irresponsible." (57)

Luckily for Wilson, by mid-October he had obtained a stamp of approval from both the AEC and the URA. According to AEC notes, after hearing the details of Wilson's design in early September, the Commissioners thanked Wilson for the "interesting and encouraging" report. Not long after, the URA submitted its vote. As URA committee member Leon Lederman explained to a member of the LRL design group, at an October 12 meeting, the associations's scientific committee recommended, and the URA board concurred, that Wilson's plans should be approved. They felt the Schedule 44 contained "no major technical decision which was so clearly wrong or unwise as to cause us to

hesitate" and their confidence in the project remained high. After obtaining URA approval, Wilson submitted the Schedule 44 to the AEC. The document gave a brief summary of the design, itemized costs totaling $243 million, and promised that the machine would be in operation by June, 1972. Although criticism resurfaced periodically, particularly during times of trouble, Wilson had won the right to build the machine his way. (58)

In the next two months Wilson and his staff produced a full-scale design report describing the design in more detail. When the report appeared in early January, it described a machine that in some ways resembled that shown in the 1965 Berkeley design. Like the Berkeley design, the NAL design featured a four-stage machine in which protons were accelerated by a Cockcroft-Walton, transferred to a linear accelerator for more acceleration, transferred to a booster synchrotron for further acceleration, and finally transferred to a large synchrotron for acceleration to design energy. In addition to this cascade design, the NAL report incorporated the long straight sections suggested by Thomas Collins, as in the LRL report. (59; See Synchrotron)

Differences between the NAL and LRL report included new innovations, such as the use of separated function main ring magnets and a new beam extraction design. Unlike beam extraction in the LRL report, in the NAL report the beam for experiments emerged from the accelerator at only one point, a scheme made feasible by a clever new septum for extracting the beam developed by former Brookhaven researcher Alfred

Maschke. In addition to saving money, the scheme was simpler and therefore more reliable. (60)

However, the most striking differences between the two designs stemmed from Wilson's insistence upon achieving maximum capability for minimal cost. The original Berkeley design featured a 200 GeV accelerator laboratory costing $348 million. Although the Berkeley group later produced schemes for a reduced scope machine that would cost about $20 million less than the total NAL budget, such schemes produced beams that were much less intense. (See Chapter 5) In contrast, the NAL design featured a $248 million 200 GeV accelerator laboratory with a machine that was expandable to as much as 500 GeV and produced a beam slightly more intense than the original Berkeley design. (61)

Comparison of the designs reveals how Wilson was able to achieve a machine with greater capability for less money. For example, one savings came from minimal magnet costs, which comprise a substantial portion of the total cost of the accelerator. While the Berkeley design required more than 17 tons of steel for the main ring magnets, the NAL design required less than 9 tons. Figure 8 gives additional pictorial evidence of the difference between the two designs, showing the LRL magnet design in the lower part of the figure and the NAL magnet design in the top figure. Note the more compact design of the magnet, the smaller tunnel size, and the absence of the crane shown in the NAL design. In the LRL design also note the substantial foundation used in the Camp Parks site, which was used for cost estimating. Ramsey explained later that Wilson took risks on about 20 aspects of the

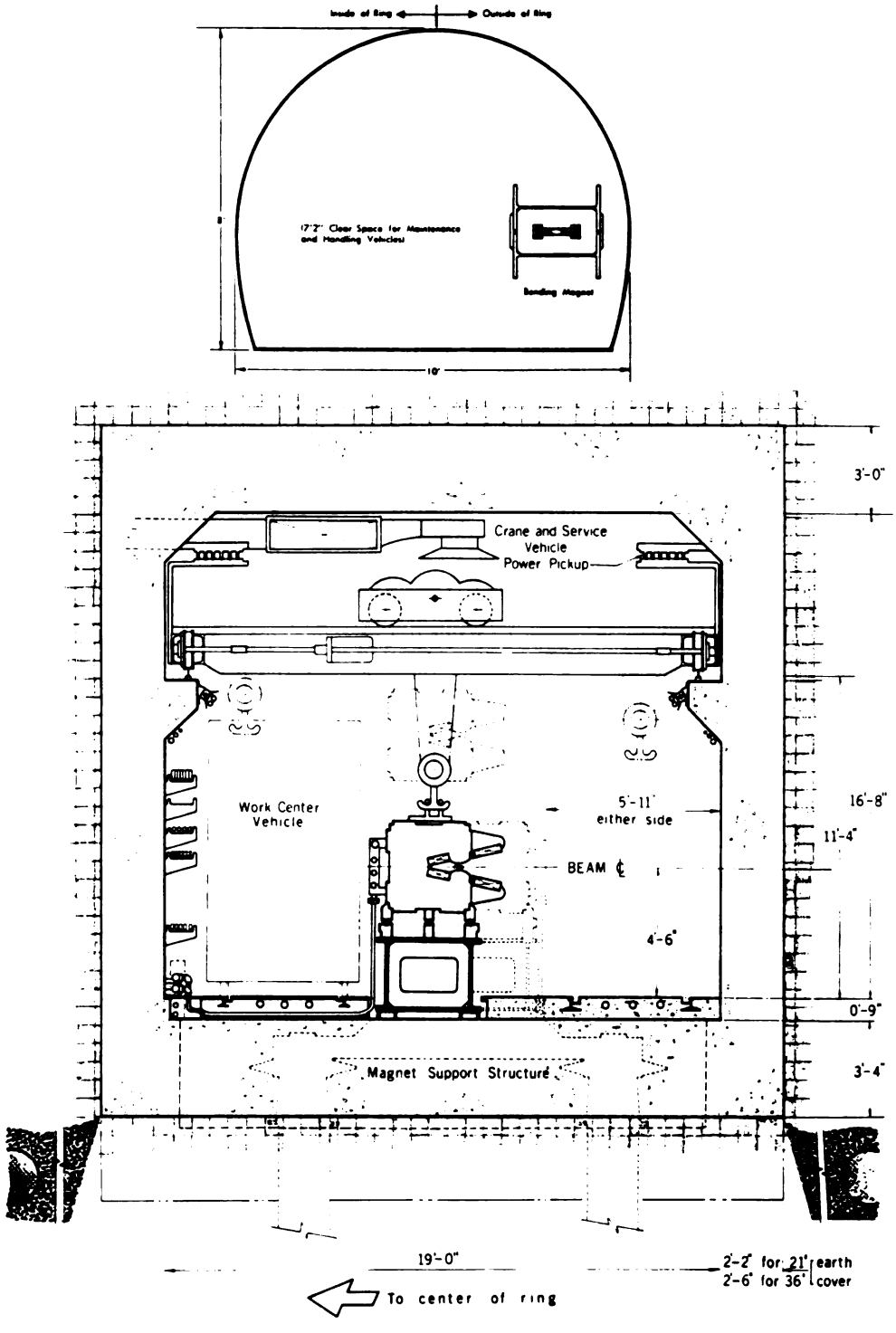


Figure 8.--Comparison of 1968 National Accelerator Laboratory Design (top) and 1965 Berkeley design (bottom). Note difference in tunnel size. From National Accelerator Laboratory, "Design Report" (Batavia: National Accelerator Laboratory, "200 BeV Accelerator Design Study," Vol. II (Berkeley: Lawrence Radiation Laboratory, 1965), UCRL-p. VI-11.

design, saving an average of about $5 million for each 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." As Frank Cole recently concluded when looking at NAL magnet plans: "It was a design pushed to the limit." (62)

Although Wilson was poised to construct the expandable machine he wanted, he also faced a task even more challenging than that faced when he accepted the directorship. To keep within the promised deadline and budget, Wilson employed a number of distinctive schemes. Goldwasser remembers that he and Wilson would sit and "connive" to find ways to save money in contracts for accelerator components. "We figured out all kinds of tricks," Goldwasser explains. Some tricks were particularly effective. "For example, instead of giving the production of magnet laminations to one contractor, we gave one-third to one and the other one-third to another, telling them the contract for the last one-third would be awarded to the competitor who finished faster and cheaper." Wilson also adhered to spartan standards when constructing the machine, as he had when directing the design. For example, J. Richie Orr remembers that Wilson was very pleased when he visited the construction of an experimental area and noted that one piece of equipment had been fashioned out of lead bricks piled on a discarded bookshelf. According to Orr, Wilson proudly remarked that such construction "was precisely his idea of proper engineering." Wilson also rotated high energy physicists to head the business office and other non-technical branches of the laboratory. As Goldwasser recently explained, Wilson felt a

trained physicist could do a much better job in finding ways to save costs in ordering parts. In addition, "a physicist will want to build a business organization that will serve the best interests of physics instead of the interests of the business office itself." (63)

Wilson was also able to save time, both for himself and for the project, by incorporating design components that had already been planned and recruiting experienced workers to construct them. For example, after in-depth study of three alternate radiofrequency systems, LRL researchers chose a system made of ferrite-tuned cavities. Wilson was satisfied with this choice and hired former LRL researcher Quentin Kerns to direct the construction of the NAL system. Although Kerns reduced the power and the number of radiofrequency cavities to accommodate the rest of the design, he ultimately built ferrite cavities designed along the lines of the LRL design. Similarly, for the first two accelerators in the four accelerator cascade of particles, Wilson decided to build a Cockcroft-Walton and 200 MeV linac similar to the one developed at MURA, in collaboration with similar projects at the Los Alamos meson facility and BNL, and hired former MURA researcher Donald Young to direct NAL development of the machines. With the radiofrequency system, Cockcroft-Walton, and linac, Wilson benefited from previous substantial research. In addition, after choosing Kerns and Young, Wilson remained fairly remote from their respective efforts, thus gaining time to focus on other accelerator components that were being developed without the benefit of extensive previous planning, such as the booster and main ring. (64)

Wilson also encouraged other time-saving tactics in linac development. Accelerator components are typically constructed in standard stages. In the modeling stage aspects of the design are built (often in miniature) to test their basic characteristics. After design refinements, prototypes are built to test the full-size component. After further refinements, final construction begins. However, in the case of linac development, the decision was made to skip both the modeling and prototype stages, which MURA researchers had already completed. By the time the linac group formed, Young decided, based on modeling done at BNL, to make revisions to the design. Nonetheless, when the group set up the first major pieces of the Cockcroft-Walton and linac (the ion source, the Cockcroft-Walton voltage system and accelerating tower, the transport system, and the first linac tank), the system worked. As Young recently explained: "We called it a prototype, but we all knew it was going to be used in the final machine." With this short-cut, the first linac cavity produced protons at 10 MeV, its design energy, by June 1969. The linac group had to assemble more linac tanks before the accelerator could produce 200 MeV protons and reach the design energy of the complete machine. Nonetheless, components for the final system were already ordered and the linac group was in a position to proceed rapidly. (65)

Philip Livdahl, second in command of the linac group, recently noted the advantages of rapid linac development. Since protons were first accelerated in the Cockcroft-Walton and linac, these components needed to be finished first. In addition, Wilson exploited the rapid

progress of linac development, using the linac progress as a "whip" to goad other groups into faster progress. (66)

The use of the linac group was only one example of Wilson's technique of using competition to spur progress. In the beginning of the project, he actively encouraged the groups working on individual components to have a strong group loyalty. As Orr recently remembered: "The laboratory was very balkanized, very competitive. Everyone wanted his group to finish first." To create additional pressure, Wilson constantly confronted his employees with challenges. As Goldwasser recently pointed out, some of these challenges were technically absurd. For example, to motivate contractors to finish the main ring tunnel, Wilson ordered that the magnet be placed inside as soon as the hole was dug and the concrete pad and arches were finished. "There was no particular technical advantage to having the magnet in place," Goldwasser explains, "at that point [the tunnel] was little more than a mud hole, swimming in water." Nonetheless, Goldwasser feels the tactic "had an enormous impact on the workers finishing the trenching and concrete work. They must have felt that scientists were right on their tails." (67)

To optimize progress Wilson also had to make adjustments to accommodate changing conditions and solve personnel problems. For example, in fall 1970, when the linac could accelerate protons to over 100 MeV, and the booster and main ring were partially completed, Wilson felt the laboratory had reached the stage when increased coordination of groups was needed and saw that personnel problems were erupting in the

midst of inter-group tension. First he set up a new technical challenge, both to force coordination among groups and to see where the technical weak spots were. In recent interviews, employees remembered the resulting "Oktoberfest" with a certain amount of irritation. The NAL Progress Report for September, announced "the goal of accelerating protons to 139 MeV in the Linac and transporting them through half the Booster and into the Main Accelerator." As Ernest Malamud recently explained: "People were in a hurry, and although it was a good idea, some people were irritated that the scheme created artificial technical problems," since the components were not designed for the low energy used. Although the staff failed to reach Wilson's goal, Malamud points out that "an enormous amount of equipment was installed in record time." (68)

Wilson remembers that the exercise also confirmed his suspicion that the beam transport system, which was headed by the brilliant, but sometimes eccentric Maschke, was particularly behind schedule. From the beginning, Wilson kept the organization of a laboratory flexible and informal, another characteristic reminiscent of Lawrence. As he recently explained: "It's best to have an administrative structure that is clear to all workers, but you don't have to pay much attention to administration until you run into trouble." Within the next few months Wilson and Goldwasser decided that they were indeed in trouble. Wilson remembers that Maschke became increasingly skeptical of the feasibility of Wilson's time schedule for completion of the beam transport system. His complaint was identical to the complaint of other accelerator

experts at this point: he objected to abandoning a project before knowing that it would work well. As Wilson recently explained, he sympathized with the desire to do a good and complete job, but he "wanted the the machine to be integrated at the earliest possible moment," and thus "tried to keep everybody jumping by making them feel there were protons right behind them." Goldwasser remembers that the problem with Maschke was exacerbated when he and Wilson began to hear reports that Maschke, in his zeal to produce high quality work quickly, was "terrifying" some of his young group members while, at the same time, alienating other group leaders. To handle such problems, Wilson formed a new Operations section in February, 1971. The section, which was placed under the direction of Young, assumed control and coordinated parts of the accelerator as soon as they came into operation. In addition, in June Wilson set a hard deadline for Maschke to complete beam transfer through the main ring, announcing that he would be transferred to one of the experimental areas and his group re-assigned to other tasks. Although the task was successfully completed, Maschke left soon thereafter. (69)

Wilson later reported that with these administrative changes underway he was quite "confident" about the laboratory's prospects as mid-1971 approached. Construction progress was extremely encouraging. As he explained:

> Both our Linac and Booster had been brought into operation [at full design energy] before . . . July [1971]. Our main-ring magnets were all in place in the tunnel and all were connected to the power supply. The radiofrequency devices which do the actual pushing of protons were also in place. The computer controls were ready. Indeed we were able to inject protons into the main ring . . . even make them go around the 4-mile-long trip several times. (70)

Fueled by this confidence, Wilson told both the JCAE and the NAL Users Group in spring that the laboratory might produce a 200 GeV beam in mid-1971. Instead of this anticipated triumph, the summer of 1971 brought a series of technical disasters that called Wilson's design style into question and jeopardized the success of the laboratory. (71)

Although NAL and URA leaders had expected some aspects of the accelerator to fail, in line with Wilson's design style, they were not prepared for the traumatic summer and fall of 1971. The main ring contained more than 1,000 magnets. When researchers tried to bring them into operation they found, to their horror, that a high percentage failed. This was frightening because of the number and expense of the magnets, and because researchers knew the entire project would fail unless the problem was solved. After several months of testing, one major source of trouble was discovered. As the 1971 URA Annual report later explained: "With the high summer humidity the magnets in the cold tunnel became saturated with moisture and any small cracks in the epoxy-impregnated fiberglass provided a conduction path to ground that could lead to a serious spark discharge." At the time Wilson felt that a contributing factor to the coil design problem was the lack of air conditioning in the tunnel, one of the controversial cost-saving

measures that emerged from the Oak Brook summer study. As he later told the JCAE, the $1 million cost of air conditioning was "clearly a mistake." Steps were subsequently taken to heat and dry the tunnel, methods for reducing cracks were devised, and more thorough initial tests were developed. By the beginning of 1972, magnet failures had declined and technicians had learned how to replace magnets quickly when they did fail. However, as Wilson admitted to the JCAE, by that time approximately 350 magnets had failed, 6 months had been lost, and about 10% of the original cost of the magnets had been spent to overcome the difficulties. (72)

Although main ring magnets caused the most worry, other problems also plagued the NAL staff during this period. As the 1971 URA report explained, main ring problems "were frustrated by linac and booster failures" which continued until late 1971 "since beam studies of the main ring could be undertaken only when the linac and booster were operating well." Another problem was caused by obstacles in the main-ring vacuum tube. As Wilson recently explained, "there were little shavings of stainless steel from cutting and welding the magnets" during magnet replacement. To solve the problem researchers first tried to train a ferret, affectionately named Felicia, to drag a harness through the tube to collect the pieces. When Felicia refused to go through some areas, an ingenious mechanical device was developed to replace her. (73)

The URA Annual Report, which was issued on January 18, 1972, noted that the previous year "had been a difficult one at the National Accelerator Laboratory." Although staff members first rallied to solve

problems, many found the continuing main ring problems exhausting and demoralizing. As Helen Edwards, who worked on the problem, explains: "The whole period is a kind of blur. All I remember is infinite trips around the main ring to find out which of the magnets had failed." The resulting stress also exacerbated tensions between Wilson and his staff. In line with the feelings of some at the 1967 summer study, some staff members felt oppressed by Wilson's leadership style, which they saw as impulsive and domineering. One participant remembers that at the height of the magnet crisis someone half-jokingly passed around a copy of Albert Speer's memoirs so that staff members could note the similarities between Adolph Hitler and their leader. (74)

The magnet problems also caused trouble outside the laboratory. Many users were terribly annoyed at the delay, since they had hurried to prepare experimental equipment based on Wilson's optimistic projections. In addition, the problems gave credence to arguments against 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. Outside pressure from irate users and opposition to Wilson, both inside and outside the laboratory, further eroded the morale of those desperately trying to fix main ring problems. As Edwards explains: "It was a grim time." (75)

In an attempt to further integrate the laboratory and form an efficient organization for solving main ring problems, Wilson devised a new management plan. In its flexibility and de-emphasis on hierarchy, the plan resembled tactics used at Los Alamos during World War II.

First, Wilson formed an Accelerator Section, merging the Accelerator Theory, Operations, Radiofrequency, and Beam Transfer Sections. He placed himself at the head of this new section and appointed Young as Associate Head and Malamud as Assistant Head. As he explained at the time, in an attempt "to form a horizontal organization where a vertical one had existed before," he appointed three "strong Managers," J. Richie Orr, Richard Lundy, and Philip Livdahl to help him implement plans. "In parallel to this, each component of the accelerator was assigned to a Commissioner." Commissioners were "expected to identify work problems and to come to one of the Managers to get the work force . . . to do the actual work . . . ." To further expedite the effort, he pulled workers from others groups, such as those working on the experimental areas, to increase the pool of those working on main ring problems. As Orr remembers, with this enlarged work force they began attacking problems "one at a time, addressing easier to solve problems first." To coordinate the effort, Wilson met daily with the three managers and other key leaders. "Gradually some of the little problems went away and as this happened we were able to concentrate more and more on big problems." (76)

Although some NAL staff members were unhappy with the reorganization, progress reports record steady gains throughout the rest of the winter. After intensive beam studies in December 1971, researchers were able to produce a 20 GeV beam on January 22, 1972. In addition to the encouraging increase in energy, researchers were happy to note that the beam seemed stable from pulse to pulse indicating "that

the injector, magnets and magnet power supply, and radiofrequency acceleration system all work properly together." In addition, the magnets seemed to be performing well. Improvement continued. On February 11, a 100 GeV beam was obtained, breaking the world's record for proton energy held by the USSR. After steady increases in energy throughout the month, 200 GeV was achieved for the first time on the afternoon of March 1, 1972. As it happened, JCAE authorization hearings were in progress at the time. (77)

Orr remembers that someone sarcastically commented that Wilson's efforts in early 1972 were aimed at "trying to set the indoor proton speed record." To get the first 200 GeV beam, several modifications were made, including schemes for keeping the magnets cool, since the water-cooling system had not yet been installed. Nonetheless, reaching 200 GeV was an important milestone, for it sent a message to physicists, both inside and outside the laboratory, and to government leaders that the project would be a success. After reaching 200 GeV, Wilson wasted no time in pushing to the next goal. In July, 300 GeV was obtained, and by the end of the year the accelerator reached 400 GeV. When the accelerator reached 100 GeV in February, the first experiment started. This experiment was devised by Malamud and Soviet colleagues and conducted by the University of Rochester, Rockefeller University, the Joint Institute for Nuclear Research at Dubna and NAL. It used an internal target to measure the scattering when the proton beam struck a proton target to determine whether the size of the proton varied with the energy of collision. (78)

Although it would take some time before beam was available to the experimental areas, and the laboratory struggled throughout 1972 to raise the intensity to the promised level, Wilson and his staff had reason to celebrate by the June 1972 deadline. The project had come in ahead of schedule and about $20 million below budget, and an exciting NAL physics experiment was already underway. In line with a 1969 AEC decision, the laboratory was renamed at this juncture to honor Enrico Fermi. Although physicists still debate the wisdom of the risks Wilson took with the design of the Fermilab accelerator, even critics agree that due to the ultimate success of the machine, Wilson's design style has set a new standard for accelerator building. (79)

Summary: Design and Construction

While facing funding allocation problems and the many challenges of establishing the non-technical aspects of the laboratory, Wilson never lost sight of his central obligation to build a 200 GeV expandable machine for $243 million. To make the necessary refinements to the existing design, Wilson gathered accelerator experts in rented offices in Oak Brook, Illinois. In a matter of months a design emerged that clearly reflected Wilson's design style. The new design had many innovative, cost-effective features, including compact, separated-function main ring magnets which were designed so that they could go to energies as high as 500 GeV with the later addition of power supplies. Wilson promised that he would build this expandable machine within the budget by mid-1972. Many were astounded by his claims.

Although Wilson felt the design incorporated the spirit of accelerator building taught by Lawrence, the nation's top accelerator physicists, from Berkeley and other institutions, insisted that the design incorporated too many risks considering the cost and importance of the project. Despite such complaints, the URA and the AEC endorsed Wilson's plans and the project continued.

During the construction phase, Wilson devised numerous schemes to save money and speed progress, using competition to encourage timely and economical work from contractors and laboratory groups, and incorporating some previously designed accelerator components. When the time came to integrate accelerator components into a single machine, Wilson established numerous technical tests to force coordination among groups and to see where the technical weak spots were. Despite some complaints from NAL staff members, who objected to abandoning a project before knowing that it would work well, by early 1971 impressive progress had been made in building the machine. In spring, Wilson enthusiastically announced that the accelerator might accelerate protons to 200 GeV by June 1971, a full year ahead of schedule.

In mid-1971, however, the laboratory was in crisis. Although various technical problems erupted, the most serious difficulty was main-ring magnet failures. By early 1972, approximately 350 magnets had failed, 6 months had been lost, and about 10% of the original cost of the magnets had been spent to overcome the difficulties. In the midst of these problems, the animosity of users grew and criticism about Wilson's

design style resurfaced, prompting opposition to Wilson both inside and outside the laboratory.

Although suffering from morale problems, the NAL staff slowly solved the host of technical difficulties after Wilson instituted a flexible, non-hierarchical management plan. After steady increases in beam energy in January, the accelerator produced a 100 GeV beam in February, and the first experiment began. On March 1, 1972, the accelerator produced the first 200 GeV proton beam. Despite the trauma of 1971, the project came in ahead of schedule and on budget. Most exciting of all, the United States had the most powerful accelerator in the world, already running interesting physics experiments. The 1972 christening of Fermi National Accelerator Laboratory, or Fermilab for short, marked the official end of a twelve year gestation. Fermilab, the nation's premiere accelerator laboratory, was born.

Conclusion: Establishing the Laboratory--1967-1972

The laboratory which was officially established in 1972 reflected the pressures and conditions which shaped its genesis. In line with the tightening funding environment of 1967, the budget-conscious Wilson was chosen as the laboratory's first director. In turn, his background, personality, and values had a strong influence on how NAL was formed. For example, thanks to his strong personality and the importance he placed on creating a beautiful environment to inspire creativity and harmony, Wilson played a dominant role in defining how the laboratory would look. Perhaps the most vivid expression of Wilson's humanistic and

aesthetic ideals is the design of the high-rise, which provides a panoramic view of the Illinois countryside, creates a central meeting area for the staff, and supplies a symbol that helps define and unify the laboratory.

The creation of the new laboratory also reflected the pressure for outside user rights which arose in the 1960s. As Lederman stressed in 1963, since only one such accelerator would be built, user support would hinge on the assurance that the facility would be accessible "as a right to any physicist bearing a competitively acceptable proposal." In light of this pressure for a "Truly National Laboratory," Wilson and Goldwasser took numerous steps to involve outside users in NAL planning and bolster user confidence in the new laboratory. In the first year of his directorship, Wilson formally agreed to devote 75% of the research program to outside users, signalling the intent of NAL leaders to make outside users the primary focus of the research program. In addition, Wilson established annual users meetings, summer studies, gathered experimental proposals, and selected a prestigious Program Committee well before the machine was scheduled to operate. (80)

Wilson was also compelled to respond to various tensions arising from the site selection contest. In response to complaints by civil-right activists, Wilson, with the help of Seaborg and Goldwasser, campaigned for the passage of both state-wide and local open-housing legislation. Civil-rights efforts within the laboratory included an aggressive minority-recruitment program and various training programs. Wilson responded to land acquisition problems in two ways. With the

help of other NAL leaders, he made special arrangements to gain access to the village of Weston before acquisition procedures were completed. Although this created challenges for the NAL staff, who had to share quarters with families on the brink of eviction, construction progress did not flag. When the title of land for the site had not been transferred by early 1969 due to resistance from farmers, Wilson complained loudly to state officials, and in April, the title was officially transferred. Although the continued presence of barns, houses, and other reminders of farm families remained, construction was able to continue at full speed.

Another important legacy of the planning years was support for continued funding from the JCAE and the AEC: since both groups had sponsored initial funding, they now had a political stake in the success of the project. This support proved particularly useful when the rapid growth of physics funding halted abruptly. Since the political power of the AEC and JCAE was declining amid growing public skepticism about the social value of science and technology, the project was also fortunate to obtain full support from the physics community. Although physicists debated the relative value of basic and applied research in the mid-1960s, little complaint emerged in the 1970s about the desirability of assigning top funding priority to high energy physics and the NAL accelerator. The general support for the project is particularly impressive in light of the sacrifices physicists had to make to fund the expensive project, which was planned assuming a much larger AEC budget. Such support confirms the continuing interest and prestige inspired by

high energy physics: even when faced with deep cuts to other programs, physicists were willing to support funding for the expensive tool needed to further advance the exploration of the fundamental nature of matter. Thanks to the support of the JCAE and AEC and the physics community, funding allocations continued. Although the rate of funding was slower than expected, Wilson, with his characteristic ability to to make the most of a difficult situation, was able to continue design and construction efforts at full speed.

Despite funding support, the budgetary pressures of 1967 placed a tight ceiling on the design and construction budget of the laboratory. This ceiling, along with his innate enthusiasm for producing maximum capability within a strict budget, prompted Wilson to create an ambitious new design for the accelerator, a design critics denounced as overly risky, considering the expense and importance of the project. While many of the controversial design elements caused no difficulty, problems with the wide-scale failure of main ring magnets threatened the success of the project in 1971. Despite this technical crisis, morale problems stemming from the tension of the period, an upsurge of criticism about Wilson's design, outside user discontent, and complaints about Wilson's management style, Wilson and his staff built an expandable 200 GeV accelerator ahead of schedule and within the budget. At the cost of considerable risk, the physics community had at its disposal an accelerator which set a new standard for accelerator construction.

The 200 GeV milestone proclaimed that Wilson had completed his part of the bargain. This proclamation of technical success affirmed the federal decision to continue its investment in ever-larger accelerators. Despite public skepticism about the value of science and technology and the tightening budget and despite the imminent extinction of the AEC and the JCAE, the traditional champions of accelerator funding, the impetus for US accelerator building continued.

Notes for Chapter 6

All interviews are located in Fermi National Accelerator Laboratory History Collection, Batavia, Illinois, unless otherwise noted. The following abbreviations are used: AIP, (Center for History of Physics, American Insitute of Physics, 335 East 45 Street, New York City , New York); Bancroft Library, (Unversity of California, Berkeley, Calfornia); BNL (Brookhaven National Laboratory, Upton, Long Island); DOE Archives, (United States Department of Energy Archives, Germantown, Maryland); FHC, (Fermilab History Collection, Batavia, Illinois); Green Papers, (files G. Kenneth Green, Brookhaven National Laboratory, Upton, New York); LANL, (Archives, Los Alamos National Laboratory, Los Alamos, New Mexico); LBL, (Lawrence Berkeley Laboratory, Berkeley, California), LBL Archives, (Lawrence Berkeley Laboratory Archives, LBL); Lofgren Papers, (files of Edward J. Lofgren, LBL); LBJ Library, (Lyndon Baines Johnson Library Archives, Austin, Texas); 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, Lawrence Berkeley Laboratory); Wenzel Papers, (files of William Wenzel, LBL).

1. Quotes, respectively, from Norman Ramsey letter to Robert Wilson, February 6, 1967, FHC; Panel on Elementary Particle Physics, "Report of the Panel on Elementary Particle Physics to the Physics Survey Committee of the National Academy of Science," Draft, November 8, 1964, pp. 5-21; and U.S. Congress, Joint Committee on Atomic Energy, Hearings 90th Congress, First Session, (Washington D.C.: GPO, 1967), p. 291. Also see Leon Lederman, "The Truly National Laboratory," in "1963 Super-High-Energy Summer Study," Brookhaven National Laboratory, AADD-6, p. 10 and Lillian Hoddeson interview with Norman Ramsey, February 26-27, 1980.

2. Quotes, respectively, from Spencer Weart interview with Robert Wilson, May 19, 1977, pp. 7, 8, and 3, AIP. Also see this source pp. 1, 2, 4, 6.

3. Robert R. Wilson, "Focusing in the Cyclotron," Physical Review, 51 (1938), pp. 408-420; Herbert Childs, An American Genius: The Life of Ernest Orlando Lawrence, (New York: E. P. Dutton & Co., 1968) pp. 254-255; Spencer Weart interview with Robert Wilson, May 19, 1977, pp. 22-23, 35, AIP; and "Robert Rathbun Wilson, Biographical," February 27, 1967, unpublished document, FHC.

4. If many nuclei had taken a long time to fission, the bomb would not have been feasible. Most physicists believed, due to earlier measurements of fission neutrons, that this would not be a problem. Nonetheless, just to make sure, Los Alamos researchers mounted more

definitive time delay experiments as soon as the project began. Several methods were used to measure the delay, including one devised by Wilson. David Hawkins, Manhattan District History, Project Y, Volume I (Los Alamos: Los Alamos National Laboratory, 1946) p. 103. Text references: "Robert Rathbun Wilson, Biographical," February 27, 1967, unpublished document, FHC; and Lillian Hoddeson interview with Robert Bacher, March 3, 1984, LANL.

5. Spencer Weart interview with Robert Wilson, May 19, 1977, p. 37, AIP.

6. NAL researcher Ernest Malamud reports that the descendants of abandoned cats remain at the laboratory; occassionally one of these feral cats finds its way into the tunnel and gets electrocuted, halting the accelerator in the process. Private communication, Ernest Malamud, February 2, 1988. Text references: Lillian Hoddeson and Richard Hewlett interview with Robert Wilson, August 17, 1984, LANL.

7. Robert Wilson, "An Anecdotal Account of Accelerators at Cornell," in R. E. Marshak, ed. Perspectives in Modern Physics, (New York: Interscience Publishers, 1966,) pp. 233-234; interview with Helen Edwards, March 13, 1987; and Francis Cole, private communication, January 7, 1988.

8. Quotes, respectively, from Robert Wilson, untitled speech, October 1, 1969, unpublished document, FHC; and Robert Wilson, "Particles, Accelerators and Society," 1968 Richtmyer Lecture, unpublished document, FHC.

9. Lillian Hoddeson interview with Robert Wilson, January 12, 1979.

10. Of the $250 million in the total budget, $7 million had been spent by the earlier LRL design effort, leaving $243 for Wilson to spend. Interviews with Goldwasser, May 15, 1987 and Robert Wilson April 1, 1987. Text references: Bradley Bennett, "Monthly Report for April 1967," unpublished document, FHC; and interview with Edwin Goldwasser, July 10, 1985.

11. Quote as quoted in George C. Herring, American's Longest War: The United States and Vietnam 1950-1975, (New York: John Wiley & Sons, 1979,) p. 246. Also see Mary Beth Norton, David M. Katzman, Paul D. Escott, Howard P. Chudacoff, Thomas G. Paterson, and William M. Tuttle, Jr., eds. A People and a Nation: A History of the United States, Volume II, (Dallas: Houghton Mifflin Company, 1982), p. 953.

12. Quotes, respectively, as quoted in Daniel Kevles, The Physicists: The History of a Scientific Community in Modern America (New York: Alfred A. Knopf, 1978) pp. 401 and 399. Also see this source p. 398.

13. Quotes from H. Peter Metzer, The Atomic Establishment, (New York: Simon and Schuster, 1972), p. 17.

14. In 1975 the AEC was dismantled, with regulatory functions transferred to the Nuclear Regulatory Commission and research and development responsibilities transferred to the Energy Research and Development Agency (ERDA), which subsequently became the Department of Energy (DOE). The JCAE was abolished in 1976. Alice L. Buck, A History of the Atomic Energy Commission, (Washington D.C.: U.S. Department of Energy, 1983), p. 8 and Daniel Ford, The Cult of the Atom, (New York: Simon and Schuster, 1982), p. 226. Text quote from Craig Hosmer, "What Ever Happened to Federal Funds?" Physics Today, 21 (1968), p. 23.

15. Quote from 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.: Smithsonian Institution Press, 1979), p. 328. Also see p. 327. High Energy Physics Advisory Panel, "The Status and Problems of High Energy Physics Today," January, 1968, unpublished document, p. 38, FHC.

16. Transcript, Second User's Meeting, December 2, 1968, unpublished document, FHC; U.S. Congress, Joint Committee on Atomic Energy, Hearings. 92nd Congress, First Session, (Washington D.C.: GPO, 1971), p. 1214; Appendix 3 in U.S. Congress, Joint Committee on Atomic Energy, Hearings. 92nd Congress, Second Session, (Washington D.C.: GPO, 1972), p. 1731; and Francis T. Cole, "Monthly Report of Activities," July 31, 1969, National Accelerator Laboratory, NAL-28, p. 1, FHC.

17. Quote from interview with Robert Wilson, April 1, 1987. Also see Ronald Kotulak, "Head of Atom Smasher Lab May Resign," Chicago Tribune, June 15, 1968; Lillian Hoddeson interview with Robert Wilson, November 18-19, 1980; and Gerald Tape, private communication, February 4, 1988.

18. U.S. Congress, Joint Committee on Atomic Energy, Hearings. 92nd Congress, First Session (Washington D.C.: GPO, 1971) p. 1214.

19. Quote from interview with Quentin Kerns, March 12, 1987. Also see interview with J. Richie Orr, March 12, 1987.

20. Quotes, respectively, from Glenn Seaborg, record of conversation, March 21, 1969, Seaborg Papers; and as quoted in "Priority List for High-Energy Physics," Physics Today, 23 (1970), p. 55. Also see Robert Mayo letter to Glenn Seaborg, January 23, 1969; and Richard Nixon memorandum to Heads of Executive Departments and Agencies, January 25, 1969, Seaborg Papers; U.S. General Accounting Office, "Report to the Joint Committee on Atomic Energy, Congress of the United States," Appendix 8 in U.S. Congress, Joint Committee on Atomic Energy, Hearings. 92nd Congress, Second Session, (Washington D.C.: GPO, 1972), p. 2090.

21. Quote from "High-Energy Politics Is Debated at APS Meeting," Physics Today, 23 (1970), p. 64. Also see interview with Edwin Goldwasser, May 15, 1987.

22. Quote from "High-Energy Politics Is Debated at APS Meeting," Physics Today, 23 (1970), p. 64. Also see Appendix 1 in U.S. Congress, Joint Committee on Atomic Energy, Hearings. 92nd Congress, First Session, (Washington D.C.: GPO, 1971), pp. 1589-1590.

23. Quotes, respectively, from Robert Wilson, private communication, February 2, 1988; Norman Ramsey letter to the members of the high energy physics community, March 6, 1967, FHC; and Universities Research Association, Inc., April 4, 1967, press release, FHC. Also see interview with Edwin Goldwasser, July 10, 1985; Otto Kerner, "Remarks by Governor Otto Kerner," April 7, 1967, unpublished document, FHC; and Universities Research Association, Inc., "Report by the President," December 1, 1967, Lofgren Papers; National Accelerator Laboratory, "Design Report," (Batavia: National Accelerator Laboratory, 1968), pp. 3-11.

24. Goldwasser explains that in choosing Aspen, NAL leaders reasoned that the temporary quarters currently being used at Oak Brook, a Chicago suburb, were barely accommodate the burgeoning staff, much less visitors, and there were still no facilities available on site. Since construction on the machine had not yet started, there was no compelling reason to hold the meeting in the muggy, Illinois summer. Moreover, Wilson and Goldwasser discovered that in the summer season housing was actually cheaper at the ski resort than in suburban Illinois. Interview with Edwin Goldwasser, July 10, 1985. Text references: interview with Edwin Goldwasser May 15, 1987 and "Summer Study Program, 1968," December 8, 1967, unpublished document, FHC.

25. Quote from interview with Edwin Goldwasser May 15, 1987. Also see M. Stanley Livingston, "Design Progress at the National Accelerator Laboratory: 1968-1969," (Batavia: National Accelerator Laboratory, 1969), NAL-26, pp. 32-34; Norman Ramsey, "History of the Fermilab Accelerator and URA," May 2, 1975, unpublished manuscript, pp. 11-12, FHC; Francis T. Cole, "Monthly Report of Activities," September 30, 1969, National Accelerator Laboratory, NAL-31, p. 10, FHC; interview with J. Richie Orr, March 12, 1987; and Francis Cole, "Monthly Report of Activities," August 31, 1970, National Accelerator Laboratory, NAL-46, pp. 1-2, FHC.

26. Other members of the original Program Committee were: Thomas Fields, Val Fitch, Wolfgang Panofsky, Robert Sachs, Nicholas Samios, and William J. Willis. Francis T. Cole, "Monthly Report of Activities," December 31, 1969, National Accelerator Laboratory, NAL-35, pp. 1-2, FHC. Also see Francis T. Cole, "Monthly Report of Activities," March 31, 1969, National Accelerator Laboratory, NAL-40, p. 1, FHC.

27. DUSAF was an acronym for Daniel, Urbahn, Seelye and Fuller. It was composed of four firms: Daniel, Mann, Johnson and Mendenhall, Los Angeles architects and engineers; The Office of Max O. Urbahn, New York architects; Seelye Stevenson Value and Knecht, Inc., New York engineers; and George A. Fuller Company, Dallas, construction contractors. George Leposky, "What's the Best Way to Build a $250 Million Atom Smasher," in Inland Architect (August/September, 1969), p. 22. Text quote from interview with Robert Wilson, May 25, 1987. Also see Bradley Bennett, "Monthly Report for April 1967," FHC, and interview with Edwin Goldwasser, May 15, 1987.

28. Quotes, respectively, as quoted in George Leposky, "What's the Best Way to Build a $250 Million Atom Smasher," in Inland Architect (August/September, 1960), p. 24 and interview with Edwin Goldwasser, May 15, 1987.

29. Quote from George Leposky, "What's the Best Way to Build a $250 Million Atom Smasher," in Inland Architect (August/September, 1960), p. 24. Also see interviews with Edwin Goldwasser, May 15, 1987 and Robert Wilson May 25, 1987.

30. Quote from interview with Denis Keefe, Glen Lambertson, Jackson Laslett, December 22, 1986. Also see interview with Edwin Goldwasser May 15, 1987; interview with Robert Wilson, May 25, 1987; Lillian Hoddeson interview with Norman Ramsey February 26-27, 1987; "Questions Raised on the Design of the 200 BeV Accelerator," no date, unpublished document, Mills Papers.

31. Quotes, respectively, from Lillian Hoddeson interview with Robert Wilson, November 11-19, 1980; and Jerry DeMuth, "Dr. King Visits Weston Tent-In; Sees Rights Legislation Erosion," Chicago Sun-Times, June 24, 1967, FHC. Also see interview with Edwin Goldwasser, July 10, 1985.

32. Glenn Seaborg, record of conversation, February 13, 1967, Seaborg Papers; Edwin Goldwasser, "Science and Man: Breaking New Ground at Batavia," in Bulletin of the Atomic Scientists, October, 1969, pp. 8-9; Lillian Hoddeson interview with Robert Wilson, November 11-19, 1980, FHC; and "It May Be A Bell Tolling For Me," Newsweek, April 22, 1967, p. 23.

33. Quote from Robert Wilson and Edwin Goldwasser, "Policy Statement on Human Rights," March 15, 1968, unpublished document; transcript, Second User's Meeting, December 2, 1968, unpublished document, FHC. Also see private communication, Edwin Goldwasser, April 22, 1988 and private communication, Norman Ramsey, April 18, 1988.

34. Quote from Edwin Goldwasser, "Science and Man: Breaking New Ground at Batavia," in Bulletin of the Atomic Scientists, October, 1969, pp. 8-9. Also see Atomic Energy Commission, "The Atomic Energy Commission During the Administration of Lyndon B. Johnson," Volume I,

Part I, Chapter 6, unpublished document, p. 147; and private communication, Edwin Goldwasser, April 22, 1988.

35. The plight of those opposing the land acquisition is dramatized in Theodore J. Lowi and Benjamin Ginsberg, Poliscide, (New York: MacMillan, 1976), which emerges as a diatribe against eminent domain. Text quote from Otto Kerner, "Remarks by Governor Otto Kerner," April 7, 1967, unpublished document, FHC.

36. Lillian Hoddeson interview with Francis Cole, May 11, 1978; and Philip Livdahl, "A Brief Summary of Staffing Levels at Fermilab During Initial Construction Years," (Batavia: Fermilab, 1983), TM-1222, p. 3.

37. On October 17 Kerner appointed a nine-member National Accelerator Laboratory Site Acquisition Committee. On November 3, he appointed an eight-member team to select professional property appraisers. Illinois Information Service, November 3, 1967, press release, FHC. Text references: Lillian Hoddeson interviews with Francis Cole, May 11, 1978 and Norman Ramsey, February 26-27, 1980, FHC; Philip Livdahl, "A Brief Summary of Staffing Levels at Fermilab During Initial Construction Years," p. 6; and interview with Donald Young, March 1, 1987.

38. Francis Cole recently explained that Weston would probably have been outside the boundary of the site, but DuPage County officials, who wanted to get rid of the housing development, pulled political maneuvers so that Weston would be ousted. Interview with Francis Cole, March 13, 1987. Text references: David Young, "Bid to Annex Ruled Illegal in Weston Survival Fight," The Chicago Tribune, May 1, 1968, FHC; "Weston Protest Plea Goes to Washington: Village Board Objects to Land Acquisition Methods Used by State," Wheaton Journal, May 15, 1968, FHC; and Karyl Louwenaar, "The Story of the Village of Weston," June 24, 1968, unpublished manuscript, FHC.

39. Due to claims against the village by creditors and the necessity of action by the state legislature, Weston was not formally dissolved until August, 1969, "Way is Cleared to Dissolve Weston," Wheaton Daily Journal, August 24, 1969, FHC; and Francis Cole, private communication, January 7, 1989. Text references: Arthur Siddon, "Weston Will End Its Fight," Chicago Tribune, June 30, 1968; Arthur Theriault letter to Francis Cole, July 8, 1968; "Agree on Final Home Sales in Weston," Wheaton Daily Journal, August 15, 1968; and Lillian Hoddeson interview with Francis Cole, May 11, 1978.

40. "Homes Left At Weston to Be Used: AEC," Chicago Tribune, June 12, 1968; transcript of the Second NAL User's Meeting, December 2, 1968, unpublished document FHC; and Lillian Hoddeson interview with Francis Cole, May 11, 1978.

41. Lillian Hoddeson interview with Francis Cole, May 11, 1978, FHC; Francis Cole, "Monthly Report of Activities," September 1, 1968,

National Accelerator Laboratory, NAL-15, pp. 1-2, FHC; Francis Cole, "Monthly Report of Activities," October 1, 1968, National Accelerator Laboratory, NAL-16, p. 1, FHC; and Francis Cole, private communication, January 8, 1988.

42. Quotes, respectively, from Arthur Theriault, draft of an untitled speech given February 16, 1969, unpublished document, FHC; and Robert Wilson letter to Arthur Theriault, November 1, 1968, FHC. Also see Lillian Hoddeson interview with Francis Cole, May 11, 1978 and transcript of the NAL Second User's Meeting, December 2, 1968, unpublished document, FHC.

43. Kathleen Burns, "A-Smasher Site Owner, State Clash," Chicago Tribune, October 4, 1968, FHC; "Weston Holdouts Push for High Prices on Land," American, October 14, 1968, FHC; "NAL Land Negotiations Rapped," Aurora Beacon-News, October 9, 1968; "Lawsuit Concerning Land for NAL Filed," Aurora Beacon-News, October 16, 1968; "Guild's Decision Gives Green Light to Land Acquisition at Weston," Wheaton Daily Journal, November 26, 1968; and Lillian Hoddeson interview withs Francis Cole, May 11, 1978 and Norman Ramsey, February 26-27, 1980.

44. Although Illinois officials insisted that appraisals were fair, when newly elected Illinois Governor Richard Ogilvie triumphantly announced the final land acquisition he boasted that Illinois had purchased the land for $25.5 million, $4.5 million under the estimated $30 million. Ray Becker, Carl Bertmann, Illinois/News, March 31, 1969, press release, FHC. Text quote from Robert Wilson letter to Glenn Seaborg, January 6, 1969, FHC. Also see Robert Wilson letter to Charles Percy, February 3, 1969, FHC and M. Stanely Livingston, "Design Progress at the National Accelerator Laboratory: 1968-1969," (Batavia: National Accelerator Laboratory, 1969), p. 7.

45. Robert Wilson letter to Kennedy C. Brooks, July 2, 1969, FHC; Kennedy C. Brooks letter to Wilson, July 15, 1969, FHC; Robert Nolte, "Last Weston Folks Prepared to 'Make Room for Progress,'" Chicago Tribune, August 24, 1969, FHC; John R. Frazier letter to James Jerz, December 1, 1970, FHC, Nancy Baumann letter to Kennedy Brooks, July 1, 1969, FHC; and Kennedy Brooks letter to Nancy Baumann, July 3, 1969, FHC.

46. Quotes from Victor Weisskopf letter to Paul McDaniel, March 2, 1967, FHC. Also see Lillian Hoddeson interview with Robert Wilson, January 12, 1979, FHC and interview with John Blewett, November 17, 1986.

47. Interview with Denis Keefe, Glen Lambertson, Jackson Laslett, December 22, 1986; interview with John Blewett, November 17, 1986; Don Getz, untitled manuscript, May, 1977, unpublished document, FHC; and Robert Wilson, "Some Aspects of the 200 GeV Accelerator," paper presented to the VI International Conference on High Energy Accelerators, September 12, 1967, unpublished document, FHC.

48. Wilson remembers that although the AEC favored a deserted school which could be rented at a low rate, he judged the building unsuitable because it was run-down and smelled like a gymnasium. Although the offices at Oak Brook cost twice as much, he insisted upon renting them, feeling that physicists would come to the old school and "just stick the nose up in the air and walk away." Lillian Hoddeson interview with Robert Wilson January 12, 1979, FHC. Text references: Lillian Hoddeson interview with Robert Wilson January 12, 1979; Don Getz, May, 1977, untitled manuscript, unpublished document, FHC; Lillian Hoddeson, "Establishing KEK in Japan and Fermilab in the U.S.: Internationalism and Nationalism in High Energy Accelerator Physics During the 1960s," Social Studies of Science, (London: Sage Publications, 1983), p. 20.

49. Quotes, respectively, from untitled manuscript, Don Getz, May, 1977, unpublished document, FHC; and Robert Wilson, "Some Aspects of the 200 GeV Accelerator," presented at the VI International Conference on High Energy Accelerators, Cambridge Massachusetts, September 12, 1967, unpublished document, FHC.

50. Quote from interview with Robert Wilson, May 25, 1987. Also see interview with Robert Serber, February 24, 1986.

51. Quote from interview with Robert Wilson, May 25, 1987.

52. James Sanford explains that "the separated function design allows for high-peak magnetic fields in the bending magnets." James, "The Fermi National Accelerator Laboratory," in Annual Review of Nuclear Science, (1976) 26, p. 165. Text references: Don Getz, May, 1977, untitled manuscript, unpublished document, FHC; Robert Wilson, "Some Aspects of the 200 GeV Accelerator," presented at the VI International Conference on High Energy Accelerators, Cambridge Massachusetts, September 12, 1967, unpublished document, FHC; interview with Francis Cole, August 29, 1986, Francis Cole, "Progress Report on the NAL Accelerator," Particle Accelerators, 2 (1971), pp. 1, 2.

53. Don Getz, May, 1977, untitled manuscript, unpublished document FHC; "Questions Raised on the Design of the 200 BeV Accelerator," no date, unpublished document, Mills Papers; and interview with Francis Cole, August 29, 1986.

54. Lloyd Smith, one of the Berkeley designers who witnessed Wilson's decision on the radius of the main ring, remembers that he was merely "mildly amused" at Wilson's behavior. He notes that in accelerator building: "Many decisions have to be arbitrary if a project is to move at a decent rate; really bad ones usually die before its too late." Lloyd Smith, private communication, February 24, 1988. Text quote from interview with Robert Wilson, May 25, 1987. Also see interview with Francis Cole, March 13, 1987; and Lillian Hoddeson interview with Thomas Collins, April 12, 1978.

55. Quote from interview with Quentin Kerns, March 12, 1987. Also see U.S. Congress, Joint Committee on Atomic Energy, Hearings. 90th Congress, 2nd Session, (Washington D.C.: GPO, 1968), pp. 1053-1054; John L. Heilbron, Robert W. Seidel, and Bruce R. Wheaton, Lawrence and His Laboratory: Nuclear Science at Berkeley, 1931-1961 (Berkeley: Office for History of Science and Technology, 1981), p. 11.

56. Don Getz, May, 1977, untitled manuscript, unpublished document, FHC; interview with Francis Cole, August 8, 1986; and "Questions Raised on the Design of the 200 BeV Accelerator," no date, unpublished document, Mills Papers.

57. Quote from Lillian Hoddeson interview with Norman Ramsey, February 26-27, 1970, FHC. Also see Robert Wilson, "Some Aspects of the 200 GeV Accelerator," paper presented at the VI International Conference on High Energy Accelerators, Cambridge Massachusetts, September 12, 1967, unpublished document, FHC.

58. Quotes, respectively, from Atomic Energy Commission, "Atomic Energy Commission Summary Notes of 200 BeV Accelerator Briefing," September 1, 1967, unpublished document Seaborg Files; and Leon Lederman letter to William Salsig, November 8, 1967, Salsig Files. Also see Norman Ramsey, "History of the Fermilab Accelerator and URA," May 2, 1975, unpublished manuscript, FHC; M. Stanley Livingston, "Design Progress at the National Accelerator Laboratory: 1968-1969," p. 27; and National Accelerator Laboratory, "Construction Project Data Sheet, Schedule 44," Draft, September 20, 1967, unpublished document, FHC.

59. National Accelerator Laboratory, "Design Report," pp. 3-2 to 3-10.

60. As explained in a 1971 article the new septum "uses thin wires as the inner electrode of a transverse electric field. This electrode can be made much thinner than the current-carrying septum of a magnetic deflector and, consequently, the particle losses on it are much smaller." The septum was crucial to single emergent beam extraction because a higher particles losses would have produced unacceptable radiation levels. Quote from Francis Cole, "Progress Report on the NAL Accelerator," Particle Accelerators, 2, (1971), p. 9. Also see interview with Francis Cole, August 29, 1986.

61. The LRL report promised a machine producing 30 trillion protons per pulse, while the NAL report promised a machine producing 50 trillion protons per pulse. The reduced scope machine mentioned in the December 13, 1965 report was estimated at $257 million. National Accelerator Laboratory, "Design Report," p. 1-1, Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, p. I-1, and Edward J. Lofgren, "On the Costs of an Accelerator With Reduced Initial Capabilities," unpublished document, December 13, 1965. Text references: National Accelerator Laboratory, "Design Report," pp. 3-10, 3-13, 3-14, Lawrence

Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, (Lawrence Radiation Laboratory, 1965), UCRL-16000, p. I-7.

62. Quotes, respectively, from Lillian Hoddeson interview with Norman Ramsey, February 26 and 27, 1980, FHC; and interview with Francis Cole, August 29, 1986. Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, p. III-10, Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume II, p. VI-11; and National Accelerator Laboratory, "Design Report," p. A-4, Figure 8-1.

63. Orr notes that the brick and bookshelf arrangement was built by Caltech researchers. "It was used as a beam collimator on a 200 foot long array of magnets, collimators, beam monitoring instrumentation and a tungsten target. All of this stuff was mounted on a series of narrow gauge railroad cars. This 'target train' was used to produce, select and focus the hadrons whose decay products would form the narrowband neutrino beam for the Caltech-led experiment E-21. The train could be rolled into a shielded underground tube for use and rolled back out for repair." Private communication, J. Richie Orr, January 29, 1988. Text quotes, respectively, from interview with Edwin Goldwasser, May 15, 1987 and interview with J. Richie Orr, March 12, 1987.

64. Lawrence Radiation Laboratory, "200 BeV Accelerator Design Study," Volume I, pp. VII1-1 to VIII-2; National Accelerator Laboratory, "Design Report," pp. 7-2; and interviews with Quentin Kerns, March 12, 1987 and Donald Young, March 1, 1987.

65. Quote from interview with Donald Young, March 13, 1987. Also see interview with Ernest Malamud, March 12, 1987; and Francis Cole, "Monthly Report of Activities," June 30, 1969, National Accelerator Laboratory, NAL-27, p. 1, FHC.

66. Hoddeson interview with Philip Livdahl, May 15, 1978.

67. Quotes, respectively, from interview with J. Richie Orr, March 12, 1987 and Edwin Goldwasser, May 15, 1987.

68. Quotes, respectively, from Francis Cole, "Monthly Report of Activities," September 30, 1970, National Accelerator Laboratory, NAL-47, p. 1, FHC and interview with Ernest Malamud, March 12, 1987. Also see interviews with Helen Edwards, March 13, 1987, J. Richie Orr, March 12, 1987, Edward Lofgren April, 24, 1984, p. 3; and David Judd, April 24, 1984, p. 19; Francis Cole, "Monthly Report of Activities," September 30, 1970, National Accelerator Laboratory, NAL-47, pp. 2-3, FHC; and Francis Cole, "Monthly Report of Activities," October 31, 1970, National Accelerator Laboratory, NAL-49, p. 1-2, FHC.

69. Quotes, respectively, from interview with Robert Wilson, May 25, 1987, p. 23 and interview with Edwin Goldwasser, April 1, 1987, p. 15. Also see interviews with Edward Lofgren April, 24, 1984, David Judd, April 24, 1984; and J. Richie Orr, March 3, 1987; Francis Cole,

"Monthly Report of Activities," February 28, 1971, National Accelerator Laboratory, NAL-54, p. 6, FHC; and Robert Wilson letter to Alfred Maschke, June 15, 1971, FHC.

70. 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.: GPO, 1972), p. 1433.

71. National Accelerator Laboratory, "The Village Crier," 2 (April 16, 1970), FHC.

72. Wilson now feels that the lack of air conditioning had no affect on the magnet problem. Robert Wilson, private communication, February 1, 1988. Text quotes, respectively, from Universities Research Association, "URA Annual Report 1971," January 1972 in U.S. Congress, Joint Committee on Atomic Energy, Hearings. 90th Congress, First Session, (Washington D.C.: GPO, 1972), pp. 2053 and 1435. Also see this source, p. 1436; interview with Francis Cole, August 29, 1986; Francis Cole, "Monthly Report of Activities," October 31, 1971, National Accelerator Laboratory, NAL-63, pp. 1-2, FHC; and Francis Cole, "Monthly Report of Activities," January 31, 1972, National Accelerator Laboratory, NAL-67, p. 2, FHC.

73. Quotes from Universities Research Association, "URA Annual Report 1971," January 1972 in U.S. Congress, Joint Committee on Atomic Energy, Hearings. 92nd Congress, 2nd Session, (Washington D.C.: GPO, 1972), p. 2053; and interview with Robert Wilson, April 1, 1987, p. 14. Also see Francis Cole, "Monthly Report of Activities," October 31, 1971, National Accelerator Laboratory, NAL-63, p. 3, FHC.

74. Quotes, respectively, from Universities Research Association, "URA Annual Report 1971," January 1972 in U.S. Congress, Joint Committee on Atomic Energy, Hearings. 92nd Congress, 2nd Session, (Washington D.C.: GPO, 1972), p. 2053; and from interview with Helen Edwards, March 13, 1987. Also see Francis Cole, private communication, January 7, 1988.

75. Quote from interview with Helen Edwards, March 13, 1987. Also see interviews with J. Richie Orr, March 12, 1987, Robert Wilson April 1, 1987, and Edwin Goldwasser, May 15, 1987.

76. Quotes, respectively, from Robert Wilson draft letter to Norman Ramsey, October 29, 1971, FHC; and interview with J. Richie Orr, March 12, 1987. Also see Robert Wilson, "Formation of the Accelerator Section," memorandum to the staff, October 21, 1971, FHC.

77. As a joke, Francis Cole reported that the 200 GeV beam was achieved on February 30, "to make it look better on paper." In line with this, the monthly report for February was also dated February 30. Francis Cole, private communication, January 11, 1988. Text quote: Francis Cole, "Monthly Report of Activities," January 31, 1972, National

Accelerator Laboratory, NAL-67, p. 1, FHC. Also see Francis Cole, "Monthly Report of Activities," December 31, 1971, National Accelerator Laboratory, NAL-66, pp. 1-3, FHC; and Francis Cole, "Monthly Report of Activities," February 30, 1972, National Accelerator Laboratory, NAL-68, p. 1, FHC.

78. Quote from interview with J. Richie Orr, March 12, 1987, p. 23. Also see interview with Ernest Malamud, March 12, 1987, p. 7; and U.S. Congress, Joint Committee on Atomic Energy, Hearings. 92nd Congress, 2nd Session, (Washington D.C.: GPO, 1972), p. 1435; and Francis Cole, "Monthly Report of Activities," March 31, 1972, National Accelerator Laboratory, NAL-63, p. 1, FHC.

79. Hoddeson reveals that Wilson used the $20 million to support research on building a superconducting accelerator. Lillian Hoddeson, "The First Large-Scale Application of Superconductivity: The Fermilab Energy Doubler, 1972-1983," in Historical Studies in the Physical and Biological Sciences, 18, Part 1, 1987, p. 35. (This source provides a history of the Fermilab "Energy Doubler.") Text references: Norman Ramsey, "History of the Fermilab Accelerator and URA" unpublished manuscript, p. 14, FHC; Atomic Energy Commission, "AEC Names 200 BeV Accelerator in Honor of Enrico Fermi," April 29, 1969, press release, Seaborg Papers; Richard Nixon letter to Glenn Seaborg, April 18, 1969, Seaborg Files, DOE Archives, Box 171; interview with Denis Keefe, Glen Lambertson, and Jackson Laslett, December 22, 1986; and interview with Francis Cole, August 29, 1986.

80. Leon Lederman, "The Truly National Laboratory," in "1963 Super-High-Energy Summer Study," Brookhaven National Laboratory, AADD-6, p. 9.

CONCLUSIONS

This discussion began with the claim that the twelve-year birth of Fermilab held the answers to two questions: Would cost and resulting pressures force changes in the way a multi-hundred million dollar accelerator project was planned, organized, and constructed? Would the federal government agree to fund a multi-hundred million dollar project even though the economic and social climate of the 1960s was less favorable to science funding than that of the 1950s? After the preceding discussion, it is possible to answer these questions and assess what the birth of Fermilab reveals about the evolution of accelerator laboratories and the federal commitment to accelerator building. This assessment, in turn, suggests new questions about the development of accelerator laboratories and other large research projects.

Cost and resulting pressures clearly forced changes in the way the multi-hundred million dollar project was planned, organized, and constructed. To get a sense of this change, it is first useful to examine the tradition for planning, organizing, and constructing accelerator projects as it existed in the 1950s. In this decade, after capitalizing on prewar credentials and wartime contacts, physicists at Berkeley and Brookhaven proposed, built, and managed the nation's largest accelerators. During this period of technical achievement, which included the development of the synchrotron and strong focusing, an elite group of accelerator specialists emerged at the two institutions.

Along with the increased complexity and expense of accelerator projects came a spirit of conservatism especially evident at Berkeley. Just before his death in 1958, Ernest Lawrence, who fervently promoted accelerator development in the 1930s and 1940s, worried that physicists would fail in their efforts to build the next high energy accelerator. In line with Lawrence's attitude in the 1930s and 1940s, Edwin McMillan, Lawrence's successor, was eager to extend experimental capabilities and enthusiastically mounted a proposal for a new accelerator. Nonetheless, Lawrence's later influence could be seen in the attitude of the Berkeley design team, which prized reliability and eschewed risk-taking.

While accelerator design at Berkeley assumed a spirit more conservative than that found in the 1930s and 1940s, attitudes toward outside users remained firmly grounded in the laboratory's past. Before World War II, when Lawrence obtained private funding, he and his students constructed the first small cyclotrons primarily for their own use. Although Radiation Laboratory accelerators were used from the beginning by researchers from other institutions, outside users were granted access to laboratory machines only by invitation, even after federal funding began in the late 1940s. Some outside users vigorously complained about this practice, which they considered unfair. While some users complained about inequitable access to Brookhaven accelerators, the tendency to favor inside users was less obvious there. Brookhaven had been established in 1946 to serve physicists at a wide range of institutions, and this mission was enforced by AUI, the consortium of northeastern universities that managed the facility.

Although Berkeley researchers attempted to plan siting and organize management of the multi-hundred million dollar accelerator project along traditional lines, their attempts were thwarted by both the expense of the project and the shifting economic and social environment in the 1960s. Midwestern physicists and politicans pushed for a midwestern accelerator after the defeat of MURA; young, active experimentalists demanded that the new facility accomodate outside users; and leaders of the physics community sought ways to bolster support for the machine at a time when the federal budget was tightening and the physics community was divided. With the help of AEC chairman Glenn Seaborg, Frederick Seitz formed a new management organization, later called URA, which had national representation, an extension of the wide-ranging representation of AUI. To determine the site for the accelerator, a new procedure was devised: the AEC organized a site competition. The new management organization and the site competition promised to soothe the concerns of midwestern physicists and outside user advocates and bolster support for the machine. However, in the process, Berkeley designers lost the traditional prerogative of accelerator designers to manage and build an accelerator at the site of their choice.

Continuing economic pressure also worked to defeat the Berkeley team's conservative approach to accelerator design. Impressed by his enthusiasm for creating optimum capability for minimal cost, the URA chose, and the AEC accepted, Robert Wilson as the laboratory's first director, after LRL physicist Edward Lofgren refused to continue with the project after the Weston site was chosen. Since the URA preserved

the tradition of a strong director when defining Wilson's position, Wilson played a dominant role in how the new laboratory would be established, including decisions on the design and construction of the machine. Thus, Wilson was able to radically revise the conservative design developed by the Berkeley team, employing a design style which he, ironically, traces to Lawrence's style in the 1930s. After successfully organizing the laboratory to attack problems arising from his much more risky design, Wilson built an expandable machine within the available budget, setting a new standard for accelerator construction.

With the advent of site competition, the URA, and a new standard of accelerator construction, the new laboratory set a new course for planning, organizing, and constructing future accelerator laboratories. The course of the development of accelerator laboratories had changed because new problems arose as accelerator projects grew larger and more expensive. In the 1950s, the continued development of high energy accelerators hinged on the solution of technical problems, most notably, the development of synchrotrons and strong focusing. In contrast, after 1960, continued development of high energy accelerators hinged on solving nontechnical problems, most notably, bolstering support for the project and adjusting to budgetary constraints. Competing, elite groups of accelerator specialists at Berkeley and Brookhaven facilitated the solution of crucial technical problems in the 1950s. The hegemony of this elite was abolished in the 1960s, when a centrally located accelerator, managed by URA, and built with maximum capability at

minimum cost, facilitated the solution of crucial economic and political problems.

In 1963 Leon Lederman imagined a "Truly National Laboratory," a national resource created to serve the needs of the entire community of high energy physicists. By 1972, such a laboratory had been established. Although no other laboratory had such a sharp focus on outside users, the first "Truly National Laboratory" revised, rather than abolished, tradition. The URA was modeled on AUI, and Wilson's design style reflected the influence of Lawrence in the 1930s. In addition, like its predecessors, Fermilab had a strong director with the power to shape the research program. Although the URA had national representation and the majority of accelerator time was allocated to outside users, the laboratory was not organized as a democracy. Aided by advice from the program committee, Wilson would judge research proposals on the basis of merit, not institutional or geographical equity. In the midst of change, the long-standing tradition of meritocracy, which had annoyed politicians since the days of Senator Harold Kilgore, remained intact.

Just as the formation of the new laboratory was shaped by tradition and new pressures, the funding of the $250 million laboratory forged a new course in the relationship between accelerator builders and the federal government. For the first time, an accelerator project attracted wide-scale attention outside the physics community. Although many physicists were uncomfortable with outside attention and influence, they adapted to the change. It was now necessary to endure the spectacle of

the site competition, the push for "the indoor proton speed record." Because of its expense, the "First Truly National Laboratory" was a major national investment, and the pressure for accountability was greater than ever before. The federal government was also forced to adapt as Fermilab was born. For the first time since World War II, the budget was shrinking and increases in science funding prompted public criticism. Despite public skepticism about the value of science and technology and the tightening budget, the federal government funded the $250 million project needed for further exploration of the fundamental nature of matter.

What explains the continuing federal commitment to accelerator building? The momentum for accelerator building generated in the fifteen years after World War II is one factor. No burst of activity was needed to initiate a large new accelerator project. By 1960 physicists expected continued accelerator funding, conduits for accelerator funding were well formed, and procedures and precedents existed for approving accelerator requests.

But the momentum for accelerator building would have certainly died in the 1960s and 1970s without further impetus. During this period, particle physics discoveries continued, and accelerator technology was clearly advanced enough to support further accelerator development. In the 1950s the funding contract between the physics community and the federal government was cemented by mutual need: the physics community needed money for accelerators while the federal government needed resources for defense and technology and the use of accelerators as a

symbol of technological, and therefore political, strength. Did the physics community and government still need each other in the 1960s and 1970s? Due to the cost of an accelerator in the 200 GeV range, physicists clearly needed the federal government more than ever before. Why the federal government needed physicists is less clear, although the willingness to continue the partnership implies that government leaders were still persuaded by the promise of particle physics. When promoting Fermilab, physicists did not stress the promise of military or technological spinoffs, concentrating instead of the cultural value of exploring the fundamental nature of matter. Since no dramatic spin-offs from accelerator development emerged during this era, it seems unlikely that government leaders made a $250 million investment strictly to advance technology.

Another possible answer emerges from the consideration of Paul Forman and others. For example, Forman notes that when federal funding for physics increased after World War II, American physics became enmeshed "in the nation's pursuit of security through ever more advanced military technologies." At first glance, the birth of Fermilab seems to contradict this assertion, unless accelerator development is seen as an important national symbol. Considering the social and economic pressures of the 1960s and 1970s, the government decision continue its investment in accelerators makes much more sense if continued accelerator development bolsters the image of the United States as technologically and militarily strong. Further research is needed to test this fascinating idea. One way to probe the symbolic value of accelerators

would be to examine the dialogue between government representatives and those promoting high energy physics projects. Does the language used reflect such symbolism? Is the dialogue different than that exchanged when decisions are made about other research expenditures? (1)

Assessment of the birth of Fermilab prompts numerous other questions for future investigation about the development of accelerator laboratories and other large research projects. How did the Fermilab's focus on outside users affect the laboratory and its research program? How did cooperative research evolve in this new institutional context as researchers faced increasingly complicated technological problems, such as the application of superconductivity to accelerators, and began international collaborations of unprecedented size, such as the Colliding Detector Facility, a joint Japanese-US-Italian project. Exploration of Fermilab's later history promises to answer such questions and reveal whether the nation's first "Truly National Laboratory" continued to set a new direction for large accelerator laboratories. Since the physics community is currently making plans for the next major high energy physics laboratory, questions arise about the continued development of accelerator laboratories. Has planning for the Superconducting Supercollider (SSC) followed the precedents set by Fermilab? Is the multi-billion dollar project subject to new pressures? If so, will such pressures further modify the planning, organization, and construction of accelerator laboratories?

Due to the wealth of recent research on the post-World War II development of physics laboratories, comparative studies also offer

numerous avenues of exploration. Was the course taken by Fermilab shaped by national conditions? Did the pressures that shaped Fermilab also affect other AEC laboratories? In particular, what affect did the movement for outside user rights have at other AEC laboratories? (2)

While such comparative studies promise to uncover information on the evolution of physics laboratories, comparing Fermilab with other large research projects promises to reveal whether research projects experience similar changes as they become larger and more expensive. Did the pressures that shaped Fermilab also affect other large research projects of comparable size and expense? If so, were similar modifications made in planning, organizing, and constructing such projects?

The history of Fermilab provides one particularly intriguing clue about the effect of increasing size and expense. The birth of Fermilab suggests that nontechnical concerns become more important in relation to technical problems in planning, organization, and construction when accelerator laboratories grow larger. Economic and political concerns shaped the planning and organization of the laboratory, economic concerns shaped the design of the accelerator, and successful management strategies were crucial to the successful construction of the machine. When examining the later history of Fermilab, Lillian Hoddeson provides further evidence of the importance of nontechnical concerns in large laboratories. In the 1980s Fermilab was the first of seven competing laboratories to successfully develop the superconducting magnet. Hoddeson concludes that this success hinged on prudent organizational

strategies, which allowed Fermilab researchers to implement clever technical innovations. Did nontechnical concerns assume increasing importance with the growth of space research, medical research, or other large research projects? The birth of the nation's "First Truly National Laboratory" answered a few questions, and prompts many more. (3)

Notes for Conclusions

1. Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," in Historical Studies in the Physical and Biological Sciences, 18, Part 1, 1987, p. 150. Also see John L. Heilbron, "A Historian's Interest in Particle Physics," Robert W. Seidel, "The Postwar Political Economy of High Energy Physics," and Sylvan Schweber, "Some Reflections on the History of Particle Physics in the 1950s," in Laurie Brown, Max Dresden, Lillian Hoddeson eds. Pions to Quarks: History of Particle Physics in the 1950s to be published by Cambridge University Press, 1988, and Robert Seidel, "A Home for Big Science: The Atomic Energy Commission's Laboratory System," in Historical Studies in the Physical and Biological Sciences, 18, Part 1, 1987, p. 136.

2. John L. Heilbron, Robert W. Seidel, and Bruce R. Wheaton, Lawrence and His Laboratory: Nuclear Science at Berkeley, 1931-1961 (Berkeley: Office for History of Science and Technology, 1981). (These authors have a longer manuscript scheduled for publication in 1988.) Lillian Hoddeson, "Establishing KEK in Japan and Fermilab in the U.S.: Internationalism and Nationalism in High Energy Accelerator Physics During the 1960s," Social Studies of Science, (London: Sage Publications, 1983); A. Needell, "Nuclear Reactors and the Founding of Brookhaven National Laboratory," Historical Studies in the Physics Sciences, 14, Part 1, 1983; Robert Seidel, " A Home for Big Science: The Atomic Energy Commission's Laboratory System," Historical Studies in the Physical Science, 16, Part 1, 1986; and Armin Hermann, John Kriege, Ulrike Mersits, Dominique Pestre, History of CERN, (Amsterdam: North-Holland, 1987.

3. Lillian Hoddeson, "The First Large-Scale Application of Superconductivity: The Fermilab Energy Doubler, 1972-1983," in Historical Studies in the Physical and Biological Sciences, 18, Part 1, 1987.

Glossary of Major Characters and Their Role in the Birth of Fermilab

Luis Alvarez. Alvarez worked at Berkeley in the 1930s as one of Ernest Lawrence's "boys" in the early Radiation Laboratory. During World War II Alvarez worked on both the radar and atomic bomb projects. Thanks to the funding Lawrence obtained from General Leslie Groves, Alvarez developed the first high-energy proton linear accelerator in 1946, which became the model for subsequent proton linear accelerators. After hearing about the bubble chamber from Donald Glaser in 1953, Alvarez built the first hydrogen bubble in the early 1950s. At the end of the decade, after obtaining a special appropriation of $2.5 million from the Atomic Energy Commission, he jumped in one big step to a 72-inch chamber. Hydrogen bubble chambers, which were built at many accelerator laboratories, greatly extended the experimental capabilities of accelerators.

John Blewett. After the McCone-Emelyanov agreement was signed in 1959, the Atomic Energy Commission asked Blewett to head the US part of the joint US-USSR effort to design a new high energy accelerator at Brookhaven. Although the Soviets abandoned the project by 1961, design work at Brookhaven continued under the direction of Blewett, an experienced accelerator physicist who had helped lead the design and construction of the Brookhaven 30 GeV Alternating Gradient Synchrotron. In February 1962 Brookhaven submitted a request for design funding for an accelerator in the 300 to 1000 GeV range. In 1963 the Atomic Energy Commission agreed to fund a design effort at Berkeley for a 200 GeV accelerator to be built immediately and a design effort under Blewett's direction for a 1000 GeV accelerator to be built several years later. However, by 1965 physicists (at Brookhaven and elsewhere) felt uncertain that the 1000 GeV accelerator would be built as planned and the efforts of Blewett and other Brookhaven physicists shifted to the unsuccesful attempt to build the 200 GeV accelerator at Brookhaven.

Vannever Bush. Bush was the principal architect of the Office of Scientific Research Development, which moblized the nation's scientific resources for national defense in World War II. The National Science Foundation was organized along the lines suggested by Bush in Science the Endless Frontier, which he wrote in 1945.

Vasily S. Emelyanov. Soviet counterpart to Atomic Energy Commission chairman John A. McCone. McCone and Emelyanov signed an agreement in November 1959 establishing several US-USSR cooperative ventures, including a joint accelerator project.

Enrico Fermi. Fermilab was named for Fermi, who won the Nobel Prize in 1938 for his contributions to nuclear physics. After working on the atomic bomb project in World War II, Fermi accepted a position at the University of Chicago's Insititute for Nuclear Studies. By the time of his death in 1954, Fermi was aware that many Midwestern physicists were resentful because they lacked the first-rate accelerator they felt they

deserved. Just before his death, Fermi appealed to Edwin McMillan to soothe Midwestern tempers and help mend the growing rift within the the community of physicists grouped around the nation's accelerator laboratories.

Edwin Goldwasser. As a University of Illinois physicist, Goldwasser was a member of the Ramsey Panel in 1963. When Robert Wilson accepted the directorship of the National Accelerator Laboratory in 1967, he chose Goldwasser to be Deputy Director.

Kermit Gordon. Gordon was Director of the Bureau of the Budget in 1963 when the MURA proposal was cut from the budget and the Berkeley proposal for designing the 200 BeV accelerator was approved.

Leslie Groves. Military leader of the Manhatten Engineering District, which administered the atomic bomb project in World War II. When the war ended, Groves took charge of postwar planning for atomic energy research while legislation was being passed to form the Atomic Energy Commission. After obtaining advice from an Advisory Committee on Research and Development, he made preliminary plans for a national laboratory system. Thus, when the Atomic Energy Commission began operation in 1947, it inherited a system that included the Berkeley laboratory, facilities at Argonne, Illinois and Oak Ridge, Tennessee and a new laboratory, Brookhaven National Laboratory, in New York.

Donald Hornig. Hornig was the presidential science advisor when the 200 BeV accelerator proposal was winding its way through the federal funding process from 1965 to 1967.

Ernest Lawrence. Lawrence invented and constructed the first cyclotron, with the help of his graduate student M. Stanley Livingston. In the 1930s Lawrence built a series of accelerators and established a world-renowned research facility in Berkeley. Lawrence won the Nobel Prize in 1939 for his contributions to accelerator development and served as a top administrator in the World War II mobilization of science. Before the war ended, Lawrence successfully manipulated his wartime contacts to obtain funding from the federal government to finance three large accelerator projects and on-going research at his Berkeley laboratory. Lawrence continued to direct his Berkeley laboratory until his death in 1958. On his deathbed, Lawrence chose Edwin McMillan to be his successor.

Leon Lederman. As a Columbia physicist, in 1963 Lederman served on the Advisory Committee for the 200 BeV Accelerator and on the Good Panel, which represented the interests of high energy accelerator users. To reinforce and extend panel recommendations, Lederman wrote an informal report for a 1963 Brookhaven summer study entitled "The Truly National Laboratory (TNL)," using the initials TNL as a pun on BNL, which he felt was not functioning as a truly national facility. Lederman described an ideal laboratory with complete on-site facilities for outside users, resources for facilitating individual experiments, an

accessible and pleasant site, scheduling and advisory committees and a strong director. Currently (1988) Lederman is director of Fermilab.

Edward Lofgren. Lofgren, who worked with Lawrence as a graduate student and helped construct the Berkeley Bevatron in the early 1950s, was appointed director of the Berkeley effort to design the 200 BeV accelerator. When Weston, Illinois was chosen as the site for the accelerator, the Universities Research Association asked Lofgren to direct accelerator design. Lofgren declined the position, explaining that he was dissatisfied with the description of his duties, felt unsure of the Weston site, and doubted whether he could assemble the necessary staff in Illinois.

John A. McCone. McCone was chairman of the Atomic Energy Commission from 1958 to 1961. In November 1959 he signed an agreement with his Soviet counterpart Vasily S. Emelyanov establishing several US-USSR cooperative ventures, including a joint accelerator project. By 1961, the Soviets had abandoned the venture, but a U.S. group, led by Brookhaven researcher John Blewett, continued design work through the early 1960s.

Paul McDaniel. As Director of the Atomic Energy Commission's Division of Research in the early 1960s, McDaniel helped make many Commission arrangements for the 200 BeV accelerator.

Edwin McMillan. McMillan worked at Berkeley in the 1930s as one of Ernest Lawrence's "boys" in the early Radiation Laboratory. In 1946 he discovered the principle of phase stability (which was independently discovered by Vladmir Vekler) which enabled the development of the synchrotron, opening the possibility for much more powerful accelerators. In 1951 he and Glenn Seaborg shared the Nobel Prize in chemistry for discovery of the first two transuranic elements. Just before his death in 1958, Lawrence chose McMillan as his successor. As Director of the laboratory, McMillan mounted the Berkeley effort to design a new high energy accelerator in 1960. The Laboratory, which in 1959 was renamed Lawrence Radiation Laboratory (LRL) in honor of Lawrence, obtained funding for this design in 1963. From 1963 to 1967, when the machine was designed and arrangements were made for managing and siting the laboratory, McMillan was LRL Director.

Emanuel Piore. In 1960, as director of research at IBM, Piore headed a joint panel of the President's Science Advisory Committee and the General Advisory Committee of the Atomic Energy Commission. Although the Panel was unenthusiastic about the Fixed Field Alternating Gradient, the high intensity accelerator proposed by the Midwestern Universities Association, Panel members supported plans for building a new high energy accelerator. In 1965 Piore was named chairman of the National Academy of Sciences Site Selection Committee, which provided a list of six finalist sites to the Atomic Energy Commission.

Isidor I. Rabi. Rabi, who won the Nobel Prize in 1944 for contributions to atomic physics, never used accelerators or other large equipment in his own research. Nonetheless, after assuming a leadership role in World War II research, he was instrumental in establishing Brookhaven National Laboratory and obtaining accelerator funding to accommodate the research needs of East Coast physicists. Rabi gave a particularly stirring defense of Oppenheimer during his security hearing in 1954, and later snubbed Edward Teller, who presented information damaging to Oppenheimer during the hearing. Rabi continued to serve as a statesman of science and advocate of Brookhaven until his death in 1988.

Norman Ramsey. Ramsey, a former graduate student of Rabi, worked on both the radar and atomic bomb projects in World War II, aided in the establishment of Brookhaven after the war, and served on a number of advisory boards in the 1950s. In 1963 he was named chairman of a President's Science Advisory Committee/General Advisory Committee. The Ramsey Panel, which became one of the most famous advisory panels of the decade, suggested that the Atomic Energy Commission give funds to the Lawrence Radiation Laboratory to build the 200 BeV accelerator at a California site. In 1965, after the Berkeley team had designed the accelerator, Ramsey served as chairman of the Scientific Subcommittee of the new accelerator's management organization, the Universities Research Association. In 1966, as president of the organization, Ramsey recruited Robert Wilson as the laboratory's first director. Ramsey continued as president of the Universities Research Association, Inc. from 1967 to 1972 as the new laboratory was being designed and constructed.

Matthew Sands. Sands, a Caltech physicist, began pioneering work on a 300 GeV accelerator in 1959, a year before researchers at Berkeley began serious discussion of a new strong focusing synchrotron. With this design, Sands presented for the first time a cascade synchrotron scheme, with a booster synchrotron that cascaded particles into a larger synchrotron. Sands recruited fellow Caltech researchers Alvin Tollestrup and Robert Walker to work on the design and helped organize the Western Accelerator Group (WAG). WAG, a group of researchers from several Southern California universities, submitted a proposal to the Atomic Energy Commission for design funding for an accelerator in the 300 GeV group in 1961. By 1962, however, Berkeley and Brookhaven had submitted competing proposals for design funding for the new accelerator. Although Berkeley researchers, who received design funding in 1963, pledged to collaborate with Caltech researchers, Sands, Tollestrup, and Walker did not participate in the Berkeley design effort.

Charles Schultze. Schultze was Director of the Bureau of the Budget in 1967 when the 200 BeV accelerator project received its first large funding allocation.

Lloyd Smith. Smith, a veteran of the effort to design the Bevatron at Berkeley, was Lofgren's deputy during the Berkeley 200 BeV design

study. In addition to administrative duties, Smith made theoretical calculations, both for the Berkeley study and later for the 1967 summer design study under Robert Wilson's direction.

Glenn Seaborg. In the 1930s Seaborg, a nuclear chemist, worked at Berkeley one of Ernest Lawrence's "boys" in the early Radiation Laboratory. In 1951 Seaborg shared the Nobel Prize in chemistry with Edwin McMillan for discovery of the first two transuranic elements. As Chairman of the Atomic Energy Commission in the 1960s, Seaborg was a firm supporter of the 200 BeV accelerator project.

Frederick Seitz. Empowered by his considerable influence as NAS President, Seitz convened a meeting of 25 university presidents in early 1965. The meeting signaled the beginning of the Universities Research Association, Inc. and the opening of the site selection contest for the 200 BeV accelerator laboratory. Seitz continued to serve as an important and influential supporter of the 200 BeV accelerator project as it wound its way through the federal funding process from 1965 to 1967.

Gerald Tape. Tape, a former Brookhaven physicist and administrator, served on the Atomic Energy Commission as the 200 BeV project was being planned. Tape, the only physicist on the Commission, acted as the liaison between the Commission and the physics community during site selection.

Jerome Wiesner. Wiesner was presidential science advisor in the early 1960s when the MURA proposal was killed and the Berkeley proposal for design funding for the 200 BeV was approved. Wiesner was succeeded by Donald Hornig.

Robert Wilson. In the 1930s, as a graduate student at the University of California, Berkeley, Wilson worked as one of Ernest Lawrence's "boys" in the early Radiation Laboratory. After serving as a group leader in the atomic bomb project, Wilson designed a cyclotron at Harvard. Next, he went to Cornell to head the Laboratory of Nuclear Studies, where he directed the design and construction of a series of electron synchrotrons. In 1960 Wilson organized an informal meeting to discuss the desirability and feasibility of a new high energy accelerator to coincide with the prestigious Rochester Conference on High Energy Accelerators. After the Berkeley design team presented their design for the 200 BeV accelerator in 1965, Wilson wrote a letter to Edwin McMillan. He complained that the design was too expensive and lacked imagination and presented an alternate design with smaller magnets and scaled down experimental equipment and research areas. Although many physicists were interested in the broad outlines of Wilson's ideas, Wilson had fewer supporters when his alternate design was more thoroughly discussed at a series of meetings with scientists and government leaders in early 1966. A year later, after Edward Lofgren declined the job of design director for the new accelerator, the Universities Research Association offered Wilson the directorship. Despite some reservations, both the Atomic Energy Commission and the

Joint Committee on Atomic Energy eventually endorsed Wilson's candidacy. In March 1967 Wilson became the first director of the laboratory, which he named the National Accelerator Laboratory. From 1967 to 1972 Wilson oversaw the design and construction of the new laboratory, which was renamed Fermi National Accelerator Laboratory in 1972.

Glossary of Scientific Terms

Beta Decay. The spontaneous transformation of a neutron into a proton, an electron, and a neutrino. If the neutron is inside the nucleus, the process is known as nuclear beta decay. (1)

Betatron. A cyclic accelerator which accelerates electrons to high energies by using magnetic induction instead of an accelerating electric field, as in other accelerators. (2)

Bubble Chamber. A type of detector in which the path of a charged particle is displayed by the trail of bubbles created as the particle passes through a superheated liquid.
Hydrogen, helium, and deuterium are commonly used in bubble chambers. (3)

Cerenkov Counter. A Cerenkov counter is a detecting device that identifies Cerenkov radiation, which is the light emitted by charged particles when they pass through a nonconducting, transparent material when travelling faster than the speed of light while in the material. (4)

Colliding Beams. Colliding beams, an idea first suggested in the 1940s, produces extremely high energies at the spot where the beams collide. As Lawrence Brown explains: "As one goes to higher energies, the available center-of-mass energy is proportional to only the square root of the laboratory particle energy for a beam striking a stationary target of similar particles. However, in beam-beam collision of identical particles of the same energy, the available center of mass energy is equal to the sum of the two energies, or twice either one." Thus, resesarchers became interested in colliding beams when considering stationary target machines in the multi-hundred GeV range. (5)

Conservation Laws. Any regularity in nature indicating that a quantity, such as electrical charge, remains the same before and after an interaction. The various fundamental interactions have characteristic conservation laws. (See Fundamental Interactions)

Cosmic-Ray Experiments. Cosmic rays are energetic particles that reach the earth from space. When these particles first reach the earth, most are protons, but in subsequent interactions with the earth's atmosphere, they produce a wide range of photons and elementary particles are produced. The primary methods used to study cosmic rays from 1945-1960 were cloud chambers, Geiger-Mueller counters, and photographic emulsions. (6)

Cloud Chamber. A cloud chamber is a particle detector consisting of a box filled with a vapor and a gas which are rapidly expanded by a moving wall or piston. The gas is cooled by adiabatic expansion which also supersaturates the vapor. In this environment, vapor condenses

around charges produced by the passage of charged particles. Tracks are stereoscopically photographed so that events can be reconstructed in three dimensions. Cloud chambers can be designed so that expansion is triggered at regular intervals or when counters surrounding the chamber detect a specified event. (7)

Geiger-Mueller Counter. In Geiger-Mueller counters, a wire is passed through a tube filled with a low pressure gas and given a high voltage. When a charged particle travels through the device, the gas is ionized. The ejected electrons are accelerated in the direction of the wire causing more ionization to occur and the emission of a photon. A large spark is thus produced which can be readily detected. (8)

Photographic Emulsions. Photographic emulsions are stacks of photographic film. When the stacks are exposed to energetic charged particles their ionization tracks expose the film. When the film is developed, the tracks can be observed. (9)

Electron Volt (eV). A unit of energy describing the energy obtained by an electron accelerated through one volt of potential difference. MeV stands for one million (1,000,000) electron volts. GeV stands for one billion (1,000,000,000) electron volts. Although BeV was originally used for this expression, the term was changed since one billion in British measurement is 1,000,000,000,000.

Feynman Diagrams. A pictorial set of rules for particle interactions. As physicist Steven Weinberg explains, Feynman diagrams "allowed one to associate a definite numerical quantity to each picture of how the momentum and energy could flow through the intermediate states of any collision process: the probability for the process is given by the square of the sum of these individual quantities. The Feynman rules were very much more than a handy calculational algorithm, because they incorporated an essential feature of quantum field theory--the symmetry between particles and antiparticles. Each line in a Feynman diagram can represent either a particle created at one end of the line and destroyed at the other, or an antiparticle going the other way." (10)

Fundamental Interactions. The four fundamental forces of nature. In order of decreasing strength these are: the strong, electromagnetic, weak, and gravitational forces. Although gravitational force is important at human-scale dimensions, it is too weak to have much effect on elementary particles. In contrast, the weak and strong interactions are exerted only at close range, that is when particles pass close to each other, and thus are important only at subatomic dimensions. Gravitational and electromagnetic forces possess infinite range. The electromagnetic force is the one force important both at subatomic and human-scale dimensions. (11)

Linear Accelerators. Linear accelerators, or linacs, accelerate particles with a number of cylindrical electrodes (or drift tubes) which accelerate particles in a straight line. Electrons can be accelerated

to higher energies in linacs than in synchrotrons because linac acceleration avoids synchrotron radiation. (See Synchrotron Radiation) Linacs have the disadvantage that increased distance of acceleration comes only with increased length, unlike the synchrotron, in which particles are accelerated through multiple circular orbits.

Magnetic Lens. As accelerator physicist Lawrence Jones explains, a magnetic lens "has the property that it may focus the particle beam in one dimension (say vertically) but only at the expense of defocusing the other (horizontal). In older synchrotrons the fact that the circular orbit motion exerted a mild horizontal focusing effect was used to compensate for the horizontal defocusing of the magnetic field lens. But all of this led to a very "weak" net focusing in both directions, in a machine possessing azimuthal homogeneity." (12)

Parity. The physical property of a wave function which indicates how it will behave on an inversion of its coordinates. If inversion does not change the wave function, it has parity of one (or even), if only the sign of the wave function is changed, it has parity of -1 (or odd).

Phase Stability Principle. Phase stability is the phenomenon which occurs when the oscillator frequency of a particle accelerator matches the velocity of the particle at all times during the acceleration of the particle. This matching allows the particle to be accelerated to very high energies in small steps determined by the voltage of the oscillating electric field. (13)

Resonance Acceleration. The acceleration used in many particle accelerators, including linacs, cyclotrons and synchrotrons. As Livingston explains: "In resonance acceleration, particles traverse an alternating radiofrequency electric field applied across a series of gaps, and acquire an increment of energy on each successive traversal of this field." (14)

Scintillation Counter. A type of detector. A scintillation counter emits a light flash when struck by a particle which can be electronically recorded. (15)

Secondary Beam. A particle beam created when the primary beam from a particle accelerator strikes a target. (16)

Storage Rings. Devices that stack and store accelerated particle beams until large intensities are obtained. (17)

Synchrotron. A type of accelerator in which particles are accelerated by alternating electric fields by means of resonance acceleration. (See Resonance Acceleration) Magnets arranged in a ring force the particle to travel in roughly circular, closed orbits. Particles are fed into the ring and then all the particles are raised in energy at the same time by simultaneously raising the magnetic field and frequency of the alternating electric field. The synchronization of the frequency of

acceleration and the magnetic field makes the particles circle many times, and at each cycle their energy increases. The major features of a synchrotron are described below. (18; See Phase Stability Principle)

Ion Source. As accelerator physicist Milton White explains, the particles that are to be accelerated in a synchrotron are "generally produced in an external device by a hot cathode or a gaseous discharge and then, after being electromagnetically focused in a narrow pencil, they are accelerated up to several tens of MeV before injection into the synchrotron magnet gap." At this point, particles are ready for acceleration in the synchrotron. (19)

Vacuum Chamber. A good vacuum must be maintained in the region in which particles are accelerated or particles will scatter after colliding with air molecules. To create such a vacuum, chambers made of ceramic or laminated metal are evacuated. The beam is then transported through these chambers. (20)

Bending and Focusing Magnets. Magnets are used to bend and focus the beam so that particles can circle many times and reach higher energies. (21)

Acceleration. As accelerator physicist Milton White explains: "The increase in particle energy actually takes place when the circling charged particles pass through electric fields spaced around the circumference of the magnet. . . . The usual arrangement is one in which the acceleration electrodes are excited by a sinusoidal voltage whose frequency is a harmonic . . . of the particle rotation frequency and whose amplitude is such that particles can gain sufficient energy to match the rising magnetic field." (22)

Ejection. When particles reach full energy, they are transported into an internal target or ejected. Several schemes are used for ejection. As accelerator physicist Milton White explains, such schemes "usually involve exciting strong radial betatron oscillations with the last oscillation carrying the particles into an ejection magnet which deflects the beam clear of the synchrotron magnet structure." (23)

Electron Synchrotron. A type of synchrotron. Since electrons can be accelerated to the speed of light with relatively low energies, the electric field frequency can be kept constant in an electron synchrotron. This allows a relatively simple design, which explains why the electron synchrotron was one of the first synchronous accelerators to be built. On the other hand, the light mass of electrons makes them much more susceptible than protons to loss of energy from synchrotron radiation, making it difficult for synchrotrons to accelerate electrons to energies higher than 300 MeV.

Proton Synchrotron. A type of synchrotron. Since protons require energies over 1 GeV to approach the speed of light, the orbital frequency and velocity of protons increases during the time required for acceleration. Thus, to achieve phase stability in proton synchrotrons, the frequency of the electric field must be increased as the magnetic field increases.

Quantum Field Theory. A kind of physical theory. These first emerged in the 1930s and 1940s when physicists applied relativity theory to quantum mechanics. When attempts to apply quantum field theories encountered problems in the 1950s and 1960s, quantum theory went into decline. In the mid 1970s, field theory came back into vogue with a new class of field theory called gauge theory. Quantum electrodynamics, quantum chromodynamics, and unified models of electromagnetic and weak interactions are all types of gauge theory. (24)

Synchrotron Radiation. Polarized electromagnetic energy caused by the acceleration of charged particles at relativistic energies. Electrons reach relativistic energies very quickly, and because of their small mass they are particularly susceptible to this phenomenon. (25)

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APPENDICES

Appendix A: List of all 200 BeV Site Proposals

(Taken From Glenn Seaborg Diary, August 27, 1965)

Alabama

- Birmingham Area
- Eufaula
- Florence
- Greene County
- Mobile
- Opelika

Arizona

- Gila River Indian Reservation
- Morristown
- Phoenix and Tucson

Arkansas

- Little Rock

California

- McCloud River Basin Area
- San Diego
- San Francisco Area

Colorado

- Denver Area
- Mesa County

Connecticut

- Plainfield

Georgia

- Atlanta Area
- Bainbridge
- Cherokee County
- Forsyth County
- Fulton County
- Savannah

Idaho

- Idaho Falls

Illinois

- Chicago Area
- Morgan-Scott Counties Area
- Princeton
- Shawneetown

Indiana

- Clinton
- Elkhart
- Indianapolis Area
- Ohio River - Aurora

Iowa

- Cedar Rapids (Sunbury and North Liberty
- New Hampton

Kansas

- Kansas City Area
- Parsons

Florida

- Broward County
- Dade County
- Fort Walton Beach
- Ft. Pierce
- Gulf County, Panama City
- Jacksonville/Gainesville Area
- Orlando
- Palm Beach Gardens
- Santa Rosa County
- Tampa Bay Area
- Tallahassee

Kentucky

- Bowling Green
- Carrollton
- Louisville
- Murray
- Paducah
- State of Kentucky Selected Sites (Covington and others)
- West Point

Louisiana

- New Orleans Area

Maine

- Sanford

Maryland

- Baltimore Area
- Dickerson
- Worcester County

Massachusetts

- Haverhill

Nevada

- Las Vegas

New Hampshire

(See Maine, Sanford, and Massachusetts, Haverhill)

New Jersey

- Burlington and Ocean Counties
- Little Egg Harbor Township
- Millville
- Mount Holly
- Warren Grove

New Mexico

- Albuquerque
- Albuquerque (Rio Rancho Estates)
- Hobbs
- Farmington
- Sante Fe

Michigan

Ann Arbor and Battle Creek

Minnesota

Dakota County
Duluth

Mississippi

Jackson
Oxford
Perry/Forest Counties
Scott County
Stoneville

Missouri

Flat River
Joplin
(Kansas City Area - See Kansas)
St. Louis

Montana

Arlee

Nebraska

Mead
Sidney

New York

Auburn
Brookhaven
Hunter
New York State
Selected Sites
(Model City
and others)
Plattsburgh
Schenectady

North Carolina

Research Triangle
Foundation Area

North Dakota

Grand Forks

Ohio

Cleveland-
Ravenna Arsenal
Marietta
Medina County
Pike County

Oklahoma

- Oklahoma City Area
- Muskogee
- Tulsa

Oregon

- Morrow and Umatilla Counties
- Willamette Valley

Pennsylvania

- Crawford County
- Pittsburgh Area

Rhode Island

- North Kingston

South Carolina

- Savannah River

South Dakota

- Badlands Bombing Range
- Black Hills Ordnance Depot
- Lemmon

Tennessee

- Memphis Area
- Oak Ridge

Texas

- Austin
- Amarillo
- Dallas/Fort Worth Area
- Houston
- Lubbock
- Odessa
- Port of Mansfield
- San Antonio
- Sweetwater

Utah

- Brigham City
- Salt Lake City

Virginia

- Norfolk

Washington

- Hanford

West Virginia

- Pt. Pleasant and Ravenswood

Wisconsin

- Madison

Wyoming

- Laramie

Appendix B: Preliminary List of 200 BeV Sites Prepared by the AEC in July 1965

(Taken From Glenn Seaborg Diary, August 27, 1965)

Arizona

Phoenix Area

California

San Francisco Area
(Alternate - San Diego)

Colorado

Denver Area

Florida

Miami Area
(Dade and Broward Counties)
(Alternate - Tampa Bay Area)

Georgia

Atlanta Area

Illinois

Chicago Area

Indiana

Indianapolis Area

Iowa

Sunbury

Kansas

Kansas City Area

Kentucky

Covington
(Alternate -
Louisville and
West Point Area)

Louisiana

New Orleans Area
(Mandeville and
Livingston Parish

Maine

Sanford

Maryland

Baltimore Area

Massachusetts

Haverhill

Michigan

Ann Arbor

Minnesota

Dakota County

Missouri

St. Louis
(Kansas City Area
- See Kansas)

Nebraska

Mead

New Hampshire

(See Maine, Sanford and Massachusetts, Haverhill

New Jersey

Warren Grove and Millville

New York

Model City

North Carolina

Research Triangle Area

Ohio

Cleveland - Ravenna Arsenal

Oklahoma

Oklahoma City Area

Oregon

Willamette Valley

Pennsylvania

Pittsburgh Area

Rhode Island

North Kingston

Tennessee

Memphis Area

Texas

Dallas/Fort Worth Area
(Alternates-
Houston-
San Antonio)

Wisconsin

Madison

AEC Sites Designated For Further Consideration

Idaho Falls, Idaho
Brookhaven, New York
Savannah River, S. C.
Oak Ridge, Tenn.
Hanford, Wash.

Appendix C: List of 85 200 BeV Sites Sent by the AEC to the NAS Site Evaluation Committee on September 15, 1965

(Taken from Glenn Seaborg Diary, September 13, 1965)

Alabama

- Birmingham Area
- Eufaula
- Florence
- Greene County

Arizona

- Gila River Indian Reservation
- Phoenix and Tucson

Arkansas

- Little Rock

California

- San Francisco Area
- San Diego

Colorado

- Denver Area

Connecticut

- Plainfield

Florida

- Broward County
- Dade County
- Fort Walton Beach
- Jacksonville/Gainesville Ara
- Orlando
- Palm Beach Gardens
- Tallahassee
- Tampa Bay Area

Georgia

- Atlanta Area
- Bainbridge
- Forsyth County
- Fulton County
- Savannah

Idaho

- Idaho Falls

Illinois

- Chicago Area
- Princeton

Indiana

- Clinton
- Indianapolis Area

Iowa

- Cedar Rapids
  (Sunbury and
  North Liberty)

Kansas

- Kansas City Area

Kentucky

- State of Kentucky
  Selected Sites
  (Covington and
  Others
- Louisville
- Murray
- West Point
- Paducah

Louisiana

New Orleans Area

Maine
Sanford

Maryland

Baltimore Area
Dickerson

Massachusetts

Haverhill

Michigan

Ann Arbor and Battle Creek

Minnesota

Dakota County
Duluth

Mississippi

Jackson
Perry/Forest Counties
Scott County

Missouri

Joplin
(Kansas City Area - See Kansas)
St. Louis

Montana

Arlee

Nebraska

Mead

Nevada

Las Vegas

New Hampshire

(See Maine,
Sanford
Massachusetts,
Haverhill)

New Jersey

Millville
Warren Grove

New Mexico

Albuquerque
Santa Fe

New York

Auburn
Brookhaven
Hunter
New York State
Selected Sites
(Model City and
others)

North Carolina

Research Triangle
Area

Ohio

Cleveland--
Ravenna Arsenal
Marietta
Pike County

Oklahoma

Oklahoma City Area
Tulsa

Oregon

Willametta Valley

Pennsylvania

Crawford County
Pittsburgh Area

Rhode Island

North Kingston

South Carolina

Savannah River

Tennessee

Memphis Area
Oak Ridge

Texas

Amarillo
Austin
Dallas/Fort Worth
Area
Houston
Lubbock
Odessa
San Antonio

Utah

Salt Lake City

Virginia

Norfolk

Washington

Hanford

West Virginia

Pt. Pleasant

Wisconsin

Madison

Appendix D: Site Criteria for 200 BeV Accelerator Laboratory

(Taken from Site Evaluation Committee, Report of the National Academy of Sciences' Site Evaluation Committee," March 1966, Appendix D)

A. Criteria presented by the AEC to the Joint Committee on Atomic Energy in April, 1965.

1. General:

A national laboratory having as its principal research instrument a 200-300 Bev accelerator will have a staff of approximately two thousand people. The resident staff will include professional scientists and engineers who will be
responsible for the design, construction, and operation of the accelerator and its associated facilities, and research scientists whose principle function will be carrying out the long term research program in collaboration with visiting scientists, particularly from university users groups. The site must be so located that management can mobilize and maintain the necessary specialized staff, both resident and non-resident, to accomplish effectively the goals of the research project.

It is difficult to establish priorities or weights among the various technical, economic and social criteria that can be described. Some items, of course, are absolutely essential such as acreage requirements, the availability of adequate power, the proximity of adequate transportation, etc. On the other hand, other items such as foundation requirements of deep piles versus shallow piles, one-pass versus recirculating water systems, tunneling versus cutting and filling for shielding the magnet ring, etc., cannot be categorized absolutely and are subject to some compromise in order to maximize the potential of each site.

In other words, there will be trade-offs between the technical and other factors in order that over-all efficiencies and economies can be obtained.

2. Land:

a. Sufficient acreage, in the continenetal U.S., should be available to meet both initial and long range expansion requirements (depending upon shape and topography, 3000 acres is tentatively estimated as minimum for a 200 Bev proton accelerator).

b. The land should be owned or be reasonably available to the Federal Government.

c. The terrain and substructure should have load-bearing capacity adequate to ensure stable foundations for both the accelerator and the other associated facilities.

d. The site should be reasonably level to minimize expensive excavations.

e. Sites with serious seismic activity, faults, or loose joints in bedrock are to be avoided; however, it is unlikely that many sites will be eliminated solely on this basis.

3. Utilities:

a. The ready availability of electric power at the site sufficient for a demand load of several hundred megawatts is required.

b. The ready availability at the site of an adequate supply of cool, clean water is desirable. Since closed recirculating water systems can be used and may be preferable technically, it is unlikely that many sites will be eliminated solely on this basis.

c. The economics of power and water acquisition and especially subsequent operational costs will be a factor.

4. Environment:

a. Proximity to a major airport having frequent service to major U.S. cities is necessary to provide easy access and minimum travel time for university users and other visiting research personnel.

b. Adequate surface transportation facilities are necessary for movement of goods and transport of personnel.

c. Proximity to a commercial industrial center which includes adequate coverage of special needs in electronics, electrical and precision mechanical equipment will ease problems of recruiting technical support and in obtaining specialized supplies.

d. Proximity to other broadly based research and development activities will provide opportunities for desirable interaction of scientific and engineering personnel.

e. Sufficient housing and community facilities must be available to accommodate the permanent operating and research staff of several thousand people and the transient staff of several hundred.

f. Proximity to a cultural center that includes a large university will provide intellectual and cultural opportunities attractive for staff and families.

g. Regional wage and cost variations as well as labor surplus areas are factors.

Appendix 4: Site Criteria for 200 BeV Accelerator Laboratory (Continued)

Revised Criteria presented by the AEC to the Joint Committee on Atomic Energy in November 1965

The principal objective in the factors governing the choice of a site is the contribution to the capability and effectiveness of the nation's research in elementary particle physics. The site must be so located that management can mobilize and maintain the necessary specialized staff, both resident and non-resident to accomplish the research goals.

The siting factors have been catagorized as follows:

I. Land Suitability.

II. Utility Availability.

III. Construction Cost.

IV. Operation Cost.

V. Transportation.

VI. Colleges and Universities

VII. Communities.

VIII. Other Considerations.

I. Land Suitability:

(A) Sufficient acreage, in the continental U.S., should be available to meet both initial and long range expansion requirements. Depending upon shape and topography 3,000 acres is estimated as minimum for a 200 Bev proton accelerator. Availability of additional land contiguous to the site described should be considered.

(B) The land configuration and dimensions should be capable of accommodating the accelerator and associated facilities. 1 1/4 miles is taken as the least linear dimensions with acceptable topography.

(C) The land should be owned by or be reasonably available to the Federal Government. Cost of the non-federal land should be considered.

(D) Compatibility of present use of the land and of the existing capital improvements with an accelerator laboratory should be considered.

(E) The site should be reasonably level to minimize expensive excavations. The maximum elevation differential should be no more than 100 feet.

(F) The surface and subsurface soils should have load-bearing capacity adequate to ensure stable foundations for both the accelerator and the associated facilities.

(G) Depth of bedrock as it affects the stability of the accelerator and the associated facilities should be considered.

(H) Depth and variation in level of the water table as it affects earth stability in the vicinity of the accelerator should be considered.

(I) Seismic activity as it affects the strength and movement of soils should be considered.

II. Utility Availability:

(A) Distance of power from site and the ready availability of electric power at the site must be sufficient for a demand load of 200 megawatts initially and increasing gradually to 300 megawatts.

(B) Impact on the electrical distribution system as it affects the availability of electrical power in the quantities necessary for efficient operation of the accelerator laboratory should be considered.

(C) The need for reliability of electrical power requires that the power source be an interconnected system.

(D) The ready availability at the site of cool, clean water should be considered. The initial minimum usage rate is estimated to be 2,000 gallons per minute, assuming the use of a recirculating system. Eventually the water requirements will be 4,000 gallons per minute.

III. Construction Cost:

(A) The effect upon construction costs due to existing facilities such as buildings, utilities, and roads should be considered. The utility of existing site facilities should be gauged in relation to the new laboratory to assess construction cost savings.

(B) Soil movement and load-bearing capacity affects accelerator associated construction costs. The requirements of accelerator stability must be accounted for in addition to the conventional construction requirements.

(C) Elevation differential affects construction cost. The elements involved are the relative costs of cutting and filling, as well as the effects of elevation differentials on earth stability which in turn affects construction cost.

III. Construction Cost (Continued):

(D) Tunnelling cost advantage over cutting and filling, if any should be considered.

(E) Cost to cope with elevation of water table should be considered. The variation of level and variation with time of the water table affects earth stability and is to be associated with drainage construction costs.

(F) Cost to bring electric power to the site should be considered.

(G) Cost to bring water to the site should be considered.

(H) Regional wage and cost variations as estimated by construction cost type indices should be considered.

(I) Natural hazards including seismic, hurricane, tornado, snow, rain, temperature, winds and dust affects construction costs. Natural hazards bear upon construction costs by way of affecting problems of stability as well as insulation and covering construction costs.

IV. Operational Cost:

(A) Electric power cost taking into account regional variations is an important consideration.

(B) Water cost taking into account regional variations should be considered.

(C) Existing and planned technological capabilities and facilities on the site area contribute to reducing operational costs. Include existing shops, electronics shops, maintenance and service shops, etc.

(D) Existing and planned technological capabilities and facilities in the vicinity of the site area contribute to reducing operational costs. Proximity to a commercial and industrial center with a well developed research and development base affects operational cost.

(E) Natural hazards affect operational costs. Include seismic disturbances, tornadoes and hurricanes as they affect operating costs.

(F) Climatic operational costs. Include heating, air-conditioning, water cooling, snow removal, etc., as they affect operational costs.

IV. Operational Cost (Continued):

(G) Soil movement and load-bearing capacity relative to operational cost should be considered. Costs to reallign experimental facilities associated with preventative maintenance programs and emergencies, can be large.

(H) Regional wage variations and labor relations affect operational cost.

V. Transportation:

(A) Proximity to a major airport having or planning to have frequent service to major U.S. cities is desirable to provide easy access and minimum travel time for university users and other visiting research personnel.

(B) Availability of adequate surface transportation facilities for the movement of goods and transport of personnel should be considered.

VI. Colleges and Universities:

(A) Proximity to colleges and universities should be considered.

(B) Strength of graduate and undergraduate programs in the physical sciences and the liberal arts available to the staff and their families is an important factor.

(C) Potential scientific and engineering interaction between the universities and the accelerator laboratory, and the relative merits of the impact of local university elementary particle physicists and engineers upon the work of the accelerator laboratory should be considered.

(D) Potential training of accelerator laboratory staff including availability of coursework and night classes for professional growth of the laboratory staff should be considered.

VII. Communities:

(A) Proximity of sizeable communities within an hours commuting time of the accelerator laboratory. A minimum population of 50,000 is required to assimilate the 2,000 staff member families and the transient staff.

(B) Availability of housing for the laboratory staff and laboratory visitors is important. The growth record of the community and its capability of adapting to change should be considered. Guest facilities in the communities should be available.

Communities (Continued):

(C) The quality and adaptability of public school systems including the growth record and community interest in education are important.

(D) Availability of medical facilities. The adequacy, growth record and community support of medical facilities are important.

(E) Churches, entertainment, recreational and other cultural facilities such as museums and libraries should be available.

VIII. Other Considerations

(A) Consideration should be given to the special responsibility of the AEC for its own laboratories and the advantages of effective utilization of present AEC facilities.

(B) Consideration to the establishment of a new center of excellence. The impact of the accelerator laboratory on a local university makes possible the transition to a new level of performance. Desirability of broadening the educational base of the nation should be considered.

Appendix E: URAI Officiers and Universities, 1965-1966

URAI Temporary Officers, appointed September 16, 1965

(Taken From "Minutes of First Meeting of Board of Trustees of Universities Research Association, Inc.," September 16, 1965, Lofgren Papers)

President: John C. Warner, Carnegie Institute of Technology
Vice President: Frederick Seitz, National Academy of Sciences
Treasurer: G. Donald Meid
Secretary: Leonard L. Bacon

Initial Universities

California Institute of Technology
University of California (Berkeley)
University of California (Los Angeles)
University of Chicago
Carnegie Institute of Technology
University of Colorado
Columbia University
Cornell University
Duke University
Harvard University
University of Illinois
Indiana University
University of Iowa
The John Hopkins University
University of Maryland
Massachusetts Institute of Technology
University of Michigan
University of Minnesota
University of North Carolina (Chapel Hill)
Northwestern University
University of Notre Dame
University of Pennsylvania
Princeton University
Purdue University
Rice University
University of Rochester
The Rockefeller University
Stanford University
University of Texas
Tulane University
Washington University
University of Washington
University of Wisconsin
Yale University

URAI Appointments By November 7, 1965

(Taken From Universities Research Association, Inc., "Minutes of the First Annual Meeting of the Council of Presidents" November 7, 1965, Lofgren Papers)

Chairman, Council of Presidents: Gaylord P. Harnell,
University of Pennsylvania
Vice Chairman, Council of Presidents: Kenneth S. Pitzer,
Rice University

15 Regional Trusees:

Robert Bacher, Professor of Physics, California Institute of Technology; Emilio G. Segre, Professor of Physics, University of California (Berkeley); R. D. Strathmeyer, Vice-President for Business Affairs, Carnegie Institute of Technology; Edwin L. Goldwasser, Professor of Physics, University of Illinois; Thurston E. Manning, Vice-President and Dean of Faculties, University of Colorado; Leon M. Lederman, Professor of Physics, Columbia University, Robert R. Wilson, Professor of Physics and Director of Laboratory of Nuclear Studies, Cornell University; Gerhard H. Henricksen, Vice-President for Business and Finance, Duke University; Norman F. Ramsey, Professor of Physics, Harvard University; Bernard Waldman, Associate Dean, College of Science, University of Notre Dame; George E. Pake, Provost and Professor of Physics, Washington University; Leon Madansky, Chairman, Department of Physics, The Johns Hopkins University; Morton Hamermesh, Head, Physics Department, University of Minnesota; Henry DeWolf Smyth, Chairman, University Research Board, Princeton University; and Fred R. Cagle, Vice-President, Professor of Zoology, Tulane University.

URA Appointments By February 1966

(Taken From Universities Research Association, Inc., "Board of Trustees," February 18, 1966, Lofgren Papers)

Trustees at Large

Maurice Goldhaber, Brookhaven National Laboratory; David Packard, Hewlett-Packard Company; George S. Schairer, Boeing Company; Charles A. Thomas, Monsanto Company; Joseph C. Warner, Carnegie Institute of Technology.

URA Officers and Universities in December, 1966

(Taken from Universities Research Association, Inc., "Annual Report by Board of Trustees," December 7, 1966)

URA Universities in December 1966,
Listed by Regional Groups:

Group 1	California Institute of Technology
	University of California (Los Angeles)
	University of California (San Diego)
Group 2	University of California (Berkeley)
	Stanford University
Group 3	Carnegie Institute of Technology
	Ohio State University
	University of Michigan
Group 4	University of Chicago
	University of Illinois
	Northwestern University
Group 5	University of Arizona
	University of Colorado
	University of Washington
Group 6	Columbia University
	The Rockefeller University
	Yale University
Group 7	Cornell University
	State University of New York at Buffalo
	Syracuse University
	Universtiy of Rochester
Group 8	Duke University
	Florida State University
	University of North Carolina (Chapel Hill)
Group 9	Brown University
	Harvard University
	Massachusetts Institute of Technology
Group 10	Indiana University
	University of Notre Dame
	Purdue University
Group 11	Iowa State University
	University of Iowa
	Washington University

URA Universities in December 1966 (Continued):

Group 12 The Johns Hopkins University
University of Maryland
University of Virginia
Vanderbilt University

Group 13 University of Minnesota
University of Wisconsin

Group 14 Princeton University
Rutgers University
Stevens Institute of Technology
University of Pennsylvania

Group 15 Rice University
University of Texas
Tulane University

URA Officers, December 1966:

Chairman Council of Presidents - Kenneth S. Pitzer, President, Rice University

Vice Chairman - Elvis J. Stahr, President, Indiana University

Chairman, Board of Trustees - Henry D. Smyth, Princeton University

Vice Chairman, Board of Trustees - Robert F. Bacher, California Institute of Technology

President of the Corporation - Norman F. Ramsey

Vice President - Frederick Seitz, President, National Academy of Sciences

Secretary - Leonard L. Bacon

Treasurer and Comptroller - G. Donald Meid

URA Board of Trustees, December 1966:

R.F. Bacher, Provost, California Institute of Technology

C. M. Blair, Vice President for Finance, Washington University

F. R. Cagle, Vice President, Tulane University

M. Goldhaber, Director, Brookhaven National Laboratory

E. L. Goldwasser, Professor of Physics, University of Illinois

M. Hamermesh, Head, Physics Department, University of Minnesota

G. Henricksen, Vice President for Business and Finance, Duke University

L. M. Lederman, Professor of Physics, Columbia University

L. Madansky, Chairman, Department of Physics, The Johns Hopkins University

T. E. Manning, Vice President, University of Colorado

D. Packard, Chairman of the Board, Hewlett-Packard Company

E. R. Piore, Vice President and Chief of Sciences, International Business Machines Corp.

N. F. Ramsey, Harvard University

G. S. Schairer, Boeing Company

E. G. Segre, University of California

H. D. Smyth, Princeton University

R. D. Strathmeyer, Carnegie Institute of Technology

B. Waldman, University of Notre Dame

J. C. Warner, President Emeritus, Carnegie Institute of Technology

R. R. Wilson, Cornell University

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