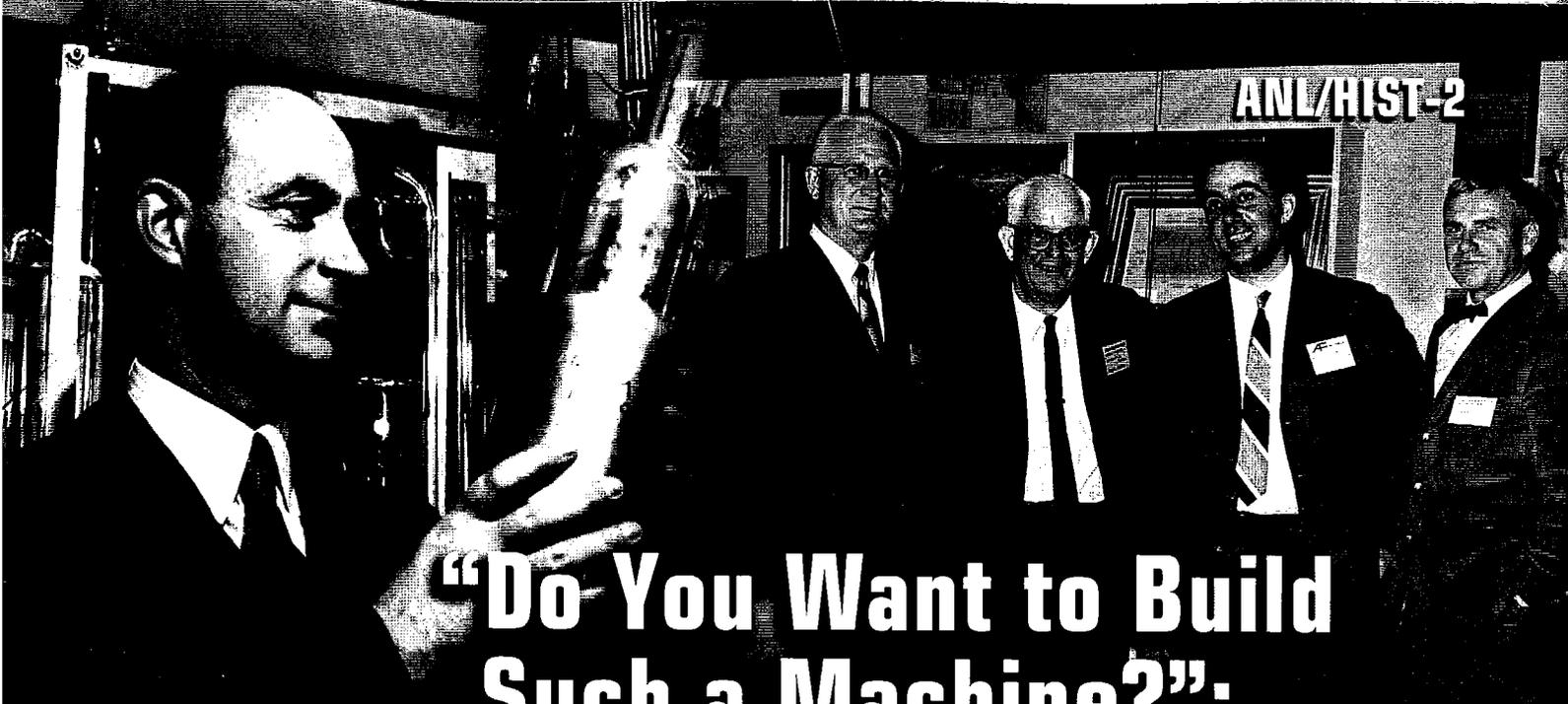
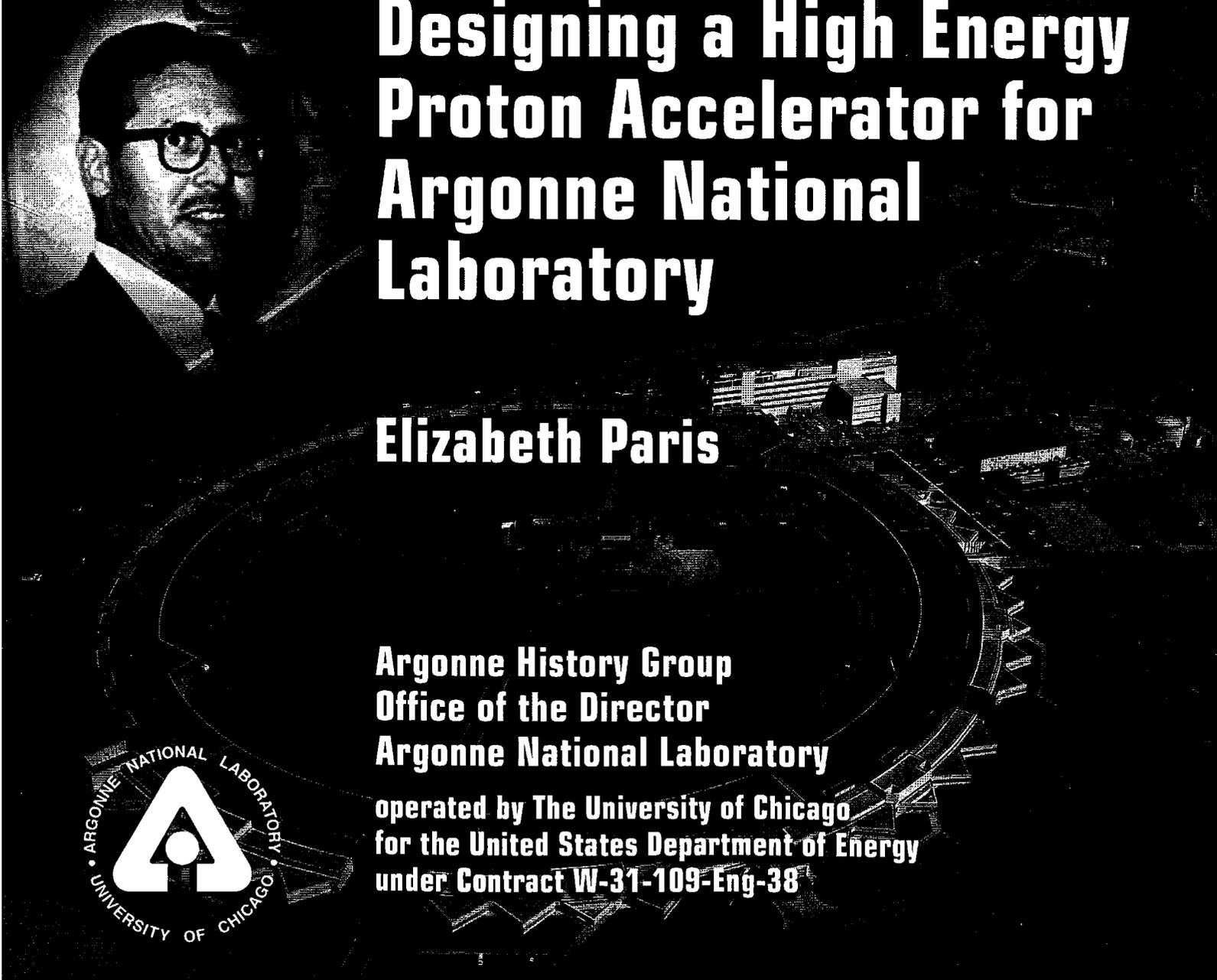


ANL/HIST-2



**“Do You Want to Build  
Such a Machine?”:  
Designing a High Energy  
Proton Accelerator for  
Argonne National  
Laboratory**



**Elizabeth Paris**

**Argonne History Group  
Office of the Director  
Argonne National Laboratory**

**operated by The University of Chicago  
for the United States Department of Energy  
under Contract W-31-109-Eng-38**



Argonne National Laboratory, a U.S. Department of Energy Office of Science laboratory, is operated by The University of Chicago under contract W-31-109-Eng-38.

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**TITLE PAGE:**

Photograph shows the Zero Gradient Synchrotron after construction.

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Argonne National Laboratory**

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**23 November 2003**





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**"Do you Want to Build Such a Machine?":  
Designing a High Energy Proton Accelerator for  
Argonne National Laboratory\***

**Elizabeth Paris<sup>1</sup>**

**Preface**

At the 2003 American Physical Society April Meeting an individual who had been around the physics community for many years asked after the subject of my latest project. I summarized: "The early years of what would become the Zero Gradient Synchrotron (ZGS) at Argonne National Laboratory." The individual became animated, "Oh, that's one machine that should have never been built. Once they knew about strong focusing, Argonne never should have built the thing." The individual was quite surprised to learn that the Argonne personnel had initially felt coerced into making the design choices this person attributed to too much free will. As one of the principal designers related in 2002,

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\* Abbreviations for archival locations found in notes: ANL = Information and Publishing Division, Argonne National Laboratory, Argonne, Illinois, 60439. NARA-II = National Archives and Records Administration, College Park, MD; NARA-GL = National Archives and Records Administration - Great Lakes Region, Chicago, Illinois; University of Illinois = University of Illinois at Urbana-Champaign. University Archives. Room 19, Library, 1408 West Gregory Drive, Urbana, Illinois 61801; UW-Madison = University of Wisconsin--Madison Division of Archives. University Archives. 728 State St. B134 Memorial Library. Madison, WI 53706

<sup>1</sup> Visiting Scholar, University of Chicago. This report was written as part of my work as a historian at Argonne National Laboratory between September 2002 and December 2003. I would like to thank the numerous individuals who agreed to be interviewed for this project, and, in particular, Fred Mills and Lee Teng for their extra advice, draft comments, and follow-up. For help with written sources, I would like to thank Janet Anderson, Mary Hale, and Shannon Savage at Argonne's Information and Publishing Division; Scott Forsythe, Glenn Longacre, and the archival staff at the National Archives and Records Administration - Great Lakes Region; Marjorie Ciralante at NARA-II in College Park, MD; William Maher at the University of Illinois Archives; and Allan Needell. Bob Seidel also provided much needed advice at an early, critical juncture. I have benefited from the time and efforts of many who read and commented on drafts of this work including: Albert Crewe, Malcolm Derrick, Tom Fields, Don Geesaman, Timothy Hefferon, David Kaiser, Catherine Westfall, and Peter Westwick.

Yeah, that's one thing that people always ask, "Why did you build the ZGS instead of the" – by that time the AGS at Brookhaven [had] already started, although it [wasn't] finished, . . . -- "because you don't believe in the AG-principle?" No, of course we believe[d] in the AG principle. But we were told to leave it alone. AEC didn't believe in the principle, maybe. (laughter)<sup>2</sup>

Strangely enough, during the conference I had attended immediately previously, one of the presenters had put up an overhead implying, without source or explanation, that Argonne's accelerator had been wholly designed by Congressmen serving on the Joint Committee for Atomic Energy .

I hope to complicate these sorts of views, held by many, that the resulting design of Argonne's first and last high energy proton accelerator can be attributed to (and more often blamed on) a single group or a single overwhelming constraint. Like most large projects, perhaps more than most, its form resulted from an enormously wide variety sources, from the behavior of protons to the behavior of nations. I will attempt to integrate local, regional, national, and international competition and collaboration with the daily work which served to create the theories and designs emanating from Argonne's Accelerator Group in the mid-1950s. It seems to me productive to acknowledge and examine the way these many factors co-exist if one is to construct a useful account for the benefit of understanding both this and other past --and future -- projects.

## **Introduction**

Argonne National Laboratory's efforts toward researching, proposing and then building a high-energy proton accelerator have been discussed in a handful of studies. In the main, these have concentrated on the intense maneuvering amongst politicians,

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<sup>2</sup> Lee C. Teng, interview by Paris, 14 November 2002, tape recording, Argonne, Illinois. AEC = United States Atomic Energy Commission; AG = Alternating Gradient (focusing); AGS = Alternating Gradient Synchrotron; ZGS = Zero

universities, government agencies, outside corporations, and laboratory officials to obtain (or block) approval and/or funds or to establish who would have control over budgets and research programs.<sup>3</sup> These “top-down” studies are very important but they can also serve to divorce such proceedings from the individuals actually involved in the ground-level research which physically served to create theories, designs, machines, and experiments. This can lead to a skewed picture, on the one hand, of a lack of effect that so-called scientific and technological factors exert and, on the other hand, of the apparent separation of the so-called social or political from the concrete practice of doing physics. An exception to this approach can be found in the proceedings of a conference on “History of the ZGS” held at Argonne at the time of the Zero Gradient Synchrotron’s decommissioning in 1979.<sup>4</sup> These accounts insert the individuals quite literally as they are, for the most part, personal reminiscences of those who took part in these efforts on the ground level. As such, they are invaluable raw material for historical inquiry but generally lack the rigor and perspective expected in a finished historical work.

The session on “Constructing Cold War Physics” at the 2002 annual History of Science Society Meeting served to highlight new approaches circulating towards history of science and technology in the post-WWII period, especially in the 1950s. There is new attention towards the effects of training large numbers of scientists and engineers<sup>5</sup> as well as the caution not to equate “national security” with military preparedness, but rather

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Gradient Synchrotron. These terms will be clarified below.

<sup>3</sup> See, for example: Leonard Greenbaum, *A Special Interest: The Atomic Energy Commission, Argonne National Laboratory and the Midwestern Universities* (Ann Arbor: University of Michigan Press, 1971); Daniel S. Greenberg, *The Politics of Pure Science* (Chicago: University of Chicago Press, reprinted 1999); and Jack M. Holl, *Argonne National Laboratory, 1946 – 96* (Urbana: University of Illinois Press, 1997).

<sup>4</sup> *History of the ZGS*, Joanne S. Day, Alan D. Krisch, and Lazarus G. Ratner, ed.s, AIP Conference Proceedings No. 60 (New York: American Institute of Physics, 1980).

<sup>5</sup> Alexander Brown, “The Rhetoric and Reality of Cold War Physics Manpower: A Quantitative Analysis of Graduate Education in Physics in Britain and Germany, 1900 – 1970,” and David Kaiser, “Putting the ‘Big’ in ‘Big Science’: Cold War Requisitions and the Production of the American Physicists after World War II,” History of Science Society Annual Meeting, Milwaukee, WI, 9 November 2002. See also David Kaiser, “Cold war requisitions, scientific manpower, and the production of American physicists after World War II,” *HSPS* 33 (2002):

more broadly – at certain points – with the explicit “struggle for the hearts and minds of men.”<sup>6</sup> There is a call for greater detail in periodization<sup>7</sup> as events such as Stalin’s death and Khrushchev’s subsequent speech, the end of the Korean conflict, the hydrogen bomb test, Eisenhower’s Atoms for Peace initiative, the 1955 Geneva conference, and Sputnik each served to drastically change the landscape in the United States. Furthermore, Harvard University Press recently published the first detailed and scholarly account of a history of the national laboratories; the work argues that the "systemicity" of the organization must be considered as a necessary piece when examining any of the myriad of related puzzles.<sup>8</sup>

Each of these issues touches high-energy accelerator history at Argonne. The ongoing tensions between Argonne and the universities, the person-power issues, the struggle for scientific and technological leadership, and the laboratory’s place and participation in the national laboratory system as a whole affected -- and were affected by -- the daily work taking place in the seminar rooms, offices and machine shops.

The following report, then, seeks to provide a well-supported account of a portion of Argonne’s high-energy accelerator history which incorporates “the view from below,” that is: attempting to understand the experiences of practitioners, incorporating the background of the more large-scale maneuvering. Deep understanding of the nationally large and the sub-atomically small can still leave quite a pickle in the middle – the very human scale we encounter most often.

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131-159.

<sup>6</sup> John Krige, “The Three Faces of Science in the 1950s,” History of Science Society Annual Meeting, Milwaukee, WI, 9 November 2002.

<sup>7</sup> Peter Westwick, “Commentary” on “Constructing Cold War Physics,” History of Science Society Annual Meeting, Milwaukee, WI, 9 November 2002.

<sup>8</sup> Peter Westwick, *The National Labs: Science in an American System, 1947-1974* (Cambridge, MA: Harvard University Press, 2003). See also Westwick, “The National Laboratory System in the U.S., 1947 – 1962,” Ph.D. diss., University of California – Berkeley, 1999.

The project begins in 1952, when alternating gradient (AG) focusing for high energy accelerators initially appeared in U.S. scientific literature. New design possibilities engendered an outbreak of research aiming to create the next generation of accelerators whose energies would eclipse then-existing predecessors by an order of magnitude. By 1954 designers and theorists had gathered into small groups all over the world in an effort to understand the new development and to coalesce technology and physical theory into plans for a machine which they, and their funding sources, could enthusiastically support. Among these groups was an Accelerator Study Group at Argonne National Laboratory. In July 1955 the group submitted its first official proposal for a high-energy proton accelerator. It incorporated even more radical ideas in alternating gradient focusing (the “fixed-field alternating gradient” (FFAG) scheme) into a multi-stage accelerator which would create final energies of 25 GeV. The group had their design goals redirected, however; in late 1955 the laboratory directorate requested that the machine they next proposed should be able to be built quickly (which was interpreted to mean that only proven technology be utilized) and should have an energy just over 10 GeV. The new criteria would eventually and dramatically change the focus of the group’s research. With at least three further incarnations, the members began imagining the specifics of what would come to be called the Zero Gradient Synchrotron. The initial phase of design work was completed in late 1957 at which time the lab received \$1.5 million to begin building construction and hire an architect-engineer. Any further design work would be paid for under construction costs.<sup>9</sup> Sverdrup & Parcel were hired as the A-E in 1958. John Livingood, the accelerator group’s leader since its inception and the Director of the two-year-old Particle Accelerator Division (PAD), felt his work was done and resigned in

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<sup>9</sup> “TELETYPE – AEC Washington to AEC Chicago – December 10, 1957,” 13 December 1957, Record Group 434, ANL, Box 342b Notebook “John J. Livingood’s collection of Memos and Notes on the ZGS,” NARA-GL.

order to write a textbook on accelerators.<sup>10</sup> There are also indications that a committee appointed by the University of Chicago to review the PAD felt the unusually complicated undertaking was being inadequately managed and suggested his re-assignment.<sup>11</sup>

The backdrop of this project naturally includes the Cold War as mentioned above but more specifically includes varying levels of enthusiasm inside Argonne (local), relationships to research being carried on by members of the Midwestern University Research Association (regional), rivalry and cooperation with other national laboratory scientists (national), and attention to progress being made by the USSR (international). Those hoping for justification or condemnation of the entire ZGS project will be disappointed, for such judgements are outside the focus and beyond the scope of this report. Rather, specifically how the early design work of the Accelerator Group (eventually the Particle Accelerator Division) affected and was affected by such local, regional, national, and international relationships will constitute the subject of this current study.

## **The Origins of Argonne National Laboratory & the Early Years of the Atomic Energy Commission**

To understand the process of accelerator design at Argonne, it is helpful to understand the founding of the laboratory itself. As Jack Holl has published in his expansive 1997 administrative history of Argonne, the origins of the Argonne National Laboratory lie in Enrico Fermi's Manhattan Project work at the University of Chicago. After Fermi's group achieved the first controlled chain reaction in the squash courts under Stagg Field on December 2, 1942, a much larger venue -- in a less-populated area -- was needed in order to build a full-scale pile for experimentation. [See **Figure 1: Painting by**

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<sup>10</sup> John J. Livingood, "The Beginning," in *History of the ZGS*, 9.

<sup>11</sup> John H. Williams, letter to L. H. Kimpton, 20 March 1958, enclosure in a memo by W. B. Harrell to Warren C. Johnson, 26 May 1958, Director's Subject Files, Box B93-00149, folder "ANL, ZGS, 1958," ANL.

**John Cadel of Enrico Fermi and His Colleagues Viewing CP-1.]** The Manhattan Engineering District moved large-scale operations to the Argonne forest, a site about 25 miles southwest of the campus, and the Metallurgical Laboratory, the Chicago portion of the MED project, now had research at both locations.<sup>12</sup>

**Figure 1. Painting by John Cadel of Enrico Fermi and His Colleagues Viewing CP-1.**

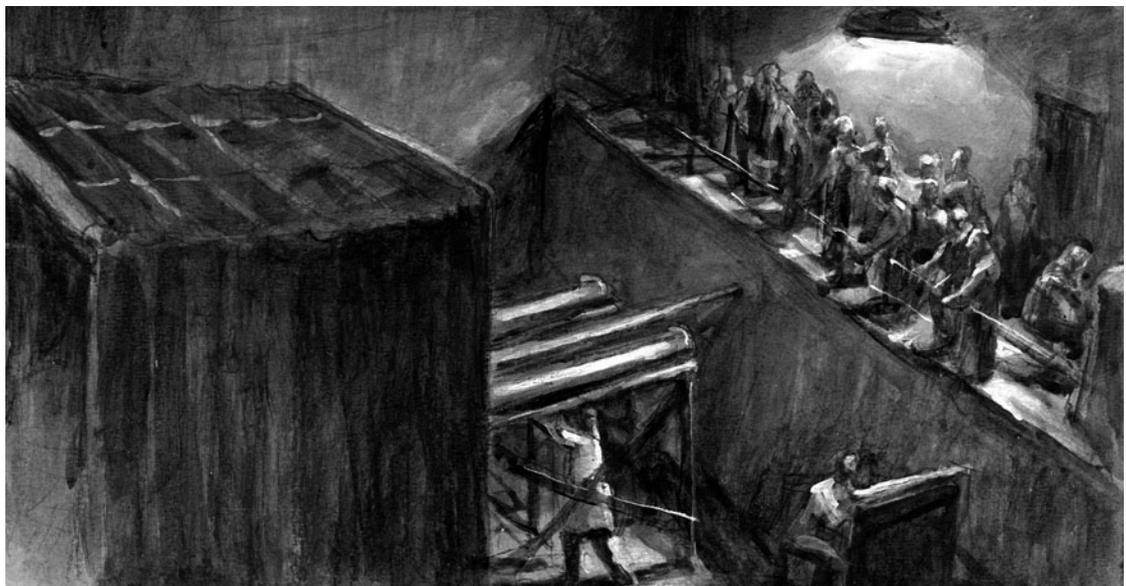


Illustration of the first controlled self-sustaining nuclear chain reaction, December 2, 1942. (Argonne National Laboratory)

On January 1, 1947, Argonne, along with four other laboratories (Brookhaven, Berkeley, Los Alamos, and Clinton), was inherited by the newly-formed Atomic Energy Commission.<sup>13</sup> [See **Figure 2: Atomic Energy Commission (AEC) Laboratories Inherited from the Manhattan Engineering District (MED) on 1 January 1947.**] The

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<sup>12</sup> For more on the Argonne site selection and further siting issues after the War, see Jack Holl, *Argonne National Laboratory, 1946-96* (Urbana and Chicago: University of Illinois Press, 1997): Chapters 1 and 2.

<sup>13</sup> Much of the information for this paragraph comes from Westwick, 2003, 43 - 49. For more on the organizational schemes of the various labs, see Westwick, 2003, chapter 2.

new AEC laboratories had a variety of operational schemes. Argonne, like both Berkeley and Los Alamos, was run through a contract with a single university.

**Figure 2. Atomic Energy Commission (AEC) Laboratories Inherited from the Manhattan Engineering District (MED) on 1 January 1947.**

<u>NAME</u>	<u>LOCATION</u>	<u>CONTRACTOR</u>
<b>Argonne</b>	Outside of Chicago, IL	U. of Chicago
<b>Brookhaven</b>	Long Island, NY	Associated Universities, Inc.
<b>Berkeley</b>	Berkeley, CA	U. of California
<b>Los Alamos</b>	Los Alamos, NM	U. of California
<b>Clinton (soon to be renamed Oak Ridge)</b>	Outside of Knoxville, TN	Monsanto (returned to U. of Chicago in September 1947 and then awarded to Union Carbide in December)

(Information from Westwick 2003, 39, 43 - 49)

The University of Chicago had responsibility for Argonne; the University of California had responsibility for the latter two. Brookhaven was run by a consortium of Northeastern universities (Associated Universities, Inc.), who had come together as WWII was ending to capitalize on their position of government-funded research and to avoid losing all major projects to their more western and southern counterparts.<sup>14</sup> Clinton

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<sup>14</sup> For more on the founding of Brookhaven, see Robert P. Crease, *Making Physics* (Chicago: University of Chicago Press, 1999).

Laboratories, which would soon be renamed "Oak Ridge" had been operated by the University of Chicago for the MED during the War and then turned over to the Monsanto company at its conclusion. In September 1947 the University of Chicago briefly resumed responsibility for Clinton until a suitable industrial contractor could be found. In December, the AEC awarded the contract to Union Carbide.

From the outset, there was tension as to the purpose of this new AEC lab system. Were these regional labs, meant to house equipment too large for any single university to afford, making such equipment accessible to scientists over a particular geographic area (e.g. Brookhaven)? Or were these centralized places where the nation could concentrate its highest possible level of effort on particular problems in a secure -- in the sense of "national security" -- environment (e.g. Los Alamos)? One's views on these questions would set expectations for the laboratory system as a whole and for each laboratory within it. The lack of unanimity either within or beyond the Commission, contributed to violent disagreements, sometimes with debilitating consequences.

When Argonne was ceded to the AEC in 1947, Walter Zinn, a Fermi collaborator at the Met Lab during the war, was at the helm.<sup>15</sup> Zinn had originally hoped for a regional laboratory with a diverse program of research -- including high energy physics -- to serve the Midwest. However, in 1947, the AEC pushed Argonne in a slightly different direction, ordering it to "stay out of the accelerator business" and designating the laboratory as the nation's center for nuclear reactor research.<sup>16</sup> And here Argonne enjoyed

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<sup>15</sup> Holl, 42, 46.

<sup>16</sup> L.R. Lunden, "Minutes of Meeting of Sub-committee on Securing Funds with Representatives of the Atomic Energy Commission Washington. D.C. June 10, 1954," 18 June 1954, box 1, folder "Minutes of Meetings 1953 - 55," P. Gerald Kruger Papers, University of Illinois, 2. Zinn likewise reiterated that Argonne had been forbidden to enter accelerator work in 1947 in his notes from a telephone conversation that he had with Lewis Strauss in October 1955. "I reminded Admiral Strauss that in 1947 we had been told by James B. Fisk and Robert Bacher that we could not go into accelerator work then. In 1952-53, I came back to this point and asked if we could not then start such work. I voiced the opinion that if in 1947 we had not been excluded from accelerator work, the present difficulty would never have arisen." (W. H. Zinn, memo to file, 21 October 1955, box 3 of 4, notebook "Accelerator Correspondence 1955," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.

much early success and an outstanding reputation. The contributions of its scientists and engineers towards the Nuclear Navy and civilian nuclear power were unparalleled, and it began and maintained important research programs in radiation health and safety.

However, by 1953, Zinn firmly believed that, if Argonne were to continue to be considered a cutting edge research institution, it would require a program -- and an accelerator -- for high energy physics. He was not alone in noticing the Midwest's lack of equipment in this high-profile area of research. The region's university researchers and leaders also had begun to feel more intensely that they were being given short shrift when compared to their East and West Coast colleagues. Although granting a sizeable portion of the nations' Ph.Ds in physics, the Midwest continued to hemorrhage talent as their best students and faculty found jobs near the state-of-the-art equipment at Berkeley and Brookhaven. Given recent and exciting developments in accelerator design, Zinn felt the time was ripe to once again push Argonne's case for a large, high energy machine.

### **The Design of High Energy Accelerators**

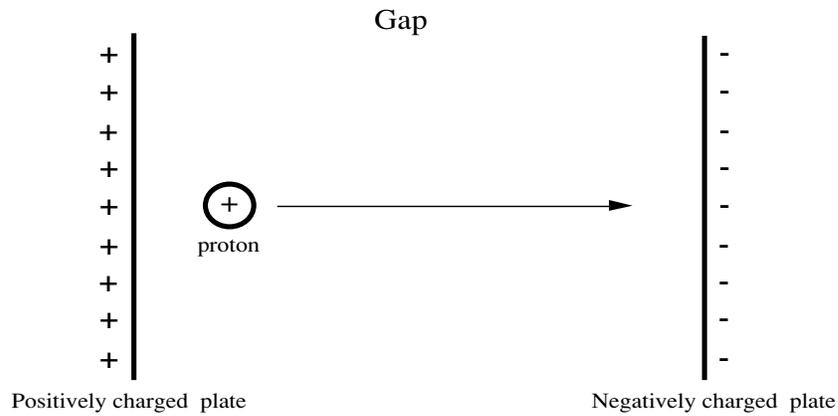
It will be helpful at this point to consider the evolution of high-energy accelerator design, the human race's attempt to create ever more particles at ever-increasing energies.<sup>17</sup> The most basic idea involves the acceleration of a charge particle across a gap created by two charged plates. The particle is repelled by the like-charged plate and accelerated toward the oppositely charged plate, gaining energy as it traverses the gap. **[See Figure 3: Single Gap.]** However, unlike the type of ideal diagrams that might be found in a basic physics textbook, no one has yet discovered a technique that will allow plates to contain ever increasing amounts of charge. Sparking or some other electrical

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<sup>17</sup> Material for this entire section has been adapted from Elizabeth Paris, "Ringing in the New Physics: The Politics and Technology of Electron Colliders in the United States, 1956 - 1972," Ph.D. diss., University of Pittsburgh, 1999, chapter 2, and further references can be found therein.

breakdown eventually results. Hence, to achieve greater acceleration, clever designs beyond the brute-force method continue to be devised.

**Figure 3. Single Gap.**



(Paris, 1999, 12.)

In order to overcome the necessity of creating a very strong field across a single gap, a scheme can be configured in which the particle passes through several gaps in succession, each with an easily achievable field strength. [See **Figure 4: Multiple Gaps.**] As the particle passes from one gap to the next, it continues to be accelerated, so that its energy at the end of the line is much greater than that which could have been obtained using a single gap with the highest possible achievable field strength.

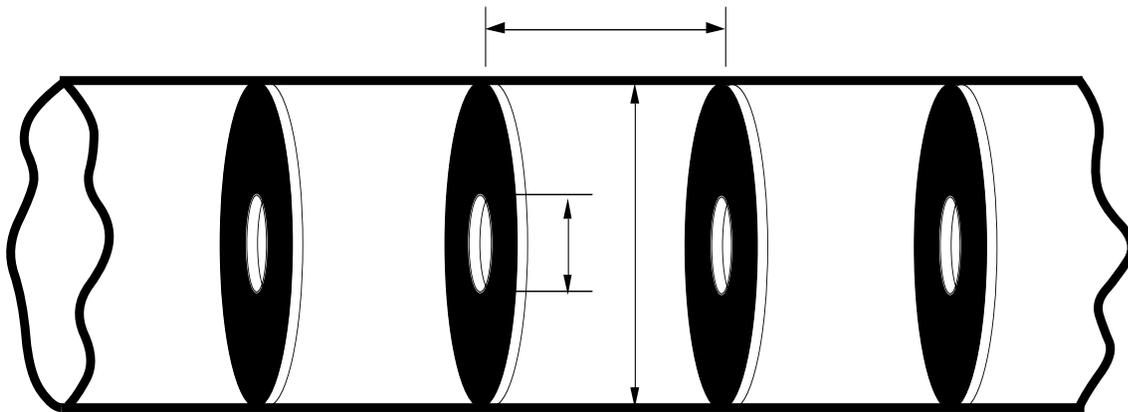
**Figure 4: Multiple Gaps.**



(Paris, 1999, 13.)

In practice, this linear scheme has also been implemented using a slightly different configuration in which the particles to be accelerated are introduced into a "wave guide". Inside the tube, diaphragms are appropriately placed so as to create a wave whose energy can be used to push the accelerated particle forward, much like a surfer rides an ocean wave. [See **Figure 5: Wave Guide Scheme.**]

**Figure 5: Wave Guide Scheme**

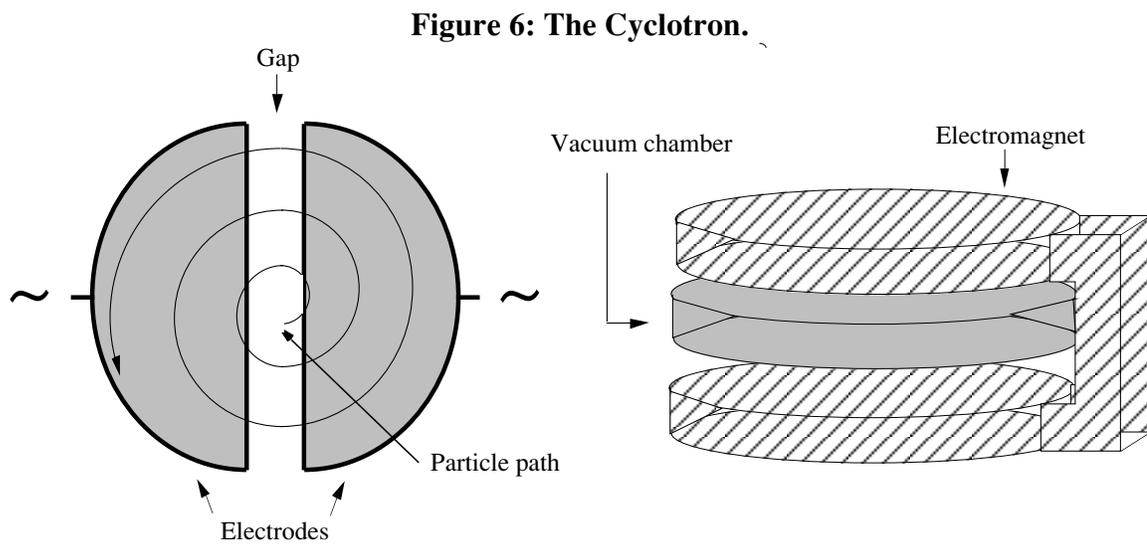


Inserting diaphragms into the wave guide slows the accelerating wave. The dimensions marked by the double-headed lines can be adjusted based on the type of particle to be accelerated and the energy desired. (As seen in Paris, 1999, 14. Based on Livingston and Blewett 1962, 324.)

Again, the length of the tube allows the particle to gain energy as they are pushed along, relieving the necessity for enormous field strengths across very short distances.

In principle, then, this linear scheme seems to overcome the practical difficulty of achieving arbitrarily high field strengths and allow particles to reach arbitrarily high energies. However, two difficulties present themselves. The first occurs when the particles begin to reach relativistic energies. The second obtains because, to impart additional energy, another segment must be added to the machine, and each segment requires its own construction materials, field-generating apparatus, housing, etc. Thus, in practice, such machines become more and more expensive to build.

One way around this second difficulty is to build a single gap with an achievable field strength, but to have the accelerating particle cross this gap many times. Such was the innovation behind the *cyclotron* in the early 1930s. [See **Figure 6: The Cyclotron.**] Under this design, the accelerated particle is made to travel on a circular path under the influence of a magnetic field, continually passing through the accelerating cavity and gaining more energy each time.



(Paris, 1999, 15.)

The electric field alternates to stay in synch with the particle as it passes from one electrode to the other and back again. Although this makes extraction of the beam much more difficult, the accelerating particle can be made to pass over a gap of reasonable field strength over and over and over again, without adding costly length to the machine.

However, the cyclotron design was not without its size limitations. As the accelerated particles gain energy, they travel in ever larger orbits which means the vacuum chamber which contains them must be large enough to encompass a particle traveling at the highest-designed energy of that particular cyclotron. The vacuum requirements and sheer tonnage of metal make very large cyclotrons highly impractical.

Furthermore, once the particles reach relativistic energies, the synchronization between the electric field and the particle orbits breaks down. The synchrocyclotron was developed in response to this difficulty. With variable electric field frequency, it could accommodate the higher energy particles although it still required increasingly large magnets. It also meant fewer particles could be accelerated at a time since the electric field frequency would now relate to the particular relativistic energy of one set of particles.<sup>18</sup> So, the next innovation involves a circular machine to take advantage of multiple passes over a single gap but mitigate the problem of the ever increasing radii of the orbits, utilizing only a ring rather than a disk.

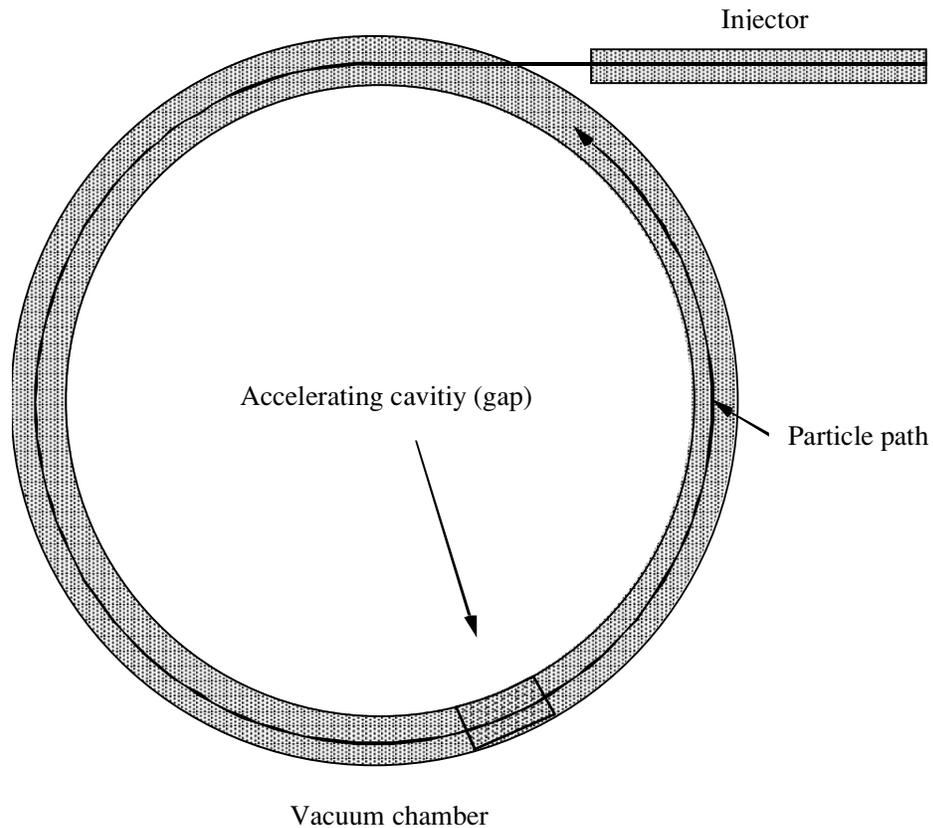
The synchrotron configuration drastically reduces the size of the vacuum chamber and the amount of materials needed by employing a variable magnetic field. Instead of the constant magnetic field of the cyclotron which results in increased orbital radii with increased speed, the synchrotron changes the strength of the magnetic steering field as the particles accelerate, bending their paths precisely the amount necessary to keep them in a constant orbit. [See **Figure 7: The Synchrotron Accelerating Configuration.**] In fact, this variation of the magnetic field can be used to accelerate the particles in this configuration, the technical details of which will not be discussed here.<sup>19</sup> This allowed large increases in the energies of the particles and the size of the machines. However, as the machines grew larger, the materials required again grew, and extensions to arbitrarily large sizes again started to become impractical.

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<sup>18</sup> For more on synchrocyclotrons, see M. Stanley Livingston and John P. Blewett, *Particle Accelerators* (New York: McGraw-Hill Book Company, Inc., 1962): Chapter 11.

<sup>19</sup> For more on synchrotrons -- and phase stability -- see Livingston and Blewett, Chapter 9, Chapter 12, and Chapter 13

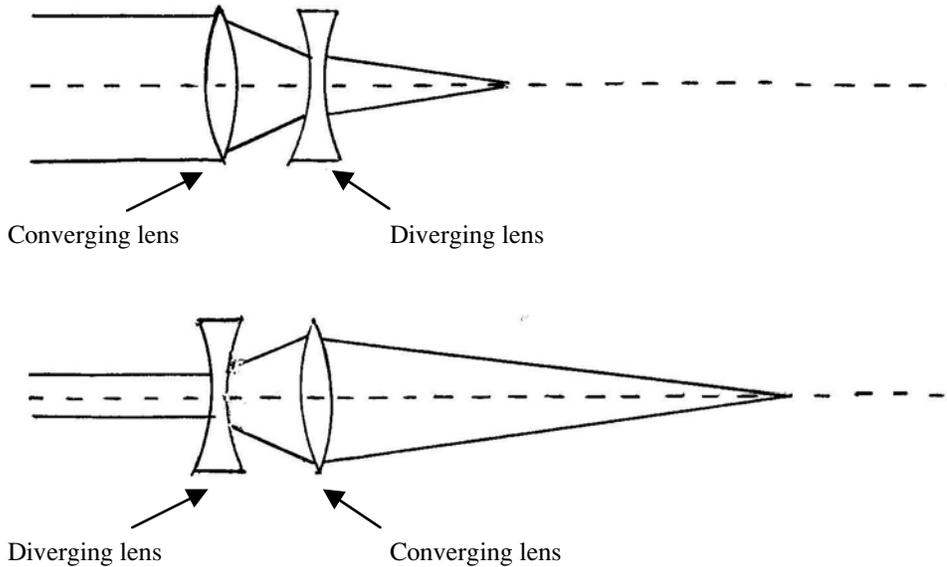
**Figure 7: The Synchrotron Accelerating Configuration.**



(Originally based on Paris, 1999, 18)

In 1952, Brookhaven scientists independently invented and subsequently publicized an accelerator design innovation that had been conceived of two years earlier by Greek physicist Nicholas Christofilos. The innovation, known as "alternating gradient focusing" or "strong focusing", provided a solution to the hundreds of tons of magnet which theoretically would have been required to drastically increase energies under the previous synchrotron design. The details of the configuration can be explained in an analogy with optical focusing through a system of lenses: a combination of converging and diverging lenses, in either order, can result in an overall focusing effect for a beam of light. [See **Figure 8: Optical Focusing.**]

**Figure 8: Focusing Light in an Optical System.**



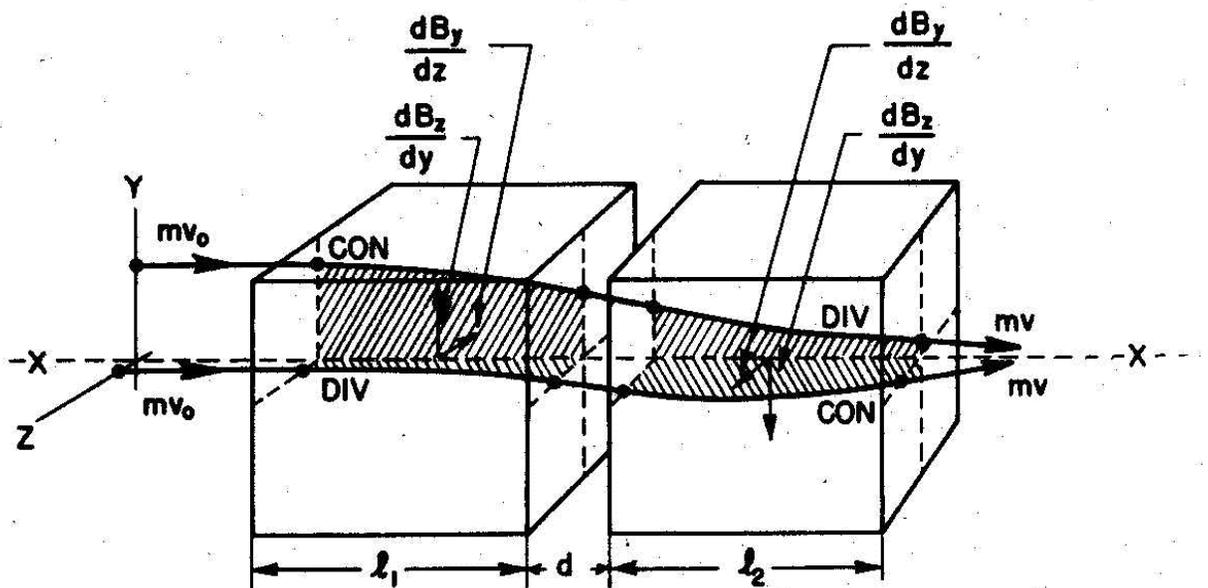
Different combinations of converging and diverging lenses can each result in an overall focusing effect on a beam of light.

Similarly, a combination of magnets which focus and defocus a beam of particles can result in an overall focusing of the beam. In the case of the magnet, the beam must receive an overall focusing effect in both the horizontal and vertical directions. Thus, one magnetic field configuration serves as a "converging lens" in the vertical direction and a "diverging lens" in the horizontal direction, while a second serves as a "converging lens" in the horizontal direction and a "diverging lens" in the vertical direction. The result, then, can be an overall converging effect in *\*both\** directions! [See **Figure 9: Strong Focusing.**]

Focusing the particle beam to a much smaller cross-section greatly reduced the size of the vacuum chamber needed to contain it and hence the size of the magnets necessary to surround the chamber. Another incredible leap in particle energy could now be achieved using the same amount of (or less!) material than machines of the older design. The older design came to be called "weak focusing", and it was quickly realized

by every accelerator designer that "strong focusing" could provide the path towards the next, highest-energy machine. Making it work was not a forgone conclusion, however. The magnet design and tolerances were more complicated and more expensive to create than in the simpler, weak-focusing machines, and the new innovation would soon become entangled in old arguments, including local hostilities, regional rivalry and the role of "national" laboratories.

Figure 9: Strong Focusing.



A 1952 publication by Courant, Livingston, and Snyder suggests a magnetic configuration which will result in an overall focusing effect on a beam of particles. In this diagram in the vertical (Y) direction, the beam first converges and then diverges, producing an overall focusing effect in that direction. In the horizontal (Z) direction, the beam first diverges and then converges, also producing an overall focusing effect, this time in the horizontal direction. Hence, the effect is to focus the beam in both directions. (M. Stanley Livingston, *High Energy Accelerators* (New York: Interscience Publishers, Inc., 1954), 130. Originally from Ernest D. Courant, et al., *Phys. Rev.* **88** (1952) 1190-1196, copyright (1952) by the American Physical Society, as reprinted in Claudio Pellegrini and Andrew M. Sessler, *The Development of Colliders* (New York: AIP Press, 1995), 21.)

## High Energy Physics in the Midwest

According to Jack Holl in *Argonne National Laboratory*, Walter Zinn began re-expressing his desire for Argonne to obtain a high energy accelerator in late 1952. He was not the only Midwestern physicist with his eye on such a prize. He had discussions with his old mentor at the University of Chicago, Enrico Fermi, who was also a proponent.<sup>20</sup> And the enthusiasm wasn't limited to the Chicago area. P. Gerald Kruger at the University of Illinois also began to make noises about the Midwest obtaining such a machine.<sup>21</sup> The interested parties gathered at Argonne on January 30, 1953, to discuss the project.<sup>22</sup> [See Figure 10: "Midwest Cosmotron Project [Committee]" First Meeting's Attendance.]

The resulting letter which nine of them sent to the Atomic Energy Commission contains information on their current design ideas as well as evidence as to their perceptions concerning the proper function for the Commission's labs. They were looking towards "a cosmotron type machine" [sic.] (a weak-focusing, proton synchrotron) justified by the assumed necessity of "maintain[ing] nuclear physics at its present outstanding level in the Midwest." This was to be partially an AEC responsibility since, "It seems unlikely that a single university or organization can undertake to carry through such a project alone."<sup>23</sup> That is to say: regional centers *are* important, and the AEC should function to help provide equipment within each region which is too expensive for

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<sup>20</sup> J. C. Boyce, memo to W. H. Zinn, 29 April 1953, box 134, folder "Selected History 1945 - 65," Argonne Universities Association Records, 1945-1982, University of Illinois.

<sup>21</sup> Greenbaum, 58. See J. C. Boyce, memo to W. H. Zinn, 29 April 1953, 1, which is referred to in Greenbaum.

<sup>22</sup> Although Greenbaum (p. 58) states that "nine physicists met," this is most likely incorrect. According to 1) J. C. Boyce, memo to W. H. Zinn, 29 April 1953, and 2) J. C. Boyce, letter to Chairman, Department of Physics, Wayne University, 20 July 1953, box 1, folder "Argonne National Laboratory," Keith R Symon Papers, 1953-1967, UW-Madison, there were 14 attendees, as is shown in Figure 10. Greenbaum's confusion most likely comes from the fact that only nine of the attendees signed the letter addressed to Thomas Johnson at the AEC.

<sup>23</sup> S. K. Allison, E. Fermi, R. G Herb, P. G. Kruger, J. J. Livingood, A. C. G. Mitchell, L. A. Turner, J. H. Williams, and W. H. Zinn, letter to T. H. Johnson, 30 January 1953, box 1, folder "Rollefson/MURA b<sub>1</sub>/f<sub>1</sub> 1953," Rollefson 7/26/14-2, Ragnar Rollefson Subject Files, UW-Madison, 1.

**Figure 10: "Midwest Cosmotron Project"**  
**First meeting, 30 January 1953**  
**Location: Argonne National Laboratory**

<u>Attendee</u>	<u>Institution</u>
J. C. Boyce	Argonne
Morton Hamermesh	Argonne
Norman Hilberry	Argonne
John J. Livingood	Argonne
Louis A. Turner	Argonne
Walter H. Zinn	Argonne
Allen C. G. Mitchell	Indiana University
S. K. Allison	Institute for Nuclear Studies, U of Chicago
Enrico Fermi	Institute for Nuclear Studies, U of Chicago
William B. Harrell	U of Chicago
Warren C. Johnson	U of Chicago
P. Gerald Kruger	U of Illinois
John H. Williams	U of Minnesota
R. G. Herb	U of Wisconsin

(Boyce to Zinn, memo 29 April 1953 and Boyce to Physics Department Chairs, letter 20 July 1953.)

any single institution to afford. The signers of the letter were an impressive collection; they included the director of the region's only Commission laboratory in Zinn, top physicists from Argonne and five different large universities, and one of the central heroes of the Manhattan Project in Fermi.

The assembly soon formed a technical working group whose first meeting was funded by Argonne and took place at Fermi's Institute for Nuclear Studies on the University of Chicago campus. The gatherings included discussions with Brookhaven physicists on a design for a large alternating gradient machine. The group continued to meet throughout the year, under the leadership of University of Illinois physicist Donald Kerst, with the participation of physicists from Argonne and from a number of midwestern university faculties.<sup>24</sup> After six weeks of meetings, three at Brookhaven and three at Madison, the technical group assigned particular problems to various university locations. These included: magnet model tests, large numerical computations requiring the use of an electronic computer, electron analogue studies, radiofrequency (rf) studies, using non-linearities to combat resonance problems, orbital stability in general, imbedding pole-tip laminations in a plastic vacuum tube, and theoretical studies.<sup>25</sup> [See

**Figure 11: MAC Conference attendance, 14-15 Nov. 1953.]**

As the technical studies and the general meetings continued, the group of university personnel overseeing the project became more solidified. Reluctant to work in a manner subordinate to Argonne management and its structure, they formed a separate organization in the Spring of 1954 "to further the development of high energy physics in

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<sup>24</sup> Donald Kerst had invented the betatron, a device for accelerating electrons using magnetic induction, in 1940. He enjoyed a reputation as one of the nation's top accelerator physicists.

<sup>25</sup> J. C. Boyce, memo to W. H. Zinn, 16 September 1953, box 3 of 4, notebook "Accelerator Correspondence 1953," RG 326, E-74, Box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II, 1. The meetings and their content are also discussed in Keith R. Symon, Memo to Russell Bright, 9 September 1953, box 3, folder "MURA Minutes," Keith R Symon Papers, 1953-1967, UW-Madison.

**Figure 11: "Midwest Accelerator Conference"**  
**14-15 November 1953**  
**Location: Institute for Nuclear Studies,**  
**U of Chicago.**

<u>Attendee</u>		<u>Institution</u>
J. C.	Boyce	Argonne
Mort	Hamermesh	Argonne
A. S.	Langsdorf	Argonne
John J.	Livingood	Argonne
N.	Francis	Indiana University
H. L.	Anderson	Institute for Nuclear Studies, U of Chicago
Enrico	Fermi	Institute for Nuclear Studies, U of Chicago
C.	Wright	Institute for Nuclear Studies, U of Chicago
L. Jackson	Laslett	Iowa State College
D. J.	Zaffarano	Iowa State College
Fritz	Rohrlich	State University of Iowa
Donald	Kerst	U of Illinois
J.	Snyder	U of Illinois
Francis	Cole	U of Iowa
Larry	Jones	U of Michigan
Kent	Terwilliger	U of Michigan
L.	Johnston	U of Minnesota
J.	Powell	U of Wisconsin
Ragnar	Rollefson	U of Wisconsin
Keith	Symon	Wayne University

(F. Cole, et al. "Minutes of Meeting of Technical Group held at Institute for Nuclear Studies, University of Chicago. 14 - 15 November 1953," box 3, folder "MURA Minutes," Keith R Symon Papers, 1953-1967, UW-Madison.)

the Midwest."<sup>26</sup> The Midwest University Research Association, MURA, legally incorporated in September of 1954,<sup>27</sup> already one of the most well-respected groups in the world on accelerator technology. MURA first requested money for design studies from the AEC. However, in June 1954 staff at the AEC made an initial decision not to support two separate laboratories in the Midwest and instead gave \$200,000 to be handled through Argonne for the same purpose.<sup>28</sup> MURA then succeeded in receiving design funding through the NSF. MURA continued research through meetings known as summer studies in which all interested parties were invited to participate, including university and Argonne personnel.

It was at one of the MURA summer studies in August of 1954 that a young researcher from Wayne State who would soon be lured to the University of Wisconsin suggested a new magnet design which looked to show enormous promise. Keith Symon's scheme for stacking beams in a synchrotron meant significantly higher intensities would be possible in machines of comparable diameter that would be much simpler and more reliable to operate.<sup>29</sup> It utilized a magnetic field that varied in space rather than over time.

The idea of a static magnetic field whose value/direction differed at different points of the beam orbits had previously been discussed for the cyclotron. In 1938 L. H. Thomas had suggested it as a possible solution to the limit seemingly reached as

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<sup>26</sup> Midwestern Universities Research Association, "Proposal for Cooperative Research in High Energy Physics Through the Establishment of a Laboratory in the Middle West Which Will Serve Best the Educational and Scientific Needs of That Area," 15 April 1955. Attachment to P. Gerald Kruger, letter to Thomas H. Johnson, 6 May 1955, P. Gerald Kruger Papers, University of Illinois, 3. For more on the complicated and acrimonious relationship between Argonne and MURA, see Greenberg (p. 223-224), Holl, (p. 158-168), and Greenbaum.

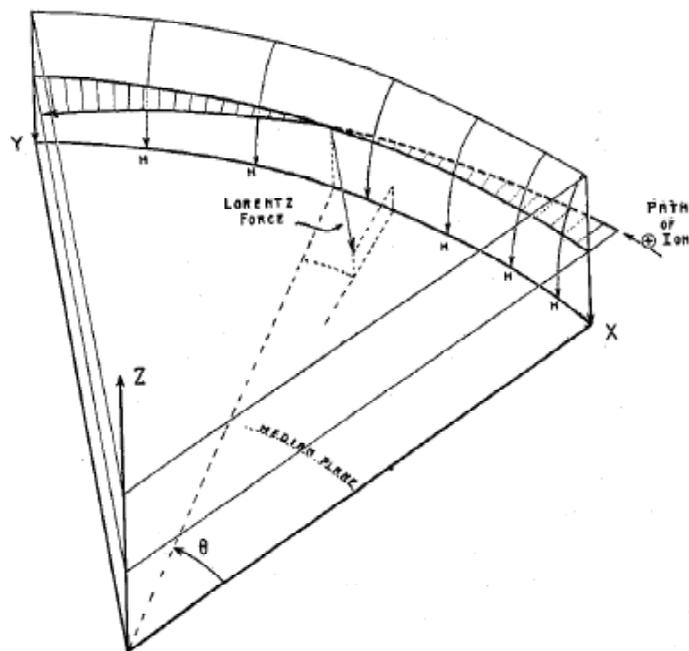
<sup>27</sup> Midwestern Universities Research Association, 15 April 1955, 3.

<sup>28</sup> L.R. Lunden, "Minutes of Meeting of Sub-committee on Securing Funds with Representatives of the Atomic Energy Commission Washington, D.C., June 10, 1954," 18 June 1954, box 1, folder "Minutes of Meetings 1953 - 55," P. Gerald Kruger Papers, University of Illinois, 2.

<sup>29</sup> L.R. Lunden, 18 June 1954, 4, B-1 - B-3.

cyclotrons attempted to accelerate particles to relativistic energies.<sup>30</sup> Seven years before the suggestion had been made to vary the frequency of the accelerating electric field as would be done in the synchrocyclotron, Thomas suggested varying the strength of the magnetic field in the various sectors throughout the orbit. [Figure 12: Focusing through Alternating Static Fields of Different Strengths.] Although this was understood to be an excellent idea, it was technically complicated, and no full-scale Thomas cyclotron for protons had yet been built successfully.<sup>31</sup>

**Figure 12: Focusing through Alternating Static Fields of Different Strengths.**



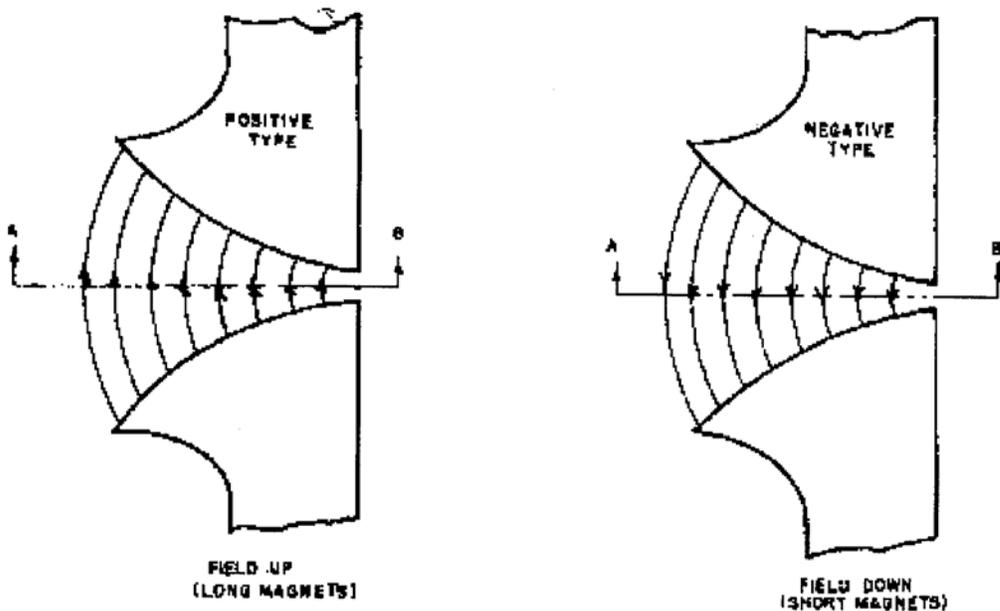
"The focusing effect due to variation of the field with polar angle." (L. H. Thomas, "The Paths of Ions in the Cyclotron. I Orbits in the Magnetic Field," *The Physical Review* 54 (1938) 580-588, copyright (1938) by the American Physical Society. Reprinted in *The Development of High Energy Accelerators*, M. Stanley Livingston, ed. (New York: Dover Publications, Inc., 1966): 307.)

<sup>30</sup> L. H. Thomas, "The Paths of Ions in the Cyclotron. I Orbits in the Magnetic Field," *The Physical Review* 54 (1938) 580-588. Reprinted in *The Development of High Energy Accelerators*, M. Stanley Livingston, ed. (New York: Dover Publications, Inc., 1966): 304-312.

<sup>31</sup> *The Development of High Energy Accelerators*, 313.

In Symon's case, the magnetic field would similarly vary by sector, this time by alternating the direction of the static field in each sector. Where the standard alternating gradient designs had to vary the strength of their magnetic fields in order to stay in step with the accelerating particles and steer them at the proper radius, the new design allowed high fields to be constantly present on the outer edge of the orbit. Hence, the field would increase as the orbital radius increased, allowing higher energy particles to exist in the same ring along with their lower energy brethren -- within magnets which could be run on simple, direct current. [Figure 13: FFAG magnet cross-section.]

**Figure 13: FFAG Magnets.**



A: Inner circumference; B: Outer circumference

Note that, for both the negative and positive types of magnets, the field strength increases as the orbital radius increases. This cross-section is for what was known as a "reversed field" magnet design. The other type, known as "spiral ridge", or sometimes "striped", was more complicated.

(Midwestern Universities Research Association. "Proposal for Cooperative Research in High Energy Physics Through the Establishment of a Laboratory in the Middle West Which Will Serve Best the Educational and Scientific Needs of That Area." 15 April 1955, Appendix B, Figure II.)

MURA immediately recognized the new possibilities for a high intensity, alternating gradient synchrotron and, by the Spring of 1955, felt ready to submit a general proposal to the AEC. The document, dated April 15<sup>th</sup> and sent to T. H Johnson the AEC Division of Research Director on May 6 outlined plans for a high energy physics research laboratory in the Midwest with a large Fixed Field, Alternating Gradient accelerator. The design and energy of the accelerator would not yet be set in stone, but the proposal provided an example of a 20 BeV machines that would require 5 years to build, cost approximately \$17 million, and have particles supplied by a 5 MeV Van de Graaff generator.<sup>32</sup> In particular, two designs had been developed by the technical group, one known as Spirally-Ridged and the other as Reversed Field. **[Figure 14: Spirally-Ridged and Reversed Field FFAG Configurations.]**

The first was more complicated and less well understood. MURA speculated that the ring, with an inner radius of 48.25 m and an outer radius of 50 m, would require 3,380 tons of iron, 154.5 tons of copper and produce two pulses per second containing  $10^{10}$  protons in each pulse. The iron requirement in particular, however, had a wide margin for error pending the continued design work and there was speculation that it could decrease by as much as 50%.<sup>33</sup> The Spirally-Ridged design was clearly MURA's preference, however, for it was the only type for which they provided a cost estimate.

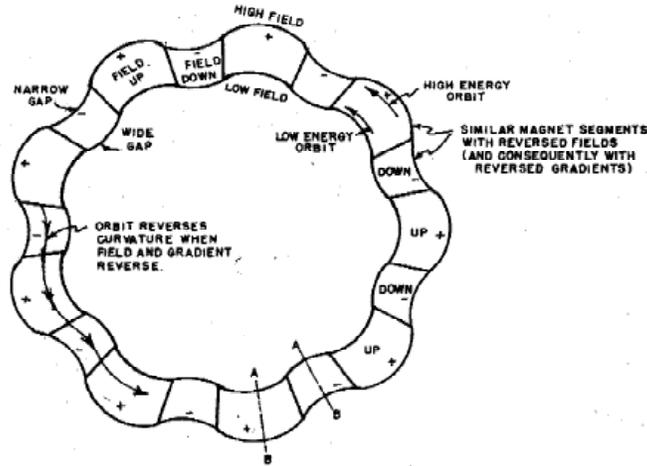
The FFAG Reversed Field Synchrotron was much more straight forward but would require a larger radius and hence more materials. MURA did provide an example of the parameters for a 10 BeV accelerator of this kind which included a 100 m radius, 9650 tons of iron, 670 tons of copper, and a yield of one pulse per second with  $10^{11}$  protons in each pulse.<sup>34</sup> The reason for their lack of enthusiasm was obvious. Such a

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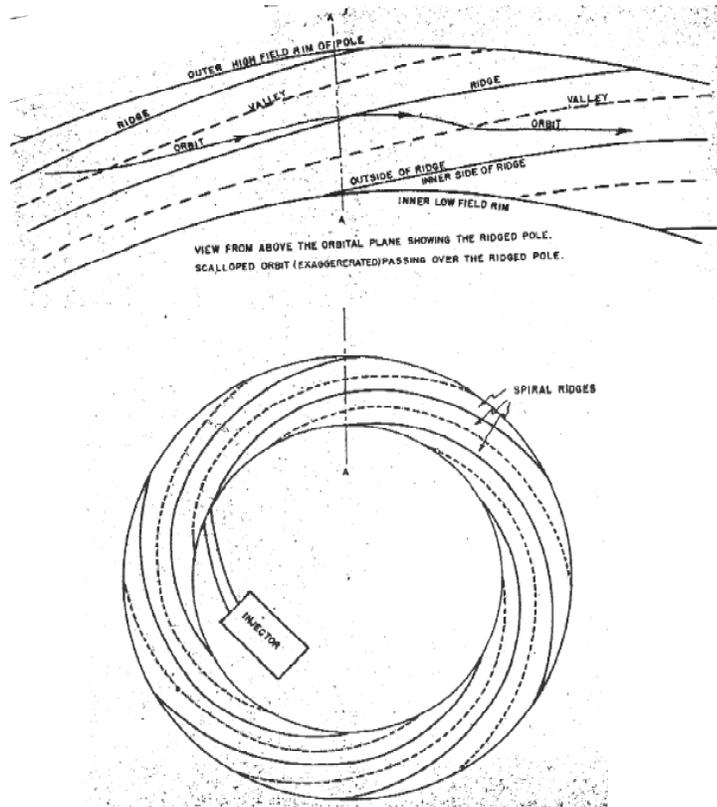
<sup>32</sup> Holl 165, 166. See also *Midwestern Universities Research Association*, 15 April 1955, 6-7 and Appendix C, page 5.

<sup>33</sup> *Midwestern Universities Research Association*, 15 April 1955, Appendix C, pages 5, 7-8.

<sup>34</sup> *Midwestern Universities Research Association*, 15 April 1955, Appendix D, pages 1, 11.



**Figure 14: Reversed Field (above) and Spirally-Ridged (below) FFAF Configurations.**  
 (Midwestern Universities Research Association, 15 April 1955, Appendix B, Figures 2 and 3.)



machine would provide one-third of the energy of the regular AG machines for roughly the same prices although it might produce 100 times the beam intensity. They openly

stated the drawbacks in their description.<sup>35</sup> Both designs assumed a 4 or 5 MeV Van de Graaff injector -- although the 20 Bev proposal did allow that a 10 or even 50 MeV linac might have to be used.<sup>36</sup> On May 18, Johnson wrote his answer to MURA stating that the AEC could not consider the proposal unless a relationship with Argonne was explicitly present. (He noted that the proposal neither "excluded" nor "ma[de] reference to" Argonne.)<sup>37</sup>

For its part Argonne had introduced laboratory seminars beginning in November 1953 to attract any personnel who might have an interest in the project and to bring them up to speed on recent research. As Argonne physicist John Livingood wrote in late September,

"[I]t is almost certain that practically none [of the potentially interested ANL personnel] are conversant with what progress has been accomplished by active groups, such as BNL, Cambridge, Princeton or Europe. . . The goal of this work would be to produce at ANL . . small groups who thoroughly understand the arguments and conclusions already arrived at by the study groups elsewhere." (emphasis in original)

Livingood hoped to engender studies in "orbital stability", "magnetic field production and control", "radio frequency acceleration", and/or "programming techniques".<sup>38</sup> The lectures were initially to be delivered by himself and by a second Argonne physicist, Mort Hamermesh. Eventually, it was hoped, audience members would begin to add to the series, presenting on their own expertise.<sup>39</sup> The initial regulars included only Livingood,

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<sup>35</sup> Midwestern Universities Research Association, 15 April 1955, Appendix C, pages 1.

<sup>36</sup> Midwestern Universities Research Association, 15 April 1955, Appendix C, pages 13-15, and Appendix D, page 1.

<sup>37</sup> T. H. Johnson, letter to P. Gerald Kruger, 18 May 1955, box 1, folder "Rollefson/MURA f<sub>6</sub> 1 Apr - 30 June 1955," Rollefson 7/26/14-2, Ragnar Rollefson Subject Files, UW-Madison.

<sup>38</sup> J. J. Livingood, memo to L. A. Turner, J. C. Boyce, and M. Hamermesh, 28 September 1953, box 3 of 4, notebook "Accelerator Correspondence 1953," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II, 1.

<sup>39</sup> "Lectures on Particle Accelerators," 12 November 1953, box 3 of 4, notebook "Accelerator Correspondence 1953," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.

Hamermesh, Ed Crosbie, who had come to Argonne in 1952 fresh out of graduate school, and a fourth addition who was destined for "the theory group," Mel Ferentz.<sup>40</sup>

After a year of study sessions and participation in what would become the MURA technical group, the outline of a possible Argonne accelerator began to materialize. Progress Reports began to be typed up by the Argonne Accelerator Group in December of 1954.<sup>41</sup> Early 1955 saw the addition of John Martin, who would later work on radiofrequency acceleration, and magnet designer Martyn Foss, who had helped to design the cyclotron at Carnegie Tech.<sup>42</sup> Machine designs began to take shape. One of the earliest suggestions envisioned a 30 Bev FFAG synchrotron injected from a 4 Mev Van de Graaff to be completed in FY1963 at a total cost of approximately \$40 million. The design would use 8600 tons of steel and 1400 tons of copper in a magnet whose mean radius was to be 960 feet. It would generate an estimated  $10^9$  to  $10^{10}$  protons every 3 to 4 seconds.<sup>43</sup>

The general justification for the machine was explained as follows:

Extremely interesting and significant results have followed laborious cosmic ray studies, and the gratifying yield of artificially produced mesons from the few accelerators of sufficient energy now in existence indicates that this field will increase rapidly if facilities for the copious production of energetic projectiles are further expanded. Such expansion is urgently needed in the Mid-west. The Argonne National Laboratory has been assigned responsibility for the Commission's research program in the Mid-west.

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<sup>40</sup> John J. Livingood, "The Beginning," *History of the ZGS*, 4 and 8.

<sup>41</sup> Argonne Accelerator Group, "Progress Report #1," 27 December 1954, RG 434, box 310b, folder "Argonne Accelerator Group Progress Reports 1 thru 10," NARA-GL.

<sup>42</sup> John J. Livingood, "The Beginning," *History of the ZGS*, 5 and 7; Malcolm Derrick, interview with Paris, 15 July 2003, tape recording, Argonne National Laboratory; and John Dawson, interview with Paris, 28 May 2003, tape recording, Argonne National Laboratory.

<sup>43</sup> J. J. L[ivingood], "Draft Proposal for ANL Accelerator," 18 February 1955, box 3 of 4, notebook "Accelerator Correspondence 1955," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.

Therefore, it is required that such a facility be constructed at the Argonne site and administered within the overall program of the Laboratory.<sup>44</sup>

Livingood sent the Argonne group's ideas to Zinn and soon added a scaled-down version whose protons would only reach 10 Bev using 9650 tons of steel and 670 tons of copper in a magnet whose mean radius would stretch 340 ft at a total cost of \$20 million and completion in FY1961.<sup>45</sup> He also included a proposal for a 10 Bev AGS machine which would cost approximately \$13 million. He noted, however, that "the FFAG principle . . . constitutes . . . a true advance in the art of particle acceleration" and that it was likely to have several advantages over AGS designs for the future. "The up shot is," he wrote, "we would prefer to build an FFAG of some type, even though it will cost more per volt than an AGS." And he hoped to persuade Zinn with a personnel rationale as well:

Furthermore, MURA has a very fatherly interest in the FFAGs; if ANL is allotted funds for an accelerator and decides on the FFAGs, MURA would be more likely to join forces with us, I believe, rather than if we decided for an AGS, in which case they would probably continue to hunt for a fairy-godmother to support their promising infant. Their group has talent and enthusiasm, and I would hate to throw away a single chance to bring about an alliance.<sup>46</sup>

At this time there was no thought that ANL would consider anything other than AG or FFAG designs. These were the designs which represented the current state of the art (and beyond).

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<sup>44</sup> J. J. Livingood, 18 February 1955, 9. For more on the history of cosmic ray research and the move to accelerators, see Peter Galison, *Image and Logic* (Chicago: University of Chicago Press, 1997): especially chapters 2 and 3.

<sup>45</sup> "Proposal for 10 Bev Fixed Field Alternating Gradient Synchrotron," [sic.] 2 March 1955. Attached to J. J. Livingood, memo to W. H. Zinn, 2 March 1955, box 3 of 4, notebook "Accelerator Correspondence 1955," RG 326, E-74, Box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.

<sup>46</sup> J. J. Livingood, memo to W. H. Zinn, 2 March 1955, box 3 of 4, notebook "Accelerator Correspondence 1955," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.

Slowly Argonne had enlarged its group. Flush with the research money provided by the AEC, Lee Teng, among others, had joined the work.<sup>47</sup> Teng proved to be a particularly fortuitous acquisition. One of the few members of the group who would enjoy a top-tier reputation in accelerator theory amongst the wider community, four years earlier Teng had collaborated with Jim Tuck to develop a theoretical scheme to extract beam directly from synchrocyclotrons.<sup>48</sup> As noted earlier, beam extraction from circular machines represented one of their main disadvantages, and any improvements meant great strides for accelerator design. The scheme had been made to work by Albert Crewe, with theoretical help from K. J. Lecouteur, on a synchrocyclotron in England. In fact, it was a Teng-like injection scheme that could be considered the unique design element that unmistakably distinguished an Argonne 25 July 1955 proposal from that of MURA's on whose heels the Argonne proposal would so quickly followed.

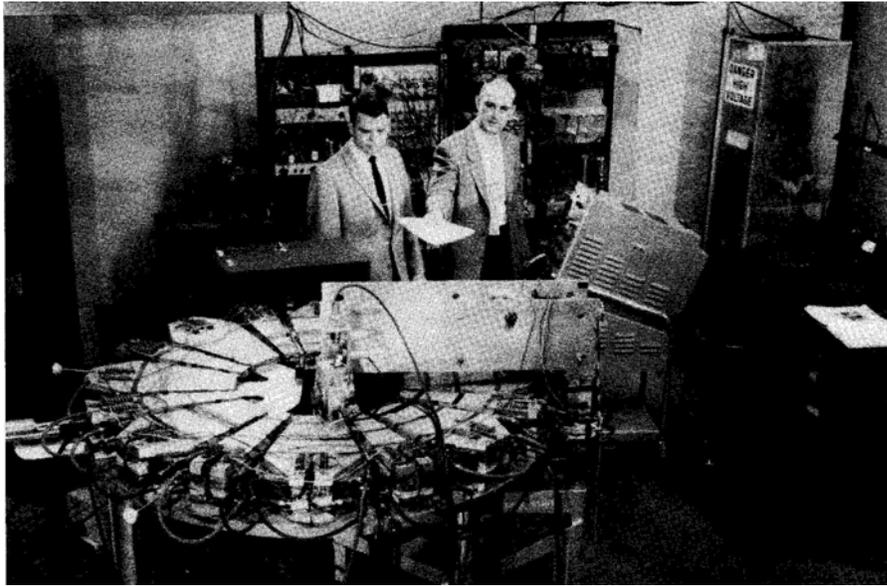
Having reinvented a focusing scheme for synchrotrons, soon Symon, Kerst, and the rest of the technical group at MURA began working on electron models for first one and then the second possible configurations. **[Figure 15: MURA FFAG models.]** So far successful at obtaining money for design work from their member institutions and the National Science Foundation, MURA still hoped eventually to garner support from the AEC for construction of its machine.

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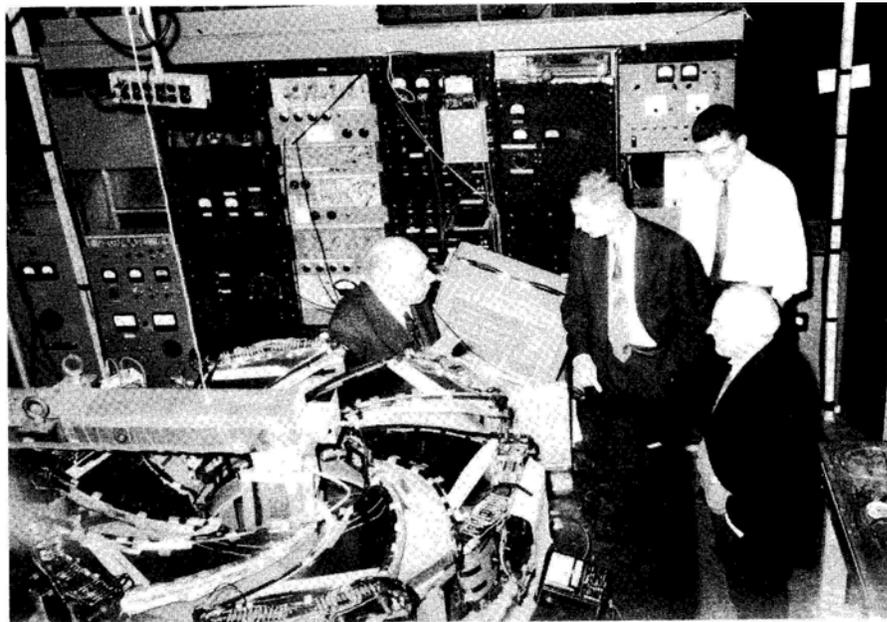
<sup>47</sup> Livingood, *History of the ZGS*, 4 - 5; and Ed Crosbie, et al., "Proposal for a High Current 25 BeV Proton Accelerator at Argonne National Laboratory," 25 July 1955, RG 434, ANL, description "Working Files of J. J. Livingood, 1955 – 1966," box 342a, folder "Genesis of ZGS: Early Proposals, etc. J. J. Livingood," NARA-GL, 1.

<sup>48</sup> J. L. Tuck, and L. C. Teng. *Phys Rev.* **81** (1951): 305.

**Figure 15. FFAG Models.**



[Reversed Field.] 1955. Left: Charles Pruett, right: Keith Symon.



[Spirally Ridged.] Built 1956 - 1959. Left: Neils Bohr, Right background to foreground: Fred Mills, [Robert O.] Haxby, E. B. Fred.<sup>49</sup>

(Both photos from Pellegrini and Sessler 1995, 7. Both photos from the personal collection of Andy Sessler, copyright Springer-Verlag and Andy Sessler.)

<sup>49</sup> In Pellegrini and Sessler, the middle gentleman is identified as "Gordon Haxby". However, it is actually Robert O. Haxby. (Fred Mills, personal communication, 30 October 2003).

## Argonne's July 1955 Proposal

On July 25, 1955, the Argonne group turned out its first official proposal for the Atomic Energy Commission.<sup>50</sup> The general configuration consisted of two circular accelerators operating in tandem. The first was a 2 Bev proton synchrocyclotron with the need for magnetic field modulation partially ameliorated by the use of FFAG magnets. This machine would also be able to be operated as a stand-alone source of high intensity 2 Bev protons. The second was an FFAG ring magnet with an average radius of 150 feet which would take the 2 Bev protons from the initial ring and accelerate them to 25 Bev. It did not explicitly discuss the method of extraction from the main ring, nor any experimental equipment such as targets or detectors -- though it did mention construction of a target building as one of the many structures expected to be financed as part of the proposal.

The method of extraction from the synchrocyclotron and injection into the main ring can be said to have originated with Teng. It involved a regenerative deflector followed by a regenerative "inflector". As the particles in the synchrocyclotron gained energy, they traveled in orbits of ever increasing radius inside the machine. Once they reached the orbit commensurate with 2 Bev, they would encounter an additional magnetic field which would increase their radial oscillations. As the oscillations became larger, these high energy particles traveled to radial distances from the center of the machine well beyond any of the other particles. As they reached this radius at a particular point along the circumference, they would encounter another magnetic field which steered them beyond their orbit altogether and into the inner edge of the main ring. As they entered the main ring, they encountered a field which steered them in exactly the opposite direction from the one they had just left in the synchrocyclotron; thus momentarily placing them back in what would basically be the same orbital radius from which they had just exited.

However, as they traveled in their first pass around the main ring, they encountered the radiofrequency cavity which served to accelerate them, boosting their energy and hence their radius in this FFAG synchrotron, so that when they again passed the spot on the circumference which contained the inflector field at its inner edge, they had moved well beyond its influence.

The proposal's regenerative deflector scheme for protons was an heir to that first dreamed of by Tuck and Teng in 1951. When Teng joined the Argonne accelerator group in 1955, he brought enthusiasm for the multi-stage acceleration scheme upon which this proposal depended. The proposal made more than one reference to the fact that a regenerative deflector was currently functioning at the synchrocyclotron in Liverpool based on Tuck and Teng's design.<sup>51</sup>

Indeed, having Teng could be said to have been a great advantage for the Argonne group. He seems to have been the ANL employee who did the most work on the FFAG idea -- calculating 2<sup>nd</sup> order (non-linear) effects among other things. The proposal claimed that FFAG was "now fairly well understood" but "not yet demonstrated in practice"<sup>52</sup> without mentioning MURA by name or the ongoing research there. In fact, seemingly the only explicit mention of MURA was on page 16 when the authors stated that further details on FFAG could be found in three places: Bulletin of the American Physical Society, MURA memoranda, and Progress Reports of the Argonne Accelerator Group.

The proposal requested \$30 million over eight years for construction beginning in FY1958 and noted that more money could result in higher energy if desired. In addition, the proposal indicated \$2.5 million over four years beginning in FY1956 for research and

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<sup>50</sup> Crosbie, et al., 25 July 1955.

<sup>51</sup> According to Al Crewe there were some problems with the original scheme that were ironed out by the Liverpool folk. In 1955 the scheme was installed (under Al Crewe's direction) on an accelerator at the University of Chicago. Al Crewe, interview with Paris, tape recording, 12 November 2002, University of Chicago; and Roger Hildebrand, interview with Paris, tape recording, 12 November 2002, University of Chicago.

<sup>52</sup> Page 25.

development. Argonne justified its proposal in two ways: scientific need – it would have high energy and high intensity; and geographic need – more machines in the Midwest. “The scientific rewards expected from a copious source of 25 BeV particles is well known in informed circles” (p.6) and high intensity 2 Bev beams have several desired uses (p. 6 and 33).<sup>53</sup> There was no elucidation on the first scientific point; the only argument offered was that two other groups already had plans to construct machines at this energy (presumably, Brookhaven and CERN). The Argonne machine would then have the added bonus of high intensity at this energy. On page 4 and 15 the proposal also mentions the “laborious[ness]” of cosmic ray experiments and the possibility of studying similar phenomena using this accelerator.

The meeting of needs **in the Midwest** is reiterated at least three times: on p. 4, “In itself, [the 2 Bev synchrocyclotron] will afford to the Midwest a research tool of unprecedented potency in the minimum possible time . . .”; on p. 6, “The adoption of two-stage acceleration will put an operating 2 Bev machine in the Middle West within 6 years”; and, on p. 15, “This facility constructed at Argonne would meet the needs of the various research groups in the Midwest.” The authors were appealing directly to the notion of Argonne as a **regional** laboratory. Their view -- on this question of the purpose of a “national” laboratory with which the AEC struggled mightily during this period -- was clear.

One might imagine that defensiveness of at least some of the MURA personnel may, indeed, have been quite similar to what John Livingood noted hearing at a MURA conference several months later, "ANL's proposal was the same as MURA's earlier one.

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<sup>53</sup> The 2 Bev particles would allow study of: “deviations from statistical behavior” of “multiplicity and charge distribution of mesons . . . as a function of energy” “in nucleon-nucleon collisions”; dependence of pion distribution on angular momentum; angular correlation of hyperon decay products (lower energies so less Lorentz contraction in

ANL has stolen the FFAG business . . . There is not room for 2 machines of the same energy in the mid west [sic.]”<sup>54</sup>

### **The 1955 Atoms for Peace Conference**

In fact, just as the Argonne proposal arrived in Washington in 1955, most of the top AEC personnel were departing for an historic meeting in Geneva. On December 8, 1953, US President Dwight D. Eisenhower's had delivered a speech before the UN General Assembly in which he suggested the creation of an International Atomic Energy Agency. In early 1954, the idea for a global conference on the Peaceful Uses of Atomic Energy was born. (Argonne was heavily involved, being the US government's primary laboratory for nuclear reactor research.<sup>55</sup>) The first International Conference was held in Geneva from August 8 - 20, 1955.

Although the conference centered on nuclear power, there were many informal sessions scheduled outside of the regular program. One such evening of talks was scheduled to take place on August 11 including presentations by US and Soviet high energy physicists on recent developments in their respective countries.<sup>56</sup> On August 7, the two well-known speakers from that session, Ernest Lawrence and Vladimir Veksler arranged to have dinner with just themselves and a few colleagues.<sup>57</sup> During that dinner

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forward direction but intensity means one can still; get lots of hyperons; and new fields of radio-chemical research. (p. 33)

<sup>54</sup> John Livingood, handwritten notes on MURA Meetings, 7 January 1956, RG 434, ANL, Description "Working Files of J. J. Livingood, 1955 – 1966," box 342b, notebook "John J. Livingood's collection of Memoranda and Letters about MURA and Argonne," NARA-GL. (Emphasis in original.)

<sup>55</sup> See *Atoms for Peace, International Conference on the Peaceful Uses of Atomic Energy, Geneva, August 1955*. United States Atomic Energy Commission. n.p., n.d. and Holl, 138-141.

<sup>56</sup> Glenn Theodore Seaborg, "Thursday, August 11, 1955," *Journal of Glenn T. Seaborg*, Vol. 9, January 1, 1955 - December 31, 1955, PUB-676, Vol. 9, July 1990, Berkeley: Lawrence Berkeley Laboratory, 1990, 252, container number 18, Glenn Theodore Seaborg Papers, Manuscript Division, Library of Congress, Washington, D.C.

<sup>57</sup> Glenn Theodore Seaborg, "Sunday, August 7, 1955," *Journal of Glenn T. Seaborg*, Vol. 9, January 1, 1955 - December 31, 1955. PUB-676, Vol. 9, July 1990. Berkeley: Lawrence Berkeley Laboratory, 1990, 247-248, container number 18, Glenn Theodore Seaborg Papers, Manuscript Division, Library of Congress, Washington, D.C. See also

the Americans present received a most surprising piece of news. When Lawrence casually asked Veksler what he would be speaking about a few evenings later, Veksler proceeded to describe Soviet work on a "10 Bev phasotron", also called by the Soviets a "sychrophasotron," a kind of machine known in the US as a bevatron. From discussions at dinner as well as Veksler's talk a few days later, it seemed that the USSR was fairly well along in their project at Dubna, and would likely have their machine up and running in a couple of years, eclipsing the energy of Berkeley's 6 Bev Bevatron and having the opportunity to work alone at energies of 6 - 10 Bev for several years to come. The next large U.S. machine was to be an 30 GeV AG proton synchrotron at Brookhaven, but it was not scheduled to be ready until 1960 at the earliest.

For the physicists this news was surprising, but for a certain set of citizens (some physicists, some not), it was alarming. For here we may invoke an argument of historian of science and technology John Krige that the ideas of "national security" during this period should not be reduced to military preparedness but were understood as a "struggle for the hearts and minds of men." Science and technology were instruments of foreign policy, a way to show non-aligned nations that that a particular form of government produced a superior, more desirable overall society.<sup>58</sup> To these individuals, it seemed imperative that the United States begin a crash course to build a machine quickly which would reduce the period of Soviet superiority in energy, at least until Brookhaven's AGS could come on line and blow the competition away. It is likely that at least one or two of the AEC Commissioners felt this way. The chairman, Admiral Lewis Strauss, was reportedly "perturbed at possible Russian supremacy in the field and will probably fight

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Laura Fermi, *Atoms for the World: United States Participation in the Conference on the Peaceful Uses of Atomic Energy* (Chicago: University of Chicago Press, 1957).

<sup>58</sup> John Krige, "The Three Faces of Science in the 1950s."

for lots of accelerator money."<sup>59</sup> Many of these same individuals were involved in the question of the purpose of the national labs and in the regional difficulties within the middle west between MURA and Argonne. Gradually, a solution -- of a sort -- seemed to unfold.

### **GAC Solution**

In the days before the Geneva conference, representatives from MURA and from the AEC, including acting chairman Willard Libby, had met in D.C. and heard presentation of two proposals.<sup>60</sup> The first, given by University of Chicago President Lawrence Kimpton, was to create a new Division of High Energy Physics at Argonne whose Director would report to the ANL Director.<sup>61</sup> The second, suggested by a MURA representative, involved a completely separate laboratory at the Argonne site "administered along the same lines as Brookhaven that is, by a consortium of area universities."<sup>62</sup> Then, four days after Libby returned from Geneva, he met again with representatives from MURA and informed them that no definite monetary support would be immediately forthcoming for them from the AEC. Three weeks later MURA, much to its relief was awarded \$100,000 for research support from the National Science Foundation. Meanwhile, a week later, on September 22, Libby reported to the rest of the Commission that, "Middle West [problems which caused delay] . . . have been resolved"

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<sup>59</sup> J. J. Livingood, Report of M. Stanley Livingston's comments at the High Energy Accelerator Conference in Urbana on October 9, 1955, memo to W. H. Zinn, 10 October 1955, box 3 of 4, notebook "Accelerator Correspondence 1955," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II, 3.

<sup>60</sup> Holl, 167. See also "Notes on Meeting of the Atomic Energy Commission with Representatives of Midwestern Universities on the Question of AEC Support of a Basic Research Center in the Midwest," 3 August 1955, box 2, folder "MURA History and Related Documents," Gerald Kruger Papers, University of Illinois, Exhibit-C, 1-8.

<sup>61</sup> Board of Trustees Minutes, 11 August 1955, University of Chicago Special Collections, Chicago, Illinois, 126 - 128.

<sup>62</sup> See also "Notes on Meeting of the Atomic Energy Commission with Representatives of Midwestern Universities on the Question of AEC Support of a Basic Research Center in the Midwest." 3 August 1955, Exhibit-C, page 6, 9-11.

and that a proposal should be forthcoming by November 1<sup>st</sup>.<sup>63</sup> In the same meeting, Thomas Johnson, Director of the AEC Division of Research, indicated that actual construction funds would not be immediately necessary, that only design funds would be needed for FY57. This represented a bit of a change since the original MAC meetings which began in 1953 had projected FY57 for the beginning of construction. Even without construction funds in the current year's budget, however, the Commission wished nevertheless to go to the 1956 Congress to request overall authorization for the project.<sup>64</sup>

Word of the new Soviet machine had rippled through the members of the General Advisory Committee to the Atomic Energy Commission including I. I. Rabi. This was an august body of well-known scientists with enormous political clout to whom the Commission looked for guidance on matters of both policy and technology. According to Holl, in an October 31 meeting called between the AEC and GAC to examine the topic, Rabi suggested a plan.<sup>65</sup> The US needed a quickie machine, and scientists in the Midwest were screaming for large accelerators. Why not ask them to build a conventional one quickly, with an energy high enough to beat the Russians, and give the next machine after the AGS to the brilliant designers at MURA? It would resolve the MURA/Argonne stalemate, address geographic inequity, and respond to the Russian challenge. The pieces appeared to fall beautifully in place.<sup>66</sup>

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<sup>63</sup> W. B. McCool, "Atomic Energy Commission, Meeting No. 1126," minutes, 22 September 1955, Volume 16, RG 326 E-19 Office of the Secretary: Minutes of the Meetings of the AEC, NARA-II, 661-662.

<sup>64</sup> W. B. McCool, "Atomic Energy Commission, Meeting No. 1126," minutes, 22 September 1955, 662.

<sup>65</sup> Holl, 169. For an indication that the meeting was being scheduled to take place, see "Atomic Energy Commission, Meeting No. 1141," 24 October 1955, Volume 16, RG 326 E-19 Office of the Secretary, Minutes of the Meetings of the AEC, NARA-II, 728.

<sup>66</sup> Holl also indicates that, at this meeting, Thomas Johnson "estimated that Rabi's idea to assemble an unscaled bevatron at Argonne would cost only \$12 - \$15 million." I was unable to examine Holl's source, but I think that this is unlikely or that Johnson momentarily mis-spoke. I believe the confusion arises from the fact that there are three different design ideas for Argonne being discussed. One is Argonne's desired FFAG tandem accelerator, the large project which was causing so much friction between Argonne and MURA. The second is a for a scaled-up weak-focusing device which, it seems, may very well have been given its first serious consideration at this meeting. In any case, the Argonne personnel located its origination to approximately this time. (See L. A. Turner, memo to W. H. Zinn, 23 January 1956, "Comments on Accelerator Program," Box 3 of 4, notebook "Accelerator Correspondence 1956," RG

Naturally MURA was pleased with the plan. They felt they had finally received the endorsement they had been looking for from the AEC, a commitment to build their laboratory and a cutting-edge accelerator without the oppressive link to Argonne. MURA would be the scientific effort, Argonne's machine would merely be a stop-gap measure, a quick and dirty engineering effort utilizing already proven designs. The MURA Director wrote his assessment of the decision in a confidential memo to a member of the MURA Board:

[W]e understand that ANL is to [go] forward with a machine on the following basis

- a) a short time crash program of 3 - 3 1/2 year[s] to build a machine.
- b) the machine is to be of essentially conventional design. . . .
- c) that the man power needed and to be used is to be mainly engineering manpower -- not scientific manpower i.e. not physicists.<sup>67</sup>

The contingencies of the Cold War seemed to have provided an opportunity for MURA to flourish without a dilution of their research effort or resources that might have resulted from of a more cutting-edge project at Argonne.

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326, E-74, Box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.) However, I have seen no other indication that Johnson or anyone else at this time so grossly underestimated the cost of such a beast, which would have been roughly twice as expensive as we shall later see. The third machine is a half-scale AGS, Those familiar with the Brookhaven project would very likely have priced this at somewhere around \$15 million, as Ken Green apparently did during a large accelerator conference held at Argonne in April of 1955. ("Minutes of the Accelerator Conference Held at Argonne National Laboratory on April 23, 1955," 6 May 1955, box 3 of 4, notebook "Accelerator Correspondence 1955," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II, 10) In fact, Livingood had communicated a similar price of \$13 million for a 10 Bev AGS to Zinn when he presented him with comparisons of different design options in March of 1955. ("Proposal for a 10-Bev Alternating Gradient Synchrotron," 1 March 1955. Attached to J. J. Livingood, memo to W. H. Zinn, 2 March 1955, box 3 of 4, notebook "Accelerator Correspondence 1955," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.) And, indeed, the \$15 million authorization request made by the AEC to the 1956 Congress was made with the AGS number in mind. (*AEC Authorizing Legislation*, 1957, 63, as quoted in Greenberg, 1999, 221; and T. H. Johnson, "General Advisory Committee, Notes on Research Matters," 29 October 1956, RG 326 E-67 Division of Research "Correspondence Relating to Physical Research Program and Policies, 1946 - 1957," box 3, folder 21 "Organization and Management: Research Management in the Commission's Laboratories, 1951 - 1957," NARA-II, 1.)

<sup>67</sup> P. Gerald Kruger, handwritten memo to Pres. Hovde, 7 December 1955, box 2, folder "AEC defection 30 Nov 1955," P. Gerald Kruger Papers, University of Illinois. (Emphasis in original.)

Zinn, for his part, was not pleased at all. He had been convinced to postpone the resignation letter that he had submitted earlier in the month when the AEC had endorsed the solution of a completely separate high energy physics lab on the Argonne campus. The new solution of allowing Argonne to build a medium energy machine (including hoping against hope that both ANL and MURA personnel would remain at least somewhat involved in each) was certainly not ideal. However, the feeling seemed to have been that Argonne could play the good AEC citizen and build the quickie machine while still being able to continue with their FFAG design work, the work that motivated Argonne's own staff. Most importantly, Zinn told both Libby and Strauss on separate occasions \*not\* to commit Argonne to any specific design strategy<sup>68</sup> (beyond those already set forth of energy and of a somewhat nebulous schedule of construction.)

### **Argonne's January 1956 Design Memo**

Thus, as far as the Argonne designers knew, they were still working on their multi-stage, FFAG idea, even as they had to come up with a design that could be built quickly. The idea for a 10 - 15 Bev bevatron-type machine seemed ludicrous. It would require inordinate amounts of iron, costing as much as the AGS for one third or one half of the energy. As early as November 12, 1955, Zinn informed various AEC employees that Argonne was most likely incapable of fulfilling the requirements of a crash program to build a 12 Bev, scaled-up bevatron.<sup>69</sup> During the rest of November and into December the character of the Argonne project was in limbo as the designers (mostly) continue to follow their hearts and Zinn lobbied in Chicago and in Washington for them to be able to

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<sup>68</sup> Walter H. Zinn, "Summary of Communication on Accelerator Situation," 11 January 1956, box 3 of 4, notebook "Accelerator Correspondence 1956," RG 326, E-74, Box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II, 1-2.

<sup>69</sup> J. C. Boyce, Argonne National Laboratory Executive Board Council of Participating Institutions, "Minutes of Special Meeting December 7, 1955, Cleveland, Ohio," box 132, folder "Council Executive Board -- Minutes of Meeting Re-Council ANL Relationship," Argonne Universities Association Records, 1945-1982, University of Illinois, 2.

do so. At a cocktail party in the middle of January in honor of the visiting GAC members, Livingood discovered from Thomas Johnson that the tandem machine was reportedly "not acceptable" to the Commissioners, but Johnson did not know why. According to Livingood, Johnson inquired after other possible designs since Ken Green of Brookhaven had added his voice to the chorus of those who disparaged the usefulness of scaling up the bevatron.

ANL members expressed themselves as not at all interested in building a copy of any existing machines, blown-up or blown down. We are interested in FFAG-ism, and feel that we have as much right to it as anyone . . . <sup>70</sup>

But the Commissioners seemed to be insisting and there seemed to be some sentiment that "if [Argonne doesn't] get construction money from Congress soon, [it] may never get it."<sup>71</sup> On January 24, 1956, Zinn requested from Livingood's group a description and cost estimate that would fit the fantastic parameters the AEC seemed to be proposing.<sup>72</sup> Perhaps Zinn put his team through the exercise to prove once and for all what an irrational scheme the AEC was suggesting. Livingood and his group complied with Zinn's request, taking seriously the parameters of a "10 to 15 Bev proton accelerator which could be built in the minimum possible time, and hence with the least developmental work." In their response Livingood and Martyn Foss noted that there were no similar machines currently in existence (10 - 15 Bev) and simply "scaling up" the Bevatron or Cosmotron was not directly possible, but would require a minimum of one

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<sup>70</sup> J. J. Livingood, memo to W. H. Zinn, 17 January 1956, "Conversation with Tom Johnson, January 11, 1956, at ANL," box 3 of 4, notebook "Accelerator Correspondence 1956," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II, 1.

<sup>71</sup> J. J. Livingood, memo to W. H. Zinn, 17 January 1956, "Cocktail Party for G.A.C. -- January 12, 1956," box 3 of 4, notebook "Accelerator Correspondence 1956," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II, 1.

<sup>72</sup> J. J. Livingood and M. Foss, memo to W. H. Zinn, 25 January 1956, RG 434, ANL, Description Working Files of J. J. Livingood, 1955 - 1966," box 342a, folder "Genesis of ZGS: Early Proposals, etc. J. J. Livingood" NARA-GL, 1.

year of magnet design studies. However, AG magnet studies would require 3 to 4 years alone, so they priced the project assuming the former design. Thus they estimated that, for a constant gradient synchrotron, the price tag would be approximately \$30 million.<sup>73</sup>

The AEC had requested a budget by February 1<sup>st</sup>, and it was more than clear (which Zinn had promptly communicated) that no reasonable machine proposal which would also meet the Commission criteria could possibly be created by then.<sup>74</sup> The FY57 construction deadline they were chasing had been appropriated by the "quickie" proponents (for Soviet-competing reasons or others). The budget they were chasing belonged to a half-scale AGS design which did not fit the timeline they proposed and about which Zinn's staff was not particularly enthusiastic. Zinn had finally had it; he turned in his final letter of resignation on January 27.<sup>75</sup> It was reported to the University of Chicago Board of Trustees on February 9<sup>th</sup> that "after extended discussions with Mr. Zinn [sic.] his resignation had been accepted with regret."<sup>76</sup> It was two days earlier that the Chicago office of the AEC posted a letter to Zinn acknowledging that ANL had communicated that it would be impossible to build a 10 - 15 Bev machine of any design if construction were required to begin in FY1957.<sup>77</sup>

### **Further Argonne Designs**

Although the question may or may not have been posed directly at the January 24th meeting, "Do you want to build such a machine or do you want to quit?"<sup>78</sup> may certainly have been the order of the day for the Argonne accelerator group. However, on

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<sup>73</sup> J. J. Livingood and M. Foss, 25 January 1956, 1.

<sup>74</sup> W. H. Zinn, letter to J. J. Flaherty, 26 January 1956, box 3 of 4, notebook "Accelerator Correspondence 1956," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II, 1-2.

<sup>75</sup> W. H. Zinn, letter to W. B. Harrell, 27 January 1956, Director's Subject Files, box B93-00129, folder "Argonne National Laboratory Directorship, 1956 - 1957," ANL. For further reasons for Zinn's resignation, see Holl, 172-174.

<sup>76</sup> Board of Trustees Minutes, 9 February 1956, University of Chicago Special Collections, Chicago, Illinois.

<sup>77</sup> J. J. Flaherty, letter to W. H. Zinn, 7 February 1956, box 3 of 4, notebook "Accelerator Correspondence 1956," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.

January 26, the physicists at the lab still believed they might continue with a variety of research approaches towards a 10 - 15 Bev machine. Foss and Livingood sent a memorandum to Zinn indicating their proposed areas of study including: innovative injection schemes using a linac or no pre-accelerator and the possible that one of these ideas would result in a reasonable cost for constant gradient machine; injection from a cyclotron; Thomas or FFAG cyclotrons for injection; flat pole magnets; AG machines; and FFAG ring magnets.<sup>79</sup>

The ANL group hung on to their tandem accelerator concept, going through several possible design strategies including an idea they wrote up in March for a 12.5 GeV machine which utilized an unusual magnet design and was to be injected from a 50 MeV Thomas-type cyclotron . They felt the new direction might be something to get excited about: rather than use magnets containing a radial gradient, they considered a resurrected design of one with parallel faces. Instead of the prohibitively bulky C-magnets of the Cosmotron, they would use H-magnets, but without the central core, resulting in a "window-frame"-like cross section. The scheme had originally been suggested to the group by Martyn Foss , so, for a while during early 1956, the design was referred to as the "Fossotron," but the name didn't stick.<sup>80</sup> The focusing force normally supplied by the gradient of magnet faces would instead be accomplished by using the edges of the entire magnet. That is to say; the focusing would be accomplished by the angle at which the incoming and then outgoing particle encountered the magnetic field and would not rely on the positive (or negative) gradients of the pole faces. Parallel pole

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<sup>78</sup> L. C. Teng, "The ZGS -- Conception to Turn-on," *History of the ZGS*, 11.

<sup>79</sup> J. J. Livingood and M. Foss, 26 January 1956, memo to W. H. Zinn, 26 January 1956, box 3 of 4, notebook "Accelerator Correspondence 1956," RG 326, E-74, box 32x "Argonne National Laboratory Accelerator Correspondence 1953-1956," NARA-II.

<sup>80</sup> L.C. Teng, "Focusing Condition for a Fossotron without Correction for Fringing Fields," 22 February 1956, ANLAD - Internal - 11 [Argonne National Laboratory Accelerator Division], RG 434, box 310b, folder "ANLAD Internal 11 thru 20," NARA-GL; E. A. Crosbie, "Some Effects of Errors in Building the 'Fossotron'," 1 March 1956, ANLAD -

faces have a zero gradient. The Argonne group felt such zero gradient magnets would be less complicated and therefore less expensive and time-consuming to build and test than the prohibitive AG design. In addition, the group believed the pole-less design would mean the ability to reach significantly higher magnetic fields than in the case of the previously utilized constant gradient magnets. Thus, they felt the idea created an affordable path toward a somewhat more interesting, weak focusing machine which might be a unique tool in its own right rather than a half-scale, half-energy, johnny-come-lately, second-class copy of Brookhaven's AGS. It seemed that the Midwest folks wanted a regional accelerator, but, just like high energy physicists in all of the other labs around the country, they wanted it to have capabilities that were nationally (or even internationally) unique.

When Jack Livingood presented the idea at one of the regular MURA technical meetings in late February at Michigan State, his notes from the experience suggest some MURA personnel may not have been too enthusiastic:

Livingood was asked to speak for ANL's plans -- and I wish I hadn't, for the result was, that having knocked ANL down, MURA proceeded to stamp on our prostrate form with bloody glee.<sup>81</sup>

The well-respected designers at MURA simply did not find the idea very technically impressive. The Argonne group was definitely discouraged. They already seemed to be fighting an uphill battle to be able even to talk about design strategies that had brought them together in the first place such as alternating gradient focusing, FFAG, and tandem acceleration at the cutting-edge. And they were struggling mightily to find some

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Internal - 13 [Argonne National Laboratory Accelerator Division], RG 434, box 310b, folder "ANLAD Internal 11 thru 20," NARA-GL; Lee C. Teng, personal communication, 2 October 2003.

innovation which could begin to fulfill their interpretation of the AEC requirements. As late as April 2, 1956, Teng was still writing internal ANLAD reports addressing specific technical aspects of the alternating gradient scheme.<sup>82</sup>

In April 1956, ANL created a new division, with Livingood at its helm, in anticipation of the organization and manpower needed to construct a machine and provide competent planning and accurate cost estimates.

The machine design evolved over the late Spring and Summer of 1956. The realization that ANL did not have time to do the proper tests for creating even an FFAG cyclotron (never mind a synchrotron) meant the ring design was confined to proven technology. In addition, they moved to a 50 MeV linear accelerator as the injector for several reasons such as the quality of the beam and the reduction of gas scattering. Since the machine would now begin operation at approximately the same time as Brookhaven's AGS, superior energy was no longer the sole selling point. ANL had continued to develop the innovative magnet design which they felt would allow much higher *intensity* than any machine then operating, and this was the aspect in which they now felt their machine could be a unique tool and a boon for Midwestern physics. Initially priced at \$22 million by Livingood alone in June, the enlarged staff was able to perform the estimates more accurately and, on November 1, the price tag sat at \$27 million.

The ANL organization had answered, "Yes." They did still want to build a large proton accelerator on their campus. Due to their zero gradient design they felt the available intensity could be significantly larger than in an AG synchrotron. Thus, their machine would not simply be a second-class version of Brookhaven's AGS, but a unique

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<sup>81</sup> John Livingood, handwritten notes on MURA Meetings, 25 February 1956, RG 434, ANL, Description "Working Files of J. J. Livingood, 1955 – 1966," box 342b, notebook "John J. Livingood's collection of Memoranda and Letters about MURA and Argonne," NARA-GL.

<sup>82</sup> L.C. Teng, "General Theory of Second Order Non-Linear Effects in an AG Accelerator with Median Plane," 2 April 1956, ANLAD - Internal - 9 [Argonne National Laboratory Accelerator Division], RG 434, Box 310b, folder "ANLAD Internal 11 thru 20," NARA-GL

tool in its own right, possessing abilities not available elsewhere. Brookhaven would have higher energy protons, but Argonne would have higher intensities of them. The group had finally found something to ignite their enthusiasm for what had been looking more and more like a lifeless project. However, did the AEC and those considering the national and international contexts still want what they had designed? In fact, closer to home, did other Midwestern physicists?

### **Spring 1956 MURA Designs**

Also in the Spring of 1956, MURA submitted a new proposal to the AEC. Contained within it were novel suggestions for the application of FFAG that had begun to be discussed in late 1955. These included taking advantage of the increased intensity provided by the design to make it experimentally feasible to collide protons head-on. The energetic advantages of such a configuration were enormous. More than twice the center of mass energy would be available over standard machines like the AGS whose accelerated, relativistic protons were to be aimed primarily at stationary protons in a hydrogen bubble chamber.

MURA continued its own campaign for a "dream" machine, the matter of Argonne's "engineering" effort no longer of much concern one way or the other. They now focused on convincing the AEC that the next big machine should be of their novel design, built in the Midwest, and at a newly created laboratory completely separate from Argonne.<sup>83</sup> The conflict between the other universities' desire for a separate laboratory and the University of Chicago and the AEC's desire to make Argonne the center of basic research in the Midwest had lead to an extensive study of Argonne's relationship with the

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<sup>83</sup> See "Notes on Meeting of the Atomic Energy Commission with Representatives of Midwestern Universities on Their Proposal for a High-Energy Accelerator in the Midwest," 25 May 1956. Attached to "Historical Sketch of the Midwestern Universities Research Association," 14 February 1957, n.a., box 2, folder "MURA History and Related Documents," P. Gerald Kruger Papers, University of Illinois.

area's universities and an attempt, at least, to positively restructure Argonne's organization.<sup>84</sup>

### Approved Design

From April 1956, ANL worked diligently to obtain the go ahead so that an Architect-Engineer could be hired and construction could begin on the buildings which would be needed to house the machine and the personnel who were to build, operate, and utilize it. Twelve and a half Bev continued to be the energy of choice since it was "adequate for the production of all known particles" and "to drop to 10 Bev would give no margin over the Soviet machine of that energy."<sup>85</sup> The high field obtainable through the use of the picture frame magnet design [See **Figure 16: Cross-section of Picture Frame Magnet.**] would result in a radius of approximately 94 feet, and the projected beam intensity stood at  $10^{12}$  protons per pulse at 12.5 Bev. Hearings were held in April 1957 to increase the appropriation from the original \$15 million to \$27 million. (This was, coincidentally, when the Soviet 10 Bev machine was initially put into operation.<sup>86</sup>) Congress approved the change and substituted the new figure for the old. However, the money did not physically change hands, held up by the Bureau of the Budget,<sup>87</sup> and there were rumblings that the project might be cancelled. With only \$1 million actually disbursed, it would not be too late.

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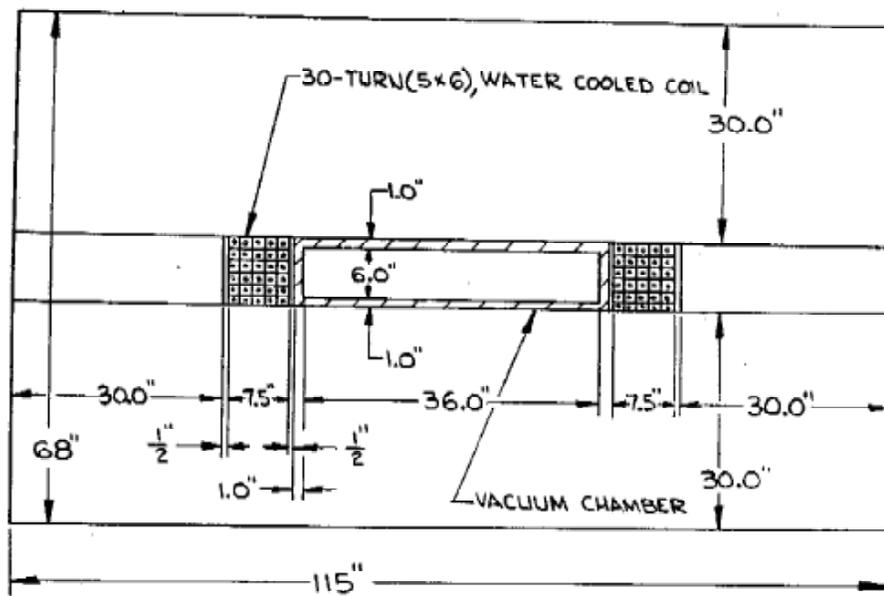
<sup>84</sup> For more on this entire effort, see Greenbaum, 1971, chapter 9.

<sup>85</sup> Crosbie, et al. "Report to the United States Atomic Energy Commission On [sic.] the Design Characteristics of the 12.5 Bev Proton Synchrotron (ZGS) for the Argonne National Laboratory," J. J. Livingood, ed., 20 March 1957, RG 434, ANL, Description Working Files of J. J. Livingood, 1955 – 1966," box 342a, folder "Genesis of ZGS: Early Proposals, etc. J. J. Livingood," NARA-GL, 1.

<sup>86</sup> V. I. Veksler, et al., "Physical Factors in the Design of the 10-Bev Proton-Synchrotron," translated by Consultants Bureau, Inc., *Uskoriteli Elementarnykh Chastits*, Supplement No. 4 of *Atomnaya Energiya*, (Moscow: Atomic Press, 1957), published as *Elementary Particle Accelerators* (New York: Consultants Bureau, Inc, 1958): 1.

<sup>87</sup> "Argonne Preferred for A-Smasher," 9 December 1957, *C & EN*, box 2, folder "Rollefson/MURA 12 1 Oct 1957 - 31 Dec 1957," Rollefson 7/26/14-2, Ragnar Rollefson Subject Files, UW-Madison, 59; "Midwest Seeking Atom-Smasher," 24 November 1957, *New York Times*, box 2, folder "Rollefson/MURA 12 1 Oct 1957 - 31 Dec 1957," Rollefson 7/26/14-2, Ragnar Rollefson Subject Files, UW-Madison.

Figure 16: Cross-section of Picture Frame Magnet from 20 March 1957.



MAGNET CROSS SECTION APPROPRIATE FOR  
A MAXIMUM FIELD OF 20,000 GAUSS

(Crosbie, et al., 20 March 1957, 8.)

In the Spring of 1957, the AEC Division of Research had sent the Argonne Group's March 20<sup>th</sup> report concerning their chosen design characteristics to 48 scientists, both accelerator designers and users, for comments. By the end of May, the AEC had received 40 replies, amassing a collection of support, criticism, and suggestions.<sup>88</sup> Among the many issues raised again by the some of the reviewers was the wisdom of using a constant gradient design rather than simply building a scaled-down AGS. As part of Livingood's "replies to many of the criticisms," he addressed this concern, not by appealing once again to greater time requirements necessary for AG magnet studies,

<sup>88</sup> T. H. Johnson, memo to C. E. Falk, "Summary of Outside Reviews of Argonne 12.5 BeV Accelerator," 5 June 1957, 1. Enclosure in letter from Charles E. Falk to John J. Livingood, 13 June 1957, RG 434, ANL, Description "Working Files of J. J. Livingood, 1955 - 1966," box 342a, folder "Genesis of ZGS: Early Proposals, etc. J. J. Livingood," NARA-GL.

which had been one of the justifications noted in the March 20<sup>th</sup> report, but by a more detailed account of the other justification. He reiterated, "It is our contention that it is well worth while to spend less than twice as much as a 12.5 Bev AGS would cost in order to obtain particles of equal energy but with an intensity 10 to 100 times as great."<sup>89</sup> As Livingood had said in 1956, the ANL group was not interested in a scaled-down machine. The enthusiasm of the group had been revived and maintained through the possibilities for a unique design with unique capabilities. As a way to make their mission palatable, the Argonne staff, "with the concordance of many experimentalists," had settled on high intensity as their goal.<sup>90</sup>

Once again, on October 4, 1957, international considerations may have entered the accelerator building game. US scientists, politicians, and citizens were taken wholly by surprise as the USSR successfully launched the Earth's first man-made satellite, Sputnik. MURA, in particular, used the event to lobby hard for complete AEC authorization of their machine. The chairman of the physics department at Wayne State wrote,

In the Spring of 1957 the Soviet Union began operating a particle accelerator . . . almost twice the size of the largest American machine. Although little publicity has been given to this Russian advance up to now, a discovery made with the accelerator could, at any time, create a furore [sic.] similar to that caused by the launching of the satellite.<sup>91</sup>

And the President of the MURA Board of Directors entreated the Head of the MURA Technical Group,

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<sup>89</sup> J. J. Livingood, [statement supplied to C. E. Faulk,] 27 June 1957, RG 434, ANL, Description Working Files of J. J. Livingood, 1955 – 1966," box 342a, folder "Genesis of ZGS: Early Proposals, etc. J. J. Livingood," NARA-GL.

<sup>90</sup> Crosbie, et al., 20 March 1957, 1.

<sup>91</sup> J. E. Thomas, "The MURA-AEC Controversy," box 1, folder "MURA-AEC Controversy (J. E. Thomas, Jr.)," 22 October 1957, Keith R Symon Papers, 1953-1967, UW-Madison, 2.

"Due to the effects of Sputnik, I think it would be appropriate if you [Kerst] would write to C. P. Anderson [a member of the JCAE] pointing out that MURA and the Midwest physicists are probably the only group competent to build an accelerator with sufficiently high energy and in short enough time to prevent Russia from another coup d'etat [with their upcoming 50 Gev machine] -- next time in high energy physics."<sup>92</sup>

The fight for the "hearts and minds of men" was going poorly. It appeared as if the Soviet system was more successful with technology and science, and the US needed to rise to the challenge. However, the General Advisory Committee apparently had already been on board (although Sputnik intervened between their meetings on September 30, October 1 and 2 and October 14th, when their recommendations were presented to the AEC),

The Committee [Subcommittee on Research of the General Advisory Committee to the Atomic Energy Commission] recommends that the Commission proceed with urgency to authorize that Argonne National Laboratory to do all things necessary to design and build the 12.5 Bev machine they have proposed for construction. Consideration has been given to the suggestion that the Argonne build, in effect a half-scale model of the AGS now under construction at Brookhaven. It does appear, however, that the weak focusing machine will, in fact, work and that it presents no unusual problems; that it will produce a beam which will be more intense by a factor of 5 to 20 than the Brookhaven AGS machine. (These figures are subject to interpretation and argument but represent a reasonable minimum and probable maximum.) To now revise drastically the Argonne design will gain little, if any, time."<sup>93</sup>

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<sup>92</sup> P. Gerald Kruger, letter to D. W. Kerst, 23 October 1957, box 5, folder "MURA Strategy 1955 -59," Donald William Kerst Papers, 1937-1959, University of Illinois.

<sup>93</sup> Warren C. Johnson, letter to Lewis L. Strauss, 14 October 1957, box 5, folder "55<sup>th</sup> GAC Meeting held in Washington, D.C. on September 30, October 1 and 2, 1957," RG 326, Entry "General Advisory Committee Minutes," NARA-II, 3; Frederick L. Hovde, letter to MURA Board of Directors, 21 January 1956, box 1, folder "Board Meeting, Jan. 23, 1956," P. G. Kruger Papers, 1937-1970, University of Illinois, 1.

Whether or not the BoB was explicitly responding to Sputnik, \$1.5 million was finally released for ANL to hire its A-E in December,<sup>94</sup> and by January 1958 the PAD staff had grown to over 70 individuals.<sup>95</sup>

### **Tidying Organizational Matters**

In spite of the authorization and the successful staff recruitment, the project seemed to be bogging down. On March 7 and 8, 1958, a committee full of nationally respected accelerator physicists was appointed by Chicago chancellor Lawrence Kimpton to "review the work and plans of the Particle Accelerator Division."<sup>96</sup> In their report submitted to Kimpton on March 20, they reiterated support for a strong Midwest accelerator program. However, they had concerns about the "level of leadership and the associated level of competency of some of the staff of the division." In particular, they felt that, given the history of Argonne's accelerator project the Particle Accelerator Division Director's job was currently an exceptionally difficult one.

A director charged with the responsibility of designing and constructing a major accelerator must continually balance the immediate needs of the accelerator construction program with the requirements of future physics research. He should have sufficient engineering knowledge that he can evaluate the work of experts, judge their qualifications and intermingle their designs. He should also be a person who is well acquainted with modern high energy physics research to the extent that he can see how the general goals of an experimental program can be achieved. He should have the ability to combine these talents in such a fashion as to channel the diverse skills and knowledge of his staff to contribute most effectively to the solutions of the overall program.

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<sup>94</sup> N[orman] H[ilberry], note to J. J. L. [John J. Livingood.], copy of "TELETYPE – AEC Washington to AEC Chicago – December 10, 1957," 13 December 1957, RG 434, ANL, Description "Working Files of J. J. Livingood, 1955 – 1966," box 342b, notebook "John J. Livingood's collection of Memos and Notes on the ZGS," NARA-GL.

<sup>95</sup> John J. Livingood, *History of the ZGS*, 9; and "Particle Accelerator Division Summary Report," 10/1/57 - 4/15/58, May 1958, ANL-5864, 62-63.

<sup>96</sup> John H. Williams, letter to L.H. Kimpton, 20 March 1958, enclosure in a memo by W. B. Harrell to Warren C. Johnson, 26 May 1958, Director's Subject Files, Box B93-00149, folder "ANL, ZGS, 1958," ANL, 1. The members of the committee taking part in the review were: H.L. Anderson, J.P. Blewett, H.R. Crane, M.S. Livingston, E.J. Lofgren, W.K.H. Panofsky, and J.H. Williams.

We have reluctantly come to the conclusion that although the present Director is a fine and devoted person and a more that competent physicist, he does not possess all of the qualifications for this difficult job. On the other hand we believe that, considering the basically disadvantageous conditions set for this project it may be difficult to obtain the services of a more suitable man. The best candidate, in our opinion, should arise from group most interested in the use of the machine in performing high energy physics research, namely the group of physicists at the University of Chicago and the other neighboring universities.<sup>97</sup>

The review committee's recommendation was followed.<sup>98</sup> John Livingood who had been the heart of the Argonne efforts from the earliest formation of the accelerator study group, having successfully engaged the services of Sverdrup and Parcel as the projects A-E, "exercised his prerogative as a Senior Scientist of the Laboratory and requested that he be relieved of his administrative duties as Director of the Particle Accelerator Division"<sup>99</sup> and stepped down to write a text book on accelerators.<sup>100</sup> Effective September 1, 1958, University of Chicago physicist Roger Hildebrand became the Associate Laboratory Director for High Energy Physics, and Albert Crewe, his colleague at Chicago, became the director of the Particle Accelerator Division.

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<sup>97</sup> John H. Williams, 20 March 1958, 1-2.

<sup>98</sup> Lawrence A. Kimpton, letter to H. R. Crane, 20 October 1958, box 8, "Review Committees = Chemistry, Met. Engr, PAD, HEP," folder "Rev. Committee - PAD and HEP, Feb., 1958 - Dec., 1961," RG 326, row 366. Argonne National Laboratory Records of Policy Advisory Boards (1957 - 67), Atomic Energy Commission, NARA-GL; and Norman Hilberry, letter to R. W. Harrison, 10 November 1960, box 8, "Review Committees = Chemistry, Met. Engr, PAD, HEP," folder "Rev. Committee - PAD and HEP, Feb., 1958 - Dec., 1961," RG 326, row 366. Argonne National Records of Policy Advisory Boards (1957 - 67), Atomic Energy Commission, NARA-GL.

<sup>99</sup> A. V. Crewe, memo to J.R. Gilbreath, "History of the Particle Accelerator Division," 27 March 1959, accession #434-91-0013, box #2, unnamed folder, NARA-GL, 4.

<sup>100</sup> John J. Livingood, *History of the ZGS*, 9. See also John J. Livingood, handwritten notes to self, 7 September 1958, where Livingood says he believes the project is now "launched successfully," and he "is 'pooped'." Being the oldest person in the Division, he says he's relinquishing to some "younger person" who can advance things "more rapidly." The textbook in question is presumably John J. Livingood, *Principles of Cyclic Particle Accelerators* (Princeton, N.J.: Van Nostrand, 1961).

By February 1959 all of the ducks were in a row, and Argonne received "full and final approval for the construction of the ZGS."<sup>101</sup> Placating the perceived need for fresh leadership was not, in fact, Hildebrand and Crewe's most important role. For, concurrent with this change, came a new emphasis on those who would use the machine and a new willingness by area university physicists to become involved with the planning of such uses. In response to this involvement, beam extraction, design alterations, and experimental areas would come into being, and the relationship between the Argonne accelerator and its users would become a highly-praised model for future machines.

## **Conclusion**

So, on June 27, 1959, Argonne finally broke ground on a weak (but unique brand of weak) focusing machine of a design that no one had originally intended or even desired. As the machine's construction evolved, so, too, did its reasons for being. By a 1960 status report, the historical information characterized the energy choice as having been made for experimental reasons, "it is high enough for production of all the strange particles and for production of baryon pairs . . . to go much below 10 Bev would offer no advantage over the Bevatron now operating at Berkeley and would not allow production of baryon pairs."<sup>102</sup> The Russian machine, which had apparently never functioned up to expectations and whose energy, in any case, would be tripled by Brookhaven's AGS in less than a month, was no longer mentioned. The magnet design continued to be cited for its ability to accommodate very high magnetic fields, allowing a smaller machine radius and creating a situation which, along with the choice of vacuum chamber dimensions,

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<sup>101</sup>A. V. Crewe, 27 March 1959, 5.

<sup>102</sup> Particle Accelerator Division, "The Zero Gradient Synchrotron: Description of the Project and Status Report," 1 July 1960, Argonne National Laboratory, Argonne, Illinois, accession #434 87-0009, box 19, item # 103 "Unidentified ZGS Records," NARA-GL, 8.

would allow "a high intensity machine."<sup>103</sup> The hope was now eventually to go as high as  $10^{13}$  protons per pulse.<sup>104</sup> The ZGS came on line in 1963 and operate until 1979. It would fulfill its role as a Midwestern regional accelerator although it would never possess world-leading energies or orders-of-magnitude-leading intensities,<sup>105</sup> the capabilities for which the ANL personnel had initially yearned both with their enthusiastic pursuit of FFAG and later with their creative compromise to a zero gradient design.<sup>106</sup>

This isn't to say that the ANL physicists involved in the design of what would become the ZGS did not find the project stimulating, for a high energy accelerator is like an old time ball field, "Build it, and they will come." Hundreds of physicists, engineers, and technicians put their heart and creative souls into the project and were and remain quite proud of their accomplishments. And the machine would be upgraded and would host a multitude of experiments before its shut down in 1979. Interestingly enough, although those involved continue to back the project with varying degrees of fervor, it is hard to find a physicist who was not directly involved who holds a positive impression

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<sup>103</sup> Particle Accelerator Division, 1 July 1960, 13.

<sup>104</sup> Particle Accelerator Division, 1 July 1960, 17.

<sup>105</sup> The original AGS capabilities for intensity had been underestimated at  $10^9$  protons/pulse. According to Fred Mills (private communication, 14 November 2003), in 1970, even before a 1972 upgrade of its injector, the AGS intensity had climbed to  $10^{12}$ . As for the ZGS, it initially had difficulties, but eventually made it to  $10^{12}$  after some trouble-shooting in 1967. (Lee Teng, private communication, 6 November 2003 and Fred Mills, interview, 29 May 2003, and private communication, 14 November 2003.) There are conflicting report as to whether the ZGS intensity ever numerically exceeded that of the AGS at any given time. Lee Teng claims that the ZGS's  $7 \times 10^{12}$  was slightly higher than that of the AGS before the latter's upgrade in 1972 (Teng, 6 November 2003). However, former MURA participant Frank Cole claimed that, while the ZGS was being built, "[Ken] Green offered to bet any sum of money the Argonne people cared to wager that at any time the AGS would always have higher intensity than the ZGS. The Argonne people refused, which was just as well for them, because Green would have won the bet hands down." (Frank Cole, *O! Camelot: A Memoir of the MURA Years*, 11 April 1994, 36, published in F. Marti, *Cyclotrons and their applications 2001: Sixteenth International Conference, East Lansing, Michigan, 13-17 May 2001*, (Melville, N.Y. : American Institute of Physics, 2001).) In either case, Argonne's desire for a machine whose intensities bested any other by at least an order of magnitude at energies of 12.5 GeV was not fulfilled.

<sup>106</sup> For a list of the experiments approved for the ZGS and their results, see *History of the ZGS*, 328-370. Although beyond the scope of this report, the design modifications, experiments, and user relations accomplished in the years between 1959 and 1979, particularly in regard to the Users Group and the 1971 decision to accelerate polarized protons, are extremely rich pieces of history of high energy physics both nationally and in the Midwest.

counter to that which I first experienced concerning the decision to build it in the first place. This seems to be true no matter what the level of knowledge about the particular technological choices involved or about surrounding events. However, no matter how one feels in hindsight about its overall right to exist, the credit or blame for the original design cannot possibly be attributed to a single entity, for proton behavior, local designers, regional relationships, national agencies, and international politics all had a hand in the shape of what would become the Zero Gradient Synchrotron. [Figure 17: Picture of ZGS.]

**Figure 17: Architect Drawing of ZGS Facilities, 1960**



ARCHITECT'S DRAWING OF ZERO GRADIENT SYNCHROTRON (ZGS) FACILITIES

Please credit  
Sverdrup & Parcel, Architect-Engineers

(138-270)

(Particle Accelerator Division, "The Zero Gradient Synchrotron: Description of the Project and Status Report," 1 July 1960, Argonne National Laboratory, Argonne, Illinois, accession # 434 87-0009, box 19, item # 103 "Unidentified ZGS Records," NARA-GL., iii)

