

# O CAMELOT !

## A MEMOIR OF THE MURA YEARS\*

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### Preface

One day Mervyn Hine of CERN and I were sitting in the Fermilab coffee lounge and I grumbled, "The way people talk about MURA, you'd think it was Camelot." He answered very earnestly, "Oh, it was, Frank, it really was! "

Perhaps it was, but few people seem to know anything about it. For years I have been saying to any historian of science I could get to listen that she or he is missing a good bet by concentrating so heavily on the history of large laboratories. Large parts of the physics underpinning of contemporary particle accelerators, including:

- (i) beam stacking,
- (ii) Hamiltonian theory of longitudinal motion,
- (iii) useful colliding beams (the idea itself is quite old),
- (iv) storage rings (independently invented by O'Neill),
- (v) spiral-sector geometry used in isochronous cyclotrons,
- (vi) lattices with zero-dispersion and low- $\beta$  sections for colliding beams,
- (vii) multiturn injection into a strong-focusing lattice,
- (viii) first calculations of the effects of nonlinear forces in accelerators,
- (ix) first space-charge calculations including effects of the beam surroundings,
- (x) first experimental measurement of space-charge effects,
- (xi) theory of negative-mass and other collective instabilities and correction systems,
- (xii) the use of digital computation in design of orbits, magnets, and rf structures,
- (xiii) proof of the existence of chaos in digital computation, and
- (xiv) synchrotron-radiation rings

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\* Note by the Editor: We have included this "Memoir of the MURA years" by the late Frank Cole, kindly provided to us by F. E. Mills and D. Young. They asked us to keep intact the manuscript we received to present Cole's point of view. The conference organizers considered it valuable to present it here in spite of not being able to discuss with the author the contents of the paper.

were first done by people associated with the Midwestern Universities Research Association group (MURA) in the 1950's and 1960's. There are certainly more items that could be added, but this list will do to start. All this development certainly deserves chronicling. Kerst<sup>1</sup> and Symon<sup>2</sup> have each given short accounts and an attempt has been made to tell the political part of the MURA history,<sup>3</sup> but nobody has ever tried to give a complete account of the technical part. I have finally despaired of interesting historians in the subject and have written this account of the MURA years.

This paper is a personal memoir, not a scholarly historical study. It is written in a roughly chronological sequence, even though this gives rise to some skipping back and forth between topics, because I want to show the interplay of the technical activities with the politics.

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## A. HISTORICAL BACKGROUND

### 1. Particle Accelerators Before World War II

#### 1.1 Electrostatic Generators, Voltage Multipliers, and Cyclotrons

X-ray tubes were used extensively in the 1920's and 1930's following the development of a sealed tube by Coolidge<sup>4</sup>. They were actively used in medical diagnosis and treatment, and produced electron beams of up to 300 keV,<sup>5</sup> but the energies of interest for study of the atomic nucleus rapidly passed them by. The later 1920's were the beginning of a time of ferment in accelerator development, most notably with the first resonance acceleration by Rolf Wideroe,<sup>6</sup> who did not pursue it as far as any nuclear-physics use.

In the 1930's, there was a vigorous nuclear-physics program in the United States based on the electrostatic generator invented and developed by Robert Van de Graaff.<sup>7</sup> Pressurized tanks were first built at Princeton by Barton, Mueller, and L.C. Van Atta.<sup>8</sup> Raymond Herb and his collaborators developed pressurized tanks much more extensively and reached 4 MeV in 1940.<sup>9</sup> A substantial part of the nuclear-physics results of the early 1930's came from electrostatic generators, particularly in the work of Tuve, Hafstad, and Dahl<sup>10</sup> at the Carnegie Institution of Washington, and from voltage multipliers such as the Cockcroft-Walton generator,<sup>11</sup> on which the first nuclear-physics experiment using an artificially accelerated beam was performed. Cockcroft and Walton made use of an existing voltage-multiplying circuit<sup>12</sup>; most of their effort went into development of an accelerating tube. Through the course of the 30's, as interest moved to higher and higher energy, more and more of the physics results began to come from circular accelerators, even though the only variety of circular accelerator existing was the classical cyclotron. It had been invented by Ernest Lawrence,<sup>13,14</sup> who was inspired by Wideroe's work (Wideroe had a sketch of a cyclotron in his laboratory notebook, but did not do anything with the circular geometry beyond this; Lawrence was unaware of Wideroe's cyclotron sketch) and extensively developed and used it at his Radiation Laboratory in Berkeley.

Following Lawrence's own way of thinking about physics, the development was much more empirical than theoretical. There was a qualitative understanding of focusing from the earliest days. His student, M. Stanley Livingston, who first demonstrated resonance acceleration, has said that he and Lawrence realized when they were working on the first cyclotron that the vertical guide field must decrease with radius for the device to operate and that they understood that the physical reason for this is that the decrease gives vertical focusing to the beam. There was some inchoate understanding that what we would now call the *horizontal tune* (the number of waves of oscillation per revolution in the plane of the orbits) was close to unity; it was discussed in terms of the "motion of the orbit center." In 1938, Robert Wilson, then a student of Lawrence's, investigated the focusing effect of the accelerating electric field in a cyclotron (important near the center) and published a paper<sup>15</sup> on it, one of the first papers that contained orbit equations. Separately, the difficulty of maintaining resonance with the radiofrequency accelerating field when particles began to be relativistic (about 20 MeV or so in the cyclotrons of

the day) was also understood. There was a paper by H.A. Bethe and M.E. Rose<sup>16</sup> pointing it out. Bethe has commented that this paper was his only contribution to accelerator physics - and a negative one at that.

## 1.2 The Thomas Cyclotron

In the same year, in an attempt to overcome the relativistic difficulty, L.H. Thomas published a mysterious paper<sup>17</sup> that proposed an azimuthally varying guide field to keep the particles and the accelerating field in resonance and discussed focusing in this field. Horizontal focusing was provided by the same centrifugal-force mechanism as in conventional cyclotrons and vertical focusing was provided by the azimuthal field variation. The increase of the azimuthal field variation with radius was tailored to keep the beam isochronous into the relativistic region.

The problem with Thomas' paper was that it was couched in very unfamiliar mathematics, an expansion in powers of  $v/c$ . Nobody understood it at the time and it remained a mystery to most until the invention of strong focusing gave people a new way to look at orbits. In the meantime, Lawrence proceeded with construction of what would become the 184-inch cyclotron, planning to use very large accelerating voltages to jam some particles through. Nothing was known about phase stability, but it was known empirically that there was a minimum rf voltage for acceleration and that beam intensity improved as the rf voltage was raised above the minimum.

## 1.3 The Betatron

Since the 1920's, many people had been attracted to the concept of accelerating particles by electromagnetic induction. For one thing, the technology of transformers was well advanced, while the technology of high-power radiofrequency systems was somewhat rudimentary; Lawrence had trouble finding rf equipment for high power at 10 MHz for the first cyclotrons. None of these attempts at electromagnetic induction had produced anything at all usable for physics research, (as in the cyclotron case, Wideroe had a notebook sketch, but did nothing with it), although there were other claims of priority that muddied the situation in later years. In 1940, at the University of Illinois, Donald Kerst built a circular induction accelerator, which he called a *betatron*. He accelerated electrons to 2 MeV in 1940<sup>18</sup> and in 1941, published a landmark paper<sup>19</sup> on its construction and operation. Together with Robert Serber, he published a companion paper<sup>20</sup> on particle focusing and injection (Serber has always said that he supplied only the theoretical, mathematical blessing for Kerst's already formed ideas). Kerst was voted a large amount of money (several million dollars) by the Illinois legislature to build a larger betatron and a laboratory. He built a 20-MeV betatron at the General Electric Company in Schenectady before the war intervened and it was copied in considerable numbers by the Allis-Chalmers Company for use in X-raying large castings for weapons (particularly the cast armored turret of the Sherman tank). It was extensively used in medical diagnosis and treatment for many years, until it was outmoded by electron linear accelerators. After the war, Kerst built an 80-MeV betatron and, finally, in 1950, the ultimate betatron of 300 MeV. Its beam intensity was very high compared with those of the electron synchrotrons of that same era and it was useful to run it (especially for training students) for many years after all the synchrotrons had been laid to rest.

## 1.4 The First Understanding of Transverse Focusing

Kerst expressed his focusing in terms of the relative (negative) magnetic-field gradient  $n = -r(dB/dr)/B$  and derived the condition  $0 < n < 1$ , where  $0 < n$  described the decrease of field with radius needed for vertical focusing and  $n < 1$  described the necessity for the field to decrease at a less rapid rate than the centrifugal-force term, in order to have horizontal focusing. If you went to graduate school in the 1940's, this inequality was the end of the discussion of accelerator theory. Kerst actually did much more in his 1941 paper, discussing injection, adiabatic damping and space-charge effects in detail.

Some time after Kerst's work, there was a controversy about the origin of the betatron and Kerst wrote a careful historical note<sup>21</sup> on the subject, trying to find every mention of the concept in the literature. Wideroe had drawn sketches of a betatron in his notebook (as he had of many other devices), but had not carried the idea any further, instead building the first cyclic linear accelerator using a geometry that had been proposed by Ising. (Of course Kerst did not know of Wideroe's notebook at the time.) Wideroe once commented that he felt that his most important role had been as a catalyst, spurring other people on to develop ideas that were originally his (although in many cases not well thought out by him).

One interesting historical point is that in his paper, Kerst pointed out that there was a much earlier paper in which E.T.S. Walton<sup>22</sup> derived the horizontal and vertical equations of transverse motion in a circular accelerator in a form that would be recognizable today. Walton did not pursue the work any further because Rutherford pressed him to work on the voltage-multiplier scheme with Cockcroft. Kerst believed that what he, Kerst, had done that was new was to understand the process by which particles are injected into the stable orbits he had demonstrated. It is also of interest that in his 1941 paper Kerst also founded the discussion of space-charge effects in accelerators, calculating the detuning of transverse oscillations (what we would now call the *Laslett tune shift*). In this time just before World War II, there had been discussions of space-charge effects by Richardson in his work on thermionic emission and in connection with high-power electron tubes by people like Ramo and J.R. Pierce, but Kerst added the idea of detuning.

There the development of accelerators stood through the war years. The large unfinished Berkeley cyclotron was converted to a *Calutron*, used to separate isotopes of uranium, and more of them were built at Oak Ridge (all with the median plane vertical, for reasons now lost in the mists of history).

## 2. Just After World War II

### 2.1 Phase Stability and the Synchrotron

Just at the end of the war, Edwin McMillan of the Berkeley Radiation Laboratory was at Los Alamos. He had finished his tasks on the bomb and was waiting for the end of the war to go back to Berkeley. He was not a person to wait idly, so he began to think about the relativistic problem in cyclotrons. McMillan once commented to me that all the best things he had ever done

(and I think he meant to imply that this should be true of everyone) were done very quickly. All the good things, he said, were really very simple. In a single evening, he considered the problem of modulating the frequency of the accelerating voltage as the accelerated particles gained mass (that is, the cyclotron would be pulsed rather than cw), showed that the oscillations in phase, energy and radius could be made stable (what are now called phase or synchrotron oscillations), and showed how to apply the idea both to a cyclotron geometry and a betatron geometry, inventing the synchrocyclotron and the synchrotron. He coined these names, too. (I once said in a conversation with him that he must have meant the synchronizing between the rising guide field and the accelerating frequency in the name synchrotron, but he said that was not it at all. It had reminded him of the hunting in phase of a synchronous motor.) He wrote a short paper in the form of a letter to the editor of the Physical Review that evening.<sup>23</sup> Later, McMillan found that Vladimir Veksler had independently done the same work,<sup>24</sup> including the synchrocyclotron, which he called the "phasetron" and the synchrotron, which he called the "synchrophasetron," in the Soviet Union (apparently, like McMillan, when his war work was at a hiatus, for he too was not a person to sit idly) and the two men shared the glory. McMillan has written a history of the synchrotron up to strong focusing.<sup>25</sup>

As soon as McMillan got back to Berkeley, he began work to convert the large cyclotron (then called the 172-inch) back from its Calutron episode and to make it a synchrocyclotron. Within a few months, it had demonstrated that the principle worked. The 172, which became 184 in a later rebuilding, had a long, splendid life in physics, chemistry, and medicine. A group under McMillan also began work on a 300-MeV electron synchrotron (others were built at Cornell, MIT, Michigan and Purdue) which was the first to demonstrate the artificial production of  $\pi$  mesons. The first electron synchrotron to demonstrate the principle was an 8-MeV conversion of a betatron in Great Britain<sup>26</sup>. A 70-MeV synchrotron at General Electric was next<sup>27</sup> and it was used by John Blewett to show the existence of synchrotron radiation. The electron synchrotrons and proton synchrocyclotrons produced vast amounts of data at energies higher than had been available previously and we learned a great deal about the properties of  $\pi$  mesons. There still existed puzzling data from cosmic rays, like V particles, that were not understood until the advent of the Cosmotron in 1952.

## 2.2 The Cosmotron and Bevatron

Design work also began in 1947 on two proton synchrotrons to go beyond 1 GeV, the 3-GeV Cosmotron at the new Brookhaven National Laboratory and the 6-GeV Bevatron at Lawrence's laboratory. The Bevatron work was held up for some time because of the concentration of the staff on the MTA accelerator discussed in Sec. 2.5. In addition, Lawrence felt quite unsure about orbit theory and a quarter-scale model was built to test it (this accelerator was later given to Cal Tech, where it became an electron synchrotron). The orbit-study results of the quarter-scale model were inconclusive and the Bevatron was designed with alternative pole pieces for larger aperture (3.5 GeV) and smaller aperture (6 GeV). The Cosmotron began operation in 1952 and quickly showed that Kerst's orbit theory was correct. The Bevatron began operation in 1954 with the smaller-aperture pole pieces.

## 2.3 The Birmingham Synchrotron

There was in fact a precursor. In Great Britain, Oliphant had proposed a proton synchrotron in 1943 (almost discovering phase stability) and detailed design studies were carried out.<sup>28,29</sup> After the war, a 1-GeV synchrotron was built, very slowly, because the group had very little money (British science was thoroughly imbued with Rutherford's "sealing wax and string" tradition). It finally began to operate in 1953, after the Cosmotron, but it had such problems from lack of room (it was built in the basement of the University of Birmingham Physics Building) and with stray magnetic fields (apparently all the vacuum-tube electronics shut down on every magnet pulse) that it was never a very useful research device, but, starting two years before the discovery of phase stability, it was a daring step. Oliphant, a person of great imagination, later became even more famous in particle-accelerator lore when in Australia he built the only particle accelerator that (at least as far as we know) never worked at all (the "White Oliphant"), a synchrotron whose magnets were coils embedded in concrete and whose magnet power supply was a very large homopolar generator (converted from a 100-MeV cyclotron already under construction and only a laboratory curiosity in those days).

## 2.4 Linear Accelerators

In parallel with McMillan's first synchrotron, Luis Alvarez was building a proton linear accelerator at Berkeley.<sup>30</sup> Sloan and Lawrence<sup>31,32</sup> had gone some way in development of the Wideroe linac and Jesse Beams had built electron linear accelerators before the war<sup>33,34</sup>. The wartime developments in radar, in which Alvarez had played a significant role before he went to Los Alamos (inventing, with Lawrence Johnston, the Ground Controlled Approach system to land aircraft in very low visibility conditions) had made rf power sources at high frequency available for the first time. McMillan's principle of phase stability applied to linear accelerators, too and Alvarez combined the new rf technology with phase stability in a 32-MeV proton linear accelerator. It had drift tubes like a Wideroe linac to shield particles from decelerating voltages, but it also was completely enclosed in a large cavity supporting a true electromagnetic wave. W.K.H. Panofsky worked on this development before he went to Stanford to work on electron linear accelerators with W.W. Hantsen, who had carried on where Beams had left off, adding the new high-frequency power sources in the gigahertz region that had been developed for radar during the war.

There was a problem in transverse focusing in linear accelerators. McMillan showed in a letter to the Physical Review<sup>35</sup> that it was impossible to have transverse and longitudinal focusing simultaneously in an electromagnetic wave. This theorem is just the classical Earnshaw's theorem of electrostatics in the wave frame, as McMillan recognized. Electron linacs of that time were too short (in the wave frame) for the transverse defocusing to be a major problem, but linacs for heavy particles were forced to deal with it. The problem was solved in Alvarez' proton linac by installing metal grids across the drift-tube apertures to change the electric-field distribution. This grid focusing worked, but the grids scattered most of the beam during acceleration and the final intensity was quite low.

Alvarez' 32-MeV linac operated for some years at Berkeley and was then given to the University of Southern California, where it had a long, useful reincarnated life. Lawrence Johnston left Alvarez after many years of collaboration and went to the University of Minnesota, where he and John Williams built a 68-MeV Alvarez linac,<sup>36</sup> the highest energy proton linac until strong focusing revolutionized linacs as well as circular accelerators.

## 2.5 Accelerators for Uranium Production

In 1950 it appeared that there might be a shortage of fissionable uranium for weapons and reactors. The Berkeley group proposed and Alvarez and a group built at a site in Livermore, California (later to become the Livermore Laboratory, but at the time done under a somewhat mysterious organization, the California Research and Development Corporation) a very large linear accelerator designed to produce 0.25 A average current of protons, using solenoidal focusing in the drift tubes (each of which was about as large as a 55-gallon oil drum) to avoid the losses on grids. Low frequency (12 MHz) was used in the first model (which began as Mark I, but was later called A-12) largely because power sources were available at this frequency. This very large linac, 60 feet in diameter, was called MTA (Materials Testing Accelerator). There was a later model called A-48 (48 MHz, 15-ft diameter) which was completed and ran quite successfully. Production linacs were to be built at Oak Ridge. But by that time, large deposits of uranium ore had been found in the Southwest and there was no need for expensive accelerator production. The MTA accelerator languished for some years and was finally demolished because its building was needed for some other Livermore purpose. It could have been useful later when there was considerable effort to design a fusion materials-testing accelerator (FMIT) with very similar performance as part of the controlled-fusion effort.

There were some useful consequences. During the building of the two Livermore linacs, methods of making large copper-clad steel sheets for the cavity walls were developed. There was quite a lot of this steel left over and it was used to build the HILAC heavy-ion linear accelerator at Berkeley, to build the Brookhaven and Argonne 50-MeV linacs, to build some development linac tanks at MURA, and finally, to build the first tank of the Fermilab 200-MeV linear accelerator. There was considerable other technical development of ion sources, rf power sources and other rf equipment.

At the Radiation Laboratory in Berkeley, two electron Thomas cyclotrons were also built in the early 1950's as models of larger proton accelerators to produce weapons materials. These were operated, but discarded in favor of the MTA linear accelerator. The Berkeley Thomas cyclotrons suffered from an overabundance of knobs, all manual. People were so busy tuning these knobs that they never got the machines settled down to steady operation. This work was later published.<sup>37</sup>

## 2.6 Importance of the Cosmotron to the Beginnings of Modern Accelerators

The Cosmotron was important not only for the particle physics done with it (the corroboration of the cosmic-ray discovery of strange particles and their quantitative exploration for example), but also for the fact that it was the first accelerator on which the 1941 orbit theory of Kerst could be tested experimentally. The 300-MeV synchrotrons had been too small, with too-high revolution frequencies, for any orbit measurements with the electronics of the day. The crude experiments on the Cosmotron, utilizing instruments like fluorescent paddles to see successive turns showed that Kerst's description of orbits was right and that the betatron-oscillation frequencies (what we now call the *tune*) were as he calculated them. The Cosmotron design and construction were discussed in detail in a special issue of the Review of Scientific Instruments<sup>38</sup>.

These crude experiments were soon improved greatly. Beam-position monitors were developed on the Cosmotron and enabled one to see the beam position directly from the control console. Continuous knowledge of transverse beam position and beam phase relative to the rf accelerating voltage made it possible to feed back these signals to control the rf amplitude and phase to keep the beam centered. The beam could be moved radially, for example, for extraction, by applying a small dc voltage to one side of the beam-position monitor. This system was highly developed on the Cosmotron, including systems for damping phase oscillations. The importance of this development of experimental methods in accelerators cannot be overemphasized - particle accelerators flourished when experimental data became available.

In later years, the Cosmotron was again involved in the advancement of accelerator knowledge. There was a mysterious longitudinal beam-breakup phenomenon that limited beam intensity. It was first investigated experimentally by Mark Barton, who was joined by Lyle Smith and Carl Nielsen of Ohio State and MURA. It had been previously conjectured by Nielsen to be the *negative-mass instability*, the first of many accelerator instabilities to be found. Transverse collective instabilities were also observed in this work.

### 3. The Coming of Strong Focusing

Almost everyone who was active in thinking about particle accelerators in the 1950's has now retired or shuffled off and it is difficult for younger people to understand the excitement and the concerns of the beginning of strong focusing. The prior invention of phase focusing by Veksler and McMillan had been important for thinking about new accelerators, but the invention of strong focusing and the almost simultaneous gathering of the first believable experimental data on orbits in the Cosmotron began an explosion of thinking about particle accelerators that fired an enormous development in understanding of particle accelerators. This development has continued steadily into the baroque era of today.

#### 3.1 Courant, Livingston, and Snyder

Strong focusing was invented at Brookhaven in 1952 by Ernest Courant, M. Stanley Livingston and Hartland Snyder.<sup>39</sup> Livingston has said that the idea of strong focusing arose because in the summer of 1952, when he was visiting Brookhaven, he asked Courant to consider whether they could turn some bending magnets of the new Cosmotron around. The magnets' back legs were all on the inner-radius side to keep them out of the way of extracted secondary beams. Operation at the highest energies was limited by changes in the relative gradient  $n$ , caused by saturation. Livingston suggested turning some magnets around to cancel this variation of gradient and asked whether this could be done without terrible damage to the focusing of the beam. Courant had a method available for this problem that he had developed in treating straight sections in a synchrotron. He found immediately that focusing was in fact improved and that alternating the focusing could lead to an entirely new class of accelerators. In a few days, he and Snyder developed a rudimentary theory of strong focusing. By the summer of 1953, there were several sets of notes on the theory by various people in the US and Europe. The best of all this work was incorporated in Courant and Snyder's classic paper on orbit theory<sup>40</sup>.

In 1953, M.G. White<sup>41</sup> and T. Kitigaki<sup>42</sup> separately invented the separated-function strong-focusing accelerator, in which the functions of bending and focusing are done in separate

magnets. It developed later that this geometry has advantages for damping by synchrotron radiation that are important in electron rings and of compactness that are important in very large rings like the Fermilab Main Ring and others of that size. It was not appreciated as much as it should have been at the time, but later came to the fore.

### 3.2 Christophilos

What Courant, Livingston and Snyder did was actually a re-invention, because Nicholas Christophilos, a Greek engineer, came forward after the publication to point out (indeed, to assert), that he had a patent application on the concept<sup>43</sup> and had communicated it to the Berkeley Laboratory, which had ignored it. He was invited to join Brookhaven and spent some years there, making contributions to the building of the AGS, Brookhaven's first strong-focusing synchrotron. In the next few years, Christophilos worked and reworked his patent until it was finally granted in 1954, so he slipped in a lot of later development and applications.

Christophilos was a very original person, a little chubby, a little larger than life. When he gave a talk, he kept generating new ideas, some of them good, on his feet whenever any objection were raised to what he was saying. Once when I was there, when Christophilos got completely tangled up in mathematics, Hartland Snyder said to him in exasperation, "Nick, you need to learn about Bessel functions." John Blewett had also been urging Christophilos to learn more mathematics. To our surprise, during the next year, Christophilos in fact learned a great deal about Bessel functions. He then designed the drift-tube shapes of the 50-MeV Brookhaven injector linac; there was a small lip around the surface about halfway between the axis and the stem, where Christophilos had matched, not quite smoothly, the inner and outer Bessel functions.

Christophilos already had a deep interest in plasma physics and later returned to that field, where he built the Astron device at Livermore and made many contributions to controlled thermonuclear fusion.

### 3.4 CERN

A European group had started work for a new laboratory, CERN, which became the harbinger of European unity. People who had very recently been mortal enemies joined together to work toward a common goal. The role of these pioneers in rebuilding Europe was extremely important. Without them, the present European Community would have come much more slowly, if at all.

They had been designing a 12-GeV synchrotron, a scaled-up Cosmotron, but immediately changed their thinking to strong focusing, because it was a way not only to catch up on the American lead in accelerators, but also a way to build a much larger, more useful accelerator within postwar Europe's very limited means. Brookhaven was very generous with help to them; Hildred and John Blewett, two key people, went to spend a year at CERN, helping them get started. CERN built the 28-GeV PS and Brookhaven built the 30-GeV AGS.

The new technology also made the idea of building an accelerator in the Midwest seem a realistic hope, because the cost of building a synchrotron of given energy would apparently be much lower with the new principle.

### 3.5 The Thomas Cyclotron Revisited

Thomas' mysterious 1938 paper discussed in Sec. 1.2 had proposed a method, of avoiding the relativistic difficulties in cyclotrons by introducing azimuthal variation of the field (not just the field gradient, as in a synchrotron) to keep the frequency of revolution constant as the mass increased during acceleration by special relativity. Courant once remarked that a significant side benefit of inventing strong focusing was that it finally enabled him to understand what Thomas' paper was about. That came later; in the meantime, the idea of azimuthally varying fields was reinvented independently by Keith Symon, Hartland Snyder, Andrei Kolemenskii in the Soviet Union, and Tihiro Ohkawa in Japan. As recounted earlier in Sec. 2.5, a group in Berkeley built two electron models of Thomas cyclotrons.

### 3.6 Strong Focusing in Linear Accelerators;

John Blewett of Brookhaven recognized immediately that the principle of strong focusing could be applied to linear accelerators and wrote a companion paper<sup>44</sup> to Courant, Livingston, and Snyder's first paper discussing alternating-gradient focusing in linear geometries, using quadrupole magnets installed inside the drift tubes. Much higher intensity beams were now possible.

### 3.7 Early-Day Concerns About Strong Focusing

In those early days of strong focusing, people worried a great deal about integral resonances, which had been discovered in rudimentary digital computations by Adams, Hine, and Lawson<sup>45</sup> of the CERN group, and about half-integral resonances, which were a natural consequence of motion in a periodically varying field. Betatron resonances were known; they had already been found in weak-focusing rings, notably the Cosmotron.<sup>46</sup> But the resonances in strong-focusing systems were so strong that there were people who doubted that such a system could ever be made to work on a daily basis, because magnets would move around as they settled over the course of time. As we know now from a wealth of experience, magnets stay in good alignment for long periods of time. In addition, once a beam is circulating, signals from beam-position monitors can be used to improve the alignment and these techniques have now been developed to a fine art with the use of computation to help (if I can align  $n$  magnets today, which set of  $n$  that I can choose will reduce the orbit deviation the most? a classic linear-programming problem).

People were even more concerned with the passage through transition energy, which was a new phenomenon. To study these effects, the Brookhaven group built an electron analog, a small ring with transverse focusing by electrostatic lenses with the transition energy contained in its energy range. By building this ring, Brookhaven made a conscious decision to let the new CERN group finish the first strong-focusing proton accelerator ahead of them. CERN first

accelerated beam in September, 1959 and Brookhaven achieved accelerated beam in the summer of 1960.

Strong focusing had in fact been demonstrated earlier. Robert Wilson and his collaborators at Cornell had built a 2-GeV electron synchrotron that accelerated beam in 1955, but it operated poorly for its first several years because it was so sloppily constructed, so that much of the force of the achievement was lost. They inadvertently demonstrated the existence of half-integral resonances because the magnets were initially so bad that the betatron-oscillation frequency was outside the stability region. At MURA, we operated the first FFAG accelerator in the spring of 1956.

## **B. THE FIRST PHASE - THE EARLY MURA YEARS**

### **4. The Beginning of MURA**

#### **4.1 The First Organization Meeting**

At the dedication of the Cosmotron late in 1952, when strong focusing was brand new, P.G.Kruger of the University of Illinois and S.K.Allison of the University of Chicago discussed ways to stimulate a high-energy facility in the Midwest (one of the sticky points in MURA always was whether Midwest should be capitalized) and a meeting was held April 17-18, 1953 at the University of Chicago. Most of those who attended were department chairmen or research project heads, but Jones and Terwilliger from Michigan went. A technical working group was formed. My own institution, the University of Iowa, did not participate in this first meeting, because there was no experimental program in particle physics there at the time. The experimental work was in low-energy nuclear physics and James Van Allen's beginning work in space physics.

Lee Haworth, the director of Brookhaven, invited the Midwest working group to visit Brookhaven in the summer of 1953 to learn about the new principle.

#### **4.2 My Own Involvement**

In that spring of 1953, I was at loose ends at Iowa, having finished my thesis, but also having come vaguely to a realization that particle-physics theory might not be what I wanted to do for the rest of my life. I was planning to teach summer school at Iowa, as I had done in 1952. James Van Allen, the head of the physics department, asked if I would like to join the Midwestern group at Brookhaven and I leaped at the chance. Not only was it a new and interesting field, but in New York I could see my mother, who had recently had surgery.

Iowa, where I was, was the epitome of a small physics department and my racing around seemed to bemuse them. I was constantly amused when I went to Urbana to hear people introducing themselves to each other at physics department lunches-- at Iowa, I not only knew everyone in the department, I knew their spouses and children's names too.

### 4.3 Learning About Strong Focusing - Brookhaven in the summer of 1953

I drove east in June and joined the Midwestern group, now called MAC, the Midwest Accelerator Conference, at the old Hotel Wyandot in Bellport, near Brookhaven. There were some established accelerator people, like Lawrence Johnston, but many of the group were, like me, young people casting about for a new line of work in physics (it was much more possible to change specialties in those days than it seems to be now). Lawrence Jones and Kent Terwilliger of Michigan were present. They had worked on the Michigan synchrotron, so were familiar with accelerators. I remember, but am not sure, that Daniel Zaffarano of Iowa State College, Courtenay Wright of Chicago, Norman Francis of Indiana, and John Powell of Wisconsin were also participants.

We spent three weeks at Brookhaven, largely hearing lectures by Brookhaven people, Courant and Snyder on theory, and others on hardware systems of the Cosmotron, including one 8-hour tour-de-force on the magnet power supply and another on the rf feedback system by G.K.Green, the head of the project.

### 4.4 Madison in the Summer of 1953

After Brookhaven and a short hiatus at home, we reconvened in Madison, Wisconsin in the Electrical Engineering Building, staying in a dormitory on the shore of the lake for three weeks to digest what we had learned and to begin work on our own. More people joined us during this time, notably Keith Symon of Wayne State University. People also came from the University of Chicago and from Argonne - the politics that separated us later hadn't started in earnest yet. These people included, at various times, Courtenay Wright of Chicago and Morton Hamermesh, Edwin Crosbie and Melvin Ferentz of Argonne. A little later, Lee Teng came to Argonne and joined in the work. Hamermesh, a physicist of broad background and a person of great sense and sensibility, wrote a set of instructive notes on the theory of strong focusing.

H.R. Crane from Michigan also came and made an important suggestion for the work<sup>47</sup>. We were, in a way that we could almost (but not quite) verbalize, looking for some part of the work in which we as a group could make a contribution to get ourselves known in the world of Brookhaven and CERN, who seemed far ahead of us. Crane's suggestion was that we should study the use of nonlinear restoring forces to overcome the effects of resonances. He had in mind particularly half-integral resonances, where the phase plane changes from the usual nested ellipses of stable motion to hyperbolas in the stop band around the resonant value of the tune. Adding nonlinear forces causes a change of frequency with amplitude, so that the tune is driven off the resonant value at larger amplitudes and the phase plot changes back to closed ellipses, with bounded amplitude. Crane, with deep intuition, hoped that one might find a window in which the nonlinear forces were not too drastic, but helped to ease the alignment tolerances. He was subliminally aware that he needed to worry about particles of different momentum, effects that we now call *chromaticity*.

We plunged into this work, doing various calculations that meant very little except that they gave an impression of motion. Whenever I feel critical of younger people because they don't seem to read the literature, I remember how little we did to learn about what was already known. For example, it was almost a year before we became aware of Poincare's work. I personally rediscovered the method of variation of canonical constants, even though it was discussed in several well-known mechanics texts. What is more, older people who were supposed to know better didn't call the literature to my attention, but kept telling me how good my stuff was.

This comment may sound disparaging of our efforts, but it must be remembered that these were very early days. It was probably possible for all the people in the US interested in orbit theory to fit into a small classroom and it didn't have to be much larger to include the rest of the world. The thinking was extremely rudimentary - we had long discussions about the differences in motion between constant gradient and constant  $n$  (relative gradient). At the same time, we were largely unconcerned with longitudinal oscillations just because we hadn't gotten there yet. At times, Kerst talked about space charge - after all, he had discussed these effects in his famous 1941 paper on the betatron - but any understanding beyond the qualitative one of a tune change with intensity was still in the future, as were all the dynamic collective instabilities.

#### **4.5 The Next Year (1953-1954)**

We went back to our universities for the 1953-1954 year, but we continued to work on accelerator problems and met approximately monthly on weekends at one of the universities. There was a little travel money from NSF (\$21,800) for these trips, but we had to scrimp. Kerst was addicted to the long-distance telephone call, but probably paid for that out of his betatron laboratory budget. I can remember going to Illinois, Indiana, Michigan, Purdue, and Minnesota; I also put on a meeting in Iowa City. It was exciting to be involved and there was plenty of motion (mostly in and out of airports), but as I look back, I find it hard to find anything of any importance that we accomplished. But the important result of the year was that we were teaching ourselves about orbits in accelerators. There was very little thought about hardware on our part, although Jones and Terwilliger built an orbit analog device<sup>48</sup> and used it to study strong-focusing motion. Some experimental work was done at Iowa State and Minnesota to study magnetic-field detectors and linear-accelerator designs, but most of the work was theoretical studies of orbits. During this time, L. Jackson Laslett of Iowa State began to participate in MURA activities.<sup>49</sup>

The MURA group did produce paper. We kept writing reports and circulating them. I felt unfulfilled if I went to a monthly meeting without some new work to report and I think many others did too. Kerst always brought a large old tan leather suitcase full of new reports, which he distributed, and took it home full of other reports he got from us. He said many years later that when we finally had a laboratory, he gleefully threw the empty suitcase off a bridge into the Chicago River.

### **5. The Beginning of Politics**

## 5.1 Feelings of Second-Class Citizenship in the Midwest

In the spring of 1953, when the Cosmotron had just begun operation and the Bevatron was being completed at the Radiation Laboratory, there were two major new proton synchrotrons to explore a completely new energy range - on the two coasts. The Midwestern universities had participated vigorously in the 300-MeV electron synchrotron era of the late 1940's (witness the development of the betatron and work on orbit theory by Kerst at Illinois, development of a synchrotron with straight sections by Crane<sup>50</sup> at Michigan, including important accompanying orbit-theory work of Dennison and Berlin<sup>51</sup>, construction of a 450-MeV synchrocyclotron at Chicago under the leadership of Fermi, with strong help from Herb Anderson, and construction of a 300-MeV synchrotron by Haxby at Purdue) and were still active in training students for research in high-energy physics using these accelerators. But it looked to many of them as if the world of physics was moving ahead and passing them by and many physicists at Midwestern universities felt left out. Adding to this feeling was the fact that Argonne National Laboratory, the major AEC laboratory in the Midwest had been chosen by the Atomic Energy Commission to be the center of fission-reactor development and almost all the AEC money for the Midwest went to that effort. The AEC seemed at best uninterested, perhaps even hostile, to other Midwest interest in high-energy physics.

As a consequence, the best students in high-energy physics all went away to the coasts as soon as they received their degrees. A lot was said in the Midwest in those days about the Midwestern brain drain and there were even statistical compilations to back up this notion. There was in those days a strong feeling among Midwestern physicists that they were second-class citizens, even though Michigan, Chicago, and Illinois had what we would nowadays call world-class physics departments, with a number of others not far behind. It was a natural ambition for Midwestern physicists to want to build an accelerator at the forefront in order to carry out their high-energy physics research closer to home and to keep close to the forefront of fundamental particle physics.

From the very beginning, at the organization meeting in 1953, there were serious questions in the minds of many of the university people about where a Midwestern accelerator should be built. The AEC was investing considerable money in Argonne and had the Ames laboratory as a smaller appendage, so it had no interest in another laboratory in the Midwest. The University of Chicago was the contractor for Argonne and made use of the management fee (several million dollars per year) for its own purposes. It had no interest in any changes in Midwestern laboratory structure. This was a subject of heated debate from the very beginning. In fact, the Midwestern universities had subsidized Argonne in 1945 and 1946 to bridge the gap until the AEC got going and always felt that they had been forced out as soon as the AEC came on the scene.

## 5.2 The Trouble with Argonne

People from other institutions found Argonne a quite inhospitable place. Its primary interest was in development of fission reactors, a field not of interest to most physicists. Walter Zinn, the director of the laboratory, was interested only in reactor development and had little use

for basic research work in the physics and chemistry departments, which he considered mere ornamentation. There was, however, one physicist who had an easy time getting research work done at Argonne. That was Fermi, to whose wishes Zinn catered in lavish detail. But that was a very special case, not applicable to anyone else. Fermi and Kerst were friends from their Los Alamos days and I heard them talk about the question, but Enrico never quite understood what concerned Don, since he, Enrico, didn't have any problem at Argonne himself.

It was hard to visit Argonne because of the strict classification rules - prior notification, escorts in many areas, all the rigmarole that went with classified work. It was even harder to work there - guards came through every night and reported any papers left out on the desk; papers were always to be put in the safe that every office had. After their wartime experiences, everyone was tired of all of these irritations arising from classification. The security umbrella was made much worse by the ravings of Bourke Hickenlooper, a demagogic US senator from Iowa, who made much political capital out of the fact that a small amount of  $U^{235}$  was not accounted for at Argonne - it was small enough that it was probably just chewed up in machining, but talk about "criminal irresponsibility" from a person who sits on the Joint Committee on Atomic Energy and thus votes on your appropriation, tends to move procedures in the direction of more secrecy and more bureaucracy.

The Radiation Laboratory at Berkeley wasn't as bad as Argonne, but there were very few visitors there - the laboratory was completely dominated by and only interested in local people. Almost no outsiders ever did experiments on the Radiation Laboratory accelerators. Later in the 1950's, when people from outside proposed experiments at Berkeley, there was some rumors and feelings, perhaps unjustified, that the Berkeley people who were on their Program Committee seemed to expropriate ideas and carry out experiments originally proposed from outside.

The shining example of Brookhaven made Argonne seem worse. It was true that a staff member at Brookhaven had to have clearance, but visitors didn't, and university people came and went freely. All papers didn't have to be hidden every night. The Program Committee at Brookhaven had university people as members, as well as laboratory staff members. Rabi, the founder of Brookhaven, had used his great management skills and his great influence at the AEC to steer around these problems and everyone else was envious. The Midwestern physicists wanted a laboratory just like that, an extension of a physics department.

Simultaneously with the technical work, agitation went on. Kerst was the leader in this furor and all of us younger people took it on from him as a cause. We buttonholed anyone we could to tell them about our vision of a truly open laboratory - what Leon Lederman later called the TNL, the Truly National Laboratory. The immediate result was that our effort became completely separated from Argonne and the two groups went their own ways technically as well as politically.

In the fall of 1954, at a Physical Society meeting in Chicago, Rabi came, billed as "the wise man from the East," representing the General Advisory Committee in an attempt to try to bring us into the fold. We all went to a meeting with him, but left completely unswayed by his arguments. He wanted us to move to Argonne and be an accelerator-development laboratory, but we wanted to build a large accelerator. In a straw vote, nobody would admit to being interested in going to Argonne under almost any conditions. We were in a frame of mind in which nothing could have swayed us.

## 5.3 The Argument for a New Laboratory

Kerst often made the argument that there was advantage for the country in starting a new laboratory. He made historical analogies to the developments of radar and the fission bomb in World War II. When in 1941 it was clear to all that the United States had to have radar quickly, the decision was not to develop it at the Naval Research Laboratory, which had experience in the field (and had in fact detected radar echoes independently of Watson-Watt) but to start a new Radiation Laboratory at MIT. In eighteen months, all the capital ships of the Navy had radar, a magnificent technical achievement and one you couldn't imagine the NRL of that day carrying out. Similarly, when it was decided to build the bomb, a new laboratory was started in the remotest possible location in New Mexico. In a little more than two years, difficult scientific and technical problems were solved and successful weapons were built, another stunning technical achievement. What existing laboratory could have done that? So the way to break new ground in accelerators was to start a new laboratory.

When I consider this argument now, after having been in on the start a new laboratory, I believe that there is a great amount of force in it. When Fermilab was new, we built twice the accelerator that had been designed at Berkeley for \$90 million less than the Berkeley estimate. The stodgy engineering of Berkeley could never have pulled that off. They had been at it for too many years in the same environment. But Fermilab couldn't do it now, either. In the natural course of aging, it has become stodgy and conservative, after too many years in the same environment. People have even been heard to claim that Fermilab's success is rooted in the conservatism of its designs.

On the other hand, the Berkeley laboratory still carries out science of the very highest caliber. There is a large difference in laboratories' performance in science and in engineering. I believe that this scientific excellence is related to the presence of students, who are not important to development and construction, but are crucial to science. They keep a place alive by continually asking irreverent questions, because they don't know enough yet to be respectfully silent in the presence of their elders.

There is another practical problem. You can't keep starting new laboratories unless you are willing to close old ones, which has become almost impossible politically, because of their economic impact on their surrounding area, or at least what their representatives in Congress think their impact is.

## 6. The Invention of FFAG

### 6.1 The Summer of 1954

We met again in Madison for the summer. There was discussion and study of magnets with long saturating lips at the low-field side (we called them "Ubangi pole tips") to provide large aperture at injection. This stimulated Keith Symon to propose<sup>52</sup> that one could accelerate particles in a fixed magnetic field, keeping the tunes constant, unlike a cyclotron, and varying the

accelerating frequency. He hoped that higher intensity could be achieved, because there was no need to pulse the magnetic field (just as a synchrocyclotron has a higher cycling rate than a synchrotron), and I believe that he understood almost from the beginning that it might be possible to combine accelerated pulses, that is, to *stack* a circulating beam. The disadvantage of his proposed *FFAG (Fixed Field Alternating Gradient) Accelerator* was that not only the gradients, but the fields alternated in sign and, because particles were bent away from a circular orbit in the negative fields, the circumference  $C$  was much larger, perhaps as much as 5 times the bending circumference  $2\pi\rho$ , where  $\rho$  is the bending radius. This ratio  $C/2\pi\rho$  was called the *circumference factor*. The first betatrons and synchrotrons had circumference factors of 1, because no-one then knew how to make straight sections. Of course, the circumference factor of a more modern synchrotron, like the Cosmotron or Bevatron, was larger than 1, because of straight sections, but in a very large synchrotron like the Fermilab Main Ring or Tevatron, where circumference factor really costs money, it can be as small as 1.1.

Symon's FFAG was closely related to Thomas's azimuthally varying field (AVF) cyclotron of 1938, as Symon understood. In both, the field variation with azimuth provides vertical focusing in the presence of an average field that does not decrease with radius to provide vertical focusing in the usual Kerst way ( $0 < n$ ). In the AVF cyclotron, the average field is independent of radius and the field variation is used to keep the frequency of revolution constant, while in Symon's FFAG, the average field increases with radius (making Kerst's  $n$  negative) and the tunes are kept constant with radius. The tune values are much larger in an FFAG than in an AVF, so the focusing is much stronger and it is important to avoid crossing resonances (thus the need for constant tune). The final energy of an AVF is limited to less than  $mc^2$  because the vertical tune  $\nu_y$  increases with radius and reaches the resonance  $\nu = 1$ . Because the revolution frequency varies with energy in an FFAG, it is possible to stack successively accelerated pulses to make an intense circulating beam (unlike an AVF ring, with constant revolution frequency, where only a degenerate, temporary form of stacking is possible). Even after FFAG had been outmoded for high energy by the development of storage rings, beam stacking was still a development of the greatest importance. We do experiments with colliding beams today because of beam stacking.

It developed that there were other precursors than Thomas. Hartland Snyder pointed out to us that he had invented something quite similar. Apparently Lee Haworth also had thought of it. Then we found a short paper from A.A. Kolemenskii, a former student of Veksler's who was at the Lebedev Institute in Moscow, with FFAG discussed, somewhat vaguely, but there nonetheless. Finally, somebody sent Kerst an abstract of a paper at a meeting in Japan where an unknown young person named Tihoro Ohkawa showed a geometry identical to Symon's. Kerst immediately began to work to bring Ohkawa to the US. All these inventions of FFAG had clearly been independent of each other.

## 6.2 Theoretical Development of FFAG

It was difficult to calculate the tune of an FFAG, even in the linear approximation, and Symon developed the "*smooth approximation*," an approximate method of averaging over the field variation<sup>53</sup>, conceptually related to the Bogilubov-Metropolis "*method of averaging*." We began a vigorous exploration of FFAG, learning about orbits in this new geometry.

In order to keep the tune constant, the average field needs to increase radially with a constant exponent  $k$  ( $= -n$ , with  $k > 0$ ), rather than a constant gradient, and nonlinear forces are therefore unavoidably present in the transverse motion of an FFAG and our previous interest in nonlinear forces was justified *post facto*, even though we had not realized anything like the gains in calculated tolerances for which we had hoped.

As FFAG was developing, we also invented jargon to describe it. The variation of the guide field with azimuth was the *flutter*, the radial motion of the closed equilibrium orbit as a function of azimuth in this field was the *scallop* and the variation with azimuth of the betatron oscillations about this closed orbit (the  $\beta$  function) was the *ripple*,

### 6.3 The Rest of 1954

When classes resumed at our universities in the fall of 1954, Kerst, Jones, Terwilliger, Symon (then still at Wayne) and Laslett began to meet weekly in Ann Arbor, developing the new FFAG principle. They invented varying geometries in an attempt to reduce the circumference factor, which for a multi-GeV ring was a large problem. Symon's original geometry was called Mark I in the jargon of the day. I can no longer remember what Mark II, Mark III, and Mark IV were like. (It would be possible to go back and look them up, but they aren't that interesting.) They had something to do with operating in higher stability zones of the linear motion. The lowest stability zone was called the "*necktie diagram*" in those days because of the shape of the plot of stability boundaries as a function of the gradients. We called the higher stability zones "*buttons*" and "*patches*." But Mark V was important. People were looking for a way to achieve good vertical focusing without the negative fields of these radial geometries and Mark V, the spiral-sector field, was the answer.

### 6.4 The Invention of Spiral Sectors

There are few cases that I know of where the invention or discovery of something is so obviously done by one person. There had been plenty of discussion in the group of the need for some new focusing system and small fumbling efforts toward something. Then Kerst got on a train in Champaign one night and got off the next morning in Ann Arbor with spiral sectors worked out. In this geometry, there is good vertical focusing and no negative fields, so the circumference factor is close to unity. It is helpful to think of spiral sectors as alternating edge focusing. The price that is paid for this ideal geometry is serious nonlinear force terms. It took us some years of work to show that these are manageable. But even though spiral sectors have not been used in high-energy rings, they have become a geometry much used in isochronous cyclotrons.

The work in Ann Arbor in 1954 culminated in the first major paper on FFAG<sup>54</sup> and a later presentation at the 1956 Accelerator conference at CERN<sup>55</sup>. There were eventually also patents on FFAG by Symon and Kerst. These were so poorly done by the lawyers that they were worthless; the wording was so wrong that they did not cover either spiral-sector cyclotrons or FFAG betatrons.

## 6.5 The First Electron Model (the Michigan Model)

The other major result of the Ann Arbor meetings was that it was decided that Jones and Terwilliger would build an electron model of an FFAG ring in Ann Arbor. It was a struggle for Kerst to find the money for this project, but he had strong feelings that we needed to get into experimental work. Jones and Terwilliger wanted very much to get back to experimental physics and, in addition, were eager for a project of their own to advance their standing in their department. It was decided to build a radial-sector ring to accelerate electrons from 30 keV to approximately 400 keV, initially by betatron acceleration. The only purpose of the model was to demonstrate the principle of FFAG; no physics experiments were contemplated, which is rare for accelerators. In the long run, even though no physics was done, this first model had a long, splendid history of demonstrating with experiments FFAG focusing, linear and nonlinear resonances, rf acceleration in FFAG, beam stacking, and phase-displacement acceleration.

I went on leave from Iowa in January, 1955 and moved to Urbana to work with Kerst on the design of the electron model. The basic problem was that a large part of the focusing came from the magnet edges and we didn't understand edge focusing in a realistic magnetic field ( we called it a "soft edge"). We struggled several months with hand numerical calculations because we didn't see how to do anything on the Illiac, which was severely limited in memory. In addition, no programs existed for magnetic-field calculation. A year later it would have been easy. The results of these hand calculations were not very precise, basically because the field edge was softer than we were guessing. When I look back on it, it seems like an exercise in ignorance, but we had to start somewhere. We specified gradients and edge angles of the magnets. Luckily, the magnets were built with a great amount of tuning capability, in order to be able to vary the tunes ( the operating point) to explore resonances.

## 6.6 Travels with Kerst

Kerst and I traveled to Ann Arbor, Purdue, Indiana, and Ames to meet the others. Haxby was building the magnets at Purdue and we went there often. There was a young man named Rowe working with Haxby on the magnet who in the next years had a very fruitful time at MURA and after at its successor, PSL. Edward Akeley, an older theoretical physicist at Purdue, did analytical work on FFAG field expansions (Akeley believed that we should be expanding in spherical coordinates rather than the cylindrical ones the rest of us used and did a lot of work toward this end without convincing anybody). On one trip to Purdue, Kerst spent the drive telling me about his development of the betatron, the patent difficulties with General Electric, the questions about who made the first betatron work, and his own rush to make it work because his wedding date was approaching rapidly - Dorothy's father was apparently not at all sure that this unusual young man would actually appear on the appointed day. Dorothy was much more sure and was right.

Our most adventurous trip that winter was to Ames, on a University of Illinois plane, a single-engine Bonanza, in bad icing conditions. Kerst and I sat in the back seat. He kept nudging me to look at a calculation he was doing, but I was distracted because I could see ice building up on the wing just outside my window. I wasn't sure that Kerst knew that this was serious. In addition, I could hear the pilot talking on the radio, trying to find out where we were. He was lost! Visibility was poor and the plane had none of the modern navigational aids. We

finally flew low around a small town in Iowa until Kerst made out its name on the water tower. Then we followed the railroad line into Ames. There the ground was so icy that we had trouble getting from the plane to the shack to call Laslett. The pilot announced that he couldn't take us back because of the icing, so when the meeting was over we took a train, together with Jones, Terwilliger and Symon. I remember a priceless moment: I had always been puzzled because Kerst would say "The magnetomotive force planes go like this" and would then make swooping motions with his hands - most definitely not planes. On the train we sat in a bleak compartment - Jones and Terwilliger were arguing heatedly (as usual) with Kerst about some technical point and Symon and I were off to the side, talking quietly. Suddenly he said "Say, what are magnetomotive force planes anyway?" I was delighted that this smart guy was having the same trouble I was. We decided that they are surfaces of constant magnetic potential (in a region where  $\nabla \times \mathbf{B} = 0$ ). Kerst and I finally got back to Urbana on Sunday morning, bedraggled after a night on the train. We went out to the airport to pick up my car and met our pilot, who had flown back that morning.

It was wonderful for me to work with Kerst - I learned how to work much more effectively and intelligently. The time was full of small events of great moment to me. Once Kerst and I were looking around the physics building late one night, looking for a calculator we could scrounge, when he stopped, looked around and said, "I think this is the room I built the betatron in." We rushed on looking for the calculator - no sentimentality about what had been a great achievement of his life! Once we met Wheeler Loomis, the famous autocratic head of the physics department, in an elevator and Kerst introduced me. Loomis' comment was "Oh, one of Kerst's boys." I was a little nettled to be called anyone's boy - after all, I was all of 29 years old, had been in the Army, had a PhD, etc. Now I would take being called one of Kerst's boys as a high compliment.

## 6.7 Digital Computation

At that time, or even a little earlier, perhaps, we began to be seriously interested in digital computation, especially because it was becoming more and more clear that we needed to deal with nonlinear forces in FFAG geometries. James Snyder of Illinois, who was one of the developers of Illiac I, Illinois's computer, took the lead in programming for us. Of course in those days there was no such thing as a high-level language. FORTRAN did not come until 1960. I'm not even sure what Illiac I had for an assembly language - all of my memories are of routines being coded in a symbolic machine language. Programming was just coming into being as a skill; even the term "*software*" had not yet been invented. Snyder put together a dynamics mapping program, which was largely used by John Powell.

On a visit to Los Alamos, Kerst met Richard Christian, who was becoming an expert in programming - Los Alamos, of course, had the top of the line of available computers to use in weapons work. Christian was becoming interested in computation of magnetic fields by relaxation methods. He taught his methods to Snyder and Snyder then wrote a program utilizing difference equations developed by Laslett.

## 6.8 Powell's Digital-Computation Work

John Powell of Wisconsin had begun using the Illiac at Urbana to calculate nonlinear orbits with a finite mapping program.<sup>56</sup> In these computations, Powell discovered nonlinear resonances, their characteristic shapes and separatrices (a word I have always thought was coined by Symon - I have never found any mention of it before he began to use it), fixed points and the combinations of stable and unstable fixed points at resonances of higher periodicity (which we called "*Powell's Pearls*" after the way they strung out enclosing the origin). I think he even saw some examples of what would now be called *chaos*, but wasn't sure enough of roundoff errors to be sure that this was a real physical phenomenon. In this work, Powell was assisted by Robert Wright, a Wisconsin graduate student.

None of us were aware that there was a considerable literature on the subject, starting with Poincaré and going through Birkhoff, Siegel, and Moser. Later, we consulted some mathematicians interested in nonlinear motion and they told us that chaos was just fine - said they had been expecting it all along. Josef Jauch of Iowa had a passing interest in the nonlinear problems and participated by recasting Birkhoff's monumental paper in simpler terms.<sup>57</sup>

## 6.9 The University Presidents' Work on Organization;

Unbeknownst to most of us in the working group, the presidents of our Midwestern universities (I have always been amused that so many of the MURA presidents had names beginning with H, as in Hovde, Hancher, Hesburgh, Hatcher, Hannah, Harrington, etc. Was it a necessary condition for the job? Or a sufficient one?) were active in discussions and letters with the AEC about support for our work.

In order to have something on the table to discuss, Kerst and P.G.Kruger of Illinois, who had now begun to help Kerst with administration, developed a simple letter proposal to the AEC in April, 1955 (this date comes from Kerst's notes and I am unsure of it, but it can't be wrong by more than a few months). It was for a single 20-GeV high-intensity spiral-sector synchrotron. The total cost for accelerator and laboratory was estimated to be \$23M. It is not at all clear whether this number was meant to be taken seriously or was put in to fill a blank - it certainly bears no relation to any reality, especially when you consider that the AGS, being built in a laboratory with considerable infrastructure already in place, was then estimated to cost \$29M.

The AEC response, in November, 1955, was that MURA should select a site and assemble a staff to design the most advanced accelerator possible, while Argonne was building as quickly as possible a conventional 12.5-GeV accelerator to meet the supposed Soviet challenge, as discussed in Sec.13 below. I think that we in the working group didn't realize it at the time, but this was a major step forward. It took us a long time to realize it, but our days of abject poverty were over.

## 6.10 The Summer of 1955

During the spring of 1955, components of the Michigan Model, as we began to call it, started to take shape. We met for our summer session in Ann Arbor and during the summer the

finished magnets arrived from Purdue and we all carried them in to the lab - they were small enough that one of us could carry a magnet unaided. Jones and Terwilliger took on a young physicist named Charles Pruett, who stayed with the model through many experiments, then stayed and made many other contributions to the work of MURA. It was also during this summer that Andrew Sessler began to participate in MURA.<sup>58</sup>

David Judd from the Radiation Laboratory in Berkeley was a participant that summer, apparently because Ernest Lawrence was curious about what we were doing. Judd had participated in the Thomas cyclotron effort in Berkeley; he had also done what may have been the first thesis in particle-accelerator theory, at Cal Tech, so he brought valuable experience and intelligence to our work. For many years through the 50's and 60's, he was the only person anywhere who taught courses and supervised graduate students in accelerators.

## 7. The Glorious Year 1955-56 in Urbana

### 7.1 The Central Working Group

By that time, it was generally agreed by all that our working at our own universities and meeting every month or so through the academic year was not productive. Kerst managed to pull together some funds from NSF and ONR to support a working group in Urbana for the academic year - AEC support began later. The resident people in the group were Kerst himself, Snyder, Laslett, Sessler, Lloyd Fosdick of Illinois, myself, and Symon (and occasionally Akeley from Purdue) visiting for a few days a week from Wisconsin (where he had moved to replace Powell, who returned to his native Oregon). Nils Vogt-Nilsen, a Norwegian physicist who was from CERN, had come in the spring and spent the year with the group. He and I began to do a serious amount of digital computation to explore nonlinear problems in spiral-sector orbits. This was not done by mapping in the usual sense of the term, but by step-by step integration of the equations of motion, using the fourth-order Runge-Kutta method. It was a marvelous productive year for the group.

### 7.2 Important Results of the Year

There were several results of the greatest importance for the future from that year's work in Urbana. First, there was design work and the beginning of fabrication on the second model, a spiral-sector electron ring sometimes called the *Illinois Model*. I participated in some of the initial discussion of the design, but the main work was carried out by Laslett and Sessler. Building of the magnets was carried out in the shops of Kerst's Betatron Laboratory. Two engineers from that laboratory, Thomas Elf, an operator on the betatron (part of the operating firm of Elf, Hastie, and Quick) and Frank Peterson, an electrical engineer from the betatron, did a large part of the work toward building components. Peterson stayed on with the MURA effort for many years.

Another important result of that year was the development of a comprehensive Hamiltonian theory of longitudinal motion by Symon and Sessler.<sup>59</sup> This work was stimulated

by comments of Wigner to Kerst discussed below. This Hamiltonian theory enabled them to do quantitative examinations of beam stacking and many other rf phenomena. A leading accelerator theorist once sniffed to me that nobody had doubted that such a Hamiltonian theory was possible and all Symon and Sessler had done was to write down the obvious. My answer was that what he said might be true, for we certainly knew that the motion was Hamiltonian, but understanding of beam stacking, phase displacement, adiabaticity, and many other new acceleration analyses and schemes flowed from Symon and Sessler's work. It clearly satisfied the predictability criterion for deciding whether a new theory is important. Symon and Sessler also saw chaos and discussed it in their paper (the equations of longitudinal mapping with rf fields are very close to what is now called *the standard mapping* ).

Laslett collaborated with Snyder in development of the magnetic-field relaxation methods and made significant progress. This work became even more important the next year when there was a larger computer and collaboration with Christian. Magnetic-field computation came of age at that time.

In March, 1956, Jones and Terwilliger completed<sup>60</sup> the radial-sector model and first accelerated beam. This was an important proof of principle - FFAG worked! The accelerator was used in subsequent years for much more detailed studies of importance to the development of FFAG.<sup>61,62,63</sup>

Another part of that year's work of the greatest importance was the beginning of our interest in colliding beams, which we discuss next.

## 8. Colliding Beams

### 8.1 Beginning of Our Interest in Colliding Beams

The first stirrings of interest in colliding beams as a real possibility came during this period of 1955-56. People had talked of doing experiments with colliding beams for many years - I can remember Philip Morrison mentioning it in passing during a course in classical electrodynamics in early 1948. Wideroe had in fact patented the concept in 1943. It had been understood that beam intensities were too low by many orders of magnitude for colliding beams to be feasible. But now beam stacking made colliding beams possible. Kerst made a visit to Princeton to give a colloquium and had an important talk with Wigner, who emphasized the importance of phase space and Liouville's theorem in the analysis of beam stacking and the need to be in agreement with it. There was considerable discussion of colliding beams in 1955 among the group and it culminated in a letter to the Physical Review, published early in 1956<sup>64</sup> and largely written by Kerst, but with all our names on it - a generous gesture by him to us all.

### 8.2 The 1956 CERN Accelerator Conference

Kerst discussed colliding beams<sup>65</sup> at the 1956 CERN Accelerator conference. At that same meeting (the first of many international accelerator conferences), he also gave a general paper on FFAG.<sup>37</sup> Laslett and Symon<sup>66</sup> discussed work on nonlinear forces, Jones and

Terwilliger gave the first complete paper on the radial-sector electron model,<sup>67</sup> and Kerst discussed spiral-sector magnets.<sup>68</sup> These papers at this conference marked the somewhat heroic entrance of MURA on the world accelerator scene.

I don't think Kerst realized what a strong impression MURA had made. He came back talking mostly of the vigorous work at the Lebedev Institute in Moscow and of Kolemenski, to whom he referred as "hyperkinetic." (If anybody was hyperkinetic, it was Kerst himself.) We knew almost nothing about the Soviet work at that time and we tended to make them into larger-than-life figures. They did good theoretical work, but we learned later that the reality was that Kolemenski's group struggled for years to build a copy of our first radial-sector model. There was a point at a later conference when they said, "Our machine is operating, but we don't yet have a beam." We wondered for a long time what that could mean. Later we began to understand that, because of the terrible inefficiency of their system, it was very difficult to get parts fabricated for experimental work in their huge laboratories.

### **8.3 The 1956 Colliding-Beams Proposal**

Most of our attention turned for some time to colliding beams. Kerst, with help from Kruger, but without very much help from the rest of us, produced a proposal to the AEC in the spring of 1956. It was to make use of two tangent spiral sector rings of 15 GeV each, with one colliding beam area at the tangent point.

It was a terribly incomplete proposal. There was almost nothing in it about experimental devices or laboratory facilities, either for feasibility or for cost. In fact, the cost added up to a neat \$75 million for construction and \$25M for operation for 10 years, which looked suspicious because they rounded so neatly. People have learned since then that having a total cost with several significant figures adds a spurious air of verisimilitude.

But \$100 million was a very large amount to propose in those days. For comparison, the Brookhaven AGS then being built had a construction cost of \$29M. What was even worse was that the proposal included a long list of unanswered technical questions, with the statement that these all needed to be answered before the accelerator could be constructed. To submit a proposal for such a large amount of money with such profound technical uncertainties seems now (and seemed to me even then) to be an exercise in confrontation and futility. Perhaps Kerst and Kruger believed that this proposal would reserve them a place in line for money at some time in the future.

Later that year, John Williams, the president of MURA, stimulated a response from AEC, the Vance letter, discussed in Sec 11.1 below.

## **C. THE SECOND PHASE - THE MADISON YEARS**

### **9. The MURA Organization Is Formed**

#### **9.1 The Organization**

The presidents had been active in organization and now formed a corporation, Midwestern Universities Research Association, or MURA for short. We didn't know at the time that there is an Indian tribe called Mura in the Brazilian jungle or that Mura in the Japanese language is a small village or hamlet. Each university put up \$10,000 as an ante. The purpose of the corporation would be to act as the contracting party for work with government agencies.

Somebody also made up a cover page for our reports, a simplified map of the MURA states with flags representing the member universities. We promptly named it "the golf course."

#### **9.2 Choice of a Site**

In June, 1955, the MURA organization invited site proposals from the Midwestern universities and evaluated them. There was no AEC involvement in this process. (My own university, Iowa, did not submit a proposal.) The proposals that did come in were terribly simple in modern terms, a few letters and simple geological and electric power analyses. Nobody promised any money or other inducements to attract us, in large part because it was not yet understood that the presence of a laboratory might be expected to attract good technical industries to the area (it's still not clear to me that this expectation has much basis in reality). I remember that there were a number of proposals, but the only site (other than Madison) that I remember anything about was the Ann Arbor proposal, which described the underlying geology of their site in the picturesque phrase "unsorted boulders." The University of Wisconsin site in Madison proposal had good bedrock, sometimes sticking up out of the ground, for foundations (an asset I wouldn't consider important any more) and good power from a nearby major transmission line.

To nobody's surprise, the organization chose the Madison site. After all, we had met there two summers and it seemed to me that all the discussion in the technical group assumed that we would end up in Madison. The Madison site was a 160-acre farm approximately 10 miles south of the city of Madison, with a mailing address of Stoughton. The corporation bought the site, using the \$10,000 that each university had put in. It was leased to a tenant farmer (who may even still be there).

#### **9.3 End of An Era**

This marked the end of the first phase of MURA's history and the beginning of a central effort in a laboratory. Forming a definite organization, choosing a site and making a proposal all put us closer, we hoped, to our goal, building a large accelerator in the Midwest. After several years of hand-to-mouth existence, with support from ONR and NSF, our work was now supported completely by the AEC, at a rate of approximately \$2 million per year. There was nothing wrong with our support from AEC. Our differences with them were in the long-range goals of the work. They wanted to think of us as an accelerator-development laboratory, but we wanted to build a large accelerator for use in physics research.

## **10. The Move to Madison**

### **10.1 Who Came**

We all moved to Madison in June, 1956. Symon was already there, and Kerst, Jones, Terwilliger, Snyder, Haxby, Ohkawa, Rowe, George Parzen from Notre Dame and I all came on leave from our universities. Sessler and Laslett stayed in Urbana to feed Illiac and commuted weekly during the summer. They visited frequently during the next years.

### **10.2 Many New People**

An important addition as we moved to Madison was Fred Mills, who had a lasting influence at MURA. In addition, over the next few years, many other new physicists joined, including Marvin Freiser, Jacob Enoch, S. Peter Rosen, Avivi Yavin, and Philip Meads, Jr. as theorists and Aaron Galonsky, Michael Shea, Roger Otte, Cyril Curtis, Donald Young, Donald Swenson, Stanley Snowdon, Gustavo Del Castillo, William Wallenmeyer and Ronald Fast as experimentalists. Most of these people had gotten their PhD's at MURA universities and so were natural to recruit. Robert Stump came from Kansas and James McGruer from Pittsburgh to join us for sabbatical years and did significant work on the third model. After a few years, it was a quite different group of people from the originals.

We also began to add engineers. Over the next few years, the mechanical engineering staff came to include Edward Day from Minnesota, Max Palmer, Glenn Lee, John O'Meara, Richard Juergens, Albert Gehm (his wife Annaliese also worked at MURA and Kerst called her Analyzer so often that he forgot her real name), Igor Sviatoslavsky, Erich Laukant, and William Winter. Winter stayed on as head of mechanical engineering and made major contributions to the design of the third model and the synchrotron radiation rings built later by the Physical Sciences Laboratory and the Synchrotron Radiation Center. In electrical engineering, Frank Peterson from Urbana was joined by Martin Berndt and Carl Radmer. Jean Van Bladel of Wisconsin was ostensibly an electrical engineer (at least he was in that department at the university), but his work was indistinguishable from that of a theoretical physicist. Richard Hilden was an engineering physicist who was active mostly on electronic problems, but contributed to many other parts of the work, including experimental work on the third model.

All in all, we developed engineering strength that was roughly comparable to that of the Brookhaven Accelerator department. When you consider these bare lists of names, you would

judge that we were much stronger in mechanical than in electrical engineers. A good part of that is because this time (1956-60) was before the digital age and all its involvement in signal transmission and processing. When you wanted to measure something, you strung a wire from it (after all, it wasn't very far) to a meter. There was no computer involvement in control and our control consoles were dominated by old-fashioned meters and potentiometers, the sort of thing that physicists of that era knew how to deal with. Fast oscilloscopes were not very fast. The electrical engineers concentrated on difficult problems of power-supply regulation and rf amplifiers.

There were also some students, who were first-year graduate students at Wisconsin, brought with assistantships to work at MURA. This group included James Mogford, Thomas Binford, Curt Owen, Margaret Foster, and Homer Meier. They had a difficult time, because they were isolated from the other beginning graduate students and lost out on the important learning experiences that come from colleagues. They were not very successful in going on to get advanced degrees with us, but they contributed to the work of the laboratory and, I hope, learned something from that. Curt Owen stayed on at MURA and did important work on the third model, then on linear-accelerator development.

In later years, Robert Dory, D.C. Morin and Donald Roiseland did theses with Symon and Phil Morton came from Ohio State and did his Ph.D. thesis with me.

### **10.3 The Garage**

A vacant automobile-dealer showroom and garage at 2203 University Avenue (south side of the street) was rented and became our laboratory. We even found an old neon "Nash" sign, which we didn't put up. The western part of the first floor of the laboratory was made into a machine shop, about the size of a typical physics-department shop. A considerable amount in this western half was not available on the first floor because of the curving vehicle ramp to the second floor. The eastern half of the first floor, which had been the showroom, had administrative offices and a conference room in the front and laboratory space in the back (south). The second floor had offices in the east half, using the rooms of two small apartments, with the computer and its cooling taking up about half the remaining space in the west half. The remainder of the second floor was used for shop and assembly work and later was taken up more and more by office cubicles as the staff expanded.

In a short time, by the fall of 1959, so many people had joined us that we outgrew the garage and a vacant lumber-yard building across University Avenue was rented and split up into office cubicles. Mostly it was physics people who moved over and engineering people who stayed in the garage.

### **10.4 Organization**

Kruger came up from Urbana a few days a week as Director, Kerst was titled Technical Director, and Symon was head of the Theoretical Physics Group. (Kerst and Symon had small

offices next to one another, so we cut out and framed for them a cartoon of two men sitting with their feet on desks, saying "Next week we've got to get organized.") We acquired a business staff, with Marshall Keith as Associate Director for Administration, Harold Wittig as Comptroller, and secretaries and administrative assistants. Robert Graewin was contracts administrator until his retirement and William Butler was head of personnel. We were for the first time a free-standing laboratory, without any need to go through our universities for purchasing, contracts, or our own salaries. As individuals, we had almost nothing to do with money. Kerst kept the purse strings to himself and the way you got money for an endeavor was to convince Kerst of its value.

Marshall Keith had worked as an administrator with John Williams at Minnesota and had had a varied work life in private industry and with government-connected projects. He had an engineering background and communicated well with the MURA engineers. But above all, he was intensely practical - when it was decided to do something, he was a master at getting the job done quickly, well, and inexpensively. This practicality was one thing we needed desperately - we were all very young and had little or no experience in buying and building things, so we needed the talents he brought. His positive "can-do" spirit was a large added bonus.

## 10.5 Computation

We rented a very modern computer, an IBM 704 (a brand-new design at the time), which was installed with a massive air conditioner (20 tons of cooling) on the second floor of the garage. We got it because our air conditioning was ready, but NYU's (the nominal customer) wasn't. The computer also came with a set of IBM engineering people, who maintained the device. Just as in the IBM fables, they always wore white shirts and ties, even when they were repairing the printer, which was a messy, inky job.

The IBM maintenance engineers spent all their free time testing vacuum tubes. A computer had several thousand vacuum tubes, with individual mean lifetimes of several thousand hours, so the estimated time between computer failures was of the order of an hour. But if weak tubes were thrown out by initial testing, the mean lifetime of the rest jumped to many thousands of hours and the computer operated without unacceptable breakdowns. Tubes were replaced in the computer on a regular regime, so no tube was in use for as much as a year.

Snyder headed the computer work. With him from Urbana he brought Lloyd Fosdick, a physicist who had become a programmer (not at all common in 1956). Richard Christian came from Los Alamos. A number of new programmers joined the group at that time (it was hard to be an old programmer, because the subject was so new). Among them were Elizabeth Zographus Chapman, Jess Anderson (who had worked for us in Urbana as an undergraduate, mostly running errands), and Melvin Storm. Storm and Laslett did important work in the next few years investigating chaos with mappings and showing that there was a real physical effect beyond round-off and truncation errors in the computation. There were many other programmers later, including George Westlund, Brandt Kehoe, Donald Dickman, Henry Carlson, and John McNall.

IBM was anxious to learn a lot more from Snyder and the others about scientific programming. They even sent two of their advanced programmers for several months, but it was remarkable how far ahead of them we were. The two IBM programmers labored for months to

produce an orbit-integration program, then had to leave before it was verified. When it was first run, it didn't work properly and Snyder decided that it would be easier to write a new program than to find out how theirs worked. So he wrote it over the weekend and it worked on Monday morning.

Snyder wrote the FOROCYL series of programs to calculate two-dimensional magnetic fields by relaxation and to do particle dynamics in these computed fields. Laslett developed methods to include currents in the program, as well as magnetostatic potentials. Fosdick developed a series of particle-dynamics programs for which I did the physics (WELL-TEMPERED FIVE and ILL-TEMPERED FIVE), where the fields were specified on the median plane and expanded away from that plane (that is, a Cauchy problem). There was also a considerable amount of development work on subroutines for use with the computer.

MURA had money enough only to run the computer for approximately one shift a day. Through his contacts in the state government, Marshall Keith rented the computer to the Wisconsin state income-tax group and they ran it for the second and third shifts. It was interesting that the rent was paid to MURA, not to the AEC, which was, after all, paying IBM through MURA. It wasn't clear to us that AEC people knew what was going on (of course they did) and no move was made to enlighten them. The MURA organization amassed several hundred thousand dollars this way and it was eventually spent on facilities for MURA work.

## 10.6 Christian

Richard Christian had been under a security cloud at Los Alamos because his life style was unusual; he had married a Native American and they lived in her pueblo. He also had what nowadays would be called an alcohol problem. I have seen him clinging to the control console of the computer in order not to fall over, but working diligently and accomplishing. He also had a very pleasant disposition. No matter how severe his problems, they never seemed to interfere with his work or his close attention to his family - he even went to PTA meetings. His alcohol problem was made worse by chronic severe back pain and at one point he had a disc-fusing operation, which didn't help at all. But all in all, he was very glad to get out of Los Alamos and had happy and productive years at MURA.

Christian at the computer was a sight to see. He had a snaggle tooth and had drilled a hole in the stem of the pipe he always carried, so the pipe swung from this tooth. He steadied himself with one hand on the console and shuffled data cards all the while. I would watch this awestruck - I had never known anyone else who could shuffle cards, apparently at random, throw them in the hopper and have the system work. I was more used to the precise, methodical Snyder. Very little came from Christian in the way of writeup - by that time, he had solved the problem and lost interest - and we learned to assign a young programmer to him to follow along and clean up the details to make a program we could use without Christian having to be there all the time. A programmer lasted about a year at this somewhat thankless task before he needed to do things on his own.

He often called me up late at night to tell me at length about his back pain, his family and his work. But Marshall Keith had a much harder time of it than I with these calls. He had the double problem of living within a block of the Christians and of being very good at fixing

household appliances. The Christians couldn't seem to keep washers, driers, refrigerators, and stoves operating and Keith was kept busy keeping their household going.

At one time there was a visit of some AEC people. In Marshall Keith's office, they raised the subject of Christian's alcohol problems. I answered with all the force I could that he did better work drunk than either they or I did sober and that seemed to satisfy them, at least for the moment.

And work he did! In those years, he made enormous advances in relaxation methods of calculating magnetic fields. He showed how to use non-rectangular meshes, to do curved boundaries, to calculate the effects of the exciting currents (with collaboration from Laslett on the physics), and to calculate the effects of saturation in the magnet steel. He showed how to do over-relaxation to speed convergence. He even had a stab at three-dimensional calculations, but our computer wasn't large enough or fast enough. When all that was in some reasonable order, he began to study standing rf waves in cavities, solving the Helmholtz equation instead of the LaPlace equation. Van Bladel, a great expert on electromagnetic fields, collaborated on the physics background. Christian worked on this problem for more than a year before he began to make sense of it. The result was that the cavity and drift-tube shapes of the Fermilab, Los Alamos and Brookhaven 200-MeV linear accelerators were all designed with his MESSYMESH programs and did very well.

## 10.7 Experimental Work

Almost immediately, Kerst stunned Jones and Terwilliger by proposing that the first, radial-sector model, the Michigan Model, should be disassembled and moved from Ann Arbor to Madison. They argued vehemently that this would hold up the experimental program for many months and that instead they should superintend Pruett's work from afar.

Kerst insisted and he was right. Six weeks after the accelerator was shut down in Ann Arbor, it was operating in the laboratory in Madison and producing data. Pruett came with it and continued to work on it. They used the accelerator to do many different experiments. They explored a wide range of tunes, finding integral, half-integral, nonlinear, and coupling resonances and they explored the effects of magnet misalignments and demonstrated that theory was in agreement with experiment. Although it had not been part of the original plan, they were able to apply an rf voltage across the insulated gap between the two halves of the vacuum chamber. They showed that beam acceleration with rf in an FFAG ring was as advertised, they demonstrated phase-displacement acceleration, and made a proof-of-principle demonstration of beam stacking. They developed rf knockout, a method of measuring betatron tunes very precisely by applying a transverse rf field and knocking the beam out when the field was in resonance with it.<sup>43</sup> This was a precursor of contemporary methods of tune measurement, where it is not possible to knock out the beam, but it is possible to detect beats between the betatron frequency and the applied rf frequency. In the Michigan Model, there were also measurements of intensity-dependent tune differences between the center and outside of the beam, as if caused by space charge.

The spiral-sector electron model had been largely built in the betatron laboratory shops at Illinois and its components were complete in the summer of 1956. These components were

brought to Madison to be tested and assembled. For reasons that were never clear to any of us, Kerst insisted on making the vacuum chamber of brass. It was to be assembled by brazing. Terwilliger and especially Jones argued with him that the chamber would collapse when hot, but he had it done anyway. It did collapse and a new vacuum chamber had to be built, assembled this time with soft solder. The spiral-sector model, also sometimes called the Illinois Model, first operated early in 1957.<sup>69</sup> Like the Michigan Model, it was used to explore a wide range of tunes and resonances, this time in spiral geometry.<sup>70</sup> RF experiments were also done, although like the Michigan Model, there wasn't room for enough rf capability to make an actual stack. Space-charge effects were studied in some detail, including the effects of neutralization by background ions.<sup>71</sup>

## 10.8 Ohkawa and the Two-Way Accelerator

A little earlier, Kerst had succeeded in his efforts and Ohkawa returned from Japan. He is one of the authors of the first colliding-beams letter. Ohkawa was a very quiet person, quite shy socially. But he eventually became very relaxed in the MURA atmosphere. He later became a highly effective executive in plasma physics. In spite of the turmoil of being thrown into a totally new culture, he began almost immediately to do important work. He soon proposed that, because the equilibrium orbit of a particle in a radial-sector FFAG scalloped outward to larger radius in positive magnets, thus into higher field, and inward to smaller radius in negative magnets, thus into smaller fields, then there was a net positive bending field even if the two kinds of magnets were the same length and strength. But then a particle going in the opposite direction simply interchanged positive and negative magnets, so it could be accelerated in that opposite direction. This *two-way accelerator*<sup>72</sup> was a natural way to do colliding beams in one ring. The circumference of a two-way ring was larger than that of a one-way radial-sector ring because the negative magnets were larger than needed for focusing.

## 11. Politics

### 11.1 The Vance Letter

The political argument went on, with the AEC still giving support to our group's work. On November 26, 1956, John Williams of Minnesota, the president of MURA, sent a telegram to the AEC asking about the status of our proposal. The response was a long letter from Commissioner Vance, dated November 30. (Is it possible to imagine that nowadays any communication to the US government could receive a reasoned, thought-out answer in four days?)

The Vance letter said:

1. The proposal had neither been accepted or rejected, but a technical review had been completed. The Commission had determined that there should be further studies to demonstrate the feasibility of FFAG and the usefulness of the accelerator for research. The Commission was prepared to support these studies. The MURA staff was complimented and hope was expressed that the studies would prove the merit of the facility.

2. The site of the accelerator would not necessarily be at Argonne, because doubt had been cast in the proposal as to the geological suitability of Argonne. but the Commission took a firm position that there was no justification for a second large-scale laboratory unless geology demanded it. In the summing up, it was stated that if the accelerator was built, construction and operation would be carried out as a fully integrated part of Argonne.

3. The AEC was not interested in a new management concept for Argonne, but desired to strengthen the ties between Argonne and the Midwestern universities. The AEC was also not considering a change in the Argonne mission, which they regarded as 53% basic research and 47% applied research and development.

From a vantage point of half a lifetime and several national laboratories later, I am deeply impressed with the reasonableness of the Vance letter and would now regard it as a good invitation to collaborate with the Argonne people to improve that laboratory's responsiveness to visiting users. But at the time we wanted them to hand us a brand-new laboratory on a silver platter, without any demonstration on our part of the soundness of our work.

In the group, we argued mightily about the Vance letter. We sat in the conference room and parsed it up, down, and sideways, trying to make it say what we wanted it to say. All through this argument, Kent Terwilliger kept saying, "It means just what it says. Give up trying to twist it into what you want." Of course he was right. After a time, the furor died down and we went back to productive work, with the issue still unsettled, and with AEC still supporting us quite well.

## **11.2 Kerst Decides to Leave**

Kerst was completely worn down by the stress of the politics, in which he was deeply involved (even though the title of Technical Director had been invented to keep the stress of politics away from him) and was emotionally receptive when General Atomic made its offer to him to head their controlled-fusion plasma work,. He seemed to have completely lost hope that we would ever reach our goal of building a large new laboratory.

## **11.3 Confusion in the Beginnings of the Third Model**

By the end of 1956, the Michigan Model was doing very well and the Illinois Model was almost complete. We began to talk about what we should do next. We rapidly reached a consensus that we should do something as large as we could within our budget, to teach ourselves more about large construction and to demonstrate to AEC that we could manage such construction. Approximately 50 MeV was what we could manage to eke out of our funds. We took as our primary goal that we should demonstrate beam stacking with an intense stack. We also wanted to include, as a slightly lower priority goal, demonstration of two-way acceleration.

Kerst had a completely different view. He had maintained good relations with the engineers at Allis Chalmers in West Allis who built and sold the 22-MeV betatron that he had

designed in 1941. They were interested in the possibilities inherent in an FFAG betatron, which could produce time-averaged intensities a factor of 100 more than conventional betatrons (because Kerst's 2 to 1 rule relating the accelerating core field to the guide field no longer applied and more intensity could be achieved simply by providing more accelerating flux). He believed that we should abandon beam stacking, build a 50-MeV betatron and begin a new technology, instead of following the goals we had had for the last several years.

Kerst was under enormous stress at that time, not only because the political arguments about the beginning of the Argonne ZGS were in full swing, but because the spiral-sector model was just coming into operation and its success was extremely important to him, emotionally as well as technically. Only a year before, when he and I were working on the design of the Michigan Model, we had disagreed on the way to attack a problem. He was so reluctant to tell me what to do that I finally had to say, "Look here, you're the boss. I'll do whatever you want me to do and willingly." Now he stood in the doorway of the office I shared with Ohkawa, literally shook his finger at me and shouted, "I want to know every single thing you're doing", then rushed away down the hall.

At the time, I was producing reams of computer output in the course of designing the ring - no graphics in those days, only long columns of numbers.. So I asked the computer operator to provide me two copies and left one on his desk every night. After a few days, he had enough of being swamped by paper and told me that I needn't do that any more.

Sessler arrived for the summer of 1957 and somehow, in a way neither of us ever understood, he became in Kerst's eyes the proponent and putative designer of the beam-stacking synchrotron and I the proponent and designer of the betatron. In reality, we worked together on the beam-stacking synchrotron and did almost nothing on the betatron, which we thought was a foolish thing for us to build at that time.

When Kerst announced that he was leaving MURA, we had a meeting at which Kerst asked each of us what he thought we should build. We were unanimously for the beam-stacking accelerator, and Kerst said resignedly, "Well, you're going to build it, so you'd better build what you want."

## **12. On Our Own**

### **12.1 Comings and Goings**

After the initial shock of losing our mentor and father figure, we felt relieved about the successful outcome of the argument on the third model and happy to be free of the stress that came with Kerst. Each one of us owed him an enormous amount, but, in part because of what he had taught us about accelerators, we were now much more ready to guide our own destinies than we had been a year or two before.

At that same time, August, 1957, there were many changes in individual people. Jones and Terwilliger returned to Michigan, deciding that a university career in particle physics rather than a laboratory career exclusively in accelerators was what they wanted, and Laslett returned to

Iowa State. They all continued to work with MURA, as will become evident below. Snyder and Fosdick returned to Illinois. Ohkawa's interests were turning more and more to plasma physics. He worked at inventing plasma devices<sup>73</sup> and a year later left to join Kerst in San Diego, leaving the field of particle accelerators.

This was the beginning of a much more down-to-earth experimental phase, where we demonstrated in the laboratory the claims that we had made of new advances and phenomena in accelerators. The inventing did not stop completely - for example, the important work on instabilities was done in this time. But the people who stayed on were more interested in building accelerators that would do new things than in inventing them.

## 12.2 New Organization

Ragnar Rollefson of the University of Wisconsin was named Director and Symon Technical Director. The difficulty that became apparent almost immediately was that they were both part-time and there was nobody with authority to mind the store. An incident that has always typified this period to me occurred a year later. Some new theoretical people (Marvin Freiser, Jacob Enoch and Peter Rosen) had come and I went to Rollefson to complain that nothing was being done to orient them and get them into the swing of things. His response was, "I don't know why you're coming to me. That's your job as head of the theory group." I was stunned. I hadn't known that there was a theory group, much less that I was head of it. Nobody had told me of this appointment, which had apparently been made a year before.

## 12.3 Design of the Third Model

The ring as designed had 32 equal magnets, powered alternately positive and negative. The current to each set could be adjusted to make the ring a 50-MeV one-way accelerator or a 40-MeV two-way accelerator. There were two rf cavities in the straight sections between magnets, one for acceleration and one for holding the stack against the energy loss from synchrotron radiation. The beam lifetime at 50 MeV was approximately 10 seconds from Robinson radial instability.<sup>74</sup>

The magnet cores were machined from solid steel, including the deep grooves for the coils (later we learned to build removable pole pieces, which were much easier). The coils were wound in place on the cores and epoxied in place for strength. This meant that the only way of curing shorts (mostly turn to turn, of which we seemed to have many) was to pound on the coils with a mallet. James Hogan became an expert at doing this. This is not the way any of us would build magnets later, but, after their initial teething troubles, the magnets operated well for almost 20 years.

The guide field increases with radius in an FFAG, although it is possible in principle to make it inside out, with high field at the inner radius (this may have been Mark IV of the Ann Arbor group). The simplest way to make the azimuthal variation independent of radius, to keep the betatron tunes constant and avoid resonances, is to increase the gap with radius, called "scaling" by us. Scaling means that even more current is needed to excite the magnet at the high-field end. For the third model, I designed and we built in a non-scaling pole, a pole roughly

hyperbolic in radius with azimuthal variation of the pole artfully contoured by computation to keep the azimuthal field variation independent of radius, that is, scaling. My simulation of a three-dimensional problem by a series of two-dimensional problems was inadequate and shims and poleface windings were later added empirically.

We found quickly that small current variations made large variations in injection field and we devised the solution of putting a supplementary winding around the return legs of the magnets, A small rotating coil at smaller radius than injection provided the input. We always meant to make this a true feedback system, but never got around to doing it, so the feedback system included a human element as part of the loop. If the magnets were turned off, a complete resetting of these backleg windings was necessary, so there was a strong urge to leave them on forever. We could also use these windings to vary the radial gradient. A number of poleface windings for varying the gradient were also included.

In addition, this was the first time that anyone attempted to stack an actual particle beam. We quickly estimated that the requirements on magnet-current regulation were extremely stringent - of the order of 1 part in  $10^5$ , well beyond what was then commercially available. Martin Berndt set to work building a system with a sophisticated regulation system.

It was also necessary to do careful field measurements and a major effort was made on this topic. Laslett designed a search coil<sup>75</sup> and Haxby made a number of ingenious devices for measurement, especially on measuring the radial field-error components in the median plane.

The vacuum-chamber fabrication was done by James McGruer, who had come on leave from the University of Pittsburgh. It was of aluminum, with metal seals, except at the rf cavities. These seals were approximately 4 feet long in the radial direction. It was a matter of concern to us that nobody could build radiation-resistant ceramic seals that were large enough, because we believed that the Teflon seals we used as a stopgap would limit the attainable vacuum and would disintegrate quickly under radiation. We never did build the ceramic seals, because the Teflon seals lasted 20 years and the limiting vacuum pressure was entirely adequate with them.

The radiofrequency system used a resistively loaded cavity to tune over the frequency range (34 to 26 MHz, above transition), a system that worked because the voltage requirements were relatively modest. A second system operated to make up the radiation energy loss of the stacked beam. Robert Stump of Kansas started the design on sabbatical and Ednor Rowe built it and made it work. It operated by phase-displacement acceleration (a prediction of Symon and Sessler's rf theory), frequency modulating down in energy through the beam, so that the beam was forced upward in energy by the displacement of phase space. There was a betatron core, split into four sections, to accelerate from injection through transition, although later we accelerated with rf directly from injection.

We were all learning how to manage projects by experience (is there some other way?). There was no central authority to coordinate our work on the third model. We had a weekly coordination meeting that quickly began to degenerate into backbiting and standoffs. At one point, Haxby, who was building the magnets, said that his problems were so difficult that he was unwilling to consider injection into the ring, leaving Mills, who was building the injection system, far out on a limb.

## 13. The ZGS

### 13.1 Meanwhile, Back at the Argonne Ranch...

The Commission wanted to build an accelerator very quickly at Argonne, because the Soviet Union would soon have a 10-GeV accelerator at Dubna and it was inconceivable to the AEC under Lewis Strauss that the Soviets should best us in anything. To get ahead of the Soviet Union, the AEC mandated that a 12.5 GeV proton accelerator should be built very quickly at Argonne, preferably by 1960 (a completely impossible goal).

### 13.2 ZGS Technical Decisions

The AEC also mandated that it should be weak focusing, because strong focusing still seemed tentative to them and they thought it would take longer. This was a very stupid decision, made by ignorant people who didn't know what they were doing. Apparently many of the Argonne accelerator staff came very close to resigning in a body as a protest against this short-sighted policy, but were persuaded to stay (Rabi apparently made a visit to convince them). Argonne had to build an engineering staff; the much-vaunted reactor engineering was busy with their own thing. The effort began with John Livingood as head, but he didn't seem to be able to keep all of the effort moving along parallel paths, so Albert Crewe and Roger Hildebrand came from the University of Chicago, Crewe to head the effort to build the accelerator and Hildebrand to head the effort to build experimental areas and detectors. I was not close enough to the work to form a strong opinion, but is my impression that Crewe did a good job, and that Hildebrand in particular deserves very great credit for organizing the experimental areas and the university users' work. When the accelerator finally came into operation, it became the best place in the country to do an experiment. Hildebrand essentially invented the modern users group.

Another very questionable technical decision was made at Argonne. Martyn Foss had built a synchrocyclotron at Carnegie Tech. He worked very hard to achieve higher fields to make the accelerator more compact. It was very difficult to do and, as a consequence, his accelerator came into operation years after the other synchrocyclotrons in the 300- 400 MeV energy range were operating. Most of the good physics had been done and Foss' accelerator was never in the forefront. To our surprise, he was put in charge of the magnet design at Argonne and did the same foolish thing again. The synchrotron was to focus by edge focusing, so it had zero gradient (hence its name, ZGS). The guide field at 12.5 GeV was 2.2 Tesla. The claim was made (and still is) that this high field was the only way to achieve 12.5 GeV within the given cost constraints, but this claim is hard to believe. To achieve this field required a very compact magnet. The laminations were not stamped, as in the Brookhaven AGS, but were individually machined into wedges, thicker at large radius than at small, which made them very expensive. The magnet is driven so far into saturation at 2.2 T that the demands on the power supply become very large and outweigh the advantage of smaller circumference. The Argonne group did a very good job of building this monstrosity, but the end result was an accelerator of less than half the AGS energy (12.5 GeV vs. 30 GeV) for approximately one and a half times the cost (\$42M for the ZGS and \$29M for the AGS, for roughly the same amount of experimental area) that finally came into operation three years after the AGS was running. Each accelerator had significant additions later on that cost more money.

Green, Haworth, and John Blewett of Brookhaven visited Argonne and offered to arrange for strong-focusing magnet construction to continue after Brookhaven's magnets were complete and to design them a 12.5-GeV lattice to use these magnets. But the AEC insisted on weak focusing and claims were made, especially by Crewe, that the ZGS would have higher intensity because of its wider horizontal aperture. 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.

### **13.3 The Life of the ZGS**

Bad as its design was, the ZGS had a good useful life of some years doing particle-physics experiments. There was an attempt by Telegdi of the University of Chicago to do a neutrino experiment, making use of the supposed higher intensity, but the experiment did not produce any useful results. The ZGS was finally turned off to save money when construction of Fermilab began, then later dismantled and the parts sold for scrap (the usual fate for old accelerators). Some enterprising ZGS people, led by Ronald Martin, made use of the 50-MeV injector linac and the small booster injector synchrotron that the ZGS group had added and built an intense pulsed neutron source from the leftover pieces. It was a great success and opened an entirely new field of accelerator neutron sources to compete with the reactor-based sources, which, because of their thermal production, had a far less favorable neutron energy spectrum

There was another part of the political argument, a part that took place largely outside the MURA laboratory group, and that concerned the geographical distribution of major high-energy facilities around the US. But one should note that in the long run the argument was successful. A responsive, user-oriented laboratory was developed in the Midwest - by changing Argonne. In the even longer run, the geographical argument did bring Fermilab to the Midwest, a greater prize than the most ardent Heartland proponent could have ever dreamed of.

### **13.4 The MURA Bubble Chamber**

During 1958 and 1959, while the ZGS was being built at Argonne and particle physicists around the Midwest were gearing up to use it, it was suggested that MURA should build a bubble chamber for use at the ZGS. The two leading suggesters were George Tautfest of Purdue, who came on leave to MURA to work on building the chamber, and William Walker of Wisconsin, who was interested in bubble chambers and was a frequent visitor to us at MURA (at that time just up University Avenue). The most powerful argument was that MURA had an excellent mechanical engineering staff, which was certainly true, and that these engineers had time for design and construction, because most of their work on the 50-MeV accelerator was finished. The engineers themselves regarded a bubble chamber as a challenge of considerable interest and were eager to get at it. We physicists were much more reluctant, because we felt it would be a diversion of the laboratory's effort and that the extra money for construction promised by AEC would not materialize, so money would be diverted from accelerators. It was decided to go ahead with the construction; the engineers indeed had a fine time doing it and did a superb job. It was also true that the promised money never completely materialized and that some of the

construction of the chamber was paid for from accelerator funds, but nobody starved. The chamber had a long, useful life at the ZGS.

The chamber diameter, 30 inches, was determined by the money available. In order to get as much as possible from this given size, a large effort was made to produce as large a magnetic field as possible. Christian did a large amount of computing and eventually a design was built that reached 3.2 Tesla. That made the magnet yoke massive and produced large stray fields, but the chamber was never operated close to our accelerator, so we weren't harmed by it. We did have some mistrust of the particle physicists, because they always seemed ready to dismiss all accelerator problems as trivial - "The rest is just a little technical problem," Walker said once in a meeting when we were trying to do something quite new and difficult. We also regarded them as overly competitive and unmannerly, which they certainly were. There were many groups and they fought one another fiercely and not always fairly. One might wonder why we were still so interested in building a large accelerator, which would necessarily mean much more contact with particle physicists. Later, at Fermilab, the experimental groups became much larger and spread over a number of institutions, so that much of their competitive fighting was carried out internally, out of our sight.

## **14. The 1958 Proposal**

### **14.1 The Need For a New Proposal**

Most of the effort of the resident people was spent in construction of components for the new model, which we were beginning to call the 50-MeV machine (or sometimes the Wisconsin Model). Jones and Terwilliger and I had been having a series of discussions when they visited and by telephone about possible next steps. All three of us were bothered by the fact that the only design presented publicly for colliding beams was the incomplete one of Kerst's 1956 proposal. We did not want the state of our technical knowledge to be judged by it. So we decided late in 1957 to work on a new colliding-beams proposal for a multi-GeV device. We were joined in this work by the other people, but Jones, Terwilliger and I did the largest part of the work, because we had more time to devote to it than the people who were building major systems for the 50-MeV model.

I had been doing extensive digital computation on spiral geometries and had gained confidence that the problems arising from nonlinear forces could be managed - that is, the stability limits, which today we would call the dynamic apertures, were large enough. But we had still not solved the problem of how to put rf cavities in the spiral geometry. In addition, we believed that there might be a cost advantage in Ohkawa's two-way geometry and we therefore based our proposal on it.

Large two-way rings suffer from a lack of vertical focusing, so the performance of the ring was not spectacular, but adequate. We were able to state in the proposal that all the technical problems listed in the 1956 proposal had been solved and that these uncertainties were therefore behind us. We were very proud of that statement.

## 14.2 Development of the Proposal

But there were new uncertainties. First, we (I think it was Jones, but I am not sure) discovered what would now be called the beam-beam effect. The electric and magnetic space-charge forces in the two antiparallel colliding beams added, rather than subtracted as they did in parallel beams, and, even though we made an estimate, we were not sure how much disruption of the circulating beams there would be. Second, there was very little room in the straight sections between magnets to put detectors for particle-physics experiments. Last, and most serious, a detector with good time resolution was needed to separate the desired beam-beam events from beam-background gas events, which were at least as likely as the desired events with the vacuum we could hope to achieve with the techniques of the day. In 1958 there was no such detector - all particle-physics experiments made use of bubble chambers. We pointed out this lack in the document.

The proposal was submitted to the AEC in the spring of 1958. They did not receive it very graciously. As we brought it in, Paul MacDaniel, the Head of Physics Research, growled at us, "What do you want us to do with this?", just to show how unwelcome it was. We replied, "Send it out to the physics community for review." Some persuasion was required to get them to do what would nowadays be a virtually automatic response to any new proposal..

## 14.3 Response to the Proposal

The reviews trickled in over the next several months. Their general tenor of the reviews was that this was a good accelerator proposal, a large step forward from the 1956 one. But the reviewers were all troubled by spending money on an accelerator without a detector available. A large number of reviewers commented that the available single high-intensity beams, which we included as a throw-in, would be valuable immediately and should be pursued. There had also been a report of a physics group to the President's Science Advisory Committee that held that all physics was asymptotic above 15 GeV, which was nonsense that helped to muddy the waters.

Later, we published a paper on the design to bring it before a wider audience.<sup>76</sup>

## 14.4 We Tilt Toward Single Beams

At this time, our views as to what to do began to diverge somewhat. Jones and Terwilliger continued to be interested in and work primarily on colliding beams. Jones worked in the direction of use of colliding beams<sup>77</sup>, and development of a detector with time resolution and spark chambers and wire chambers were developed within a few years as a result of his and others' efforts. Terwilliger invented what is now called the *zero-dispersion insertion*, a set of magnets to make particles of all momenta within the beam cross at a single point, to increase the collision rate<sup>78</sup>. he also showed how to reduce the amplitude function  $\beta$  to reduce the beam size. At MURA, Swenson also studied the beam-stacking process computationally.<sup>79</sup>

Those of us still at MURA felt the need to get going on something acceptable to the community and began to orient our thinking toward the use of single intense beams. Aaron Galonsky carried out detailed studies<sup>80</sup> of the production rates of  $\pi$  and K mesons and

antiprotons. Morton and I went back to spiral geometries, more suitable for single beams, and studied how to put radial straight sections in for rf cavities.<sup>81</sup>

A significant amount of work on beam extraction flowed from this interest in single beams. It was out of the question to utilize internal targets at these intensities, because the targets and their mechanisms would last only a few minutes in the beam. Slow extraction was also needed in order not to destroy an external target. Resonant extraction had been invented by Tuck and Teng<sup>82</sup> in 1950. LeCouteur<sup>83</sup> is sometimes given equal credit, but in his paper he clearly acknowledges the prior work of Tuck and Teng. Resonant extraction was independently reinvented by Hammer at Iowa State and extensively treated experimentally and theoretically by him and coworkers.<sup>84</sup> At MURA, theoretical contributions were made by Laslett and Symon<sup>85</sup> and Jones and Terwilliger.<sup>86</sup> Experimental work was led by Mills for the spiral-sector ring<sup>87</sup> and done later by a large group on the 50-MeV ring.<sup>88</sup> This work showed that extraction was not only possible, but could be made very efficient, which is particularly important at high intensity. We discussed these design efforts at the 1959 Accelerator Conference.<sup>89</sup> At that conference, Meier and Symon also reported<sup>90</sup> their work on two-space-dimensional nonlinear motion.

It seemed to us that colliding beams was not any less worthwhile as a long-term goal, only less immediately useful, and we needed something to keep MURA going toward building a new laboratory. We put together a new proposal for single intense beams, discussed in Sec. 16.3 below.

## 15. 1958 and 1959

### 15.1 The Invention of Storage Rings

It was barely noticed at the time, but a way had been proposed to avoid the difficulties of getting a detector into an FFAG ring. Donald Lichtenberg, Roger Newton, and Marc Ross of Indiana University proposed in a MURA report<sup>91</sup> and Gerard O'Neill of Princeton independently proposed in a letter to the Physical Review<sup>92</sup> that a ring of synchrotron magnets powered dc provided a medium in which to stack and circulate beams. At approximately the same time, Kenneth Robinson of the Cambridge Electron Accelerator showed that the relative amount of synchrotron-radiation damping or antidamping of the two betatron modes and the longitudinal motion could be varied by varying the ring lattice and, in particular, in a separated-function synchrotron lattice, all modes could be damped, an enormous advantage for electron rings.

Our reaction was that of course you needed to build an accelerator to supply the storage ring with high-energy particles and that this would bring the construction cost to a level comparable with an FFAG ring. I'm not sure how much I believed this as I was saying it, because FFAG magnets were obviously considerably more complex, larger and hence more expensive than synchrotron magnets, whose technology was becoming very well developed. Further, if a laboratory happened to have an accelerator already there, as a little later CERN did for the ISR and SLAC did for SPEAR, and, still later, CERN did for the SPS and Fermilab did for the Tevatron Collider, then the cost argument didn't work anymore. Storage rings can have long

straight sections for large detectors - almost arbitrarily long with Thomas Collins' invention<sup>93</sup> of the long straight section a little later - and this has proved to be vital in practice, as detectors have grown to very large sizes.

## 15.2 Discovering and Understanding Instabilities

Carl Nielsen of Ohio State University, who was working closely with MURA, had done some work on longitudinal space charge forces with Sessler and intuitively thought of the *negative-mass instability*. Above the transition energy, when revolution frequency decreases with increasing energy, particle A exerts a repulsive electric force on particle B, which is ahead of it in azimuth, causing particle B to gain energy, effectively slow down and move back toward particle A, increasing the electric force between them, thus making the effect grow exponentially. This force causes particles to move out of the rf bucket and lose stability. Sessler cleaned up Nielsen's mathematics, adding Landau damping. Symon joined this theoretical effort and added to the physics. They showed<sup>94</sup> that there is a threshold intensity above which there is exponential growth. The threshold depends on the energy spread of the beam - greater energy spread gives more mixing in phase and negates the bunching. This is the same physical effect as Landau damping in a plasma.

At this time (1959), Mark Barton of Brookhaven, experimenting with the beam of the Cosmotron, began to observe breakup of the well-behaved beam into a complex, disordered set of smaller bunches, with attendant beam loss. He showed experimentally that the phenomenon was intensity-dependent, so that it was caused by interactions among the beam particles. He was joined in the later parts of the experimental work by Nielsen and by Lyle Smith of Brookhaven. Barton and Nielsen showed<sup>95</sup> experimentally that the phenomenon could be explained as the negative-mass instability. Transverse instabilities were also observed in the Cosmotron and cured by feedback systems. Mills and others at MURA later built feedback systems and used them at the MURA 50-MeV ring and at the ZGS to make significant improvements in intensity.

The negative mass was the first of a number of instabilities to be discovered and treated. Sessler moved to Berkeley in 1960 and Laslett also moved there in 1963, after a stint in the AEC in Washington. They, together with V. Kelvin Neil of Berkeley and Livermore, did definitive work on instabilities.<sup>96</sup> These are now so well understood that people make careers out of calculating the impedances that go into instability calculations and feedback systems to contain them are part of the design of all modern high-performance accelerators.

## 15.3 Laslett's Work on Chaos

We had seen examples of wandering of phase points and extreme dependence on initial conditions (what would now be called *chaos*) in digital computation as early as 1954. There had always been a nagging uncertainty as to whether the phenomena we saw could be just an effect of computation, although mathematicians were delighted to see the results and said they had been predicting it all along on topological grounds. The Runge-Kutta method we usually used for studying FFAG orbits does not conserve phase space exactly and there are roundoff and truncation errors as well. Symon and Laslett developed an algebraic transformation<sup>97</sup> that conserved phase space in every term, so non-Liouvillean errors were not present. Laslett, with the

help of Storm in the programming, developed this into a dynamics program and studied it extensively. He used double-precision arithmetic to study effects of truncation and tested the roundoff errors by mapping forward through a very large number of steps, then mapping the numerical results backward through the time-reversed system. It took many months of effort to make this system work, because there were extremely subtle roundoff effects that were very hard to cure. Finally Laslett could demonstrate<sup>98</sup> that there was truly a physical basis to chaos beyond computational imprecision. I believe that this was several years before people working in astronomy achieved the same results. Laslett also discovered the self-similar nature of chaotic motion.<sup>99</sup>

## 15.4 Work on the 50-MeV Model

Our original plan was to assemble the 50-MeV accelerator in the University Avenue laboratory. But the physical space was very small for the ring and, as our estimates of radiation got more precise (this was the first time that our planned energy was high enough that we needed to deal with neutrons), we became more and more worried about shielding. The laboratory was, after all, in a residential area, with a public road and sidewalk quite close to where the ring would have to go, so we needed to shield the general public, not just radiation workers. After considerable agony about shielding estimates, we finally gave up and squeezed out enough money to build an underground lab out at the MURA site fifteen miles away, taking a few acres at the north boundary of the farm back from our tenant farmer. This area was not very useful to him for farming because there was a steep little hill, but the same hill was perfect for shielding. A prefabricated steel-frame building for the control room, power supplies, and lab space was built on top of the hill next to the underground vault. Marshall Keith managed all the building construction, without benefit of architects, except those of the steel-frame building company. The area was a pleasant spot in the spring and summer and we put a picnic table outside for eating lunch, but in the winter it was a cold, windswept place. There were contests among the physicists and technicians in the fall to see who could squirt the most field mice with laboratory alcohol squeeze bottles as they tried to come in out of the cold. Construction and assembly of the 50-MeV model continued through 1958 and most of 1959. We gave a reasonably optimistic report at the 1959 Accelerator conference.<sup>100</sup>

## 15.5 First Attempts at Operation

At Christmastime of 1959, we began beam tests and, after considerable effort, managed to accelerate beam. But there were very large losses during acceleration because resonances were crossed and there was not enough beam at the end to stack and observe. The beam lifetime was also too short, because of gas scattering and small dynamic aperture. The joke of the moment was that we must be accelerating at least two electrons, because we could see one being lost partway up the cycle. In spite of these problems, two-way acceleration was demonstrated at this time. The largest part of the trouble was that the magnetic fields were not good enough and were not well enough measured for us to use them effectively. In addition, the injector and the magnet power-supply regulation needed work.

This was a time of enormous stress for us. We had built the accelerator we wanted for our next step, but we hadn't built it well enough to work properly. All we were doing was getting

a black eye in the particle-accelerator community. All the snide things the older laboratories said about our abilities as builders and experimentalists seemed to be coming true. But at that time, in the winter of 1959-60, the laboratory was not yet ready to recognize that we needed to stop operating to fix and measure the magnetic fields, so we drifted along for several months working very hard trying to accelerate enough beam to make a worthwhile stack. I was deeply discouraged. My feelings seemed to be shared by the members of the MURA board. Crane, then the president of MURA, came over from Michigan a number of times to observe for himself and to discuss the situation. After some deliberation, the board's response was to bring in Bernard Waldman from Notre Dame as director in place of Rollefson and to relieve Symon of the post of Technical Director, starting in July, 1960.

## **16. The 1960 Era**

### **16.1 Waldman**

Waldman had spent several summers at MURA and was well aware of the program and people. He would go on leave from Notre Dame and would be full time at MURA. Even before he arrived on the scene, he stopped the 50-MeV operation and put us to work remeasuring and correcting the fields.

Almost everything that needed action by the director was in disarray from neglect when Waldman came. Even people's salaries had little to do with their work and it was frequently the case that a group leader was paid less than some of the people in his group. That state of affairs is offensive to younger people, who are often struggling to make ends meet. When we told Waldman about this, he said calmly, "Don't worry. I'll fix that within two years." And he did. Over the next several years, he gradually brought some order to the laboratory.

Waldman did not consider himself capable of making strong technical contributions to the work, but he wanted very much to understand the work, partly to be able to answer questions from Washington visitors. So he worked at asking questions and understanding the progress we were making. He worked mightily at encouraging all of us.

All during Waldman's early MURA years, his first wife was very ill and he had to cope with that, as well as the problems of the laboratory. It was a very difficult time for him personally, but he maintained a cheerful, positive presence throughout and buoyed us all up by his example and his actions.

### **16.2 The 50-MeV Model is Successful**

Remeasuring and correcting the fields of the model, led by Wallenmeyer, Pruett and Young, took all the rest of 1960 and the first half of 1961. Berndt redid the regulation system and Mills built a new injector. During the summer of 1961, this work was finished, together with much more sophisticated correction windings, and the accelerator was turned on and did very well. It was soon accelerating more than  $10^{12}$  electrons per second. Two-way acceleration was demonstrated with larger beams. Beam stacking was demonstrated. Beam could be stacked up

to 3 amperes circulating, but ion clearing was needed to go beyond. Swenson built a very clever set of clearing electrodes that could be installed through a vacuum port to clear positive ions trapped in the negative potential well of the electron beam. Feedback to cure instabilities was also needed to go to higher stacked currents.

The stack eventually reached more than 10 amperes circulating. We had thought to put in a winding on the vacuum chamber to compensate for the lost ampere-turns caused by the current of the stacked beam. The stacked beam decayed from radiation antidamping in a time of approximately 10 seconds, as predicted, and phase-displacement acceleration was demonstrated on the stacked beam.

We had arrived experimentally. We had done what we had said we would do on the 50-MeV ring. Now nobody could dismiss us any more as a bunch of ethereal, impractical dreamers. McMillan came to see our stacked beam, watched for a bit, puffed at his pipe and said, "Now that is a phenomenon!" The MURA experimentalists had done their stuff and come of age. It was a great triumph, made even better by the catharsis of remeasurement and field correction that had been undergone for more than an agonizing year.

We published a number of papers discussing the construction and operation of the 50-MeV accelerator, including a large collection in the *Reviews of Scientific Instruments*<sup>101</sup> that we patterned after the Cosmotron issue.

### 16.3 A New Single-Beam Proposal

We put together a new proposal with no colliding beams at all. We chose a proton energy of 10 GeV to be high enough above the antiproton production threshold to make usable intensities, but were constrained from going higher by concern about the total cost. We claimed we would reach a time-average intensity of 30 microamperes or  $2 \times 10^{14}$  protons per second, three orders of magnitude above what the synchrotrons were then doing (of course their higher energy took away some of that advantage in antiproton production). It was a spiral-sector ring with radial straight sections for rf cavities. It relied entirely on external beams for physics, because at these intensities, internal targets would create completely unmanageable radioactivity problems. We also tried to do a better, less wishful job of cost estimating and came to a total of approximately \$80M. I chose the cover this time, so this was "the Blue Book" (my favorite color).

This was the proposal the AEC wanted and they cheerfully sent it out for review. While the reviews were going on, the AEC sent us a cost expert, Phil McGee, who spent many weeks with us, checking, but doing very little to change our original estimates of the cost of components. But he introduced us to the wonders of EDIA (engineering, design, inspection, and administration), escalation and contingency and following AEC guidelines on these pushed the cost up to approximately \$100M.

I must remark that cost estimates are self-fulfilling prophecies. Once the sponsor and the builders agree on an estimated cost and it is authorized, nobody ever spends less, even if they claimed earlier that it could be done for less. Why spend less when it's there for the taking and

there are so many good things you can do with it? Suppliers know these things too and it's rare to receive a bid for much less than the original laboratory estimate. In the present climate of governmental relations, it is impossible to build anything at all economically.

## 16.4 The Move to the Site

We were doing well enough that it was decided to build a laboratory building out at the site, to concentrate the work there, to make it the center of the laboratory rather than an outpost. A little of the capital was profit from operation of the farm, but most we had saved up from rental of the computer. We had the undoubtedly quite naive idea that the AEC would not be involved in how we spent it. That is, we could build a laboratory, then charge AEC with overhead for operating this laboratory, even though some of the money for building the laboratory had come from rentals of a piece of computer equipment for which AEC was paying rent to IBM.

The AEC wasn't all that dumb and there followed a long, tortuous negotiation about paying for the building and what rental in the form of overhead would be charged. They had watched benignly while we collected the capital, giving us lots of leeway for our work, but they had to draw the line when, in essence, we were charging them twice for the same work. All of that long negotiation was Waldman and Keith's problem and they must have managed it well, because we built and occupied the building. It was complete and we moved in early in the spring of 1963.

The new laboratory was a steel-frame building built by the same local manufacturer who had done the control structure for the 50-MeV accelerator. It was located south of the nominal site border on additional land purchased for it - we were saving the site itself for later construction. There was approximately 30,000 (here I am guessing) square feet of offices and computer space, including a large meeting room, a small seminar room, a library, a duplicating room (we were still good at producing paper), a drafting room and offices for everybody. Behind (to the south) was approximately 50,000 square feet of shop and laboratory space, so much that different groups had permanently assigned space where they could leave apparatus set up. We had all participated in the outlining (the "conceptual design") of the building and Marshall Keith carried the major load of seeing it through.

It was heaven on earth! We could all see and talk to one another by just going down the hall, without bundling up for the trip across University Avenue. Our experimental work flourished in this laboratory, because we finally had space to build models and prototypes and test them.

It was also a symbol of how much MURA had changed. We were no longer a tattered group of rabble-rousing young academics, bouncing new ideas around as we hurtled between our universities. We had become somewhat staid, proper laboratory people with responsibilities and a mission. Of course this change had to take place if we were going to manage a large construction project, as we hoped, but it was very noticeable. Symon, Haxby, Pruett, Rowe and I were now the only physicists left who had been active in MURA before the move to Madison. Had we wanted to continue as an accelerator-development laboratory, it would not have been appropriate to move to the country, so far from the university, but if we wanted to build a large new laboratory for high-energy physics, this was the right place for our new laboratory building

## 16.5 Back to the Betatron

The Allis-Chalmers Company had been building copies of Kerst's 1941 22-MeV betatron for use in industrial radiography and cancer therapy. The betatron did very well in its day, because it was so simple and reliable and had such simple controls that it was easily managed in a factory or hospital environment. It was such an exact copy that they worked hard to find pieces of maple large enough for the top cover of the magnet, because Kerst had used maple and they thought it must have some magic property. Once I asked Kerst why he had used maple and he said that he probably had found a piece of the right size lying around the physics building and scrounged it.

But by 1960, the betatron was becoming obsolescent because of the competition from electron linear accelerators, which were also simple and reliable, and could be built for higher energy. Kerst had kept Allis-Chalmers informed on the FFAG betatron we hadn't wanted to build in 1957 and, in 1960, they approached us to carry out a design study of a 50-MeV FFAG betatron that would have milliamperes of average current (as well as large duty factor, which wasn't important to the industrial or medical applications). Mills, Haxby and I carried through a study as consultants, employing most of the MURA physicists and engineers as consultants to help.

The finished design looked reasonably good, although the magnetic field was quite complicated for an industrial device, and we urged building a magnet model as the first step. The Allis-Chalmers people, who had enormous amounts of machining capability, but little experimental space or knowledge (the 22-MeV betatrons were assembled in an area with a dirt floor right next to a drop forge powerful enough to shake the entire building) built the model, but then it languished. The problems with the accelerator were that the accelerated current was so large that no possible customers knew how to use it (it was certainly inappropriate for a therapy accelerator on safety grounds) and that the cost was very large compared with the competing linacs. Eventually, after they lost interest, they gave the magnet model to Haxby, who had gone to Iowa State, and he carried through some measurements looking toward further development of the device. He passed away before he had time to finish the device.

Instead, Allis-Chalmers decided to upgrade their 22-MeV betatron to 35 MeV. Charles Hammer, of Iowa State (one of the inventors of resonant beam extraction) was a consultant to them and asked me to help with the redesign. It was simple to do - all we had to do was enlarge Kerst's 1941 magnet cross section, run it on the computer, and find that he had designed as good a field by intuition as we could with the computer. That project also languished - Allis-Chalmers couldn't seem to bring itself to do anything new.

An interesting sidelight on the company came when I suggested to them that there were much better magnetic steels available than those of 1941. You could read steel-company tables and find quickly that there had been significant gains in permeability, losses, and coercive force in those intervening years. But the Allis-Chalmers betatron people didn't know about any new steels and were not at all sure how to find out. I pointed out to them that their company was a major manufacturer of transformers and that there must be somebody there who knew about the

magnetic properties of steel. With quaking hearts, they set out to find someone and called me two weeks later to tell me happily that they had found a person who knew all that we needed. His office was only two cubicles down from their cubicle, but they had never met him before.

## 16.6 Laslett's Work on Space Charge

In 1963, Laslett carried through and published<sup>102</sup> a detailed calculation of space-charge effects including the presence of conducting walls and magnets. He solved it as a boundary-value problem. Mills and I had done some work in 1959 and 1960, summing up the electric and ac and dc magnetic images of the beam in the conducting vacuum chamber and magnets. We had gotten some preliminary answers, but didn't push the entire calculation through to the point of publishing. Our partial calculation must have been right, because our answer was the same as Laslett's. Since his definitive work, the tune shifts are called the "Laslett tune shifts" and nobody has felt the need to go back to improve the work.

## 17. The Synchrotrons Catch Up

### 17.1 The 1959 Summer Study

One of the responses to our 1958 proposal was that we didn't have enough contact with experimental work in particle physics. To try to correct this lack, we held a summer study in 1959 on the uses of high intensity in particle physics. We went to some length to prepare for this influx of people, but, as often happens, the summer study went off on different tangents and perhaps the seeds for MURA's demise were sown there.

Instead of thinking about FFAG, Matthew Sands of the California Institute of Technology proposed<sup>103</sup> to build a 300-GeV proton synchrotron, the novel feature of which was that a smaller synchrotron was used to inject into the large ring. This was an idea that had occurred to a number of people. In 1956, Lee Teng had proposed<sup>104</sup> using a cyclotron as the injector to a synchrotron and Wilson had mentioned the idea in his 1955 *Handbuch der Physik* article,<sup>105</sup> although in a sentence so convoluted that most people thought he was saying that Salvini had invented the idea. Like all first proposals, Sand's work went too far in claiming small aperture and very small costs, but it was a powerful idea, not to be neglected. There were other things done at the summer study, but compared with Sands' idea, they paled into insignificance. Perhaps the most important part of Sands' proposal was that it made the concept of a much higher energy accelerator seem well within the bounds of possibility.

The concept of a smaller synchrotron injecting into a larger one became a feature of the LBL 200-BeV design and was taken over by us at Fermilab, where we built a rapid-cycling (15 Hz) 8-GeV synchrotron that injected into the Main Ring. We worried greatly about matching the momentum, phase, and radius of the particles in the smaller ring to the existing buckets in the larger ring, but that problem became much more tractable when Quentin Kerns, who built both booster and main ring rf systems, slaved one from the other. Later, CERN returned to Sands' and Wilson's original concept and injected into the large ring from a slow-cycling synchrotron, the existing CERN PS.

It was also interesting as a sidelight that Kitigaki was present at this summer study. But he was interested in a new concept of his, the "scanning-field accelerator," a rival to FFAG, and he never looked back at his separated-function geometry for large rings. The separated-function geometry was much used in electron-positron storage rings, but the first time I ever heard anyone point out its advantages for very large rings was when Gordon Danby visited Berkeley in the summer of 1966. It would have been interesting had we realized these advantages sooner.

Even without particle-physics data from the Brookhaven and CERN strong-focusing accelerators, most people would, if asked, have agreed immediately that there would have to be a step in energy beyond 30 GeV. This was, after all, at the time of proliferation of particles, but before there was any theoretical idea putting them into some kind of order. Gell-Mann's eightfold way and its confirmation by the finding of the  $\Omega^-$  at Brookhaven were a few years away, so all physicists felt a great need for new data. Even earlier, in 1958 a group at Berkeley who called themselves the Young Turks began an effort to stimulate their laboratory to design and build a strong-focusing synchrotron of approximately 100-GeV energy. Ernest Lawrence stopped this effort, telling them that it was not the right time politically. The Young Turks bided their time, somewhat impatiently, but people in the Radiation Laboratory couldn't do anything to oppose Lawrence. Nobody could - his word was law.

Sands was not of course at Lawrence's laboratory. Even had he been there, it would not have stopped him from proposing a sweeping new idea. That appealed to the swashbuckler in him, a pose he cultivated with bandannas and unusual dress. But his ideas could never be dismissed out of hand.

Sands went home to Cal Tech that fall and immediately formed a group to work further on the idea, the Western Accelerator Group, or WAG. Ernest Courant, Hildred Blewett, Kenneth Robinson of the Cambridge Electron Accelerator and others contributed to this effort on visits there.

## 17.2 The Abortive National Effort

There was not much actual significant technical progress at Cal Tech. It was a little like the first few years of MURA, before there were full-time people gathered at one laboratory. But just the existence of this group stimulated both the Berkeley and Brookhaven people to try to stake out positions for the next US accelerator after the AGS and there was some political activity working toward a national group. The Berkeley group especially made it a cause to do WAG in and leave Berkeley the accelerator group of the West Coast. There was a series of meetings in 1960 and 1961 at various places. There was one at Cal Tech, sponsored by Robert Bacher of Cal Tech, a former AEC commissioner, and one later at the Miramar Hotel in Santa Monica. Waldman and I were deeply involved in this effort, because of the growing doubt, both inside MURA and outside, about high intensity as the road to the future.

The movement toward a national effort failed because neither of the two large laboratories was willing to have a study at any other location than its own site, fearing that the choice of a site for studies would have a strong influence on the choice of site for the actual laboratory. We offered the Midwest as neutral ground, but that wasn't what Berkeley and

Brookhaven wanted - they each wanted the study in their backyard and would rather not have a national effort than have it at any place other than their own laboratory.

### 17.3 PS and AGS Start Operation

On the third day of the 1959 International conference on High Energy accelerators at CERN in September, John Adams announced to the meeting that the PS had accelerated beam to full energy (28 GeV) the previous night - a glorious coup for the CERN people to announce it during the conference. The AGS was completed and began operating in July, 1960, later than CERN because the Brookhaven people had stopped to build the Electron Analogue.

### 17.4 High Intensity of the New Synchrotrons

From their very beginning, the operation of the two synchrotrons was very smooth and the intensity high (unexpected by many, but predicted by Green in 1953). In September, 1960, I went to a conference at Berkeley on instrumentation for particle physics. Mervyn Hine of CERN gave a talk on the performance of the PS that stunned me. The beam manipulation was very good- the beam could be moved onto a target with great precision in both time and space and multiple internal targets could be used simultaneously. There was even some talk about extracted beam (the Lambertson magnet did not come until 1965 and the wire septum until 1967. The combination of these two devices by Maschke led to today's very high extraction efficiency (98% or more) and to total reliance on external beams for fixed-target physics.) But the really important thing was that the intensity was so high. CERN was accelerating more than  $10^{11}$  per pulse (more than the mature Cosmotron) even at their early stage of development and one could see that  $10^{12}$  was coming and that  $10^{13}$  was by no means out of the question. The MURA advantage in intensity melted from a factor 100 to a mere factor 10 or 20 and it was not clear that even if this factor held up that it was worth the large additional cost of an FFAG. I began to worry that our high-intensity arguments had been outmoded by events and came home to tell Waldman and Mills. The three of us had many soul-searching discussions.

### 17.5 Our Response and a New Proposal

Waldman and I, with Mills when he could get away from lab work, began a series of trips to discuss this problem with leading people - McMillan, Panofsky, Haworth, Green, etc. Their general advice was that MURA was not well enough established to compete with the larger labs to build a synchrotron and that we should stay with FFAG and high intensity. Of course, many of these people had their own axes to grind and that colored their advice, even if they were trying to bend over backwards to help us. They certainly didn't need or want our competition. We dutifully (perhaps too dutifully) produced early in 1962 a design for a high-intensity single-beam 10-GeV FFAG ring (as opposed to the colliding-beams rings of our 1956 and 1958 proposals). It included Galonsky's detailed estimates of  $\pi$  and K mesons and antiprotons. We could swamp any synchrotron in production of  $\pi$ 's and K's, but we were not high enough above the threshold for antiprotons to have as large an advantage (a little less than one order of magnitude). This was the Blue Book discussed in Sec. 16.3 above. Simultaneously, in 1961 Berkeley began a major effort to design a synchrotron of 100-200 GeV and Brookhaven began a smaller effort to design a much

larger synchrotron for 700-1000 GeV. I participated briefly in the Brookhaven effort in the summer of 1961.

## 18. The Point of Decision

### 18.1 The Ramsey Panel

There had been several *ad hoc* panels formed to advise the AEC, the first of which was called the Piore panel. Each panel gave rise to the authorization of a new accelerator- that was really the point, but the process gave it a fine gloss of scientific objectivity. There had been one for the ZGS and one for SLAC. Late in 1962 the Ramsey Panel was formed to give the government (NSF and AEC) advice on the efforts of MURA, Berkeley, and Brookhaven and all the groups appeared before it.

By the time we appeared before the Ramsey Panel, we were, in spite of our deep doubts about the future of FFAG, in very good shape technically. We had done the things we had set out to do to prove the soundness of FFAG. We had built three accelerators and showed that they could operate reliably and well even in a strongly nonlinear environment. We had demonstrated with theory and comparison with experiment, particularly in the work of Parzen,<sup>106</sup> that we could calculate and design FFAG rings in the greatest detail and predict their performance quite precisely. Experimentally, we had demonstrated multiturn injection,<sup>107</sup> which we needed for high intensity. We had stacked a large beam,<sup>108</sup> we had built and measured a large spiral-sector magnet,<sup>109</sup> showing that it worked as designed. We had made major contributions to the design and construction of linear accelerators, because of our need for one as an injector.<sup>110</sup> MURA people had made major contributions to many other accelerator problems<sup>111</sup>. We had arrived as an accelerator laboratory, but it was very late for FFAG.

We prepared mightily for the Ramsey panel. We spent many days dreaming up questions they might ask us and preparing written answers. We were thorough enough that when we got through our appearance, Panofsky, who was sitting in front of me, turned around and asked whether we had any more paper covering things they should have asked. I doubt that all this virtuosity in presentation made any difference - after all, they were sophisticated particle physicists who could be expected to see the questions clearly, no matter what we said.

The Ramsey panel recommendation was that Berkeley should come first with a 200-GeV synchrotron, then Brookhaven later with the 1000-GeV ring. A lower-energy, high-intensity facility was considered of lower priority, to be built only if there was money left over after the synchrotrons. This was just what I would have recommended had I been a member of the panel. It seemed to me that high energy was much more important than high intensity for the future of particle physics and that the means existed to make the next step in energy needed to explore quarks and the weak interaction.

The panel also recommended increasing the energy of the MURA FFAG from 10 to 12.5 GeV, putting it in direct competition with the ZGS. Perhaps it was thought to be a replacement. We made a new proposal to AEC (the Gold Book, the color chosen by me to symbolize money), increasing the energy from 10 to 12.5 GeV. By now we were getting very practiced at proposals.

My most vivid memory of the Gold Book is that I stayed home from the 1963 Accelerator Conference in Dubna to work on it.

After this recommendation, Berkeley was given the bulk of the study money, several million dollars a year for several years. The Brookhaven effort went on at a very low level with very little money. They were very busy developing the AGS and nobody had any time to give to this work, in contrast to Berkeley, which had large numbers of people ready and eager to work on the design. Perhaps time could have been made available if Brookhaven had been given any substantial money, but the Brookhaven effort was considered by the AEC to be in the future. There was eventually a small development group, the Advanced Accelerator Development Department ( $A^2D^2$ ), which went on for some years and did some interesting development of superconducting devices for energy storage and power transmission, although nothing to do with the 1000-GeV ring.

## 18.2 The Good Panel

Immediately after the Ramsey Panel report, a second panel was formed of younger people who objected that the Ramsey panel was too full of directors and the like to represent them. The chair was Myron Good of Stony Brook. After a number of meetings and debate, and considerable thrashing around (John Blewett, Lloyd Smith of Berkeley, and I were their accelerator consultants), the panel ended up endorsing the Ramsey Panel recommendations.

## 18.3 Political Action

The MURA university presidents began a political effort to try to save MURA. At their behest, the congressmen from the MURA region, especially Hubert Humphrey, did a great deal of political work to try to keep MURA alive. There was some feeling that President Kennedy could be persuaded to support the MURA effort, because he was supporting science vigorously. After Kennedy was assassinated, the Midwestern political drive made it all the way to the White House, where President Johnson met with the Midwestern people early in 1964. It was clear to many of those present that he had made up his mind before the meeting to turn it down. Many of these people believed afterward that Kennedy would have approved it, but this was probably only wishful thinking. There were many issues higher in priority for the government. There were also already some scientists in other fields concerned about the amount of money being spent to support particle physics.

There was considerable criticism of MURA by Berkeley and Brookhaven for this political action. We were accused of introducing politics into physics. In fact, the only difference was that we had to go see our legislators. The established laboratories had their captive legislators and didn't have to do anything so unseemly as visit a senator - the senator came to them for pleasant visits, always with a good lunch!

## 18.4 The Laslett panel

Irked by the Ramsey Panel recommendation of 12.5 GeV for MURA, Albert Crewe, the Director of Argonne, made a public claim that the theoretical intensity limits given by space charge were approximately as large for the ZGS as for the proposed MURA ring. A panel of accelerator people was formed to investigate, headed by Laslett, who by then had gone to the AEC to serve a term as the first director of high-energy physics research, in Paul MacDaniel's department of physics research. Had it been anyone but Laslett, eyebrows would have been raised about conflict of interest, but his reputation for probity was so strong that there was no objection at all. I was the MURA representative and Lee Teng was the Argonne representative. It soon became clear that Crewe had bent space-charge calculations wildly out of shape and used quite different assumptions for the ZGS and the MURA ring. It was also clear from little signals, although he didn't say anything, that Teng was embarrassed to be put in a position of trying to support Crewe. The matter was settled after one short meeting and the committee report flatly denied Crewe's assertion, but by then it didn't matter. There wasn't going to be enough money for the FFAG accelerator after the step to 200 GeV.

## 19. Life Goes On

The FFAG game was over. Azimuthally varying fields were carried on separately to the development of isochronous cyclotrons, with many accelerators of varying geometries and sizes being built. Henry Blosser at Michigan State University did pioneering work in this field, building the first superconducting cyclotron.

Some of the MURA people moved on to other places and concerns. Waldman went back to Notre Dame, which was where he had always wanted to be, and soon became Dean of Science. Galonsky went to Michigan State and back to his earlier interest in experimental nuclear physics. Haxby went to Iowa State, where he worked on development of the FFAG betatron. Christian went to Purdue, where he worked on programming for bubble-chamber analysis. Swenson went to Los Alamos to work on LAMPF. Shea, whose interests had begun to turn toward electronics and controls, went to California to work in private industry, but returned to work at Fermilab when it began. Parzen went to Brookhaven to join their Accelerator Department. Berndt went to SLAC. Radmer joined LeCroy Electronics to work on electronics development. Del Castillo and Storm went to Argonne.

While Laslett was at AEC, he had invited MURA to send a staff member for a term appointment in his office. Waldman suggested to Wallenmeyer that this might be interesting for him. Indeed it was - he went and stayed on, succeeding Laslett and becoming the great mogul of particle physics. In this capacity, he always carried a large briefcase (it may have been our going-away present to him) and we all used to carry on the fantasy with him that it was stuffed with cash ("unmarked bills," of course). After he retired from government, he served a term as president of SURA, a latter-day MURA in the lower right-hand corner of the US.

I went to the Radiation Laboratory in Berkeley, because I wanted to participate in building the next large accelerator, because I was tired of being a rebel and thought it would be interesting to become part of the establishment (I actually spent a lot of time in Berkeley rebelling quite vocally against the extreme conservatism of the design), and because my family and I were intrigued with the idea of living in California. But I had misgivings. On one of my last days at MURA, Lee Teng and some of his colleagues from Argonne came to talk about a

joint effort to design a large synchrotron, in competition with Berkeley. I looked at them and thought that it might be great fun to compete with Berkeley rather than join them. (As an interesting sidelight, Martyn Foss was part of the group on this visit and talked about achieving higher fields.) In addition, at the Washington APS meeting that spring, Luis Alvarez asked me where I was going. When I told him that I was coming to Berkeley "because that's where the action is," the reaction of this person I thought of as a pillar of the Berkeley establishment was, "Wow, are you ever making a mistake!" That was a little unsettling.

But a lot of people stayed and did interesting work. After a year of support from the AEC through Argonne, the MURA organization was dissolved (I am told that the last meeting was so well lubricated that afterward nobody remembered voting on an actual resolution to dissolve) and the laboratory absorbed into the University of Wisconsin as the Physical Sciences Laboratory, which had the purpose of building technical equipment that was too large for university people to manage in their own shops. Fred Mills became Director, which meant that he had to spend a large part of his time getting work for the laboratory (the prospect of helping with that effort was a strong factor in sending me to Berkeley). But he carried it off very well, found work to keep the laboratory busy and managed to find time to do accelerator work himself. He was also teaching, working on the synchrotron-radiation ring and participating in a cosmic-ray experiment in Colorado with Jones - a full plate indeed! He and others built feedback devices to control collective instabilities in the ZGS, then installed them and made them work.

What was even more important was that he and Rowe invented the synchrotron-radiation storage ring and built one (Tantalus), using the 50-MeV FFAG as an injector. They founded an entire new field of accelerator applications. (Apparently there had been some thinking on the subject of using synchrotron radiation at Cornell, but nothing seems to have come of it.) After some effort on the part of Rowe and Mills, an active users group came into being. It has continued to the present, when the 1-GeV ring Aladdin is the center of its attentions. There are now very large national and international projects for synchrotron-radiation facilities, with many eager users. It all started with Rowe and Mills.

Snowdon and a group around him, including Del Castillo (before he moved to Argonne) and Fast, worked on magnet development, intertwining computation and hardware to advance the art of magnets. They designed and built beam-line magnets for the ZGS. Snowdon became one of the premier magnet designers in the US and played an important part in the design of many Fermilab magnets.

Young and a group around him, including Owen, Palmer, O'Meara, and Lee, went back to Young's first love, linear accelerators. Intertwining computation and hardware again, they built a series of more and more realistic models and prototypes of linac cavities. They also worked to improve the performance of the 50-MeV ZGS injector linac. There were serious discussions about building a new 200-MeV linac for ZGS injection, but it was decided to use the available money for a new large bubble chamber. When, a few years later, three 200-MeV linacs were built, the technology Young and his group had developed was basic to all of the linacs. The group moved bodily to Fermilab in 1967 and built the 200-MeV Fermilab linac. Closely tied to this work was an effort in ion-source development carried on by Curtis, who also moved to Fermilab when it started.

## EPILOGUE

Was it worth it ? You bet it was! It was certainly worthwhile for us young participants. It was the vehicle that led us to our careers, to continued work in accelerators and many related fields of physics. MURA alumni have participated in many large developments and even some important accelerator physics.

It was also worth it for the development of physics. One example alone makes it all worthwhile. Colliding beams is the experimental basis today for the forefront of particle physics. It is true that colliding beams had been conceived before MURA, but it was made to work by the invention of beam stacking and by the theoretical underpinning of Symon and Sessler and the experimental demonstration in the 50-MeV ring. FFAG didn't turn out to be the optimal method of doing colliding beams, but the storage ring, which is, was conceived at MURA, as well as by O'Neill. The FFAG geometry did have a major impact in the development of isochronous cyclotrons, which nowadays make extensive use of spiral-sector focusing.

In addition, there was the exploration of nonlinear motion, both experimentally and theoretically. The work of Laslett on algebraic transformations has certainly had lasting value. Coupled with this was the understanding and use of phase-space concepts in accelerator physics. Before MURA, that had been an exclusive province of mathematicians.

There was also the exploration of space charge and, much more generally, the exploration and curing of dynamic collective instabilities. Finally, as a last gasp of MURA and an auspicious beginning for the Physical Sciences Laboratory, there was the invention by Mills and Rowe of the synchrotron-radiation storage ring.

All these things would probably have been found out eventually without the existence of MURA, for almost nothing in physics ever seems to be done by only one person or group at one time. But the MURA work clearly speeded the development of all kinds of accelerators and accelerator applications. The MURA group never did achieve its goal of building a large accelerator and laboratory, but the individuals who wanted to, participated in such large construction. Seventeen people from MURA went to Fermilab and did their things there. And there are many fates worse than being outmoded by technical developments you helped to invent!

In retrospect, our war against Argonne and the AEC looks overdone. It wasn't the noble cause we made it out to be. But its existence provided us important independence. One can look long and hard before finding the far-ranging MURA kind of development work in large, well-established laboratories. In those laboratories, there is too much pressure to work on short-range problems. So perhaps the pose of rebels helped us in some non-rational way.

And what a marvelous group of people. After all these years, they are all still friends. I wouldn't have missed MURA for anything!

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## REFERENCES

- 
- <sup>1</sup> D.W. Kerst, *Terwillger and the "Group" : A Chronicle of MURA*, in the Kent M. Terwillger Memorial Symposium, Ann Arbor, Michigan, AIP Conference Proceedings 237, AIP (New York), 1989, p.22
- <sup>2</sup> K.R.Symon, *Reflections on the MURA Years*, in the Kent M. Terwillger Memorial Symposium, Ann Arbor Michigan, AIP Conference Proceedings 237, AIP (New York), 1989, p.53
- <sup>3</sup> Notably an unpublished manuscript by Donald Moyer, then at Northwestern University
- <sup>4</sup> W.D.Coolidge and A.H.Kearsley, *Am. Jour.Roentgenology* , **9**, 77 (1922)
- <sup>5</sup> W.D.Coolidge, *Jour. Franklin Inst* , **202**, 693 (1926)
- <sup>6</sup> R. Wideroe , *Arch. Elektrotech* **21**, 408 (1929)
- <sup>7</sup> R.J.. Van de Graaff, *Phys. Rev.* **38**, 1919A, (1931)
- <sup>8</sup> H.A.Barton, D.W.Mueller, and L.C.Van Atta, *Phys. Rev.* **42**, 901A (1932)
- <sup>9</sup> R.G. Herb, D.B. Parkinson and D.W. Kerst, *Rev. Sci. Inst.* **6**, 261, (1935)  
D.B.Parkinson, R.G.Herb, E.J.Bernet, and J.L.McKibben, *Phys. Rev.* **53**, 642 (1938)  
R.G.Herb, C.M.Turner, C.M.Hudson, and R.E.Warren, *Phys. Rev.* **58**, 579 (1940)
- <sup>10</sup> M.A.Tuve, L.R.Hafstad, and O. Dahl, *Phys. Rev.* **48**, 315 (1935)  
L.R.Hafstad and M.A. Tuve, *Phys Rev.* **47**, 506 (1935); **48**, 306 (1935)  
L.R.Hafstad , N.P.Heydenburg, and M.A.Tuve, *Phys. Rev.* **50**, 504 (1936)
- <sup>11</sup> J.D.Cockcroft and E.T.S. Walton *Proc. Roy. Soc. (London)* **A129**, 477 (1930)  
J.D.Cockcroft and E.T.S. Walton *Proc. Roy. Soc. (London)* **A136**, 619 (1932)  
J.D.Cockcroft and E.T.S. Walton *Proc. Roy. Soc. (London)* **A137**, 229 (1932)  
J.D.Cockcroft and E.T.S. Walton *Proc. Roy. Soc. (London)* **A144**, 704 (1934)
- <sup>12</sup> H.Greinacher, *Z. Physik* **4**, 195 (1921)
- <sup>13</sup> E.O.Lawrence and N.E.Edlefsen *Science* **72**, 376 (1930)
- <sup>14</sup> E.O.Lawrence and M.S. Livingston, *Phys. Rev.* **37** (1931), 1707; **38**, 136, (1931); **40**, 19 (1932)
- <sup>15</sup> R.R.Wilson, *Phys. Rev.* **53**, 408, (1938)
- <sup>16</sup> H.A.Bethe and M.E.Rose, *Phys. Rev.* **54**, 588 (1938)
- <sup>17</sup> L.H.Thomas, *Phys. Rev.* **54**, 580, (1938)
- <sup>18</sup> D.W. Kerst, *Phys. Rev.* **58**, 841 (1940)
- <sup>19</sup> D.W. Kerst, *Phys. Rev.* **60**, 47 (1941)
- <sup>20</sup> D.W. Kerst and R. Serber, *Phys. Rev.* **60**, 53 (1941)
- <sup>21</sup> D.W. Kerst, *Nature* **157**, 90 (1946)

- 
- <sup>22</sup> E.T.S. Walton, *Proc. Cambr. Phil. Soc.* **25**, 469 (1929)
- <sup>23</sup> E.M. McMillan, *Phys. Rev.* **69**, 143 (1945)
- <sup>24</sup> V.I. Veksler, V.I., *Doklady USSR* **43**, 444 (1944), and **44**, 393 and *J. Phys. USSR* **9**, 153 (1945)
- <sup>25</sup> E.M. McMillan, A History of the Synchrotron, *Physics Today*, February 1984, p.31
- <sup>26</sup> F.K. Goward and D.E. Barnes, *Nature*, **158**, 413 (1946)
- <sup>27</sup> F.R. Elder, A.M. Gurewitsch, R.V. Langmuir, and H.C. Pollock, *J. Applied Phys.*, **18**, 810 (1947)
- <sup>28</sup> M.L. Oliphant, J.S. Gooden, and G.S. Hide, *Proc. Phys. Soc. (London)* **59**, 666 (1947)
- <sup>29</sup> J.S. Gooden, H.H. Jensen, and J. L. Symonds, *Proc. Phys. Soc. (London)*, **59**, 677 (1947)
- <sup>30</sup> L.W. Alvarez *et al.* *Rev. Sci. Inst.* **26**, 111 (1955)
- <sup>31</sup> D.H. Sloan and E.O. Lawrence, *Phys. Rev.* **38**, 2021 (1931)
- <sup>32</sup> D.H. Sloan and W.M. Coates, *Phys. Rev.* **46**, 539 (1934)
- <sup>33</sup> J.W. Beams and L.B. Snoddy, *Phys. Rev* **44**, 784 (1933)
- <sup>34</sup> J.W. Beams and H. Trotter, Jr., *Phys. Rev.* **45**, 649 (1934)
- <sup>35</sup> E.M. McMillan, *Phys. Rev.* **79**, 493 (1950)
- <sup>36</sup> E.A. Day, R.P. Featherstone, L.H. Johnston, E.E. Lampi, E.B. Tucker and J.H. Williams, *Rev. Sci. Inst.* **29**, 457 (1958)
- <sup>37</sup> E.L. Kelly, P.V. Pyle, R.L. Thornton, J.R. Richardson, and B.T. Wright, *Rev. Sci. Inst.* **27**, 493 (1956)
- <sup>38</sup> Cosmotron Staff, *Rev. Sci. Inst.* **24**, 723-870 (1953)
- <sup>39</sup> E.D. Courant, M.S. Livingston and H.S. Snyder, *Phys. Rev.* **88**, 1190 (1952)
- <sup>40</sup> E.D. Courant and H.S. Snyder, *Annals of Physics* **3**, 1 (1958)
- <sup>41</sup> There was an unpublished Princeton-Pennsylvania Accelerator technical report that I remember seeing, but do not have. I also heard a talk by White on the subject in July, 1953.
- <sup>42</sup> T. Kitagaki, *Phys. Rev.* **89**, 1161 (1953)
- <sup>43</sup> N.C. Christophilos, U.S. Patent 2,736,799 (1950)
- <sup>44</sup> J.P. Blewett, *Phys. Rev.* **88**, 1197 (1952)
- <sup>45</sup> J. Adams, M.G.N. Hine, and J.D. Lawson, unpublished CERN Note
- <sup>46</sup> E.D. Courant, *J. App. Phys.* **20**, 611 (1949)

- 
- <sup>47</sup>H.R. Crane, Midwestern Accelerator Conference Report MAC-8, MURA 9, August 15, 1953 and Sept. 4, 1953.
- <sup>48</sup>L.W.Jones and K.M.Terwilliger, An Electromechanical Analogue for the Study of Strong Focusing Synchrotron Betatron Orbits, MURA Report 20, March 15, 1954
- <sup>49</sup>L.Jackson Laslett, Discussion of Space-Charge Effects in the Alternate-Gradient Synchrotron, MURA Report 14, January, 1954
- <sup>50</sup>H.R.Crane, *Phys. Rev.* **70**, 800 (1946)
- <sup>51</sup>D.M. Dennison and T. Berlin, *Phys. Rev.* **69**, 542 (1946), *Phys. Rev.* **70**, 58 (1946)
- <sup>52</sup>K.R. Symon, A Strong Focussing Accelerator with a DC Ring Magnet, MURA Report 32, August 13, 1954
- <sup>53</sup>K.R. Symon, A Smooth Approximation to the Alternating Gradient Orbit Equations, MURA Report 25, July 1, 1954
- <sup>54</sup>K.R. Symon, D.W. Kerst, L.W. Jones, L.J. Laslett and K.M. Terwilliger, *Phys. Rev.* **103**, 1837 (1956)
- <sup>55</sup>D.W.Kerst, K.R.Symon, L.J.Laslett, L.W.Jones and K.M.Terwilliger, Fixed Field Alternating Gradient Accelerators, *Proc. 1956 CERN Symposium on High-Energy Accelerators and Pion Physics*, CERN, Geneva, (1956), p. 32.
- <sup>56</sup>J.L.Powell, Non-Linearities in the AG synchrotron, MURA Report 11, Oct. 23, 1953
- J.L.Powell and R.S.Wright, Non-Linearities in A.G. Synchrotrons, MURA Report 49, Jan. 12, 1955
- <sup>57</sup>J.M. Jauch, The Stability of Orbits in a Non-Linear AG Synchrotron, MURA Report 47, Dec. 19,1954
- <sup>58</sup>A.M.Sessler, Determination of Sigma in the Model FFAG Mark 1b Accelerator, MURA Report 71, June 24, 1955
- <sup>59</sup>K.R.. Symon and A.M. Sessler, Methods of Radiofrequency Acceleration in Fixed Field Accelerators with Applications to High Current and Intersecting Beams, *Proc. 1956 CERN Symposium on High-Energy Accelerators and Pion Physics*, CERN, Geneva, (1956), p.44
- <sup>60</sup>L.W. Jones, K.M. Terwilliger and R.O. Haxby, *Rev Sci Inst*, **27**, 651 (1956) (letter)
- <sup>61</sup>F.T. Cole, R.O. Haxby, L.W. Jones, C.H. Pruett and K.M. Terwilliger, *Rev. Sci. Inst.* , **28**, 403 (1957)
- <sup>62</sup>L.W. Jones, C.H. Pruett, K.R. Symon and K.M. Terwilliger, Comparison of Experimental Results with the Theory of the RadioFrequency Acceleration Process in FFAG Accelerators,*Proc. 1959 Intern. Conf. on High-Energy Accelerators*, CERN, Geneva, (1959), p.58
- <sup>63</sup>K.M. Terwilliger, L.W. Jones, and C.H. Pruett, *Rev. Sci. Inst.*, **28**, 987(1957)
- <sup>64</sup>D.W. Kerst, F.T. Cole, H.R. Crane, L.W. Jones, L.J. Laslett,T. Ohkawa, A.M. Sessler, K.R. Symon, K.M. Terwilliger and N. Vogt-Nilsen, *Phys. Rev.* **102**, 590 (1956)
- <sup>65</sup>D.W. Kerst, Properties of an Intersecting Beam Accelerator System, *Proc. 1956 CERN Symposium on High-Energy Accelerators and Pion Physics*, CERN, Geneva, (1956), p. 36

- 
- <sup>66</sup>L.J. Laslett and K.R. Symon, Particle Orbits in FFAG Accelerators, *Proc. 1956 CERN Symposium on High-Energy Accelerators and Pion Physics*, CERN, Geneva, (1956), p.279
- <sup>67</sup>L.W. Jones and K.M. Terwilliger, A Small Model FFAG Radial Sector Accelerator, *Proc. 1956 CERN Symposium on High-Energy Accelerators and Pion Physics*, CERN, Geneva, (1956), p.359
- <sup>68</sup>D.W. Kerst, Spiral Sector Magnets, *Proc. 1956 CERN Symposium on High-Energy Accelerators and Pion Physics*, CERN, Geneva, (1956), p.366
- <sup>69</sup>D.W. Kerst, H.J. Hausman, R.O. Haxby, L.J. Laslett, F.E. Mills, T. Ohkawa, F.L. Peterson, A.M. Sessler, J.N. Snyder and W.A. Wallenmeyer, *Rev.Sci. Inst.* **28**, 970, (1957) (letter)
- <sup>70</sup>D.W. Kerst, E.A. Day, H.J. Hausman, R.O. Haxby, L.J. Laslett, F.E. Mills, T. Ohkawa, F.L. Peterson, E.M. Rowe, A.M. Sessler, J.N. Snyder and W.A. Wallenmeyer, *Rev.Sci. Inst.* **31**, 1076, (1960)
- <sup>71</sup>R.O. Haxby, L.J. Laslett, F.E. Mills, F.L. Peterson, E.M. Rowe and W.A. Wallenmeyer, Experience with a Spiral Sector FFAG Accelerator, *Proc. 1959 International Conference on High-Energy Accelerators*, CERN, Geneva, (1959), p.75
- <sup>72</sup>T. Ohkawa, On the Two-Beam FFAG Accelerator, MURA Report 318, July 5, 1957 and *Rev. Sci. Inst.* **29**, 108 (1958)
- <sup>73</sup>T. Ohkawa, Electronic Devices Using Plasma in a Magnetic Field, MURA Repor-310, June 14, 1957
- <sup>74</sup>K.W. Robinson, *Phys. Rev.* **111**, 373 (1958)
- <sup>75</sup>I have been unable to find a MURA reference for this work, although I remember a note containing a sketch of a search coil that resembled the spools used for household thread. Laslett wrote two long, instructive Brookhaven reports on the subject, LJL-1, Some Aspects of Search Coil Design, July 5, 1954, (49 pages) and LJL-2, Coil Systems for Measurement of Field and Field-Gradient in Two-Dimensional Magnetic Fields, July 16, 1954 ( 28 pages).
- <sup>76</sup>MURA Staff, Design of a 10-BeV FFAG Accelerator, *Proceedings of the 1961 International Conference on High-Energy Accelerators*, USAEC, Publisher, (1961), p. 57
- <sup>77</sup>L.W. Jones, Experimental Utilization of Colliding Beams, *Proc. 1959 Intern. Conf. on High-Energy Accelerators*, CERN, Geneva,(1959), p.15  
L.W. Jones, Recent U.S. Work on Colliding Beams, *Proceedings of the 1963 International Conference on High Energy Accelerators*, Dubna, USSR, (1963), p.379
- <sup>78</sup>K.M. Terwilliger, Achieving Higher Beam Densities by Superposing Equilibrium Orbits, *Proc. 1959 International Conference on High-Energy Accelerators*, CERN, Geneva, (1959) p.53
- <sup>79</sup>D.A. Swenson, A Study of the Beam-Stacking Process, *Proceedings of the 1961 International Conference on High-Energy Accelerators*, USAEC, Publisher, (1961) p. 187
- <sup>80</sup>A large part of Galonsky's extensive pioneering work on production estimates appears to have been written down only in the proposals. There is some in F.T. Cole and A. Galonsky, Use of a 10-GeV High Intensity Accelerator as a Pion Factory, *Proceedings of the 1963 International Conference on Sector-Focused Cyclotrons and Meson Factories*, CERN, Geneva, (1963). I cannot understand at this late date why this work was published in a cyclotron conference and why my name is on the paper.

- 
- <sup>81</sup>F.T. Cole and P.L. Morton, Radial Straight Sections in Spiral Sector FFAG Accelerators, *Proc. 1959 International Conference on High-Energy Accelerators*, CERN, Geneva, (1959) p.31
- <sup>82</sup>J.L.Tuck and L.C.Teng, Institute of Nuclear Studies 170, Synchrocyclotron Progress Report III, Chapter 8 (University of Chicago), 1950.  
J.L.Tuck and L.C.Teng, *Phys.Rev* **81**, 305, 1951.
- <sup>83</sup>K.J. LeCouteur, *Proc. Phys. Soc. of London B* **64**, 1073 (1951)
- <sup>84</sup>C.L.Hammer and A.J. Bureau, *Rev. Sci. Inst.***26**, 594 (1955)  
C.L.Hammer and A.J. Bureau, *Rev. Sci. Inst.***26**, 598 (1955)  
C.L.Hammer and L.J.Laslett, *Rev. Sci. Inst.***32**, 144 (1961)
- <sup>85</sup>L.J. Laslett and K.R. Symon, Computational Results Pertaining to Use of a Time-Dependent Magnetic Field Perturbation to Implement Injection or Extraction in a FFAG Synchrotron, *Proc. 1959 Intern. Conf. on High-Energy Accelerators*, CERN,
- <sup>86</sup>L.W. Jones and K.M. Terwilliger(1959), *Proc. 1959 Intern. Conf. on High-Energy Accelerators*, CERN, Geneva, p.48
- <sup>87</sup>F.E. Mills, J.A. Mogford, C.A. Radmer and M.F. Shea, Beam Extraction from an FFAG Accelerator, *Proceedings of the 1961 International Conference on High-Energy Accelerators*, USAEC, Publisher, (1961) p.415
- <sup>88</sup>J.E. O'Meara, C.H. Pruett, E.M. Rowe, C.A. Radmer, M.F. Shea, D.A. Swenson and D.E. Young, Beam Extraction from the MURA 50-Mev FFAG Accelerator, *Proceedings of the 1963 International Conference on High Energy Accelerators*, Dubna, USSR, (1963) p. 947
- <sup>89</sup>F.T. Cole, Typical Designs of high-Energy FFAG Accelerators, *Proc. 1959 International Conference on High-Energy Accelerators*, CERN, Geneva, (1959) p.82
- <sup>90</sup>H.K.Meier and K.R.Symon, Analytical and Computational Studies on the Interaction of a Sum and Difference Resonance, *Proc. 1959 International Conference on High-Energy Accelerators*, CERN, Geneva, (1959)p.253
- <sup>91</sup>D.B. Lichtenberg, R.G. Newton and M.H. Ross, Intersecting Beam Accelerator with Storage Ring, MURA Report-110, April 26, 1956
- <sup>92</sup>G.K. O'Neill, *Phys. Rev.*, **102**, 1418 (1956)
- <sup>93</sup>T.L. Collins, *Cambridge Electron Accelerator Report* CEA-86, (1961)
- <sup>94</sup>C.E. Nielsen, A.M. Sessler, and K.R. Symon, Longitudinal Instabilities in Intense Relativistic Beams, *Proc. 1959 International Conference on High-Energy Accelerators*, CERN, Geneva, (1959), p.239
- <sup>95</sup>M.Q.Barton and C.E.Nielsen, Longitudinal instability and Cluster Formation in the Cosmotron, *Proceedings of the 1961 International Conference on High-Energy Accelerators*, USAEC, Publisher, (1961) p. 163
- <sup>96</sup>L.J. Laslett, V.K. Neil, and A.M. Sessler, *Rev. Sci. Inst.* **36**, 429 (1965) and *Rev. Sci. Inst.* **36**, 436
- <sup>97</sup>L.J.Laslett, Concerning the  $\gamma$ -Growth Exhibited by Algebraic Transformations, MURA Report 246, March 11, 1957

---

L.J.Laslett, Supplemental Note Concerning the Algebraic Transformation of MURA-246, MURA Report 247, March 18, 1957

<sup>98</sup>L.J.Laslett, Round-Off Errors From Fixed-Point Linear Algebraic Transformations Computed by IBM-704 Program 117, MURA Report 302, June 11, 1957

<sup>99</sup>L.J.Laslett in *Topics in Nonlinear Dynamics - A tribute to Sir Edward Bullard*, AIP Conference Proceedings No. 46, Siebe Jorna, Ed., AIP, New York, 1978, p. 221.

<sup>100</sup>MURA Staff, Progress on the MURA Two-Way Electron Accelerator, *Proc. 1959 International Conference on High-Energy Accelerators*, CERN, Geneva, (1959), p.71

<sup>101</sup>MURA Staff, *Rev. Sci. Inst.* **35**, pps. 1393-1480 (1964)

C.D.Curtis, A. Galonsky, R.H. Hilden, F.E. Mills, R.A. Otte, G. Parzen, C.H. Pruett, E.M. Rowe, M.F. Shea, D.A. Swenson, W.A. Wallenmeyer and D.E. Young, Beam Experiments with the MURA 50-MeV FFAG Accelerator, *Proceedings of the 1963 International Conference on High Energy Accelerators*, Dubna, USSR (1963), p. 815

<sup>102</sup>L.J. Laslett, *Proc. 1963 Summer Study on Storage Rings, Accelerators, and Experimentation at Super-High Energies*, Brookhaven National Laboratory, Upton, N.Y., (1963), p.324

<sup>103</sup>M.W. Sands, Ultra-High Energy Synchrotron, MURA Report 465, June 10, 1959

<sup>104</sup>L.C.Teng, *Rev. Sci. Inst* **27**, 106 (1956)

<sup>105</sup>Robert R. Wilson, The Electron Synchrotron, in *Handbuch der Physik*, Vol. XLIV, S.Flugge and E. Creutz, eds., Springer Verlag, Berlin, 1959, p.176

<sup>106</sup>G. Parzen, C.H. Pruett, W.A. Wallenmeyer and D.E.Young, *Proceedings of the 1961 International Conference on High-Energy Accelerators*, USAEC, Publisher (1961), p. 478

G. Parzen and P.L.Morton, Effects of Field Perturbations in FFAG Accelerators, *Proceedings of the 1963 International Conference on High Energy Accelerators*, Dubna, USSR, (1963), p. 1089

G. Parzen and P. Morton, *Rev. Sci. Inst.* **34**, 1323 (1963)

<sup>107</sup>D.C. Morin and F.E. Mills, Multiturn Injection into FFAG Accelerators, *Proceedings of the 1961 International Conference on High-Energy Accelerators*, USAEC, Publisher, (1961), p. 395

<sup>108</sup>MURA Staff, *Proceedings of the 1961 International Conference on High-Energy Accelerators*, USAEC, Publisher (1961),p. 344,

<sup>109</sup>R.O. Haxby, R.S. Christian, G. del Castillo and S.C. Snowdon, Spiral Sector FFAG Magnets, *Proceedings of the 1961 International Conference on High-Energy Accelerators*, USAEC, Publisher (1961), p.476  
S.C. Snowdon, R.S. Christian, G. del Castillo and R.W. Fast, Spiral Sector FFAG Magnet Design, *Proceedings of the 1963 International Conference on High Energy Accelerators*, Dubna, USSR, (1963) p 746

<sup>110</sup>T.W. Edwards, Proton Linear Accelerator Cavity Calculations, MURA Report 622 (1961)

D.E. Young, R.S. Christian, T.W. Edwards, F.E. Mills, D.A.Swenson and J. Van Bladel, Design of Proton Linear Accelerators for Energies up to 300 MeV, *Proc. 1963 International Conference on Sector-Focused Cyclotrons and Meson Factories*, CERN 63-19, CERN, Geneva, p. 372

B. Austin, T.W. Edwards, J.E. O'Meara, M.L. Palmer, D.A. Swenson, and D.E. Young, The Design of Proton Linear Accelerators for Energies up to 200 MeV, MURA Report 713 (1965)

<sup>111</sup>L.J. Laslett, Particle Motion in the Proposed Budker Accelerator, *Proceedings of the 1963 International Conference on High Energy Accelerators*, Dubn, USSR, (1963) p. 1438.