EARLY FFAG DEVELOPMENT

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1 INTRODUCTION

After hearing Dave Clark's interesting talk on Departed Cyclotron Pioneers yesterday, I decided that a discussion of a fifty year old topic can best be started by recalling the names of the "pioneers" who began it all and took part in the work. FFAG development began with the Midwest Accelerator Conference (MAC), which led to the formation of a Multi-University Corporation called Midwestern Universities Research Association (MURA) which operated a laboratory in Madison, Wisconsin from 1956 to 1967. These activities were set in motion by a group of physicists who had built and operated accelerators in the 1930's and 1940's in university environments, and who wanted to take part in the next step of accelerator development. They were frustrated by the existence of regional accelerators at Lawrence Radiation Laboratory and Brookhaven National Laboratory, and the refusal of their regional laboratory (Argonne) to do anything but reactor engineering. This group included Don Kerst, Gerry Kruger, Jackson Laslett, Bob Haxby, John Williams, Allen Mitchell, Bernie Waldman, Ragnar Rollefson, Dick Crane, Lawrence Johnston and possibly others. This group enticed bright young scientists to address problems in accelerator physics, including Keith Symon, Jim Snyder, Ed Akely, Carl Nielsen, George Parzen, Frank Cole, Larry Jones, Kent Terwilliger, Charles Pruett, and Andy Sessler. When the MURA Laboratory was formed, many of these people came for a year or more to help start activities. They were joined by some new people, Dick Christian, Ed Day, Bill Wallenmayer, Ed Rowe, Frank Peterson, Lloyd Fosdick, Tihiro Ohkawa, Marshall Keith, myself, and, in the next few years, Curt Owen, Don Swenson, Carl Radmer, Martin Berndt, Bill Winter, Glenn Lee, Roger Otte, Mike Shea, Gus del Castillo and Jim McGruer. In 1959 the staff was significantly augmented by Stan Snowdon, Aaron Galonsky, Don Young, Cyril Curtis, and Peter Rosen. Phil Morton, an USU graduate student, joined soon after.

An extensive memoir of the history of MURA was written by Frank Cole just before he passed away in 1994. I have learned with pleasure that the Cyclotron Conference will publish that as part of the proceedings of this conference. I recommend it to you highly. I shall attempt to incorporate some of my own views on this subject below. I apologize in advance for the brevity of this report. It is, after all, difficult to describe ten years of the activities of all those (and more) people above in a few pages.

2 BEAM DYNAMICS

FFAG accelerators can be either "non-scaling" or "scaling" as described below. The MURA group developed the scaling type, while most modern interest is in the non-scaling type. The scaling type can be described by a median plane magnetic field (Radial Sector)

$$B = B_0\left(\frac{r}{r_0}\right)^k f(\theta)$$

where k and r_0 are constants and f is a periodic function with N periods in the circumference, or by (Spiral Sector)

$$B = B_0\left(\frac{r}{r_0}\right)^k f(\psi)$$

where

$$\psi = K \ln\left(\frac{r}{r_0}\right) - N\theta$$

and the spiral angle ζ is given by

$$\tan\zeta = \frac{K}{N}$$

If we Fourier expand (taking the average value of f to be 1) the function f in sine and cosine functions of nNθ with coefficients g_n of cosines and f_n of sines we can define two numbers F^2 and G^2 where

$$F^2 = \sum_1^\infty\left(g_n^2 + f_n^2\right),$$

$$G^2 = \frac{1}{2}\sum_1^\infty\left(\frac{g_n^2 + f_n^2}{n^2N^2}\right)$$

The smooth approximation estimates of the tunes of the radial sector accelerator are

$$\nu_x^2 = k + 1 + k^2G^2,$$

$$\nu_z^2 = -k + k^2G^2 + \frac{1}{2}F^2$$

The G term is the AG term, while the F (Flutter) term is due to the edge focusing, which does not affect the radial frequency because of the orbit scalloping. In the spiral sector configuration, the AG term is negligible and the tunes are approximately

$$\nu_x^2 = k + 1, \quad \nu_z^2 = -k + \frac{F^2K^2}{N^2}$$

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In standard parlance, the chromaticity is zero, so resonances are not a problem as they are in a cyclotron.

Orbits of different momenta p are similar in shape and are related by

$$\frac{p_2}{p_1} = \left(\frac{r_2}{r_1} \right)^{k+1}$$

and the momentum compaction relating orbit lengths and momenta is

$$\alpha = \frac{1}{k+1}.$$

This gives the usual slip factor relating frequency and momenta, and RF acceleration is essentially the same as in a DC synchrotron, say, the ISR. The fixed field allows many opportunities to obtain higher average intensity by intermediate stacking and high rep rate injection, while also allowing the accumulation of dense stacked beams, of interest for colliders. Resonance extraction, invented first for FFAG accelerators, poses no especially difficult challenges.

3 THE MODEL PROGRAM

The Radial Sector Accelerator was invented by Keith Symon and simultaneously by Tihiro Ohkawa at Tokyo in 1954. The Spiral Sector Accelerator was invented by Don Kerst shortly thereafter. A program of model building began, first the Michigan Model, built at Ann Arbor by Terwilliger, Jones, and Pruett. It was a so-called Mark II with unequal length focusing and defocusing magnets. Injection was at about 20 keV, and the beam was accelerated with either a betatron core or RF to 400 keV, well below transition energy. The model worked well, and a variety of experimental tests were made, most notably in beam stacking and recapture. The model was first assembled in Ann Arbor in March of 1956, but was moved to Madison in the Autumn of 1956.

The second model, the Illinois Spiral Sector Model, was started at Champaign by Don Kerst, Frank Peterson, et. al. at the Betatron Lab, but was also moved to Madison in the autumn of 1956. Magnet measurements were done by Bob Haxby and Bill Wallenmayer, vacuum by Ed Day and me, injection, acceleration and control systems by Carl Radmer, Ed Rowe and me. Injection was at 25 keV, and γt was about 1.3. The machine was operated in 1957 and was used to investigate transition crossing, resonances, space charge, instabilities and extraction.

The third model, the Wisconsin Model (or the Stackatron as it was dubbed by Aaron Galonsky) was started in 1957. It was planned to be in the two-way configuration proposed by Ohkawa because of the interest in colliding beams, so it had identical focusing and defocusing magnets with injection at 100 keV (0.3 MeV/c) and a final energy at 45 MeV, so the momentum ratio was 150. The magnets were to start with conical surfaces with surface back wound conductors, proceed through a region of progressively deeper slots for the back windings, and then launch into a "non scaling pole" region with no windings, which terminated a gap or so beyond the maximum energy orbit where the forward windings and the back leg were to be found. In the absence of three dimensional magnetic field calculation programs, it proved impossible to design correctly the non scaling pole. The machine was turned on in December 1959, but crossed major resonances as the non scaling pole region was entered. In addition, the injection system was marginal for its purpose, as was the magnet power supply.

It was decided to fix the problems, but interest had waned for the two way operation, so a more conservative one way point was chosen. The magnets were shimmed from measurements by Bill Wallenmayer, Don Young, Charley Pruett and Roger Otte. The corrected fields were measured and put into a mesh orbit program (Parmesh) by George Parzen and the process was iterated until a satisfactory solution was achieved. A final mesh of the whole accelerator indicated the presence of two nonlinear resonances at intermediate energies. Aaron Galonsky and I, measuring beam lifetime, verified the effect of these resonances. We chose the stacking energy to avoid them. A new high current, low emittance gun was developed by Cy Curtis, Don Swenson, and me. A new magnet power supply by Martin Berndt gave $2 \bullet 10^{-5}$ stability. When the machine was operated again in the summer of 1961, it met all our hopes and expectations. High efficiency stacking was routine, and currents of 3 Amperes were achieved.

We soon decided that the beam was limited by captured ions (this phenomena having been observed in the Spiral Sector Model), and so we installed a full circumference clearing electrode to sweep out the ions. The ion bombardment of the Titanium electrode liberated a lot of gas, and the sputtering did no pumping, as in ion pumps, much to our disappointment. Gradually the pressure reduced, the lifetime improved, the current increased, until one day the beam began vanishing suddenly. We diagnosed this as a vertical transverse instability caused by the resistance in the walls. We built a feedback system which damped the unstable modes up to several hundred MHz. As the current came up, it became strong enough to change the injection field enough to stop injection. A feedback system was devised to buck out the beam current by supplying a counter current to stabilize the injection fields. Soon, we reached 10 Amperes of stacked beam whose phase space density was only several percent less than that of the beam being stacked. We then built an extraction system which extracted the 45 MeV beam. We had come to the end of our quest. It had taken six years.

4 THE LAST PROPOSAL

The High Energy community decided that they (and we) were not yet ready for colliding beams, and that a "Kaon Factory" of 12.5 GeV protons would be just the thing to have. We designed a Spiral Sector proton accelerator with a 200 MeV Linac injector which would provide 30 μA ($2 \cdot 10^{14}$ per sec) of protons. Although I took part in the decision to pursue the Kaon Factory, and did not object, I felt that we would be swept away by the high energy wind, which was then blowing for a 300 GeV proton synchrotron. After all, high energy was why we were there, and the reason for our interest in colliding beams. The Kaon Factory was rejected after President Kennedy's death, and the Laboratory was disbanded.

5 LIFE AFTER DEATH

Many of the staff left MURA in 1964. I joined the University of Wisconsin, and agreed to help make the Laboratory part thereof. I was appointed Director of the MURA Laboratory and later the Physical Sciences Laboratory, which it became. Rowe and Pruett stayed, as did a substantial engineering staff. All those who would go to NAL in 1967 also stayed, and worked on projects of importance to the future Lab. The Atomic Energy Commission decided that it would no longer talk to us directly, but financing would proceed administered through Argonne to give a safe landing for those who would work at NAL, and the others who, it was presumed, would work at Argonne. Budgets for the laboratory were submitted to the Associate Director for HEP at Argonne. Most of the money was used to help the limping ZGS achieve better operation. Although this program in principal was for all phases of operation, the most successful was the extension of our feedback methods to cure the transverse instability in the ZGS. This one system raised the intensity from $2 \cdot 10^{11}$ to 10^{12} protons per pulse.

On the other hand, I had become very interested in effects of Synchrotron Radiation in accelerators. The beam lifetime of the FFAG had finally been determined by the radiation antidamping of the radial betatron oscillations. This also happened in AG synchrotrons such as CEA or the Cornell machines. Was there a way to fix this? When I spent the winter of 61-62 at Saclay in Henri Bruck's group, we collaborated with the Orsay Linac Laboratory on what became ACO, the French e+-e- storage ring. I attacked the radiation problem from the most fundamental level I could imagine. Surprisingly the problem yielded. The solution was simple: no bend field and gradient at the same place: i. e. separated function magnets. (Claudio Pellegrini came to the same conclusion at the same time at Frascati, leading to ADONE.) With that in mind I really wanted to build a very simple electron storage ring because of the stalemate between SLAC and CEA on the construction of a collider.. I had in mind using magnet ends for vertical focusing and a few quads for radial focusing. (We learned from Ken Green of BNL almost 20 years later that we had stumbled onto building a "low emittance" lattice). In the discussions at MURA in 1964 about the future I raised the possibility of building such a ring for accelerator physics studies. Ed Rowe and Charley Pruett were very interested, as was the engineering staff. We also knew from Gerry Kruger that there was the need for such a ring for condensed matter and other research. As we drew up our budgets with Argonne I began inserting the electron storage ring as an item. Bob Sachs, previously of UWM and then Associate Director at Argonne, felt that he should not interfere with our future plans. We were doing our job for the ZGS. He felt the money we received was ours, not Argonne's. Argonne in fact built some key components: the magnet coils. Altogether we spent about $300,000 on components, and used some labor. The modest building addition was paid for by the MURA Corporation.

In 1967 the laboratory formally became the Physical Sciences Laboratory of the University of Wisconsin at Madison. I wrote separate charters for PSL and the Storage Ring, which was promptly funded by the Air Force. I felt that the storage ring needed some autonomy from the lab, even though at that time I was wearing both hats, so to speak. Ed Rowe, Charley Pruett, Bill Winter and many others did a remarkable job, first by completing the ring economically and putting it in service to users, and then making it grow into a place of international reputation with a higher energy ring. In those early days, no one worked only on the ring. Each of us did projects for others at UWM and other places. For example, in the summer of 1967 we built the first magnet model for NAL and 200 feet of elliptical vacuum chamber to test the proposed (and accepted) vacuum system for the Main Ring PSL, meanwhile has put in 34 years of service to UWM, and is a very respected institution also. It does work for many different departments, most particularly the Plasma Physics and Particle Physics groups.

Finally let me say that the Midwestern Universities honored their promise and made sure that each of us found suitable employment. No one was left stranded. It was altogether a remarkable experience.