Some Recollections on the Story of the Cyclotron and Comments on Higher Degrees

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In the following account I make no claim to unreasonable objectivity – it is based upon my recollections together with some notes and papers.

It was in the fall of 1934 that I asked Ernest Lawrence at Berkeley about research possibilities for a PhD thesis. Upon his invitation to try out for a year, I found a fascinating world across the way from Physics in the building which later became known as the Old Radiation Laboratory. There was a feeling – an atmosphere, that a research program of world importance was under way here, based on the development of the cyclotron by Lawrence and Livingston in 1930–32. Future research stars Ed McMillan, Jack Livingood, Franz Kurie, Bob Thornton and Malcolm Henderson were already on board – joined soon by Luis Alvarez. Lowly graduate students like me were Jackson Laslett, Hugh Paxton, Paul Aebersold and Glen Seaborg.

As an aspiring graduate student I had to learn the following techniques:

a) How to make a vacuum-tight seal using beeswax and resin – or later and worse how to use glyptal.
b) How to bend tungsten filaments for primitive ion sources or for the home-made triodes of the rf oscillator.
c) How to taylor the magnetic field in the cyclotron gap by adding or moving thin sheets of iron between the upper pole face of the magnet and the upper lid of the vacuum chamber. This is one of the tasks that Lawrence enjoyed doing himself – one of my most enjoyable memories is that of Lawrence wearing a greengrocer's smock, using a large brass hammer to bang away at the iron shims. This was to improve both the shape of the magnetic field vs radius contour and to adjust the azimuthal variation of the field to maximize the beam through the deflection channel to the outside.
d) Another skill students should learn is adjusting the resonant frequency of the dees by bending the water cooled coil forming the inductive part of the circuit.
e) I was a dismal failure at glass-blowing.

Five years later (1939) there had been considerable improvement in understanding the physics of the cyclotron but also the technology had improved greatly. Gaskets replaced glyptal and the dees were mounted on the ends of resonant lines in order to achieve greater dee voltage. Dee voltage must be higher to circumvent the relativity problem.

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The relativity problem for cyclotrons was first stated in print by Bethe & Rose (1937). The cyclotron frequency for an ion of charge q and mass m in a magnetic field B is given by $\omega_c = \frac{qB}{m}$. One would like to have the dees excited by a constant frequency ω_c but the mass increases with the kinetic energy T as $m = m_o + \frac{T}{c^2}$ and so B should increase with the energy or radius R. In a magnet with azimuthal symmetry, however, B must decrease with radius R in order to obtain a vertical focusing force from the bowing out of the magnetic lines. This is the cyclotron relativity problem. Other papers by Rose and R.R. Wilson (1938) followed but the only clear-cut solution to the problem appeared to be increasing the dee voltage, thus decreasing the number of turns and the growth in the difference in phase α of the ions relative to the rf.

In practice the relativity problem was first circumvented by a trick. The magnetic field was adjusted to take advantage of the small electric focusing at low energies. In this case the ions would start with $\alpha = -\frac{\pi}{2}$ and then gain in phase until they are $\frac{\pi}{2}$ radians ahead in phase. At this point the magnetic field has become less than the resonance field so the phase decreases to $-\frac{\pi}{2}$ when it presumably enters the deflector channel. There are obvious difficulties at the beginning and end of this program but it does yield a calculated minimum dee voltage of 95 kV for the 16 MeV (deuteron) 60 inch cyclotron - in good agreement with the observed value. These considerations were presented in the 1940 paper entitle "Theory of the Cyclotron" by R.R. Wilson.

In fact, Ernest Lawrence was already seeking funding for a much larger increase in cyclotron energy. Expressed in terms of their pole diameters, the series of magnets at the Berkeley Radiation Laboratory had been:

11 inches – 27 inches – 37 inches – 60 inches.

Now there was to be a major increase of a factor of 3 to 184 inches. I was always impressed by Lawrence's profound faith in experimental ingenuity, either his own or that of others. And he was right, of course, the difficult thing to do at that point was to raise the money and then to build the huge magnet. If the only path had been that of going to dee voltages of 1 MV or more, the task would have been very difficult indeed. The failure of the huge linac in California in the 1950's demonstrated that the technology of that time was not adequate for such extrapolations.

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On the other hand, the existence of the 184 inch magnet, finished in 1941-42, proved to be of very significant advantage in the development of the calutron (the electromagnetic or mass spectrometer method of isotope separation). Many different development programs could be carried out simultaneously in the huge volume of uniform magnetic field available between the pole faces of the 184. In fact, the success of the program was dependent in large part on the existence of the 184 inch magnet.

The fall and spring of 1945-46 was one of the most exciting periods of my professional life. The war in the Pacific had just ended and we physicists could turn thankfully to plans and dreams for the future. Naturally my thoughts turned to ways of solving or circumventing the cyclotron relativity problem. I am sure that many physicists had considered the possibility of modulating the frequency of the rf system in accordance with a set energy gain for a packet of ions. The magnetic field could be constant with radius or it could decrease with radius to ensure axial focusing. However, the precision required in matching the rf frequency to the timing of the accelerating process seemed impossibly strict.

The discovery of the principle of phase focusing and stability by Vexler (1944-45) and independently by McMillan (1945) changed the situation completely. If the peak energy gain per turn qV is made larger (e.g. by a factor of two) than that required to keep ω in consonance with the total energy, this principle shows that there will be an equilibrium phase α_0 (60° in this case) about which the phase will oscillate. Eventually the desired energy will be attained, if the other conditions of focusing, etc. are satisfied. It was generally believed that a cyclotron of this type would require a moderately high injection energy. However, some ion path work I had done during the war, combined with further calculations, convinced me that ions could be picked up directly from an ion source at the centre of the machine. I estimated an efficiency of 3–5% at the optimum α_0=60°.

In the fall of 1945, then, I proposed to Lawrence that the 184 inch cyclotron be finished using frequency modulation and that as proof of principle we use the old 37 inch cyclotron. In the relation $\omega = \frac{qB}{m}$ I suggested we simulate the acceleration of deuterons to 200 MeV (11% mass increase) by an 11% decrease of magnetic field with radius.

Within a period of three months we had changed the magnet, installed one dee, a non-hooded ion source, probes, set a rotating capacitor whirling around and had brought a beam out to full radius. (Richardson, MacKenzie, Lofgren and Wright, 1946). The yield as a function of $\frac{d\omega}{dt}$ verified my calculations on the ion pick-up process.

Lawrence had been following our efforts closely because he was concerned about the feasibility of the design now under resumed construction - 1 MV on the dees for 100 MeV deuterons! After I demonstrated the full radius beam to him one day, Lawrence became very excited and rushed out of the laboratory to drive up the hill to the engineering office. I understand he passed a truck carrying one of the huge dee stem tanks necessary for 1 MV on the dees. Stopping the truck, he told the driver to turn around and take the tank back to storage (or the dump!) The FM cyclotron required only a few kV on one dee. I think the story speaks for itself.

At that time (Fall of 1945) there were three teams of physicists at Berkeley working on the development of higher energy accelerators.

1. A new linear accelerator concept (later called the Alvarez type) being developed by Luis with the very able assistance of Pief Panofsky.
2. A 500 MeV electron accelerator using the phase focusing principle under Ed McMillan's leadership.
3. The "proof of principal" of the FM or synchrocyclotron.

It was an exciting time with each group reporting weekly progress at a meeting usually chaired by Lawrence. The progress of our group was much more rapid than that of the other groups because our task was much, much easier. I always found that an FM cyclotron was eager to run.

Since our work was the first experimental test of the Vexler-McMillan principle we spent quite a lot of time exploring the ramifications of phase focusing, including the concept of the bucket, which was clearly described in our later paper (Richardson, Wright, Lofgren and Peters 1947)

List of Operating FM cyclotrons in 1950
(four years after "proof of principle")

UCLA	Jan	1946	20 MeV p originally at Berkeley
Berkeley	Nov	1946	350 MeV α (pions) 190 MeV d
Rochester	Jan	1949	240 MeV p
Princeton		1949	18 MeV p
Dubna		1949	? later 700 MeV p
Amsterdam		1949	28 MeV d
Harvard		1949	140 MeV p
Harwell		1949	180 MeV p
McGill		1949	100 MeV p
Columbia		1950	385 MeV p

The following FM cyclotrons were under construction:

Uppsala	180 MeV p (modified to SF)
Liverpool	380 MeV p
Chicago	450 MeV p
Carnegie Inst. Tech.	440 MeV p

I. The Sector Focusing Cyclotron 1950 -

Actually the first step in the solution of the cyclotron relativity problem was taken by the well-known theorist L.H. Thomas in two papers in 1938. Two years later the results were mentioned very briefly by R.R. Wilson in his article "Theory of the Cyclotron" "Variation of B with Polar Angle Can Produce Focusing and also Preserve Resonance and Stability". As far as I know that was the last serious mention of the Thomas papers for 10 years.

One of the early experiments on the 184 (Kinsey & Lawrence) demonstrated the copious production of neutrons from bombardment with deuterons of several hundred MeV. And so, when production requirements appeared in terms of a gram of neutrons per day, it became natural for Luis Alvarez to suggest the use of high energy. high current accelerators for this purpose.

The prime candidate was a linac but I thought the Thomas cyclotron might be better so I suggested to Lawrence that we could get a "proof of principle" for the latter by modeling 250 MeV deuterons ($\beta = \frac{v}{c} = 0.47$) with 70 keV electrons. The electrons at their final energy would have the same velocity as the deuterons but a momentum smaller by a factor of 3700. This program was born classified so for most of a year I spent 4 day weekends in a small locked shack up the hill in Berkeley and 3 days teaching at UCLA. Initially it was rather lonesome with Lawrence the only visitor but when the 54 orbital trim coils proved quite adequate and 80% of the beam could be extracted I was joined by several valuable colleagues. (Kelly *et al.* 1956) David Judd made several valuable orbit calculations. In 1955 Lawrence was allowed to describe the results at the first Atoms for Peace Conference in Geneva 1955. The next important contribution to the solution of the relativity problem was made by the MURA group (Kerst *et al.* 1955) in their development of the FFAG accelerator. They pointed out that shaping the Thomas hills and valleys in a spiral (azimuth changing with radius) would greatly increase the axial focusing force of the magnetic field. The importance of this effect is shown by the simplified expression

$$\text{focusing impulse} \quad \sim \quad 1 + 2\tan^2 \epsilon$$

where the first term is the Thomas effect and the second term comes from the spiral angle ϵ. In TRIUMF at high energies, for example, the second term is 15 times larger than the first term. The idea of the spiral completed the magnetic field of the SF (sector focusing) cyclotron.

The first SF cyclotron accelerating nuclear particles to velocities higher than the velocity achievable by the classical cyclotron was that of UCLA (Richardson, Wright, Clark *et al.* 1960) accelerating protons to 50 MeV. Hill fields of 2.4 T were employed. This cyclotron operated very productively for 15 years but it was also considered by the builders to be a model for a much larger cyclotron in the 500–600 MeV energy range. Because of the foreseen difficulty of beam extraction at the proposed high energies, the acceleration of H^- ions was adopted, following the test at Colorado and the extensive development at UCLA. Also I coined the term meson factory in order to make the high cost of the proposed facility more palatable to the funding agencies.

After the meson factory decision in favor of Los Alamos was announced the work at UCLA tapered off. However, there was a complementary interest arising elsewhere. A trio of universities in Western Canada had become interested in the H^- SF cyclotron concept of a meson factory. Thus many of the original contributions of Ken MacKenzie and Byron Wright at UCLA lived on in the TRIUMF design at Vancouver. I became a long-time consultant to the group and finally in 1971 I was asked to become Director of the Laboratory. This turned out to be a mixture of fun and heavy responsibility that I found to be very rewarding indeed. As part of my reward I insisted on personally working the beam out from the center to the final radius, about 2000 turns or 60 km of ion path. I had 54 orbital trim coils to adjust – the same number as I had adjusted on the electron model of the Thomas cyclotron, twenty years earlier. Despite some minor failures, success was achieved in four weeks. Of course, everyone knew that I was having fun – reaping the reward for thousands of hours of careful work by the other members of the TRIUMF group.

In 1976 at the scientific dedication of the 520 MeV TRIUMF facility I invited Hans Bethe to give the key note speech, reminding him of his 8 MeV limit in cyclotron energy.

The decision to accelerate H^- ions at TRIUMF has had a number of favorable consequences – some foreseen, like the ability to bring out simultaneous beams of differing energies and others not foreseen. Among the latter is the ability to extract H^- ions using electric pumping of the $\nu_R = \frac{3}{2}$ resonance. This concept, initiated by George Mackenzie, makes it possible to by-pass Liouville's theorem by stripping the H^- ions into an accumulator ring and thence, as H^+ ions into a series of rings culminating in a 30 GeV KAON Factory. If TRIUMF had accelerated H^+ ions the injection process would have been extremely difficult.

The KAON Factory is described by Mike Craddock in paper CGC2 of this session.

The other cyclotron meson factory, PSI, Zurich has been very successful, with high extraction efficiency of 590 MeV H^+ ions.

I have sponsored or co-sponsored some 25 PhD students, of whom five wrote their thesis in what is now called Beam Physics. Now I find my thunder has been stolen by the APS when it introduced the new section on Beam Physics, presumably blessing the acceptance of theses in that field. I believe this move was long overdue. Accelerators and beams are not flashes in the pan. Research in their physics must be carried on in the future.

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