THE HISTORY OF THE CYCLOTRON

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I first met Prof. Ernest Lawrence as a student in his Electricity and Magnetism course when I went to Berkeley as a graduate student in 1929. He was a young associate professor in his second year at the University of California and I was greatly impressed with his enthusiasm and his vivid personality. He seemed always to emphasize the important concepts, but took a rather cavalier attitude toward factors of 4π or other details in theoretical developments.

In the summer of 1930 I asked Prof. Lawrence to propose a topic for an experimental thesis. He suggested a study of the resonance of hydrogen ions with a radiofrequency field in the presence of a magnetic field -- the phenomenon now known as cylclotron resonance. He showed from simple physical principles that ions would have a constant frequency of circular motion in a uniform magnetic field, regardless of energy, and they would be accelerated in resonance with a transverse electric field of suitable frequency. He claimed that light ions would make hundred of resolutions in the magnetic field, gaining energy on each turn and attaining final energies of 1 MeV or more in a magnet of suitable size. Lawrence had asked another student, N.E. Edlefsen, who had completed his thesis the previous winter and was waiting the June degree date, to make a preliminary experimental effort to observe this resonance. Edlefsen had used a small laboratory magnet and a low-power radiofrequency generator; he observed currents on an electrode inserted at the outer edge of the vacuum chamber. Lawrence considered his results promising, but he was unable to demonstrate resonance. However, Lawrence described the concept at a meeting of the American Association for Advancement of Science at Berkeley that spring and submitted a brief article to Science.

In discussions with Lawrence in later years I learned that he had conceived the idea of a magnetic resonance accelerator in the early summer of 1929, while browsing through the current journals in the library at the University. He saw the illustrations in a paper by Rolf Wideröe in the Archiv für Elektrotechnik for 1928, and recognized the resonance principle involved, although he could not read German readily. Wideröe's paper described an experiment in which positive ions of Na and K were accelerated to twice the applied voltage while traversing two gaps at the ends of a tubular electrode to which a radiofrequency potential was applied. This was an elementary linear accelerator, depending on the resonance of the motion of the ions with an impressed alternating electric field. Wideröe described the process as "kinetic voltage transformation". He chose the type of ions, the accelerating frequency, the applied potential, and the gap spacing to achieve resonance. The doubled energy was confirmed by electrostatic deflection measurements on the ions.

Lawrence had been searching for a method of accelerating particles to higher energies than could be attained with d.c. potentials, in order to study "nuclear excitations". He realized that extension of Wideröe's technique to such high energies would require a very long array of electrodes. He speculated on variations of the resonance principle, including the use of a magnetic field to deflect particles in circular paths so they would return to the first electrode and reuse the electric field in the gap. He found that the equation of motion predicted a constant period of revolution in a uniform magnetic field regardless of particle energy, so the ions would remain in resonance with an accelerating field of fixed frequency. Charged particles could be made to traverse the same set of electrodes many times, gaining energy on each traversal of the gap between them; the orbit radius would increase as the velocity increased. This was the cyclotron resonance principle, and the resonance frequency is now called the cyclotron frequency.

I started experimental work that summer. I first reassembled and recalibrated the 4-inch laboratory magnet used by Edlefsen, built a replacement for the glass vacuum chamber, and studied the effects that Edlefsen had obtained when the magnetic field was varied. I soon found that this was not due to hydrogen ions but probably to heavy ions from the residual gas, accelerated once in the radiofrequency field and which reached the unshielded detection electrode at the edge of the chamber.

It was now my responsibility to demonstrate true cyclotron resonance. The Physics Department glassblower built for me a sequence of flat glass chambers in which electrodes were mounted on greased-joint seals. Glass was traditionally used for vacuum systems in the laboratory. But this thin, flat glass chamber defied our technical skills. I then built a chamber formed of a brass ring and flat brass cover plates, using red sealing wax for a vacuum seal, in which the several electrodes could be mounted. The radiofrequency electrode was a single hollow D-shaped half-pillbox facing a slotted bar placed across the diameter of the chamber which we called a "dummy D". The radiofrequency potential was developed by a simple Hartley oscillator; the need for a more efficient RF circuit came later with the effort to increase energy. A 10-watt vacuum tube was used as an oscillator and provided up to 1000 volts on the electrode, at frequencies that could be varied by changing the number of turns on an external inductance coil formed of copper tubing. Hydrogen molecular ions (H_2^+) were produced through ionization of hydrogen gas in the chamber by electrons emitted from a tungsten-wire cathode near the centre of the chamber. Ions that reached the edge of the chamber were observed in a shielded collector cup located behind slits.

During the fall I continued the technical development with Lawrence's continued enthusiastic interest and supervision. It was in November 1930 when I first observed sharp peaks in the collector current as the magnetic field was varied over a narrow range. Several techniques were used to prove that the resonance peaks were due to high-energy ions.

A deflecting plate and slit system were placed in front of the collector, and gave a rough check of ion energy. But the basic proof was that the magnetic field at resonance was just that calculated from the resonance equation using the measured value of applied radiofrequency. Other resonance peaks showed at lower magnetic fields, explained as due to the 3/2 and 5/2 harmonics of the applied frequency. Incidentally, these resonance values provided a highly accurate check of the magnetic field calibration, since the precision of frequency (wavelength) measurements was better than for the magnetic field. When the graph of wavelength versus magnetic field was plotted, the points fell on a smooth hyperbolic curve, as predicted by the resonance equation; it is a linear relation for frequency versus magnetic field.

The small magnet used for the first studies had a maximum field fo 5200 gauss, for which resonance with H_2^+ ions occurred at a wavelength of 76 m; the ion energy was calculated to be 13 keV (kilo electron volt) at the radius of the collector. This goal was reached on January 2, 1931, after working straight through the Christmas and New Years' holiday. A stronger magnet was borrowed for a time, capable of producing 13'000 gauss, for which the resonance occurred at 30 m wavelength or 10 MHz frequency, and for which the calculated ion energy was 80 keV. This was obtained with an applied peak RF potential of about 1 kV, so the ions traversed a minimum of 40 turns (80 accelerations). This result was reported by Lawrence and Livingston at a meeting of the American Physical Society early in 1931 [Phys. Rev. 37, 1707 (1931)].

An important consequence of our studies with this prototype was the experimental observation of electric and magnetic focusing. In Lawrence's original conception the electric field inside the hollow RF electrodes should be zero, and the electric field in the gap should be parallel to the plane of the particle orbits. Otherwise it was expected that small transverse fields would produce spiraling orbits that would intersect the electrodes. Accordingly, the electrodes initially had a grid of fine tungsten wires tightly stretched across their apertures at the gap. Resonance peaks were first observed as currents of 10^{-10} and 10^{-11} amperes, requiring our most sensitive electrometers and galvanometers. I knew that these wires were intercepting the circulating beam and felt intuitively that they might not be needed. So, while I was on my own during a trip Lawrence made to the East Coast, I removed the grid wires and obtained greatly increased currents, in the 10^{-9} A range. On his return Lawrence recognized the focusing properties due to the shape of the electric field between the open electrode faces and we never again used grids. Similarly, when thin shims of iron were inserted in the gap between the chamber and one pole of the magnet, beam intensity was increased for certain sizes and locations of the shims. The caused me to study the effect of the shape of the magnetic field. The transverse focusing due to the concave-inward shape of the fringing field at the periphery was recognized and checked experimentally by observing the thickness of the beam at the edge of the chamber with a probe mounted on a greased joint. From then on the "shimming" of the magnetic field became an important and somewhat mysterious technique for tuning up the accelerator and obtaining maximum beam intensity.

Lawrence moved promptly to exploit the promise of this new technique. In early 1931 he applied for and was awarded a grant from the National Research Council, for $1000, for construction of a machine that could give energies useful for nuclear research. Lawrence urged me to complete my Thesis, so I could get my Ph.D. degree and be eligible for an Instructorship in the Department for the following year. He wanted me to continue the development by building a larger magnet and accelerator. The time was short, but I did complete and present by Thesis, dated April 14, 1931, and stood for my Doctor's Examination. I was a poorly prepared candidate. In following Lawrence's enthusiastic lead I had been working nights, weekends and holidays in the laboratory, with no time for reading or study. At my examination some members of the Committee were appalled to find that I had not studied "Rutherford, Chadwick and Ellis", the basic reference on natural radioactivity, which they considered essential for a person presuming to enter the field of nuclear physics. Again, Lawrence's enthusiasm and personal recommendation prevailed and I received the degree in May.

During the summer and fall of 1931 I designed and installed a 10-inch diameter magnet and built other components for a magnetic resonance accelerator capable of reaching 1 MeV energy, located in Room 339 of LeConte Hall, the Berkeley Physics building. As before, the vacuum chamber was a flat brass box and the cover plate was sealed with wax. It contained a single hollow D-shaped electrode for the RF potential, a thermionic cathode, a shielded deflecting electrode and collector cup mounted at a radius of 11.5 cm. The RF oscillator used a 10-kW Federal Telegraph water cooled power tube in a tuned-grid-tuned-plate circuit which produced peak RF potentials up to 50 kV across the accelerating gap, at frequencies up to 20 MHz. I was greatly aided in the development of this first high-power RF oscillator by David Sloan, another graduate student who had been a ham radio operator and was an ingenious student of high-frequency radio techniques. A deflecting electrode was used to draw the beam out of its circular orbit and into a collector; its radius determined the ion energy.

This first practical cyclotron produced H_2^+ ions of 0.5 MeV energy by December 1931 and H_1^+ ions (protons) of 1.22 MeV, with beam currents of about 10^{-9} A, in January 1932. The progress was reported in several abstracts and a paper was sent to the Physical Review and published February 20, 1932, by Lawrence and myself. This was the first time in scientific history that artificially accelerated ions of this energy had been produced. The original vacuum chamber of 1.2 MeV cyclotron is now on permanent display in the Kensington Museum of Science in London.

As a personal footnote to history, I recall the day when I had adjusted the oscillator to a new high frequency and, with Lawrence looking over my shoulder, tuned the magnet through resonance. As the galvanometer spot swung across the scale, indicating that protons of 1 MeV energy were reaching the collector, Lawrence literally danced around the room with glee. The news quickly spread through the Berkeley laboratory and we were busy all that day demonstrating million-volt protons to eager viewers.

We had barely confirmed our results and I was busy with revisions to increase beam intensity when we received the issue of the Proceedings of the Royal Society describing the results of Cockcroft and Walton at the Cavendish Laboratory in disintegrating lithium with protons of only 500 keV energy, for which they later received the Nobel Prize. At that time we did not have adequate instruments to observe disintegrations. Lawrence sent an emergency call to his friend and former colleague at Yale, Donald Cooksey, who came out to Berkeley for the summer with Franz Kurie, a Yale graduate student. They helped us develop the necessary counters and instruments for observing disintegrations. With the help of Milton White, then a graduate student at Berkeley, we installed a target mount inside the collector cup and a thin mica window on the side of the chamber facing the target, outside which counters could be located. Within a few months after hearing the news from the Cavendish we were ready to try for ourselves. Targets of various light elements were inserted, the counters clicked, and we were observing nuclear disintegrations. These early Berkeley results confirming Cockcroft and Walton and including several additional targets were published that fall, in 1932.

Lawrence was planning his next step even before I had completed the 10 inch machine as a working accelerator. His aims were ambitious, but supporting funds were difficult to obtain in those early years. He was forced to economize and use many substitutes to reach his goal. In late 1931 he located two magnet cores from obsolete Poulson-arc magnets owned by the Federal Telegraph Co., which he requested and was given. Each magnet had a 45 inch core, but only one long pole and one coil tank. Both cores were used and machined to form a symmetrical pair of poles, each with its coil tank. The magnet coils were formed of strip copper wound in layers and immersed in oil tanks for cooling. (Note: the oil tanks leaked! We all wore paper hats, when working between coils, to keep the oil out of our hair.) This magnet was installed in December 1931 in a frame warehouse on the campus near the physics building, later known as the "old Radiation Laboratory", which was the centre of cyclotron activities for many years. Early in 1932 I turned the 10 inch machine over to White to use for his thesis problem, and applied my time to the construction of the larger machine.

The vacuum chamber was a 27-inch brass ring fitted with iron disks for top and bottom plates. The top "lid" was removable and vacuum sealed with soft wax. Initially a single RF D was installed, supported by a Pyrex glass insulator, with a slotted bar across the diameter for a "dummy-D". This allowed us to locate the deflection electrode and collector at any chosen radius. The accelerated beam was first observed at small radius, and shimming and other adjustments were made to maximize intensity. Then the collector was moved to a larger radius, and the tuning and shimming were repeated. If we made too large a step and could not find the beam, we made a strategic retreat to smaller radius and found it. Thus we learned, the hard way, of the necessity for a radially decreasing field to maintain focusing, and produced it with thin disk shims of selected radii placed in the shimming gaps. Eventually we reached a practical maximum radius of 10 inches and installed two symmetrical D's with which higher energies and intensities could be obtained. Technical improvements were added day-by-day as we gained experience. The progress during this period of development of the "27-inch cyclotron" was reported in several publications from 1932 to 1934. A brief chronological account shows the following:

June 13, 1932	16 cm radius, 28 m wavelength, 1.24 MeV H_2^+ ions
August 20, 1932	18 cm, 29 m, 1.58 MeV H_2^+ ions
August 24, 1932	Sylphon bellows installed on filament stem to allow adjustment
September 28, 1932	25.4 cm, 25.8 m, 2.6 MeV H_2^+ ions
October, 20, 1932	Radius fixed at 10 inches, two "Dee's" installed
November 16, 1932	4.8 MeV H_2^+ ions; beam current 0.001 µA
December 2-5, 1932	Target chamber installed with Geiger-Muller counter, start of long series of experiments
March 20, 1933	5 MeV H_2^+, 1.5 MeV He^+, 2 MeV $(HD)^+$ ions, deuterium ions accelerated for first time
September 27, 1933	Neutrons observed from targets bombarded by D^+ ions
December 3, 1933	Automatic magnet current control installed
February 24, 1934	3 MeV D^+ ions, beam current 0.1 µA, radioactivity induced in C by D^+ bombardment
March 16, 1934	1.6 MeV H^+ ions (protons), beam current 0.8 µA
April, 1934	5.0 MeV D^+ ions, beam current 0.3 µA

Those were busy and exciting times. Other young scientists joined Lawrence's group; some worked on accelerator developments and others on detection instruments. We joined in teams for taking data and publishing results. In my list of publications I find 17 abstracts or articles on disintegration results, in addition to several technical papers on accelerator development, during the years 1933-34. David Sloan and Wesley Coates developed a linear accelerator, using tubular electrodes in line, which produced 2.8-MeV Hg^+ ions. Sloan and B.B. Kinsey built a linac for Li^+ ions for energies up to 1.0 MeV. Sloan and J.J. Livingood built a resonance transformer which produced electrons and X-rays of 1 MeV energy. Malcolm Henderson came in 1933; he developed counting equipment and magnet control circuits, and also spent long hours helping to repair vacuum leaks on other developments. Incidentally, Henderson invented the name "cyclotron", first used only as laboratory slang but eventually pick up by news reporters and populized. Ed McMillan joined the group in 1934, and made major contributions to the planning and interpretation of research experiments; he overlapped my time at Berkeley by several months, and is the only co-worker still there; he became Director on Lawrence's death. And we all had a fond regard for Commander Telesio Lucci, retired from the Italian Navy , who was our self-appointed laboratory assistant and a friend to all. As the research results became more interesting we depended heavily on Robert Oppenheimer for discussions and theoretical interpretations. But always, Ernest Lawrence was the leader and the central figure, enthusiastic over each new result, intent on each new technical problem, in and out of the laboratory at all hours up to midnight, convinced that we were making history and full of confidence for the years ahead.

I left Berkeley in July 1934, to go to Cornell and later to MIT as the first missionary from the Berkeley group. At Cornell I built a 2-MeV deuteron cyclotron in one year, which I like to think trained more Ph.D.'s per dollar than any other cyclotron built. One useful development was the first gas discharge ion source, which increased beam intensities far above the earlier tenth-microamp range into the multi-microamp range. There, with Hans Bethe and Bob Bacher, we started the era of nuclear physics at Cornell. Then in 1938 I went to Massachusetts Institute of Technology to design and build the 16-MeV MIT 42-inch cyclotron, which included a variety of improvements and new technology.

Meanwhile, at Berkeley many others joined with Lawrence to continue the development. Cooksey returned to stay permanently and join in the expansion of pole faces to 37-inch diameter and an increase in energy to 8-MeV deuterons. Professionally trained engineers such as W.M. Brobeck and Winfield Salisbury joined in building the 60-inch Crocker cyclotron, completed in 1939. During construction a group photograph was taken of Lawrence and his group which is now historic. The year 1939 was also notable as the year in which Ernest Lawrence received the Nobel Prize.

Perhaps this is the place to leave my story of Ernest Lawrence and the cyclotron. The prestige of the Nobel Prize gave him the opportunity to implement his dreams of ever larger cyclotrons and made him a scientific figure of international importance. The Lawrence Radiation Laboratory, first with its 184-inch synchro-cyclotron, then the 6-GeV Bevatron and now other more recent developments, is a continuing memorial to this great innovator of science.