R.R. Wilson Prize Lecture: Adventures with Accelerators

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In this paper I plan to cover 60 years of accelerator history in half a hour. In 60 years there are a little over a million half hours, so this will be a compression by a factor of 10^6. I can't be very complete—just a few glances at that history as seen from wherever I was at the time.

My first contacts with big accelerators were in the mid-thirties when I was a graduate student at Princeton. Every year we students made our way to Washington for the spring meeting of the American Physical Society, held at the Bureau of Standards which then was a cozy affair out Connecticut Avenue about a mile past the Shoreham Hotel. First we found a cheap rooming house, then we made our way out to the meeting; about five hundred physicists attended in those days and everybody knew everybody else. We looked forward to the annual debate between Millikan, Compton, and Swann about what were the cosmic rays and where did they come from. Then we would troop to the Department of Terrestrial Magnetism to see Merle Tuve, Larry Hafstad, and Odd Dahl and their 1.3 million volt Van de Graaff machine—one of the most spectacular physics operations to be seen anywhere in those days.

The next contact was back at Princeton where Milton White and Malcolm Henderson had just arrived, bringing us the cyclotron gospel from California. They were to build a machine with a 40-inch pole; its magnet yoke proved to be so huge that it would not go through the laboratory door. A big hole had to be knocked out of the outside wall. The rest of us, doing tabletop experiments for our theses, were stupefied by the grandeur of this operation.

From Princeton I went to England—to the Cavendish Laboratory—for a year of work under the direction of Lord Rutherford and Mark Oliphant. Cockcroft and Walton's voltage multiplier was still standing, but was superseded by a new multiplier built by Philips in Holland. An intriguing feature was an electric typewriter built by Wynn-Williams to record data from the Cockcroft-Walton set. But Wynn-Williams had left and nobody else could make the typewriter work.

My work was on the range-energy relation for slow alpha particles—relatively unimportant, but every day "the Lord," as Rutherford was affectionately called by his students, came around about noon to ask how the work was going.

I returned to the U.S.A. to a job, paying $3,000 a year, in the Research Laboratory of the General Electric Company in Schenectady.

Coolidge, the Lab's Director, was famous for his development of X-ray apparatus. He considered the betatron to be an important X-ray source and, in 1941, he welcomed Don Kerst who had built the first betatron at the University of Illinois. With the help of GE's engineers, Kerst built a 20 MeV machine and then returned to Urbana. But GE's interest continued and it was decided to build a 100 MeV machine at the GE Research Lab. This was done, primarily under the direction of one of GE's best engineers, Willi Westendorp. This was quite a spectacular machine operating at 60 Hz, making a deafening noise and producing a truly lethal X-ray beam.

During the war we were distracted from accelerators by the need for equipment to jam the German and Japanese radar systems. This, with the cooperation of the Radio Research Laboratory at Harvard, we accomplished quite easily.

In 1944 a letter to the editor of the *Physical Review* came to my attention. The Russians, Iwanenko and Pomeranchuk, presented a formula for energy losses by electrons in circular paths and suggested that this might set a limit to the energy to which electrons could be accelerated. Since the formula indicated energy losses proportional to the fourth power of the energy, this evidently deserved attention.

All innocent of the fact that the same formula had been worked out by Lienard in 1899, I worked through the electromagnetic theory and concluded that the Russians were right. Moreover, it appeared that the effects on a 100 MeV beam should be easily detectable. The theory indicated also that the radiation should be in a very narrow beam directed straight ahead of the radiating electron, and so should exist in a spectrum with a great many harmonics of the revolution frequency. But the vacuum chamber of the 100-MeV machine at GE was opaque, so nothing could be seen. However, it was easy to show that energy losses should make the radius of the electron orbit shrink appreciably at the end of the acceleration period. Westendorp had already observed an unexplained orbit shrinkage; I calculated its predicted amount and found complete agreement with the observations.

I published my calculations and our observations on the 100-MeV machine in 1946. Only later did I become aware of Lienard's work (remember, he was the one who invented retarded potentials) and of another monumental opus, the 327-page Adams Prize Essay of 1908 by G.A. Schott, scholar of Trinity College, Cambridge on "Electromagnetic Radiation." That essay contained the complete theory of radiation from accelerated charged particles. Its only weakness was that the formula describing the spectrum of the radiation was expressed in Bessel functions of very high order not to be found in available tables. This was rectified in 1949 when Julian Schwinger published his elegant paper, "On the Classical Radiation of Accelerated Electrons," in which he converted Schott's spectrum formula to one using tabulated functions.

It should be noted that, at that time and for some time thereafter, this radiation was regarded as nothing but a nuisance, setting an upper limit to achievable energies. No one appreciated the possibility that the radiation might be a useful experimental tool.

It was then that the synchrotron appeared on the scene. We read Ed McMillan's paper with fascination. Did his phase oscillation equation indicate a stable oscillation? To the mathematically sophisticate it was evidently OK, but I thought it would be nice to solve it with a new calculating engine that GE's engineers had created. It performed integration with a system of discs revolving in contact with the surfaces of other rotating discs and took up a room 85 feet long. It had been quite useful in solving some problems of electron orbits in vacuum tubes, so I fed it the phase oscillation differential equation. To my distress, it supplied a solution that continually increased in amplitude. After some thought, I decided to feed it the equation for an undamped sine wave. To my relief, the machine again yielded an increasing amplitude. The conclusion was that backlash in the integrating mechanisms made the machine unsuitable for equations with periodic solutions.

About this time Ed McMillan himself appeared in Schenectady to ask for advice on how to construct the laminated magnet for his proposed 300-MeV synchrotron. Westendorp and I, with several others, spent a good deal of time with him, showing him our solutions to problems with the betatron magnet. Then he went home and we began to think about our skill in building machines and about all the parts we had lying around. Herb Pollock, Bob Langmuir and I, with several of our associates, said to ourselves, "Suppose we put together a machine for, say 70 MeV? We could be the first in the world to produce an operating synchrotron." And, indeed, that is what we did. Our machine operated in 1947; it had a transparent vacuum chamber and almost immediately the electron radiation was observed, a small bluish white spot at the side of the chamber where the beam was approaching the observer. At lower energies the spot changed color; at 40 MeV it was yellow and at 30 MeV it became red and very faint. So now it is called "synchrotron radiation" although it was first detected in a betatron.

We were not the first to produce an operating synchrotron. The British team of Goward and Barnes had very quickly thrown together a tiny 8-MeV machine which worked long before ours was finished.

In 1946 I heard about the founding of the Brookhaven National Laboratory; it was to be a center for all sciences involving the nucleus and it was to build equipment so big that single universities could not afford it. Stan Livingston who, with Ernest Lawrence had built the first cyclotron, was to head the accelerator group. He wanted to build the largest cyclotron ever seen for 750 MeV but he was overruled by I.I. Rabi who decreed that Brookhaven's first accelerator should surpass a billion volts. This meant that it had to be a proton synchrotron—a machine that presented many unsolved problems. All of this was very exciting for me and I decided to leave GE and get in on the founding of the new Laboratory. So I departed Schenectady and moved to Long Island. Unfortunately the 70 MeV synchrotron at GE wasn't finished yet, so I missed the first eyeballing of synchrotron radiation.

The Cosmotron, as we named our prospective proton synchrotron, presented rather terrifying problems associated with size and shape. Electron synchrotrons like McMillan's 300 MeV machine involved relatively small magnets arranged around a circular orbit about six feet across. His vacuum chamber was a toroid with an aperture of a couple of inches in the radial direction and about one inch high. But our energy was ten times higher—we chose to aim for three billion volts—and protons are not bent into circular orbits as easily as are electrons; our orbit would be at least sixty feet in diameter. How much space needed to be provided in the vacuum chamber for proton excursions from the equilibrium orbit restrained only by weak focusing? Remember, strong focusing had yet to be invented.

The problem of aperture also was faced at Berkeley at Ernest Lawrence's Radiation Laboratory, which had been approved at the same time as we were for a machine twice as big as ours to be called the "Bevatron." Estimates of necessary aperture at Berkeley ran from one by four feet to four by fourteen feet. The Bevatron finally was built so that the aperture could be four by fourteen feet, but pole pieces could be inserted if it turned out that all that space wasn't needed. As a check on these guesses, they built a quarter-scale model of their machine with shutters that could be closed down to see at what aperture the beam disappeared.

Our approach was quite different. Our little group included two very bright young theorists, Ernest Courant and Nelson Blachman. They worked out the details of the proton orbits, taking into account the achievable vacuum and all effects of component misalignments, and concluded that a gap nine inches high and thirty-six inches wide would be quite adequate. With considerable daring, we decided to disregard the Berkeley guesses and build as we were told by Courant and Blachman. After deep thought, I proposed the magnet structure shown in Fig. 1. The final shape of the gap was the result of magnet model measurements and many hours of tedious calculation. We tried to get help from a "computing bureau" in New York that consisted of twenty people with mechanical adding machines, but they were virtually useless and the complex relaxation calculations of field patterns in the end were done by Hildred Blewett.

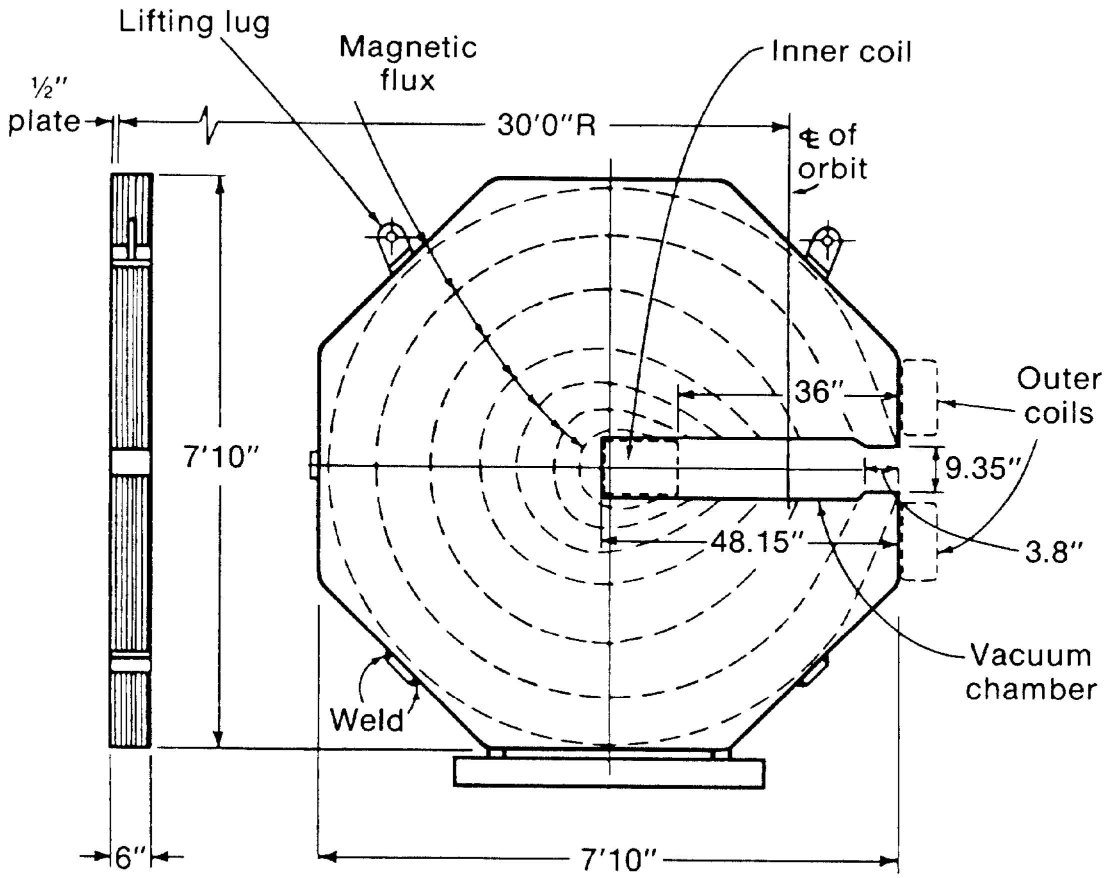

Figure 1. Cross section of the Cosmotron magnet

There were a host of other problems to be solved in the first proton synchrotrons. With injection at 4 million volts from High Voltage Engineering's first Van de Graaff machine, the proton velocity is less than one tenth of the final velocity—which is quite close to the velocity of light. So the radio frequency accelerating field must change in frequency by more than a factor of ten and the accelerating cavity had to be tuned over that range. For this purpose, we introduced to the United States the ferromagnetic ferrites that had just been discovered at the Philips Laboratories in the Netherlands.

We wanted very badly to be first in the world to a billion volts, but we were terrified that the mighty Berkeley powerhouse of talent and experience would beat us to it. Then, in 1949, the Berkeley quarter-scale model was completed and tests on it showed that the beam was lost at an aperture larger than we were building into our 2000-ton magnet. Today it is evident that the model did not meet the mechanical tolerances that we built into the Cosmotron, but at the time we were very scared.

Then Fortune befriended us. At Berkeley and Livermore a huge, very secret project was started aimed at production of fissionable material for the national defense program. The whole Bevatron staff was transferred to this project for two years and we were given the chance to be first to a billion volts. We made it in May of 1952. It was a glorious victory; it was indeed a critical victory. If the Cosmotron had failed, Brookhaven's splendid program in high energy physics might never have been started.

Consequently, I feel that the Cosmotron was about the most important project in my career.

That summer of 1952 we heard about the organization of the CERN Laboratory, to be a new European center for high-energy physics. They were well impressed by the design of the Cosmotron and planned to build a scaled-up version of our machine to reach 10 billion volts. We learned that they would visit us during the summer of 1952 to ask our advice about a variety of problems. Livingston was anxious to be of all assistance possible and he suggested a design change to reach higher fields in our magnet. The Cosmotron magnetic field pattern above about 13 kilogauss was affected by saturation effects so that the field gradient increased to levels where weak focusing was no longer possible. Livingston suggested placing some magnet returns outside the orbit so that the high gradients would average out to the low gradient required. He asked Ernest Courant to analyze the effects of alternating gradients on the orbits and to Ernest's surprise, he found that the focusing was much improved when the gradients were such as alternately to focus and defocus the beam. The higher the gradients, the better was the focusing strength. Hartland Snyder came by and reminded us of the optical analog: the combination of a focusing lens and a defocusing lens of equal strength is focusing, no matter which comes first. Thus was strong focusing born. Immediately it was evident to me that this was the solution to the fundamentally defocusing accelerating fields in linear accelerators. All that is necessary is to include alternating focusing and defocusing quadrupoles—either electrostatic or magnetic—and proton or ion linear accelerators of high intensity become possible. Our papers on the strong focusing synchrotron and focusing in linear accelerators appeared in *The Physical Review* in December of 1952.

The visitors from CERN—Odd Dahl, Frank Goward and Rolf Wideröe—appeared shortly after this discovery. We told them our story; they lost no time in abandoning their 10-GeV Cosmotron and raised their sights to a new strong focusing machine in the energy range of 30 GeV—almost exactly the machine which we now had in mind. Also, they invited Ernest Courant, Hildred Blewett and me to come to Europe and help organize CERN's proton synchrotron effort. Hildred and I accepted, and early in 1953 we found ourselves located in Bergen, Norway where Odd Dahl, the head of CERN's PS group, lived as head of the Mikelsens Institute. We soon made friends with a promising young Norwegian protege of Dahl's—Kjell Johnsen—and found ourselves established in the Physics Department of Bergen University. They had just built a betatron and had some magnet laminations left over, so we very quickly built a small model of a strong focusing, high-gradient magnet that provided the basis for the first CERN magnet design. But the most important project we undertook for CERN was to persuade them that PS design projects scattered all over Europe would make no real progress until all were assembled in Geneva, the final site of CERN. In September of 1953 at last we all moved to Geneva. A month later the CERN group was subjected to a public examination by accelerator experts from all over the world. Though suffering from beginners' terrors, they passed the examination with flying colors. In 1954 we came home, confident that CERN was headed for a brilliant future. We indeed were justified.

I note in passing that confidence in strong focusing was not universal. We, CERN and Bob Wilson at Cornell proceeded with strong focusing machines. But, even after our announcement of the new method, old fashioned weak focusing machines were built at Argonne (the ZGS), at Princeton (the Princeton-Penn accelerator), and at the Rutherford Laboratory (Nimrod).

We returned to find Brookhaven's AGS well underway. While we were at CERN, BNL had decided to build an "Electron Analog" to test our ability to pass through the "phase transition"—a sudden phase shift that hadn't existed in the Cosmotron, but would be necessary in the AGS. I took a dim view of this project. Kjell Johnsen had analyzed the problem and concluded that it would be easily solved. Anyway, the analog worked as Johnsen had predicted and people no longer feared the phase transition.

The AGS and the CERN PS worked as expected and, indeed, are still working after over 30 years. Meantime a group of us turned our attention to the next big accelerator. Official Washington decided that this was to be for 200 GeV, that it was to be designed at the University of California, that the Atomic Energy Commission would conduct a site search and that Brookhaven should study future machines for up to 1000 GeV. After a fair amount of administrative foolishness and disagreement within the high energy physics community, the Fermilab emerged to become the distinguished center that it now is.

Meantime at Brookhaven, Luke Yuan and I organized a number of studies of "super energy" machines and raised questions with the high energy physics community about what they wanted in the future. In 1963 at a Brookhaven summer study, we could find only three experts who favored colliding beams. Under pressure from the Columbia group headed by Leon Lederman, it was decided that colliding beams should be left to CERN. Brookhaven should concentrate on souping up the intensity of the AGS for improved studies on neutrinos.

Eventually, we came to a design for a new colliding beam machine, known as ISABELLE—ISA for "intersecting storage accelerators" and BELLE for good measure. ISABELLE lived long enough for us to construct a two-mile tunnel, now about to house RHIC, a heavy ion colliding beam ring. I think it fair to suggest that ISABELLE was an important stepping stone on the way to the SSC.

During President Carter's regime, I organized a program at Brookhaven for development of energy sources, but eventually I found my way back to accelerators and formed an alliance with Ken Green, Rena Chasman and Jules Godel to start a project for a synchrotron light source for Brookhaven. By now it had become evident that synchrotron radiation could be useful in a great number of fields. The pioneers in this area were at Cornell and at the University of Wisconsin, where the remains of MURA (Midwestern Universities Research Association) had refused to die and had begun to exploit the radiation from one of MURA's electron models. Now it seemed appropriate for Brookhaven to provide a "National Synchrotron Light Source." I edited the proposal and Ken and Rena provided the basic design. We were quickly approved and, as I am sure you know, the project has been a great success with, by now, over two thousand users from universities, industry and other institutions.

Now, light sources are springing up world wide. I am particularly attached to a 1.3 GeV ring in Taiwan having been a member of its Technical Review Committee since 1984. It is a beautiful machine and is just now coming into operation. Its staff reminds me of Cosmotron days when a collection of beginners gradually turned into professional accelerator builders.

I conclude with a mention of a graph which I evolved around 1960 of logarithm of achieved accelerator energy against date. This proved to be a reasonably straight line indicating an increase of energy by a factor of ten every six years. Stan Livingston and I wrote a book about accelerators in 1962 in which we published this graph. It is interesting to see how this plot looks at present. In adding points for colliding beams I use the equivalent energy required for a static target. The linearity of the graph persists quite nicely—still a factor of ten every six years. Drawing the line as carefully as I can, it seems to predict that the LHC will be completed around the year 1998, while the SSC is not due until the year 2003.