IEEE Transactions on Nuclear Science, Vol. NS-28, No. 3, June 1981

EARLY DAYS AT THE RADIATION LABORATORY

William M. Brobeck
William M. Brobeck & Associates
1235 Tenth Street
Berkeley, CA 94710

This year is considered the fiftieth anniversary of the Lawrence Radiation Laboratory at Berkeley, the birthplace of the cyclotron and its line of amazing successors. The Lawrence Laboratory is also recognized as the place in which a particular type of teamwork appeared and in fact was found necessary to develop and exploit the capabilities of large scientific machines. I will talk about the Lab as I saw it from the standpoint of a non-scientist but an enthusiastic machine builder, one of a group that today is represented by this conference. I will talk about the years 1937 to 1939, from the time I joined the Lab until the beginning of the war work. What went on before 1937 can best be told by others and what went on after 1939 is a fascinating but different story which has already been well told.

The Laboratory, when I arrived, was very much a going concern. It was housed in an old wooden building on the Berkeley campus which had the valuable feature that no one objected if you drove a nail into it anywhere. Surrounded by the building on three sides was a courtyard full of high voltage equipment that Marconi would have recognized. The tremendous magnet of the 27-inch cyclotron was a gift from the manufacturer of then-obsolete Poulsen arc radio transmitters. The magnet carried an impressive brass plate stating that its power output was 1000 kilowatts. The megawatt had not yet been invented.

The Laboratory staff was about 25 people, mostly graduate students. Alvarez, Kurie, Livingood, McMillan and Cooksey were the middle management with, of course, Ernest Lawrence very much in charge.

Although my knowledge of nuclear physics was nil, it was obvious that scientific knowledge was pouring in. On one wall was hung a huge isotope chart with brass hooks in each space for an active isotope. The properties of the isotopes were written on tags which were hung on the hooks and changed whenever new data appeared. Most of the tags showed properties that had been measured right there in the Laboratory, and the rate that they were being added and filled out was a graphic display of the progress of science.

The distinction between machine users and builders had not yet been recognized. Almost everyone was a machine user and a builder also, but he only built when it was absolutely necessary. The 27-inch machine was apparently capable of doing plenty of physics, but its reliability was woeful. Fortunately from the standpoint of machine progress, Lawrence was primarily a builder. In fact, during that period he was not a user at all. Although he was vastly interested in the physics being done, and I am sure a great inspiration to the other scientists, I don't remember his working on any of the experiments. On the other hand, he was close to everything that went on about the machine and was continually pushing improvements and planning larger cyclotrons.

Donald Cooksey, an associate of Lawrence from his Yale days, was the other principle builder. Don kept a drawing board in his office on which the more complicated improvements in the machine took shape. At the time I came to the Lab, the 27-inch "tank" was about to be replaced by the 37-inch one. A major innovation of the new tank was the use of rubber gaskets. The 27-inch was sealed by wax in the classical "string and sealing wax" style. With such a large vessel made of separate pieces, the brittle wax was highly unsatisfactory. The frequency of leaks was indicated by the presence on the crew card - one was filled out for each shift - of a printed diagram of the tank on which the location of the leak currently plaguing the crew would be marked. Leaks were found by squirting illuminating gas at the wax and watching current of the ionization gauge - a radio-tube to which a tubulation had been attached by the glass blower. If all else failed, one would "flame the wax" which consisted of melting the surface over the whole tank in the hope of sealing the leak.

I was fascinated by the control desk and the array of meters and knobs, only approached in complication today by the instrument board of a Japanese sports car. The control desk had been a kitchen table to which instruments had apparently been attached one by one. The magnet was powered from a campus-wide DC system through a large switchboard at the side of the control desk. When the magnet was to be shut down, the operator tripped a circuit breaker at the top of the switchboard after first carefully crouching as low as possible to keep his hair out of the arc. Visitors were shown a terminal lug in the magnet circuit in which the solder was always liquid while the magnet was on.

Soon after my arrival, the campus DC connection was replaced by a World War I motor generator set. The starter for this equipment had a large lever which was pushed forward to close the main oil circuit breaker for the motor. One night, when the operator pushed this lever, all the lights on the campus went out. A few hours later, after power was restored, he did it again; and this time the lights went out all over Berkeley. The motor generator set was soon replaced by another with a more modern starter.

My first work at the Laboratory was to help Luis Alvarez on an experiment to look for negative protons, developing film taken of a cloud chamber close to the target. Once when the beam came on by accident while I was standing beside the chamber, I got a good idea what radiation was. The chamber and I were solidly filled with knock-on protons.

At the time of the changeover to the 37-inch tank, a second cyclotron was being planned for a new radiomedical laboratory, the Crocker Laboratory next door. The building construction had started and steel for a 50-inch cyclotron magnet was on order. Pumping down the 37-inch went poorly. For the first week the pressure stayed high, and the atmosphere was one of deep gloom. There was talk of pulling out the gaskets and going back to wax. However, the pumping continued and during the second week the vacuum and spirits reached new highs. In fact everything looked so good that Lawrence decided to increase the 50-inch Crocker cyclotron to 60 inches! This was my first encounter with the suddenness with which plans could change at UCRL.

Operation of the cyclotron was a chore which all the physicists accepted whether or not it was being operated for their own experiment. This was perhaps the unusual organizational feature that led to accelerator laboratories becoming noted for scientific "teams." Each week a crew schedule was made out covering the

0018-9499/81/0600-2004$00.75©1981 IEEE

seven days from 8 a.m. to midnight in two shifts. The crew generally consisted of two people who did their best to keep the machine running and to meet the "bombardment schedule." When the machine was working, operation was quite boring. One night I brought a visitor to the Laboratory who was obviously in awe of the apparatus. We found the wife of one of the students not only operating the cyclotron but knitting at the same time. In other situations things were much more hectic. Improper operation, particularly forcing the radio frequency or ion source power too high, could cause much trouble. Good operating adjustments, the information now collected by a computer, were noted on the blanks on the crew cards. That is, except when Lawrence had been running the machine, when the crew card frequently read "EOL operating - all meters off scale."

Often the cause of trouble was not at all obvious. There were always plenty of theories to proceed on but the actual cause, when it was found, usually turned out to be not very subtle. Once when the RF voltage strangely dropped to zero every few seconds and immediately recovered, the explanation was that each time the RF went off a drop of molten copper fell from a dee feeler.

Because the cyclotron was such an effective instrument for nuclear physics, there was a strong tendency to keep the machine running at all costs by patching and improvising to get back "on the air" in the shortest possible time after a failure. As a result there were many marginal conditions and systems just on the point of failure. As a matter of necessity, "clean-up" periods had to be scheduled to remove the accumulated "haywire," and if possible, improve reliability. There were too many things that the operator had to remember which if forgotten would cause much lost time. One such system was the magnet oil cooling. When shutting down for the night, a valve had to be closed to prevent the oil in the top tank from flowing into the bottom tank and hence overflowing into the basement where it drenched the deflector high voltage supply and other equipment, not to mention the loss of oil. Although sparks jumped between any closely spaced metal parts anywhere in the building when the RF was on, all this oil was not considered a particular fire hazard. Various "Rube Goldberg" mechanisms had been invented and applied to close the oil valve automatically, but as soon as one was relied on it failed. A proper fix included cutting a slot in the oil tank over which the oil could flow, removing the need for a valve. This was done. Sawing the slot was an arduous and unpleasant job. Each of the crew sawed until he got too tired and then turned it over to the next. What impressed me was that Lawrence took his turn sawing like everyone else. This magnet tank with the slot is now standing outside the Lawrence Hall of Science high in the Berkeley hills. Visitors do not realize that it represents not only the intellectual accomplishment of Lawrence but the manual as well.

During this period the physics department held a "Journal Club" on Monday evenings at which recently published papers and current Laboratory results were presented and discussed. I attended these meetings in the hope of picking up some understanding of the scientific work going on. I remember particularly Robert Oppenheimer in his corduroy coat and pork-pie hat. His talks were particularly unintelligible to me as could be expected. I could, however, understand his mannerisms which included the sound "huh" every few seconds. At the end of his explanations which were largely mathematical, I had the hope of understanding what he was saying when he came to his conclusion. I was always frustrated, however, because he always dropped his voice to the point of inaudibility when he came to stating the conclusion of his talk, presumably because restating what he had already proved would insult our intelligence. Another remembrance was of a talk by Abelson on the results of bombarding uranium with neutrons. He could make nothing out of the activities produced - fission had not yet been discovered.

After each journal club meeting, Lawrence would hold a stand-up meeting of the cyclotron crew members to discuss the coming week's work. At one of these sessions, I suggested that areas of responsibilities be assigned to the various people to become better organized. Lawrence explained that "everyone was responsible for everything" - a principle that I was later to find was highly successful.

The sixty-inch cyclotron took advantage of the experience with the earlier machines and hence was much more reliable. Still it had its problems. The oscillator in particular was very tempermental. The power tubes, which were made at the laboratory, were enclosed in a large aluminum house with a door large enough to walk through. The door was important because the oscillator wouldn't work if it were closed. Furthermore, if you stood in front of the door and your ankles got hot, you knew that the oscillator was on a particular parasitic at which you were a half wave antenna with your ankles at the current node.

The 60-inch cyclotron control system designed by McMillan was a model of convenience which I believe has hardly been equalled since. All the auxillaries were turned on with a single pushbutton, a stepping switch brought them on one at a time to avoid too great a surge on the AC line. The high voltages were then separately turned on and adjusted by the operator. The beam could be produced in less than a minute from the time the shift started, something that took hours in later years.

One of the striking things about those days was the lack of application of physics to the design of the cyclotron. The resonance requirement was of course recognized and magnetic focussing was known to occur in the "fringing" field, but in 1937 there was very little knowledge of the beam dynamics of the rest of the machine. The reason was probably that the cyclotron worked well enough when it was working at all. The development effort went into keeping it working and removing the causes of breakdowns rather than into basic improvements.

Cyclotrons in those days were built with air gaps between the iron vacuum chamber heads and the magnet poles. Metal plates could be inserted in those gaps to change the shape of the magnetic field. The object was to make the field uniform and to the extent that it was accomplished, magnetic focussing in the accelerating region was lost. Beam energy was determined by observing the range of the beam in air after passing through a thin foil. Often the range was short or there could be several energy components at the same time resulting in a stepped beam. This was corrected by trying various sizes and shapes of "shims" in the air gaps. This "shimming" was an important part of "snouting," the process of obtaining a useful external beam.

At the laboratory the first serious investigation of beam dynamics to my knowledge was made by Robert Wilson - then a graduate student. McMillan had suggested and built a "circular shim" consisting of a stack of concentric disks to be used on the 60-inch machine when it was first turned on. This was considered a rather daring, if not naive, reliance on theory in a subject that had a long tradition of "cut and try."

In spite of the insecure basis in theory, Lawrence was energetically promoting a "great" cyclotron with an energy of a hundred or more MeV's. As there was no room for such a large machine on the campus, sites were investigated on university property near the bay and in the hills. Above the big letter C on the hillside was a knoll which provided enough flat area for the circular building planned and had satisfactory soil conditions. With support from the Rockefeller foundation and others, design of the 184-inch cyclotron was begun. The policy of naming the machine after a dimension rather than a energy, to avoid possible future embarrassment, was continued. 184 inches was chosen because the price of steel plates, pairs of which formed the vertical legs of the magnet, increased above 92 inches.

While Wilson was working on his calculations, Bethe's paper on cyclotron beam dynamics appeared with the concluding statement that due to relativistic effects cyclotron energy would be limited to about 25 MeV. This, while the 100 MeV machine was being designed! The first reaction, which lasted only briefly, was that Bethe was wrong. The design of the great cyclotron was changed to 100 MeV deuterons with 1 million volts dee-to-ground. The background of 1 million volt technology was the million volt X-ray tube then in operation at the University of California Hospital in San Francisco, designed and built by Sloan and Lawrence before the cyclotron. The fact that the X-ray tube target was only about a square inch in area compared to some 40,000 square inches of the cyclotron dees would have caused an unpleasant surprise if the design had been built. Nevertheless work proceeded until the war effort intervened and the cyclotron magnet, with a six-foot air gap, was diverted to uranium separation.

Frequency modulation as a means of avoiding the relativistic limitation was discussed at the time but dismissed as impractical because it was impossible to vary the frequency fast enough to keep up with the acceleration of the ions. Apparently no one thought to investigate what would happen if the acceleration occurred much more slowly until Veksler and McMillan looked into the question five years later.

At about this time the paper by Thomas on what is now known as the sector-focussed cyclotron appeared. This was another possible solution, also dismissed at Berkeley because it required azimuthal variations in the magnet that were inconceivable to those who had spent years trying to improve field uniformity. Both frequency modulation and sector-focussing were ideas whose time had not yet come. It may be significant that it required the interruption by the years of war work to remove the restraints in thinking which prevented early recognition of what have proved to be practical improvements. If the war had not occurred, necessity would probably have brought these developments to life much sooner.

During those days the radiation laboratory was the center of cyclotron development world wide. There were usually physicists from foreign countries visiting to learn the lore so as to build their own machines at home. The Japanese were partuclarly interested. A copy of the 27-inch cyclotron had been built in Japan, complete with errors and their corrections that were part of the Berkeley machine. Lawrence always defended the Japanese policy of exact copying as a way of entering a new field. The results in later years certainly proved it correct. A famous picture in Life magazine of the exact copy of the 60-inch cyclotron disappearing below the surface of Tokyo Bay after the war gave all of us who worked on the 60-inch a very strange feeling.

At the end of the period I have been describing, the 37-inch machine, still in its prime, was shut down and converted in a few days to a mass separator for uranium. The 60-inch machine was in use in an active radio-medical program as well as on isotope production mostly for medical purposes. Construction of the 184-inch magnet was under way - it had been started before the building which was built around it - to become the center of the uranium separation process development. The enthusiasm and intensive effort shifted rapidly from pure science to the winning of the war.