EARLY ACCELERATORS AND THEIR BUILDERS*

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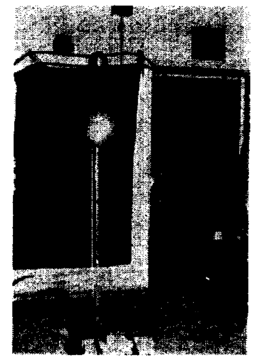
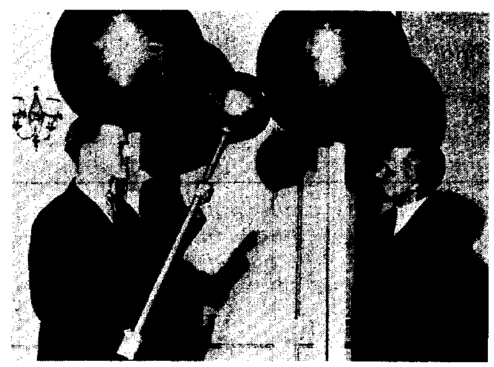
I would like to begin by explaining that what I am going to give is not an address, it is a slide show. I started with the idea of covering accelerators and their builders and soon, realizing the time limits, I had to limit it to early accelerators, and then I had to pretty well leave out accelerators and just concentrate on builders, and then I realized that there wasn't time to say very much, so I just put in a whole lot of slides, mostly involving people. There are a few actual pictures of machines in here which got in by accident. Actually what happened was, I got in about as many slides as I thought I could show, but they didn't quite fill the box I had, so I shoved in a few more just so they wouldn't rattle around in the box. That's how the accelerators got in.

The inception of the idea of accelerators for nuclear purposes I would put around 1930, centering around 1930; take a few years before and a few after, and you'll find that most of the ideas, the basic methods of accelerating, started in that period. The basic types of accelerators might be classified as first, the simplest, those that accelerate simply by application of an electrical potential difference, so that an ion falls through a certain potential difference and acquires a certain energy; then there is the resonance type of accelerator in which one successively applies an impulse which is properly timed; and the third general type is the induction accelerator which works something like a transformer, typified by the betatron, or the linear induction accelerator designed by Christofilos. I'm afraid I am going to slight the induction accelerators. I take a somewhat personal point of view here; namely, I will speak about the things that I have been most closely in contact with. I also want to make a couple of apologies. One is to those people who got left out. Among the slides I went through to get these together, I'm afraid that I didn't get everybody in that I should have. The other apology is for the fact that I, myself, appear in a number of these slides. Usually I have been very shy about showing pictures of myself, but this time I forgot all my shyness, so you will see some pictures of me.

I'd like to start with my first contact, my first knowing contact with the idea of accelerators, which occurred when I went to Princeton as a graduate student in 1929. Van de Graaff was already working on the belt-type electrostatic machine with the idea of accelerating particles, and this first slide is one of the earliest models which he built. (*Slide 1*) I remember seeing that model. In fact, that picture is one that I brought with me when I left Princeton. I don't think I took it but somebody gave it to me. That was the very simple beginning of the belt-type electrostatic machine. I have another slide showing the next-size model, and the man on the left is Van de Graaff and the one on the right is Karl Compton, who was the head of the Physics Department at Princeton when I went there. (*Slide 2*)



Slide 1



Slide 2

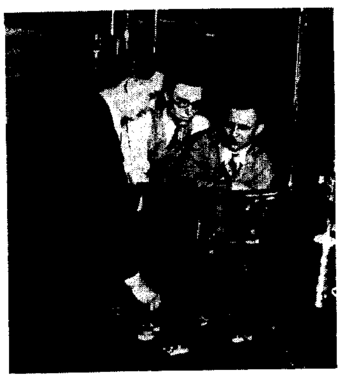
I'd like to tell a little story about Van de Graaff, how Van de Graaff became interested in high voltages. Van de Graaff, I might say, for those who didn't meet him, was a completely charming man, one of the nicest people I have ever known. It was a great pleasure to me to spend time listening to him tell these stories about how he got started. He grew up in Alabama and as a boy took summer jobs, and one summer he took a job at a power plant in a place called Bug Tussle. He said that the inhabitants of the city thought that was an undignified name and it's now changed to Warrior, and if you look on the map of Alabama, there is a Warrior. While working at that plant, he became fascinated by power. As a young boy, power just took over his mind, and so he started designing a complete power system, using electrostatic machines. He showed me the drawings he made. He calculated efficiencies, he had electrostatic generators, transmission lines which were to be coaxial evacuated lines running at a million volts, and electrostatic motors. This particular thing never came to fruition, but it is what led him into the idea of static machines, and then he was guided into designing these for nuclear physics and was started in this line of development.

Carrying on that line, the next figure is Merle Tuve, and you'll find out that Merle was associated with Lawrence, but this will appear later. Actually, in those days it also helped to be Scandinavian. Scandinavians played a big part. I have a picture of Merle Tuve as a young man in South Dakota, and for some purpose that I don't know, he is baptizing that car. (*Slide 3*) The next slide shows the team, Tuve, Hafstad and Dahl, who were the first ones to adapt the Van de

*Dinner address, March 6, 1973

Graaff machine to do nuclear experiments. (*Slide 4*) Van de Graaff, himself, went on to MIT and later was a founder of the High Voltage Engineering Corporation, in the business of making accelerators. But the physics development was carried on by many others and Tuve was the first one. Here is a more modern picture of Merle Tuve, who was one of the real pioneers in this field. (*Slide 5*)

Slide 3

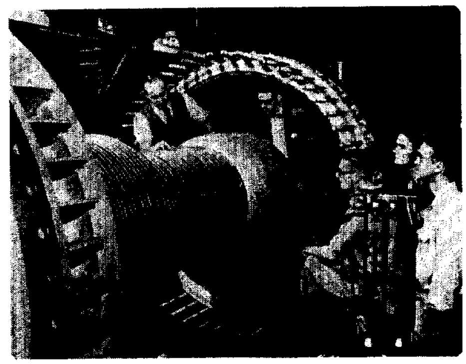


Slide 4

Slide 5

The next important development in the Van de Graaff art was the pressurized Van de Graaff. Of course, the limitation of a machine operating in atmospheric air is simply that it gets too big when you go to high voltage, so Van de Graaff himself and some of the men who came to work with him at Princeton tried running machines at vacuum and pressure, but for various technical reasons did not succeed. The first success was achieved by Herb, Parkinson and Kerst at Wisconsin. I have this picture of Herb, that is Ray Herb on the left; he is present here tonight. (*Slide 6*) When I got this picture, I thought maybe the third man there was Don Kerst, which was going to be my representation of the induction accelerator, but Herb tells me it's not. He said this picture was taken after Parkinson and Kerst had left, and so all those who are lovers of induction accelerators will just have to be sorry that I didn't get Don Kerst in. The man on the right, Al Hanson, is here. Neither he nor Herb could identify the others. The next slide shows Herb at the controls. (*Slide 7*)

Now I go back to another line. It's very hard to tell these things in a logical order because you have several lines starting up almost simultaneously, all starting around 1930 in different places by different methods, so I just have to mix things a little bit. But I cannot let this go without Charlie Lauritsen; it's a late picture. (*Slide 8*) Lauritsen had started in the



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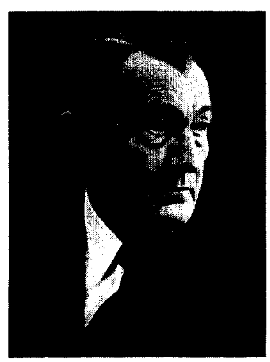
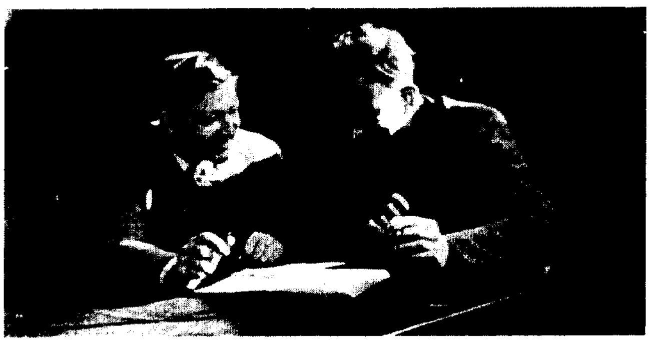


Slide 8

late '20s in Pasadena working with a high voltage transformer. It happened that the engineering department at Cal Tech had a million volt transformer outfit they built for testing transmission line components. And using that, Lauritsen started a very extensive development of high voltage accelerating tubes. I should say that at that time getting high voltage was really no problem; a million volts was technically easy. The problem was building a vacuum tube on which you would apply this voltage to accelerate a particle.

Lauritsen was carrying on that development in Pasadena, and in England Cockcroft and Walton were carrying on their development using a rectifier voltage multiplier outfit, and I have a picture of Cockcroft. (*Slide 9*) Cockcroft is on the left; on the right is George Gamow. Gamow was not an accelerator builder, but at that time Gamow had developed his theory of barrier penetration, and it was on the basis of that that he encouraged Cockcroft and Walton to go ahead and try their machine, although they had thought that the voltage they had was too low to do a nuclear experiment. They tried it with a few hundred kilovolts and of course, got the first disintegration. There were other people in the world at that time who could have done it, who had enough voltage, but they were the ones who had the credit for doing it first. I have here a later picture of Cockcroft, who became a great figure in British science in his later years. (*Slide 10*)



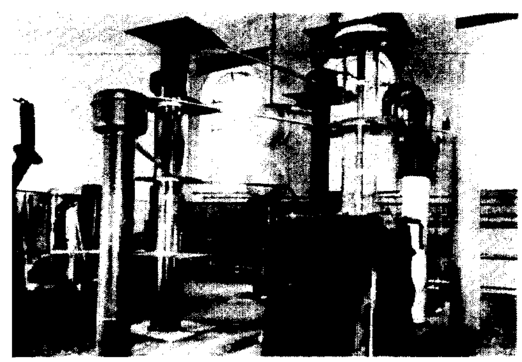
Slide 9

Slide 10

I also have one of Walton, the only picture I have of him, taken at the Berkeley Laboratory a few years ago, when he was visiting. (*Slide 11*) That's outside my office looking down over Berkeley and that's Walton on the left with Mrs. Walton. He is living in Dublin and is retired as far as I know, but probably he is still thinking about ingenious devices. He thought of a lot of other things besides the machine to which his name is attached. I'll close this particular sequence with the original Cockcroft-Walton machine on which the first disintegration experiments were done. (*Slide 12*) The covered cubicle on the lower right is the place where an observer sat in the dark counting scintillations visually; they used scintillation counters then! The scintillation counter had to be reinvented. It was used for early experiments and then forgotten, or considered sort of old fashioned, until it was reinvented in a modern form, with the help of photomultiplier tubes.

That closes the particular skein of thought which I was going to carry out. Now I have to go back in time into the next skein, and that was the resonance accelerator. The earliest idea for a resonance accelerator of which I am aware is an idea which occurred to a man named Ising, who by the way, for any solid state people who may be here, is not the same as the Ising with the magnet model. This Ising (whom I met in Stockholm) is a pleasant white-haired gentleman who runs a private laboratory outside of Stockholm and apparently invents things and tries them out. He had the idea published in 1924. I don't know how to get a picture of him so I represent that development by a drawing from his paper, showing the basic idea of a resonance accelerator in a linear form, in which a series of cylindrical electrodes with ions going down the axis is fed by a spark through lines of different lengths, so that the pulse arrives in the proper sequence at the different electrodes. (*Slide 13*) Namely, sort of a one-shot linear accelerator.

Slide 11



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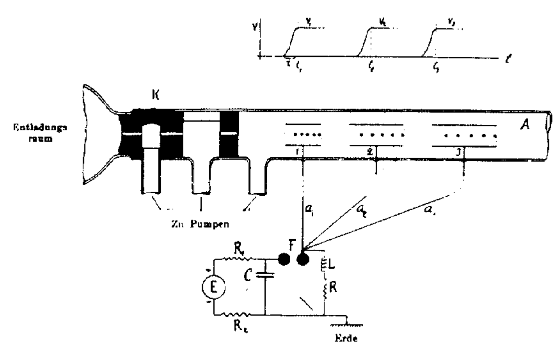


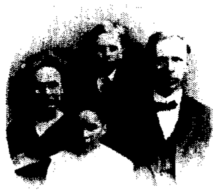
Slide 13

Ising did not succeed in making this work. Then, a few years later in 1928, another Scandinavian, Wideröe, who is a Norwegian, working in Aachen in Germany for his doctor's thesis in electrical engineering, picked up the idea of Ising and built a two-stage linear accelerator using two gaps and doubling the voltage. The voltage wasn't great; it was something like 20 kilovolts, I think. The ions were potassium ions. But it did work and I do have a picture of Wideröe which was taken in 1961 at a conference in Berkeley. (*Slide 14*) The posing of that picture shows a lamp over his head, which means he is having an idea. He is another one of those people who is a great inventor. He was with Brown-Boveri in Switzerland, he probably still is, he's invented many things, and is still active.

Wideröe published his thesis in the Archiv für Elektrotechnik, which Ernest Lawrence saw in the University of California library. Ernest said his German wasn't very good, but the pictures were self-explanatory and he picked up this idea and very quickly started a two-pronged approach. One was to build linear accelerators with more than two gaps, and the other was to curl it up into a circle with a magnetic field, using the fact that the angular velocity of ions in a magnetic field is a constant independent of energy.

Lawrence, of course, not only made this invention, but carried it out extremely effectively over many stages in the Laboratory and, in my mind, is without a doubt the greatest figure in this field. So I put in several pictures of Lawrence. (*Slide 15*) The one in the back is Ernest Lawrence, with his mother and father and his younger brother, John. John was the Director of Donner Laboratory and is now a Regent of the University of California. Here is a later picture; the one in the middle is Ernest, John is on the right, and the other is a family friend. (*Slide 16*) This was taken in South Dakota, where he grew up. At the same time, Merle Tuve was growing up in the same town and they were going to high school together. Next is a picture taken in 1930

Slide 14



Slide 15

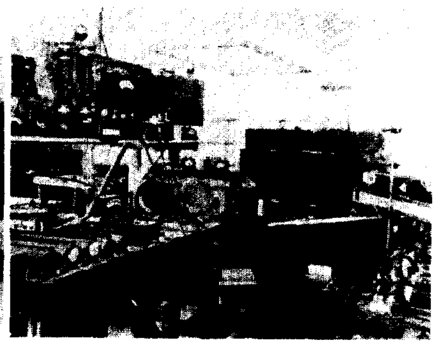
Slide 16

at a meeting of the National Academy of Sciences in Berkeley, when he had just announced the cyclotron idea which he had invented late in 1929, and that is one of the first models in his hand. (*Slide 17*)

This is one of those apparatus pictures that got in just to fill the box; that's the 11-inch cyclotron. (*Slide 18*) That was the first of the cyclotron series with which actual experiments were done, which was used to repeat the Cockcroft-Walton results. As soon as they were announced it was possible to go ahead and verify them.

Here is Ernest Lawrence at the controls of the next cyclotron, the 27-inch. (*Slide 19*) I don't have time to go into details, but maybe I can show you some of the people. That's Ernest Lawrence sitting at the control table. See that scale, it shows the spot from the galvanometer, indicating the type of currents one was dealing with then. These currents were microamperes, and he measured them on a galvanometer. Next is the 27-inch cyclotron from the oscillator side, showing Lawrence and Don Cooksey, who is now retired but who had a very important role as general helper during that time in getting things done around the Lab. (*Slide 20*)

Slide 17



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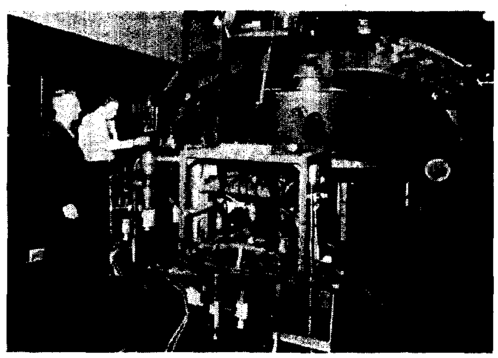
Slide 19

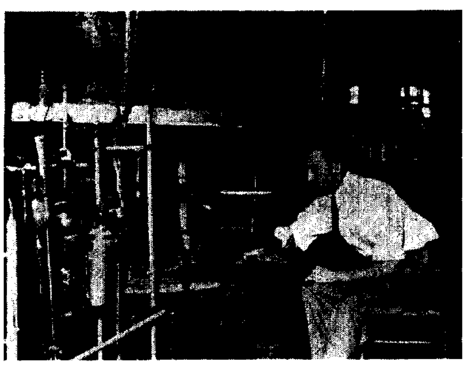


Slide 20

This is the first extracted beam, the first beam that was taken clear away from the cyclotron, and which, if we had had strong focusing (which we didn't), could have been led to any distance. (*Slide 21*) That was on the 37-inch cyclotron. The date of that is 1937. The beam was let out into the air through a thin window and you see the bright glow. I think nowadays the safety people wouldn't let you do that, but we did it all the time, then.

Here I have Stan Livingston, who is with us at this Conference, who was with Lawrence through this early period, through all these early periods, and whose contribution to the success of the early cyclotron was very, very important. (*Slide 22*) I will refer to him again later on. Now I have a miscellany of people from Berkeley. That's Dave Sloan. (*Slide 23*) Dave Sloan worked in the Laboratory; he built x-ray tubes, he designed and built oscillators and vacuum pumps. The fact that Dave Sloan was able to make such equipment in the Laboratory was one of the things that made this work possible, because commercial equivalents were very expensive or unobtainable at the time.

Slide 21



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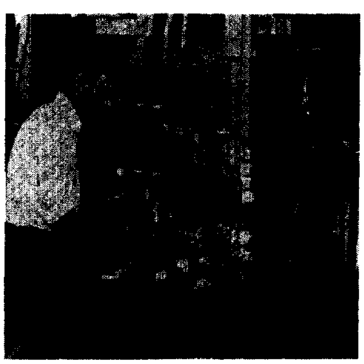
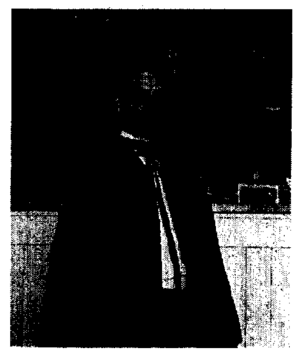


Slide 23

Next we have Bill Brobeck. (*Slide 24*) He is shown in front of the old radiation laboratory on the UC campus, with some 5-gallon cans that had been filled with water and used for shielding the 37-inch cyclotron. He was the first professional engineer at the Laboratory, and had a great influence in weaning away the physicists from their old sloppy habits of sticking everything together with sealing wax, introducing sound engineering practices in the accelerator art. And here is one of the earliest accelerator theorists, Bob Wilson, taken on the day that he received his PhD at Berkeley in 1940. (*Slide 25*) In the same year he published a very important paper on the theory of the cyclotron. It is true that Hans Bethe had preceded him by three years with a paper on the energy limit of the cyclotron, but this was only an incident in Bethe's career, so I count Wilson as the first man to make an extended effort to understand the orbit dynamics in a circular accelerator. As you all know, he has since gone on to greater things.

After the 37-inch cyclotron, the next at Berkeley was the 60-inch, and here are the dee-stem tanks for that machine. (*Slide 26*) The man on your left is

Slide 24



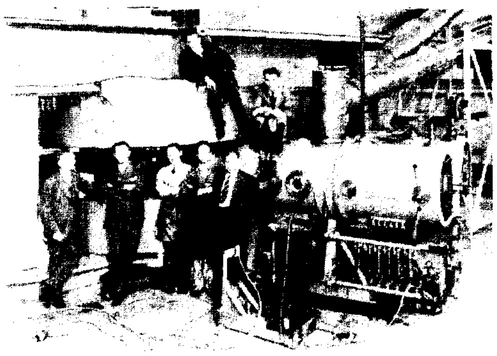
Slide 25

Slide 26

Laslett, who has now made a very great name for himself as an accelerator theorist. The man on top is Bob Thornton, who worked with cyclotrons extensively, and the one on the right is John Backus, who went to USC and I believe now does acoustics, but at that time he worked on accelerators. And here is a picture of the 60-inch cyclotron complete. (*Slide 27*) It may be that some of the missing people are in there. There's Cooksey next to Dale Corson, now President of Cornell University, the third one is Lawrence, then Thornton, Backus, Winfield Salisbury, then going up are myself and Luis Alvarez. I had hoped that maybe some of the missing people would be in that, but I'm afraid not.

Now I go on to another skein, branching off to the side, to the further development of the linear accelerator. When the cyclotron was such a great success, the linear accelerator became sort of a museum piece and nobody spent much time developing it until, during the war, the development of radar advanced high frequency techniques to the point where one could do things that couldn't have been dreamt of before. So the whole linear accelerator concept had a great reblossoming.

One of two important lines is the traveling wave linear accelerator for electrons, such as the SLAC machine, which was developed extensively both in England (I'm sorry I don't have any pictures of that) and in the United States at Stanford. I do have a picture of Bill Hansen. (*Slide 28*) Perhaps not many of you would have known him; he died many years ago, but he was one of the really great pioneers. Without Hansen we wouldn't be anywhere near where we are now. There's Bill with some sort of a wave guide structure to the left; that may be part of Mark I. One thing I do remember about that machine. Hansen was, of course, really a microwave man and interested in the accelerator itself, not particularly in experimental areas. When the machine was finished, the accelerator extended to within about 6 inches of the wall, and that space was the experimental area! What was done then was simply to knock out the wall and go on from there.

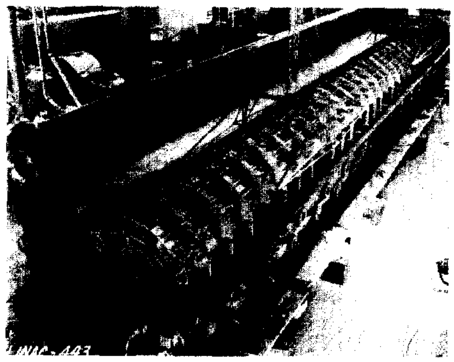


Slide 27

Slide 28

The other chief line of linear accelerators is the proton linear accelerator, which is a standing-wave type machine. It was pioneered by Alvarez. I have a picture of Alvarez here, taken during the time that the machine was being built, and Panofsky was with him. (*Slide 29*) I don't have to tell you which is which, I think. The next slide shows the original Alvarez machine with the vacuum tank open, showing the resonant cavity, which was then built as a separate structure. (*Slide 30*) This machine was the prototype of the linacs now used as injectors for proton synchrotrons, and of the heavy ion linear accelerator, the HILAC.

Slide 29



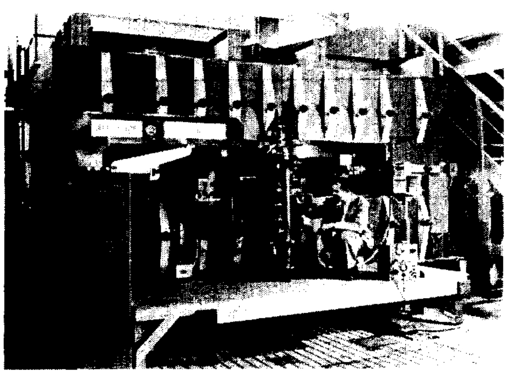
Slide 30

Now we go on to what I'll put down as the only really important new things since those early days, that period around 1930, when most of the ideas already existed. But two of them, I think, did not exist. The idea of phase stability had flickered through peoples' minds. In talking to people, I find that Ernest Lawrence said he had thought about something like that, and I think others had. But it never had been recognized as an important principle, certainly not recognized to such an extent that anyone had worked it out and published it, making it useful to the world. So I have to put in something about phase stability. And of course, as Panofsky said in the introduction to my talk, Veksler invented that in the Soviet Union and I invented it here in this country. Veksler started about a year ahead of me but the Russian journals were not coming across at that time, and it was only later that I found out what he had done. When I first saw his paper (it was published in the English language in a Soviet journal) I felt that I could have written it myself. He even used almost the same terminology. It was a remarkable convergence! Later, I had the great pleasure of getting to know Veksler, who was a very fine gentleman and a very clever man.

The next slide shows Veksler in the middle, I'm on the right, and the man on the left is an interpreter. (*Slide 31*) This was taken at a meeting in Berkeley in 1959. Veksler and I served a number of years together on the IUPAP commission on high energy physics, which sets the times and so on for the big international meetings in high energy physics. And, as I say, he was a really very fine person, and it was a great loss when he died a few years ago.

I threw in a few slides here of my own efforts in building a synchrotron at Berkeley, the Berkeley electron synchroton (I said I had some pictures of myself), and there I am. (*Slide 32*) That was the 300 MeV machine. Electrons were injected at the left and circulated inside a quartz doughnut between the magnet poles. We didn't extract the electrons, but we had an internal target which made an x-ray beam which came off more or less to the right of my head, toward you in that picture. The next slide shows how one operated the electron synchrotron at a development stage when the timing of the injector pulse was exceedingly critical. (*Slide 33*) Here I am raising the voltage, increasing the amplitude of the pulses driving the magnet with one hand and adjusting the timing of the injection with the other. We did not have computers to run accelerators then.

Slide 31



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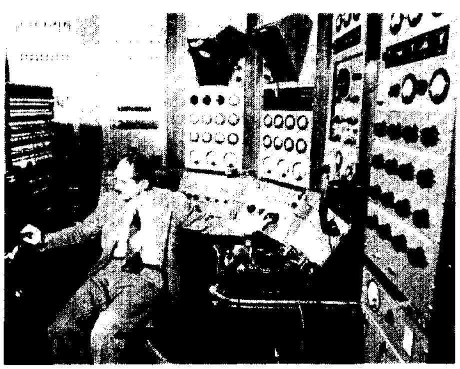
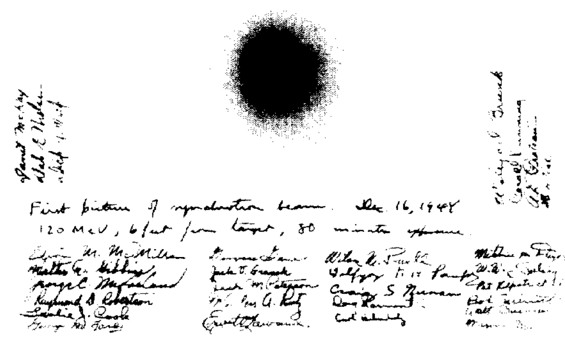


Slide 33

One more electron synchrotron picture I have here. This is just a nostalgic souvenir, such as one collects. The first time you get a beam in a machine you measure it somehow, and then you put a film in the beam (the x-ray beam) and get a spot, and everybody signs it, so I stuck that picture in (*Slide 34*). That was on December 16, 1948. That was the first time there was a picture of a beam, and the energy was 120 MeV; it wasn't up to full voltage yet. This wasn't reached until a month later, and you will note that the exposure time was 80 minutes, so the intensity wasn't very high either! Lawrence, you see, has signed it. And Panofsky is on there, and perhaps a number of other people whose names you recognize.

The other of the important new ideas which I wanted to mention was that of strong focusing. I don't know if anybody had thought of the idea of strong focusing before the individuals that I am going to mention in a minute. I don't know of any. In the case of phase stability, it is easy to approach in an intuitive, elementary way. When I first started telling people at Los Alamos (where I was when I got the idea) about it, I found that I hardly needed to finish the explanation before they understood, and at least one said that he felt stupid in not having thought of it himself. But strong focusing is not the kind of thing that one thinks of in an elementary way. It's a little hard to come upon it unless something leads you into it. The team that did this in the United States was Stan Livingston (whom you've seen already), Ernie Courant, who is here tonight, and Hartland Snyder, who is no longer with us. (*Slides 35 and 36*)



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Slide 36

In their case the idea came up, I believe, in the course of calculations with regard to the cosmotron, which had straight sections in it so that one had to use a more sophisticated way of calculating beam dynamics than for a circular orbit. After getting the right equations, they realized that by pushing the parameters to the proper extreme they got strong focusing, which is the thing that makes possible the huge machines that one has now, that have become geographical features, rather than architectural features. But there was an independent inventor. He was working entirely isolated from contact with anybody else, in Greece, and was somewhat earlier than these three. That was Nick Christofilos, one of the real geniuses of our time. Those of you who were here yesterday would have heard the appreciative remarks about him by one of his colleagues, and he didn't have a picture to show, so I was very glad that I have this picture in my series, so you can see what Nick looked like. (*Slide 37*) He was a difficult man to argue with, he would talk your ear off, but he was a man of tremendous originality, of tremendous intellectual power.

Slide 37

Slide 38

I will close this little slide show with a couple of things I found which express the idea of cooperation. I have one picture symbolizing international collaboration. (*Slide 38*) This was taken at Dubna in 1963. There's Lofgren on the left, who is responsible for the Bevatron; next is Dzhelepov, responsible for the synchrocyclotron at Dubna; myself; and Dmitrievsky, who also worked with the synchrocyclotron at Dubna. I cannot remember what we were arguing about, but we certainly were collaborating! The last slide I have illustrates that, contrary to some rumors, we also collaborate with our own government. (*Slide 39*) There is John Teem, who just spoke to you, representing the Atomic Energy Commission, and Louie Rosen, of LAMPF, representing the U.S. accelerator effort. The picture was taken at LAMPF fairly recently. So you see we collaborate with our own government, and in a very fruitful way. I thank you.

Slide 39

Picture Credits

Slide 4:	Merle Tuve
Slides, 5, 6, 7:	AIP - Niels Bohr Library
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