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Lawrence Berkeley Laboratory
UNIVERSITY OF CALIFORNIA

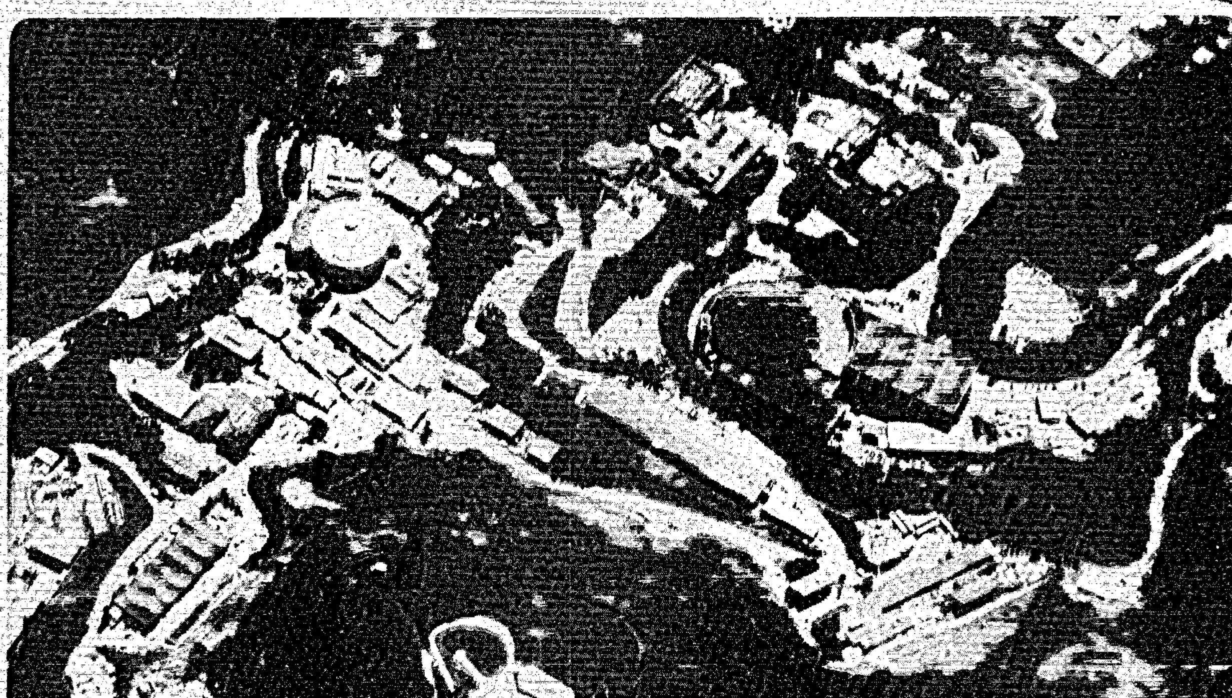
Physics Division

Presented at the 1986 Linear Accelerator Conference, SLAC, Stanford, CA, June 2-6, 1986

HISTORY OF PROTON LINEAR ACCELERATORS

Luis W. Alvarez

January 1987



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HISTORY OF PROTON LINEAR ACCELERATORS

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I thought I had better think about linear accelerators for the first time in about 35 years, so I brought the Bible[1] along with me which has lots more in it than I ever hoped to know about linear accelerators.

It's hard to believe that the work that I'm going to describe and that Pief was just talking about was done forty years ago, because I try to imagine what I would have thought had I been listening to somebody who had done some work forty years earlier when I was a young post-doc at Berkeley for example, that was in 1936, so if I subtract forty years from that I get to 1896 which was before J. J. Thompson had discovered the electron or Roentgen had discovered X-rays or Becquerel had discovered radio activity. I couldn't believe that I could ever hear somebody that old coming in and talking about some research, so you'll have to excuse me — I am that old, and I will be talking about some things I remember fairly well.

When Pief was a young kid, I have a picture of him that I was going to show in a slide, but somehow or other it got lost in the photographic department. So I'll tell you a little bit first of all about why I decided, at the end of the war to build a linear accelerator. First of all, all of us had gone off to war pretty much secure in the knowledge that when we came back we would build big accelerators and we would explore the thing then called the mesotron, discovered by Neddermeyer and Anderson, which we now know as the muon. Everybody was sure at that time that that was the particle that had been invented by Yukawa as the mediator of the strong force and it was a big shock, of course, at the end of the war when three young Italians working in a basement, hiding away from the German occupying forces showed that the muon couldn't care less about whether it was around a nucleus. It was obviously not the mediator of the nuclear forces but the pion was found a couple of years later, that did fill the bill. But anyway, we all went away saying that when we come back we'll build big accelerators and we'll work with mesotrons and that will be the physics of the future.

Most of us didn't think anything about physics for about four or five years. I went off to work on radar in 1940. November 11th, which is Armistice Day, was the day I got on the train to go to MIT to fight the radar war. When I was there I learned for the first time how to use Maxwell's equations in a practical way. I had been taught by theoretical physicists that they were just a bunch of differential equations and an occasional integral — nothing practical was ever done, no problems were ever solved to boundary values; it was just highly theoretical. I found it very dull. Fortunately Bill Hansen was very expert at this and he gave us a course up at MIT. He was working at Sperry at the time on continuous wave radars, the things we now call police radars using klystrons. We were at MIT all working on pulsed magnetrons. We weren't very interested in Bill's radar sets but we were terribly interested in what he told us about Maxwell's equations from the practical standpoint. The thing I remember particularly about the lecture notes for that series was that they were all stamped SECRET – Maxwell's equations were secret in those days!

Let me just tell you a little bit about some of things that Bill taught me because I find that most of the young people in accelerator physics don't know them and I found them very useful. Bill said that the first thing you have to remember in using Maxwell's equations is that the velocity of light is 30 ohms – that's number one! You all laugh, but if you look in the books to see what the definition of the absolute ohm was you'll find that one ohm is equal to 10^9 cm per second. That's true, and so the velocity of light then is equal to 30 ohms and Bill said that was of great practical importance. For example, if you want to know what the impedance of a coaxial transmission line, z is equal to $2c$ times the natural log of a over b.

I looked in Terman's handbook last night and I saw there was a formula giving z as equal to 138 times the log to the base 10 of a over b. Such a formula is completely lacking in physics understanding, as contrasted to Bill Hansen's formula. Another thing is that the impedance of space is equal to $4\pi c$ which is equal to 377 ohms, as everybody knows, per square. If you want to stop a radio wave cold you put up some of this "space cloth," with a resistance of 377 ohms per square which we used to make by just taking canvas, getting some aquadag dissolved in water and a paint brush and you just paint until you get 377 ohms per square with an ohmmeter. (Actually you use two copper cylinders with an impedance of z ohms, and push it against the cloth, and read the resistance on a DC meter.) You just measure the resistance and if it's more than z then you paint some more aquadag and pretty soon you have got a large area of space cloth and if you take that and put a reflecting surface, say copper, a quarter wavelength beyond that then you will absorb all the radiation, it will just stop; and that was very useful.

Another nice feature of space cloth is if you take a transmission line with an unknown characteristic impedance and any old shape; you make a scale model of it, and push it against the space cloth; you measure the resistance with an ohmmeter, and that is the impedance of that line. And all of this is just because the velocity of light is 30 ohms and when I mentioned that most everybody laughed, so it clearly indicates that you have never heard that idea before. To me it's a very real thing and very very important.

The other thing Bill said was you should work with a resonant cavity, which was a new idea to most of us in those days; nobody had used resonant cavities to make accelerators. Suppose you have a spherical cavity and you feed some power into it with a loop; then he said there are two important things, one is Q and one is the shunt impedance, and Q is essentially equal to some small numerical constant on the order of one, times the radius divided by the skin depth. That's all you need to know. That's fairly obvious. The shunt impedance is of course equal to c times a over d. So that is where our large shunt impedances came from – the velocity of light times the ratio of wavelength to skin depth in copper. Of course shunt impedances also depend of the length but, to an order of magnitude this is the way things go.

I was reading in this book last night about how the authors thought we measured in the early days the things like field configurations in the cavity. Suppose you have a cavity like this (one unit of a modern proton linear accelerator) and you want to know what the electric field looks like and the magnetic field, and it was quite impossible for the people writing this book in 1970 to imagine how we did it. They knew at that time that you did it by using digital computers. I think that Nick Christofilos was the first one to do that, solve Maxwell's equations to arbitrary boundary conditions and come out with the answer. And so the person who wrote this book, this particular chapter, said, and I'll quote it, he said

> "... the first methods applied and the only ones available before the recent development of digital computers, used finite expansions for the fields in terms of cylindrical harmonics..."

The reader is referred to Chapter C.1.b for a survey of such methods. The person didn't understand how it was really done and when I read this I remembered a story that I think is pertinent to show how you can't imagine something that you have never experienced. This is a story about a school; Park Avenue kids were in this school, and the teacher asked the children to write a story about a poor family. So one little girl wrote, "This is a story about a poor family. Everyone in the family was poor, very very poor. The chauffeur was poor, the upstairs maid was poor, the butler was poor..." I think this statement that I just read is of that nature. People trying to imagine something that they had no way of ever experiencing.

So, the way we did it, and now I'll talk a little bit about that, is simply by building models. We had a model which was a cylindrical tank with one wall fixed on it like this, then we had another wall which could be moved back and forth, with spring contacts to carry the currents. And then we had a tube that could be pushed back and forth just like this. And then what we did was to put a little coupling loop in there and measure the resonant frequency of that device. We would set this up to some value with always the same outside diameter, we just had one tank, and then we would move this flat diaphragm back and forth, set it for various values and then we would push tubes of various diameters back and forth and measure the resonant frequency. So we came out with a whole series of curves of the resonant frequency at a particular distance here and a particular diameter, and that gave us essentially all the points on a multi-dimensional space where we eventually wanted to end up with what happened for a diameter that was a particular fraction of the wavelength and then for tubes that were various fractions of the wavelength and various betas — this is of course beta over two, from here to here. And, by doing it that way, we just got a whole series of curves and came out with the data that we needed. And nobody ever put a spherical or cylindrical harmonic in the thing; nobody knew how to do it, perhaps Pief did, he had been one of the star pupils in Professor Smythe's course down at Cal Tech and so he had solved problems like that, but none of the rest of us had. We did it in the simplest way imaginable and we did it in an analog computer using electromagnetic waves as the analog for electromagnetic waves.

Let me back up a few moments and say how I personally came to get involved in building a proton linear accelerator. I actually thought of building an electron linear accelerator; that seemed to me to be the most straightforward thing. As Pief said, I knew that there were thousands of SCR, that's Signal Corps Radar, SCR 268s which had been used during the war to train search lights on airplanes so that the antiaircraft guns could be aimed at them using optical sights and computers and all the rest of it and, of course, that was of no use whatsoever once we got in the war. I think the British used things like that, but they were of no use to us. But the longer the war went on the more of these things the Signal Corps ordered, so at the end of the war there were I think two or three thousand of these devices in existence. They operated at a frequency of 200 MHz. That's how it is that proton accelerators all run at 200 MHz! I read in this book that a few accelerators had been operated at two meters instead of a meter and a half, but essentially all proton linear accelerators have operated at one meter and a half, 200 MHz. And that is simply because the SCR 268s were there and I had this fine idea that the Signal Corps would love to get rid of them; they would be happy to give them to us and as a matter of fact they did give us 2000. We had a whole warehouse down in the Oakland Port area that was filled with SCR 268s. Of course we didn't want the antennas, we let the Army keep the antennas. I guess they bulldozed them into the Pacific Ocean, but the power supplies and the oscillators, those all came to us. I never had the nerve to go down and look at these 2000 SCR 268s that were in Oakland and were technically owned by me; they were signed over to me by the Department of Defense or whatever it was called in those days.

As Pief said, we didn't use any of them, we used the power supplies; they were great power supplies and we were very popular fellows because we had thousands of these things which we gave to our friends because we couldn't use all of them! As the development of the accelerator went on we found ways to make better oscillators, much higher power, better behaving oscillators, and that work was done by Don Gow and Jack Franck and a few other people and so when you see a picture of the accelerator which I'll show you before long, you've all seen it anyway, you'll see that we had our own oscillators using more modern tubes; the SCR 268 used tubes that had four triodes in one envelope, and it wasn't a very efficient way to generate power. I think it generated 50 kW.

As I said at the end of the war, I guess it was probably in January or February of 1945, the Germans had been defeated and it looked like the Japanese would be by the end of the summer, so we all started thinking about what we would do when we got back to our home base, for me Berkeley.

I was at Los Alamos at the time and so I started designing an electron linear accelerator using 200 MHz cavities, they were half-wave cavities and every other one was of opposite phase, and it looked like it could be built. I'm terribly happy that it didn't ever get built because Ed McMillan was also at Los Alamos and he was thinking along similar lines only he wasn't thinking about linear accelerators, he was thinking about circular accelerators. As Pief pointed out, there was an upper limit energy for circular accelerators and for linear accelerators at the time. Well, there weren't any linear accelerators I should point out. Linear accelerators had been built in the middle thirties based on a design by Wideröe, nobody had ever made one to accelerate protons or electrons. If you simply plugged the numbers in, in 1935, you would have found out that there weren't any oscillators that gave enough power to give you enough voltage per gap, to give you any reasonable acceleration. Ernest Lawrence and Dave Sloan built an number of so-called Wideröe accelerators at Berkeley in the thirties and when I first went to Berkeley on a visit in 1934, there were two such accelerators — one accelerated lithium to several million volts and one accelerated mercury ions to a good many millions of volts.

As far as nuclear physicis was concerned they were both absolute duds. I should say that the lithium accelerator finally did produce a nuclear reaction — I'm sure you can guess what it is — they shot the lithium at hydrogen and made the Cockroff-Walton reaction, lithium seven plus a proton gives you two alpha particles, but that's the hard way to do it. Its much easier to accelerate protons to a few hundred kilovolts.

So, Lawrence and company had a long history of making linear accelerators but they didn't do any nuclear physics and the reason they couldn't do any nuclear physics was because they couldn't accelerate hydrogen; there just weren't any oscillators in those days that had short enough wavelength to give high enough power to give you enough voltage to make a linear accelerator in a reasonable size. Nobody had made any linear accelerators for electrons but it was obvious that that was a perfectly reasonable thing, given the high powers and short wavelengths that were available because of the existence of radar. As I said I started thinking in terms of accelerating electrons using the SCR 268s, lots of half-wave cavities and the most fortunate thing that ever happened to me was that Ed McMillan put me out of business. One afternoon he said to me "Luie, I've just invented the neatest thing; I'll tell you about it." So he described to me the synchrotron which he had invented the night before. I realized immediately that that was so much better than what I had in mind that I immediately stopped thinking about linear accelerators for electrons and said, "Be my guest, Ed," and so he did a very good job of making a 300 MeV synchrotron for electrons when he got back after the war.

Now I had gotten myself committed to building a linear accelerator using SCR 268s. Hardly anybody has ever mentioned that commitment is probably more important as the mother of invention than is necessity. Once you tell people you are going to do something, then you had better do it!

One example that comes to mind is that of Edward Teller. Edward Teller said that he was going to make a hydrogen bomb. The way that he had planned to do it didn't work, and everybody knew that and they would have been laughing at him if he hadn't sat down and thought very hard with Stan Uhlam and come up with a way that did work. His commitment was the important thing.

I watched Ernest Lawrence invent color television tubes, getting some money from Paramount Pictures to build them, and they were a total disaster! But Ernest got himself committed to making a better color television tube than anybody had ever seen, so when his first ones didn't work, he sat down and thought very hard and came up with a beautiful one that did work very very well.

So I had the same experience of having gotten myself committed to building a linear accelerator, at 200 MHz, and had to change it from electrons to protons – the only other thing I could think of. Had Ed McMillan not come up with that invention, I'm sure I would never have come up with a proton linear accelerator simply because it's too hard. If you try to do it with individual cavities, you have to start at a beta of a tenth or a twentieth or something like that.

The cavities just get to be unreasonable in size and shape, with no appreciable shunt impedance and so you give up. But having gotten myself committed, as I said, I then was forced to look hard at proton linear accelerators and did come up with a scheme that worked. There were no data on shunt impedances and resonant frequencies of cavities of this sort. The only cavities that had been investigated were ones where you had, for example, ellipsoids of revolution and hyperboloids of revolution like that, so here was a cavity and you could put in steeper hyperboloids like that. Not very practical things to ask somebody to build in the machine shop and there was no real way to get a feeling of what the shunt impedance was or how it varied with various parameters.

So I started out thinking in terms of cavities like this, another one like this. So here's one cavity and another cavity here, but it turned out that most of the power was used up driving currents back and forth in these end plates, and that bothered me and I didn't know what to do about it. Finally I realized that the lines of force in this cavity would end here on a charge and another one would start out here from the opposite charge so there were currents flowing back and forth in these walls that didn't do any good. The obvious thing to do was take these walls out and so then you had a big long cavity with a whole bunch of drift tubes, as you have all seen, and the electric field lines link together, without benefit of charges or copper sheets.

The question then was what would the shunt impedance of such a thing be? As I said there were no tables to go and look at and there was nobody to go and ask about it so I finally realized, and this took me quite a long while, that the shunt impedance just had to do with how many lines of force there were in here, and all you had to do was go back and ask Mr. Faraday how many volts you would have if you had so many lines of force oscillating back and forth at the resonant frequency? And so whether or not you had drift tubes sticking in here or not you had essentially the same shunt impedance for a given current in the walls or a given power dissipation. That was a great discovery for me – it's obvious to anybody right now but it took me a few months to come up with that notion.

At that point then, I realized that we could make a proton linear accelerator and I assembled a really first class crew of people, one being Pief, who had signed up to go to the Bell Laboratories. But I rescued him and brought him back to the academic world and the linear accelerator business. In fact, in this "Bible" it says that was one of my main contributions to linear accelerators was rescueing Pief from the Bell Labs and bringing him in to the linear accelerator business and I think that is probably true! Of course, if he had stayed at the Bell Labs he probably would have been a co-inventor of the transistor, but he had some other things to do. Frank Oppenheimer joined the crew; we had a number of people whose names you would recognize.

Then the problem was how do we build such a thing and make it work? First of all we had to do all of these tests with models to get the size for the size of the drift tubes, the diameter of the drift tubes for various betas, and an outside diameter that would work from the lowest energy to the highest energy. There we were both lucky and fortunate that we picked the lowest energy, four million volts. You might wonder why I didn't use RFQ or something like that where you can now start out? The answer is, of course, that we didn't know about such things. The other thing was that Ed McMillan had pointed out that phase stability was a very important thing. If we were going to build a long accelerator, you had better have phase stability. Ed once went to Ernest Lawrence and said, "Look Ernest, I just read the paper that you and Dave Sloan wrote fifteen years ago and in there you talk about phase stability. You point out that if the voltage is rising as the particles go in, the ones that come in later get a higher acceleration and will catch up with the ones that were there before and that they will oscillate back and forth. That's the concept of phase stability and it's in your paper so you ought to be given the credit for discovering phase stability." Ernest would have none of that. He said, "We really didn't realize its importance. You are the guy that realized its importance and you are going to be the father of phase stability."

At any rate we realized, as Ernest Lawrence and company had, that we needed phase stability to make a linear accelerator work. The difficulty was that if you were on the rising part of the curve where you got the phase stability, then you had radial defocusing. You can easily understand that if you just imagine that you are moving along with the wave and you are looking at a region in space where there is no charge. You would like to have forces bringing the particles back in — that's radial focusing – you would like to have particles moving in, both radially and axially, but that violates the Earnshaw's theorem and so you can't do that. So we had to get another way to do the focusing and my solution was to use berylium foils. They had to be very thin because we couldn't stand too much scattering. It was to be a berylium foil; I forget how many microns thick each one was, it was pretty small. Hugh Bradner made the berylium foils, and they looked as though they were tough enough to do the job. That's also what set the four million volts for the injection energy, and that presented a problem because nobody had ever built a four million volt Van de Graaff – the highest energy anyone had ever achieved was three and a half MeV, which Ray Herb had done at Wisconsin, and that machine had been moved to Los Alamos.

So we had not only to build the first proton linear accelerator we had to build the highest energy Van de Graaff anybody had ever seen, and that presented some problems too.

To get back to the beryllium foils, we did put them into the accelerator, turned on the voltage, and then opened the tank up again, to see that all the beryllium foils had disappeared. Sparking or whatever, we never found out. Anyway the beryllium foils all disappeared.

I'll tell you one interesting story about what the laboratory was like at Berkeley in those days. Hayden Gordon who designed the beryllium foil holder – here's a drift tube with an arrangement in here that had some screw threads back here pulling a copper piece in against the beryllium foil which is resting on another copper piece, and there were some pins in here so that when you turned the screw threads you didn't tear the beryllium foil. It was a very complicated piece of apparatus. When the drawing was sent into the shop it was called "beryllium foil holder." Down at the bottom someplace it said make out of copper. But the machinist didn't see the words that said make it out of copper, so he made the whole thing out of beryllium! Complicated screw threads and everything. Nowadays if you sent in such a thing and the machinist read it as to be made out of beryllium he would get you on the phone and say, "Do you really mean you want this thing made out of beryllium?" In Berkeley in those days, nobody asked the question — so the beryllium foil holder was made out of beryllium!

I've told you how we measured the resonant frequency of the cavity. You might wonder how we measured the shunt impedance, since we didn't know the configuration of the fields; this was an idea I think came from Pief. It was very simple. You took a cavity and you excited it and then you had a little hole in here with a thread going through and then you had a little thing we called a beebee that ran down the center line, on its thread. The beebee could be either made of a little spherical piece of copper or of some dielectric and that, of course, is going to perturb the fields here. It's clear that if the beebee is down inside the drift tube, it's not going to change the distribution of fields, it's not going to change the frequency. So what we did then was to put a frequency meter out here and we would tune the thing and as we would run the beebee back and forth we would see what Δf over f was. And Δf over f was proportional to the volume of the beebee times the square of the electrical field. And so we measured shunt impedances by the beebee method. It was the only way we had; there was no other way. And again that's one of those things that people writing a book like this would not have ever heard about and probably if they did would not believe it! That's how we measured the shunt impedance.

The other main difficulty we had, and I think I wouldn't have known how to solve this, was we had all these drift tubes in here and when we excited the whole cavity we measured the magnetic field at the edge, and of course that had some waves like that, it was supposed to be flat to give constant increase in energy per foot of path length. The difficulty with this was, it was clear that individual cells were all not tuned to the same frequency. If they were all tuned to the same frequency the magnetic field would have been flat. But we found out that if we changed the tuning at one of these drift tubes, it would not only change the height of the magnetic field there but it would change it from one end of the tank to the other, and it seemed that there was no way to solve the problem. Pief, using his experience with Smythe, recognized that if he did a Fourier transform of this thing and did some perturbation theory on it we could find out then exactly how much the frequency of every individual section should be changed. And so we put a shim of calculated thickness behind the front of each drift tube, which could be screwed back and forth, and after one or two iterations we flattened the tank.

I tried to find something in this book on flattening tanks, and there wasn't anything. I expect that means that the constants of the various sections are so well known from the computers that you build the thing and the tank is flat. Is that true or not? Do you have to flatten tanks? I can't tell. Anyway, I couldn't find anything about tank flattening in here but we did flatten the very bumpy-looking tank and make it work.

An earlier problem had of course been, what should the tank look like? Nowadays you build tanks that are strong enough so that they don't deform when you put the vacuum load on them. We did it another way. We built our tank so that it was like an airplane. Very flimsy. It was an inside-out airplane in fact and it really was an airplane structure; it was built by the Douglas Aircraft Company down in Santa Monica and you've probably all seen pictures of it. It has forming rings like you see on the inside of an airplane except rather than having a skin on the outside it has the skin on the inside. A copper skin, aluminum rings and I'll just show you the pictures of that in a moment — first of all some numbers, we ended up at 32 MeV starting at four, so the tank gave 28 MeV. It was eight wavelengths long and I read in the book last night that you don't want to have it more than 20 wavelengths long or you'll have trouble with mode flattening the magnetic field from one end to the other. (So we were just lucky; we had never heard of the problem when we designed the tank.)

Our tank length was set simply by the length of the shop building in which we put the accelerator. So we got around the mode problems because the building wasn't long enough, and we got around other things because we had obsolete radar sets to pick our frequency for us, and so we finally got the thing to run. Oh, I should say after the foils all broke, we were really in deep trouble because everybody had concluded that the only way you could focus a linear accelerator was with foils. You couldn't make them thicker because they would scatter too much. Finally we realized that we could take tungsten sheets and put them on end and use that to focus and that was lucky, but when you've got that much invested in something like this, you find a way out as long as it doesn't violate the laws of physics. The main thing we had to do was put some charge inside the beam.

Well, I'll just remind you that we started this thing at the end of 1945. We had our first beam near the end of 1947. That was going from no accelerator, no theory of the accelerator, no experience whatsoever, to an accelerator that was really putting out a beam. I shouldn't say this, but we gave this accelerator to another university in 1958, I think it was, and we gave them our best accelerator technicians who had been running the accelerator for the last several years and it took them three years to get the machine running. So we always think we did not do too bad a job in getting the thing going in two years.

We also had the experience of putting the first quadupole focusing devices in any real accelerator right after the Brookhaven people invented the strong focusing principle. Craig Nunan and Bob Watt, you all know here, put electrostatic quadrupole focusing elements in each drift tube, and that was the first test in real life of a strong focusing system. I've got four slides which I'll just show, then I'll stop.

First slide: the first slide is one that shows our tank down here with all of the people connected with the building of the accelerator, the secretaries, shop people, everybody on the tank as it came in to Berkeley. And Bill Hansen who is the guy on the left in the picture above, the great person who is largely responsible for electron accelerators in the world, Bill thought this was very funny and so he got his accelerator which was that long and instead of having everybody sit on it they supported the accelerator on their shoulders! I've always thought of these two pictures as going together. Of course, the people at Stanford can't laugh any more at this because after this they built one that's two miles long, which is considerably longer than this.

Next: this is the linear accelerator with the tank opened. This airplane structure here, these are the Dural forming rings, these are the copper flat pieces and the drift tubes hang down from these points here. This doesn't show the oscillators feeding in, but these are the feed-through holes here.

The next picture, shows the accelerator with the oscillators in place. These are not 268 oscillators, but they are the ones which Don Gow and Jack Franck built. Vacuum pumps are over here and the injector is coming in from this side. The Van de Graaff is sitting about here.

Next: this is kind of a funny looking slide. People put it together because when the beam first came on, which was on October 16, 1947, we had tried the whole evening to get a beam with no success. So I had finally come up with a reason why we were not getting a beam. Something was wrong with the structure that had to be changed, so I gave them this lecture on what the needed changes were, and these are the diagrams I drew. These are the diagrams I put on the board to show why the accelerator couldn't work. Then at 2:40 AM, we got our first 32 MeV beams. Somebody photographed this slide for posterity, and I'm glad to show it to you.

Thank you very much.

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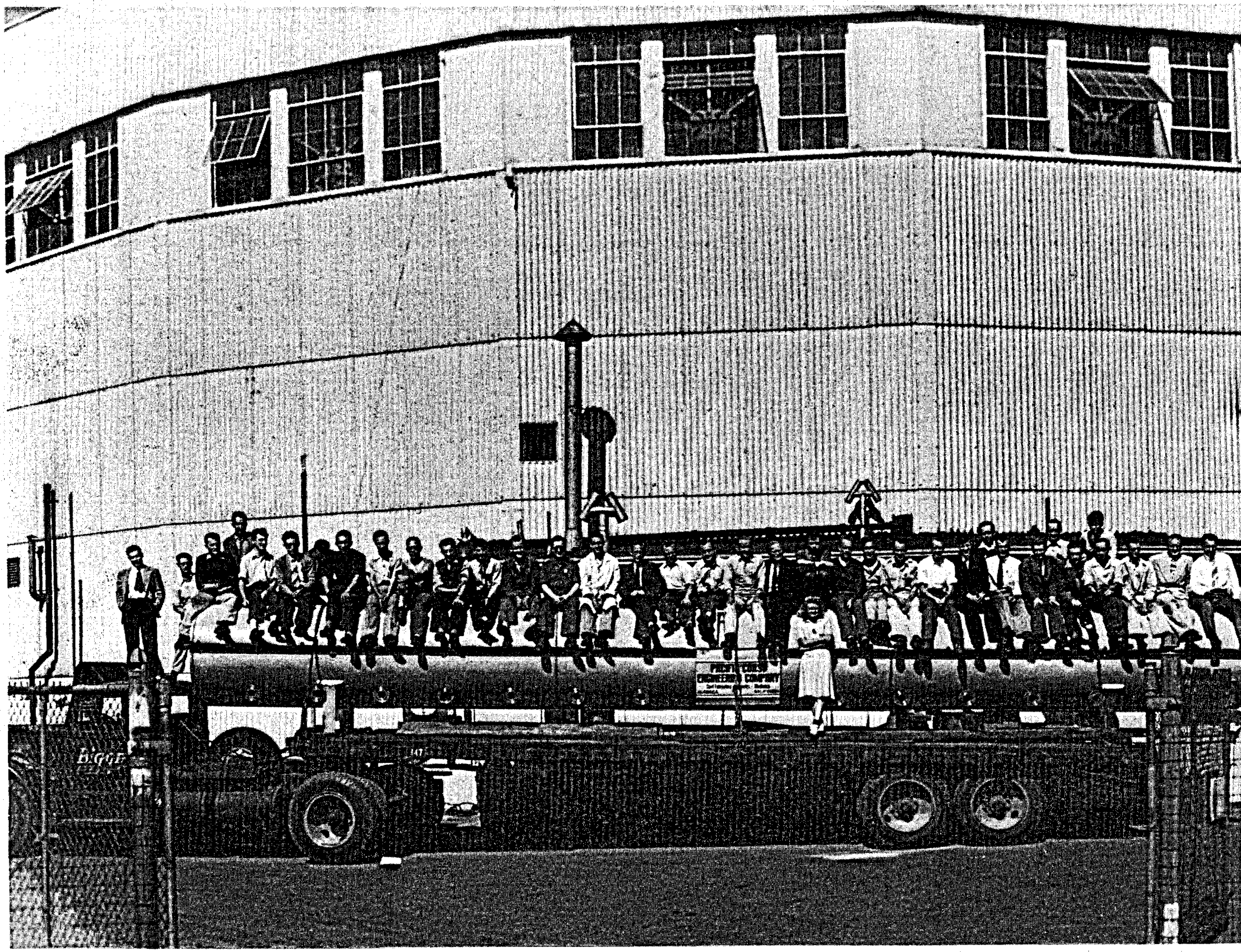

Fig. 1

LINAC 159

Fig. 2

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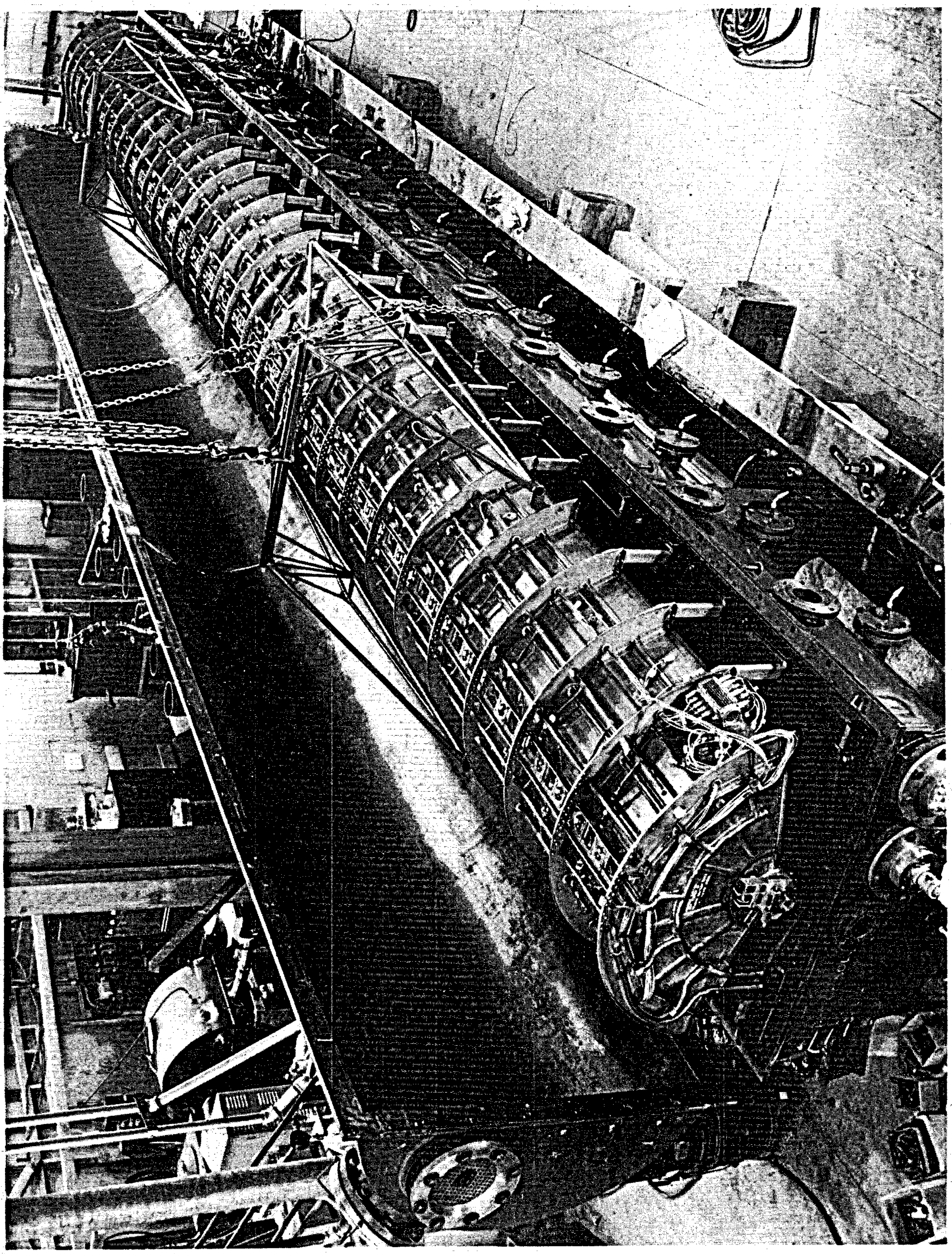

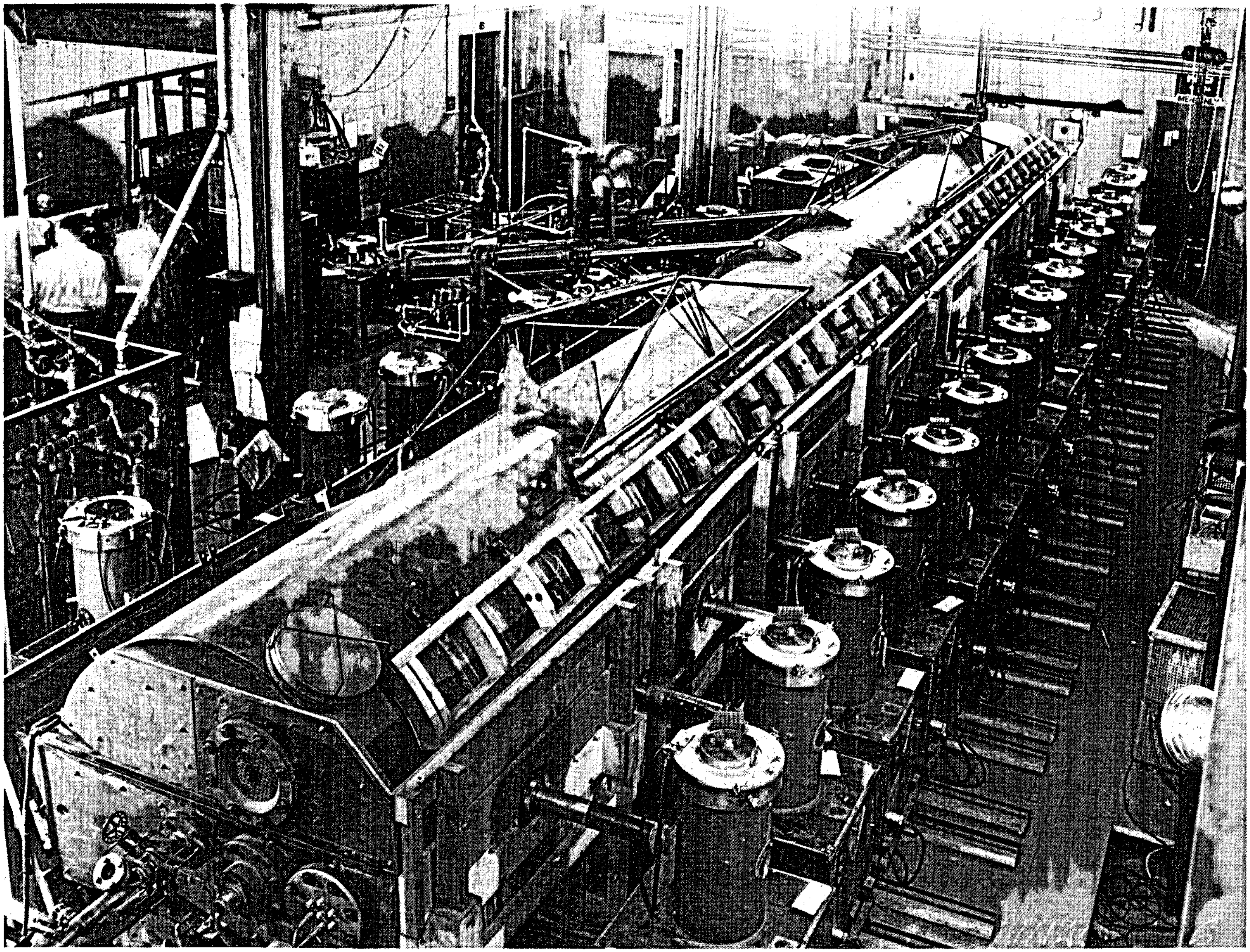

Fig. 4

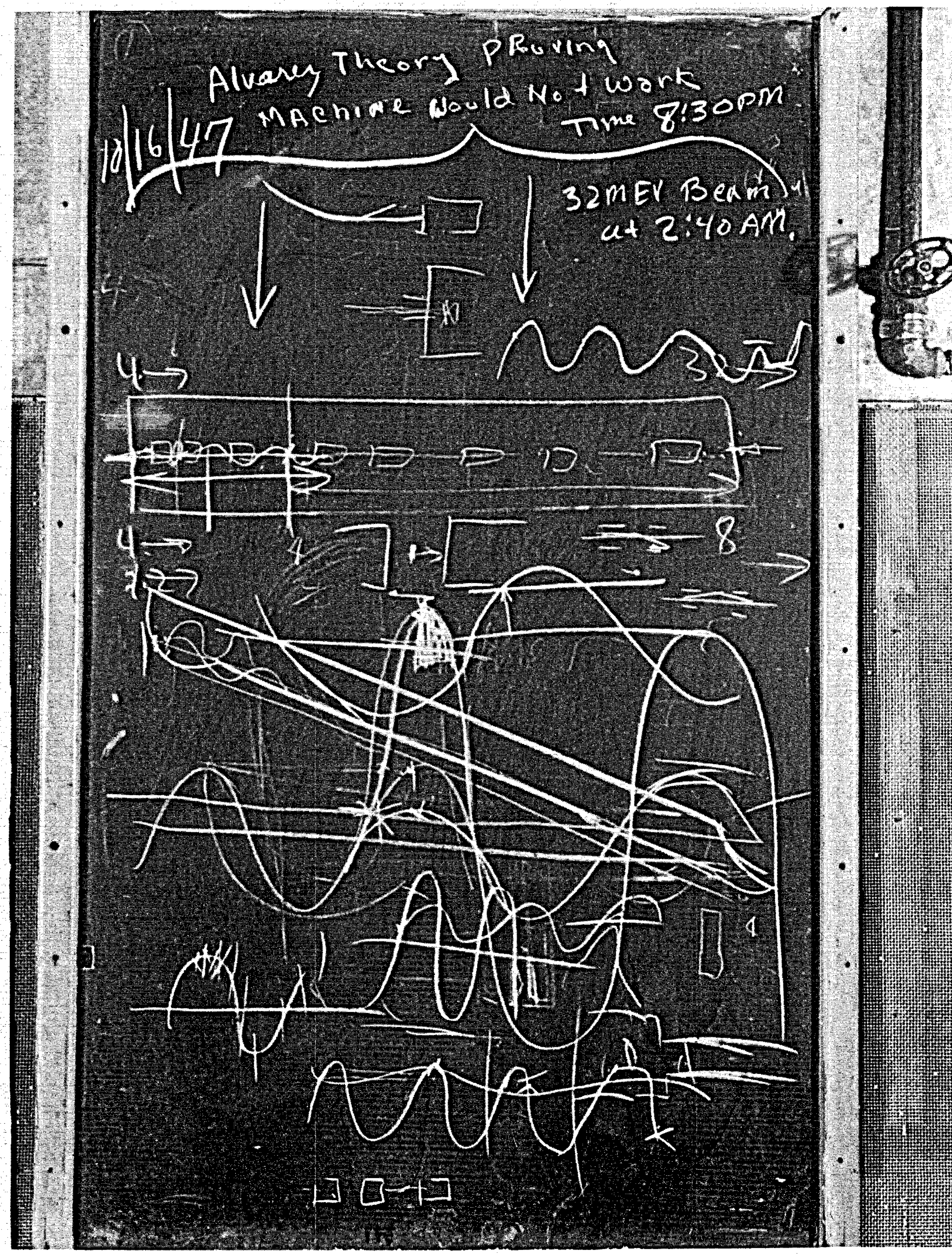

Fig. 5

LINAC 515

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