Jean Deken [00:00:00] This is Jean Deken. It's Friday, November 15th, 2019, and I'm speaking with Gregory Loew. Greg, I'd like to start with your early life. Where were you born?

Gregory Loew [00:00:17] I was born in Vienna, Austria, but never lived there. It was just an accident of my family's history.

JMD [00:00:25] Oh, okay.

GL [00:00:26.] My father was from Bucharest, Romania and my mother was from Hamburg, Germany. We first lived in Bucharest, but a couple of years later we moved to Paris, France and that's where I grew up until I was 9 years old.

JMD [00:00:51] And what did your parents do? What did your father do?

GL [00:00:58] My father was a businessman and my mother took care of me, my sister and my brother. She was very active at home, teaching us, helping us with school work, taking care of our apartment or house but she never worked for money.

JMD [00:01:] Did you grow up speaking French?

GL [00:01:17] Yes, I spoke some German because at first my parents were speaking German with each other, but as soon as we moved to Paris, I learned French and from then on, my first language was French.

JMD [00:01:33]. And what kind of education did your parents have?

GL [00:01:42] They both finished high school. My father went to a small business school in Vienna, it was what he called “a school for commerce”. He was also an accomplished pianist. My mother was very interested in art, water coloring and dance, and spent some time studying languages in a school in Geneva, Switzerland. They were both intellectual and curious, they loved music, opera, theater and movies, they read a lot of books, but they did not have what we call a college education per se today.

JMD [00:02:11] Oh, Okay. And when you started school, what kind of school did you go to?

GL [00:02:19] That was an interesting thing. My parents loved to travel, and so I was signed up to go to a school in Paris called Le Cours Hattemer. It was a school where you went once a week for two or three hours. You were accompanied by your parents who sat in the back of the classroom with all the other parents. And there was a teacher who would run the class. She would examine you and you would get a chance to speak and to write and so on. And then you would go home with a pile of homework for the next week. If you were on travel with your family, the homework would be sent to you by mail. And for the first five years of my childhood, I would study at home with my mother. She would help me and supervise my homework, and then, the next week, we would go back to the school, take the tests, and so on. That was until I was nine.

JMD [00:03:17] Okay .What happened after that?

GL [00:03:18] Well, after that, when WW2 started, we moved to Buenos Aires, Argentina. And from then on, I went to a regular school, which was.the French Lycee in Buenos Aires. There you learned everything in Spanish in the morning, and everything in French in the afternoon up to 6^{th} grade. four hours in the morning and four hours in the afternoon After that, you could choose the French program or the Argentine program full time, but not both.

JMD [00:03:48] Okay. And were there any teachers that you had in school that were especially memorable or influential?

GL [00:03:58] Yes, I had several wonderful teachers. I had a science teacher. Madame Baccarat. she was a French biologist. She had a great influence on me. And then I had a French mathematics teacher for two or three years in a row. His name was Monsieur Corbineau. He was also a wonderful teacher and I learned a lot from him. Those two people plus several others had an influence on me in two ways. One, they interested me in what they taught, and two, they showed me that I could be pretty good in math and science. And so they had a marked effect on my subsequent career.

JMD [00:05:20] Oh, okay! So did they or did your parents, encourage you or expect you to go on to college?

GL [00:05:29] Both. But there was never any question in my parents' mind that I would go to college. No question.

JMD [00:05:33] Even though your parents had not really gone to college?

GL [00:05:36] Absolutely, they were totally set on sending me to university. The only question was, what would I study?

JMD [00:05:54] OK. And was this the same for your siblings, that they expected them to go to college?

GL [00:05:59] Yes. Absolutely..

JMD [00:06:00] And so when you went to college, at what point did you decide to major in physics or did you major in physics?

GL [00:06:09] Well... My university career was complicated by a number of circumstances. When I finished the two successive French baccalaureates in Buenos Aires in1946 and 1947 (degrees officially sanctioned by the French Government), I had to decide where I would go to college, and there was a conflict. One was that my parents thought that it would be good if t first got a more general, humanistic education in France and then came to the United States to become more specialized in science and technology. Actually, the world situation with the Cold War and the Berlin Airlift in Europe was very bad at the time, so my parents were worried about that plan,. But, eventually it worked out. I went for three years to the University of Paris, the Sorbonne, and there I got what's called a “Licence es Sciences,” in mathematics, physics, chemistry and electronics. These four subjects were studied fulltime, one year at a time.

JMD [00:07:40] Okay.

GL [00:07:41]Then, after that, I came to the United States as per the original plan. And when I came to the US, I was admitted to Caltech in the Electrical Engineering Department to study electrical engineering, electronics and physics. Richard Feynman’s lectures on Quantum Mechanics were the best show in town, even though they were a bit too advanced for me to take for credit. After I got my M.S. Degree from Caltech in two years, at the encouragement of a professor who had just moved from Stanford to Caltech I applied and I was admitted to the EE Department at Stanford....

JMD [00:08:32] Who was the professor at Caltech who encouraged you?

GL [00:08:36] It war Lester Field from Stanford, who had gotten a tenured position at Caltech, with a lucrative consultant-ship at Hughes Aircraft. He was one of the inventors of the traveling-wave tube, which was a microwave device which worked very much like an accelerator in reverse: beam in, microwaves out! I took his class and he saw that I liked it very much. And he said, 'I think you will be happy at Stanford. If you want to get a Ph.D., go to Stanford.' So that's what I did.

JMD [00:09:13] Okay. So when you came to Stanford, what year was that?

GL [00:09:19] That was in 1954.

JMD [00:09:22] Who became your thesis advisor?

GL [00:09:29] My thesis advisor at Stanford was Karl Spangenberg, who was a well-known vacuum tube specialist, also later involved with transistors. He was my supervisor, and Frederick Terman, famous for his book on Radioengineering, was his boss. When I came to Stanford, all those people were still very accessible. So I met Frederick Terman many times. He was a bookworm and could often be seen walking across the Quad reading a book! In EE, we had a very close connection with physics and so I took several physics classes: as a matter of fact my degree was half with EE and half with physics courses.

JMD [00:10:16] Your PhD?

GL [00:10:16] Yes. But the thesis was on a special traveling- wave tube, which was a microwave generator.

JMD [00:10:2 You worked with Karl Spangenberg on that thesis?
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GL [00:10:34] Yes, I worked with Karl Spangenberg and I also worked with a very sharp colleague of mine, C.T. Sah, who later became a very well-known specialist in transistors. When I was finishing my PhD, the transistor began to compete with vacuum tubes,

JMD [00:11:04] Transistors, I see. And so, as you are finishing up your PhD, were you looking around to decide what you were going to do next?

GL [00:11:17] Absolutely. That was a hard question for me, I didn't know what I wanted to do next. So I did several things .In those days, if you earned a degree in EE from Stanford, all doors were open to you everywhere. So, I was invited to interview with Bell Labs, IBM, University of Illinois. Westinghouse, Hewlett Packard and others, and I got offers from every one of these companies, but I still couldn't decide. And then I ran into one of my ex-professors at Stanford, Marvin Chodorow, who was very well known at the time for his contributions to the design of klystrons,, and I told him about my dilemma, that I really liked the idea of university work, and I didn't quite know whether any of these other companies would be as exciting. And he said, "Let me see if I can find anything of interest to you here at Stanford." After a couple of days, he called me and said: "Look, I can't promise you a job for life at this place because we don't know what's going to happen, but over in the Hansen Labs (right next to where I was in the Electronics Research Lab), there's a new project which is proposing to build a big linear accelerator. And they're looking for people like you -- there are only about three or four people working on this project right now, but you would have to take a risk because there's absolutely no guarantee that this project will ever be approved." (This was in May of 1958, it turned out to be my job for fifty years!)

JMD [00:13:54] Yeah?

GL [00:13:54] "No guarantee whatsoever the project will ever be approved by Congress because it's a big deal but if you want to do something interesting for a year or two and get a temporary job there, there's a man who is looking for people like you. His name is Richard Neal. He's basically the technical head of this project at the Hansen Labs, and he's willing to interview you, and if you guys can get together, you might get a job there. Again, no guarantee, but it should be interesting." So I went to see Richard Neal and we hit it off immediately. He was a wonderful man, a very supportive person, as a matter of fact, I worked with him for my first 25 years at SLAC. But anyway, in those days there was no "SLAC," it was called "Project M."

JMD [00:14:48] Project M?

GL [00:14:49] Yes, "M" stood for "Monster." The idea was that this machine, this accelerator if it ever got built, would be a monstrous affair, three kilometers long. So anyway, I thought about it, got very excited by this work, and decided to go for it. When I started to work with Richard Neal, there were several other people in the Microwave Lab aside from him that were influential in helping me and supporting me: Ken Mallory and Bill Gallagher, both very knowledgeable about microwave measurements, and En Lung Chu, the accelerator theorist. However, the main person I saw every day was Richard Neal. and that's how I started to work for what eventually became SLAC.

JMD [00:15:40] OK, so you started out on campus working for Richard Neal. What were the main challenges that you were faced with on Project M?

GL [00:15:53] OK. The first challenge for me was to design the heart of the accelerator, a structure made out of copper, supplied by microwave power to accelerate electrons to about 20 GeV. Four smaller linear accelerators had been built at Stanford so far. The first was the MARK I, which was the creation of William W. Hansen, the founder of this electron accelerator technology at Stanford. The second was the MARK II which was a short experimental two-section accelerator, the third one was the MARK III. The MARK III accelerator was built in stages and eventually reached about 1 GeV in electron energy. It was where Prof. Robert Hofstadter got his Nobel Prize for his work on electron scattering of numerous atomic nuclei. and where Pief Panofsky did his pioneering experiments with pi mesons. Richard Neal had written his entire PhD thesis on the Mark III. I will come back to the Mark IV later. All these linacs used cylindrical disk-loaded waveguides (arrays of pill-box resonating cavities) for their accelerating structures.

The first thing that Richard Neal asked me to do was to see if I could explore all kinds of other structures to see if any might be more efficient. After a few months of research, the answer turned out to be no: the disk-loaded waveguide was the best. But a question remained: Should the cavities in the array all have the same dimensions (so-called uniform structure) which would lead to an exponentially decaying electric field in each section, or might it be better to design a new structure tailored to produce a constant electric field (a constant-gradient structure) by gradually decreasing the dimensions of the cavities along its length. The constant field was more conservative in that it was less prone to electric breakdown. But the constant-gradient structure design was more daring because it had never been done before. Each ten-foot accelerator section would consist of 85 cavities and two end-couplers to feed microwave power in and out, but each of the 85 cavities would need to have different dimensions, tapered to achieve the constant field. So that's the task I took on. To do the job I was offered the help of a very meticulous technician/engineer of German origin called Otto Altenmueller and a couple of graduate students and together, we designed and figured out the dimensions of these cavities. There were no computers at the time and everything had to be done experimentally, cavity by cavity. After about two years we came up with the design that became the SLAC accelerator structure for the next 50 years.

GL [00:19:15] There's a picture of Otto Altenmueller doing a microwave measurement in the SLAC Two-Mile Accelerator book .

JMD [00:19:27] Oh, The Blue Book. Okay. Okay. Very good. So this had never been done before?

GL [00:19:32] No, this kind of structure had not been built before. The idea of the constant gradient was something that Dr. Neal had only explored theoretically.

JMD [00:19:58] Okay. So when you were experimenting and you were modeling the structures, were you actually working in copper?

GL [00:20:07] Absolutely, copper was the only way to go at the time.

JMD [00:20:07] Okay, okay, okay. Do you even remember how many different structures you tested?

GL [00:20:28] Well, the way we did it was to divide the measurement into 13 sub-measurements. We made 13 different stacks of cavities and we figured out what kinds of apertures, diameters, and so on they would have. We worked very well together with the Fabrication Department under a very capable mechanical engineer called Arnold Eldredge with another microwave engineer, Richard Borghi and a whole army of mechanical engineers/fabricators, machinists, furnace designers, etc. After they produced a few prototype sections, I had to test them on the Mark IV linac in the Hansen Laboratory. This linac had been run before with standard structures and it was also being used for cancer electron radiation therapy trials. When our prototype structures were installed inside the vault, I spent many nights there testing them with two klystrons and looking at the beam at the end. There was only room for two prototype sections at a time. We tested probably four or six structures until we finally decided that the design was fine.

JMD [00:22:32] Okay. So you said that there was cancer treatment going on. Was that something you participated in at the time?

GL [00:22:39] No. The electron beam was tuned up by an operator and the radiation treatments were done entirely by M.D.s from the Stanford Medical School which was just being moved to the Campus from San Francisco. X-ray radiation treatment was already in use but direct radiation with electrons was a new idea which looked promising for certain eye and other diseases. I had nothing to do with that work.

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JMD [00:26:44] OK, Meanwhile, what was the status of the entire project?

GL [00:26:44]I In 1959 our 1957 design for Project "M" had been proposed by President Eisenhower, but the Congress had not approved it because the price tag was over a hundred million dollars, In those days this was a huge request of money for something not well understood, a two-mile-long linear electron accelerator. It took Dr. Edward Ginzton (then our director) and his Deputy, Dr. Panofsky many, many trips to Washington to convince the AEC, the Atomic Energy Commission, and eventually the Congress to approve and fund the project. In those days, just as today, most senators were not scientists and they did not know what this whole project was about. I vividly remember that there was once a senator who asked Dr. Ginzton: “Well, Professor, what is all of this for, what do you want to discover with this accelerator,?' And Dr. Ginzton looked at the senator and replied, 'Senator, if I had an answer to this, I wouldn't be here.” Surprisingly, the senator accepted this answer: it would be inconceivable today! He would be shown the door!

JMD [00:28:48] [laughs].

GL [00:28:48]. So eventually these many discussions in the AEC Joint Committee and eventually in the Congress led to the approval of the project for a hundred and fourteen million dollars. And at the end of 1962, Stanford got the contract to build the project and our jobs became a little more secure…

JMD [00:29:43] So at that point, did you move to the SLAC campus from the main Stanford campus? Or when did that happen?

GL [00:29:56] Well, before the SLAC campus was ready, we moved out of the Hansen Microwave Lab into two large temporary buildings, M1 and M2 near the football stadium. They contained just cubicles, no offices. And that's where Dr. Ginzton and Dr. Panofsky ran the project together, although Ginzton was still Director. And then a couple of things happened. When the project was approved, Professor Ginzton, who was a very charismatic person, ran into a big problem because he had also collaborated with the Varian brothers who were running their company in Palo Alto. And the two brothers, Sigurd and Russell Varian both died within a very short time, leaving nobody of their stature at the helm.. At that point, Professor Ginzton was offered the job of running Varian Associates, which still exists today, and he decided that being an engineer and not a physicist like Pief Panofsky, he would be better off running this company than to staying at SLAC, So, he transferred over to Varian in Palo Alto and Panofsky became Director. Meanwhile, we were looking for the best site for SLAC. The preferred site at the time was mostly under the Stanford golf course, and extended for about three miles between Page Mill and Alpine Road. Very few people remember this today.

However, this site had to be built mostly deep underground, which would not have turned out to be very practical. So after some further studies, we zeroed in on the current Sandhill site, between Alpine Road and Whiskey Hill Road. I remember, when I was still at Stanford, I once came up to look at this site, all by myself in my car. I entered on foot, there was no road and no people, just some nice oak trees and a cow. I took a picture of the cow It may well be the first picture ever taken of the lab! I may still have this picture somewhere in my files… Shortly thereafter, the Sandhill site was selected because it would be less costly to build a machine not entirely underground.

The first building that was built for people coming up from the campus was the Test Lab. The Test Lab was the place where we were going to do our microwave experiments and also where the klystrons were going to be built. Both myself and Dr. Lebacqz were the first to come up!..

JMD: What else what happening at the time?

GL: It was a very momentous epoch in international physics because in September of 1963, the International Accelerator Conference was scheduled to be held in Dubna, USSR. This was the first such meeting to be hosted by the Russians.. I was invited with Drs. Neal, Panofsky, Matt Sands, Richard Taylor and a few others to attend the conference. This trip was an incredible occasion, both scientifically and politically. because for all of us it was the first time that we went to Russia and interacted with Russian people and scientists under the Communist regime A few weeks before the meeting, the US and the USSR had signed the Partial Test Ban Treaty agreeing to ban all nuclear tests in the atmosphere, which was a good omen for international relations and pleased all scientists Also, it was a special time in my career because, along with my experience in designing accelerator structures, I had designed and built one of the first microwave particle deflectors that was capable of pushing charged particles sideways instead of forward The Russians had built a device with the same purpose but much more cumbersome than mine, and my presentation was a great success.

JMD [00:35:58] It was called a particle separator? What was its purpose?

GL [00:36:28] Yes. The idea was that if you could synchronize the radiofrequency wave in the deflector with a particle beam, you could separate some desirable particles from others at different speeds, that were undesirable. I built this device, again with the help of Otto Altenmueller. By 1966-1967, we installed four of these structures for different groups in the SLAC end-stations, to separate K-mesons and protons from electrons...They still exist today and 50 years later, one of them is used as a deflector for the LCLS!

JMD [00:38:47] Oh, okay. So once you're settled into the test lab, you are in your second floor office, the one that Laura [O'Hara] and I helped you close down.30 years later.;
Once you knew and you let Dick Neal know that you had solved the constant-gradient structure problem, what were your next challenges?

GL [00:24:51] Well, the next challenges were that in order to make a linear accelerator work, you had to have something to drive it. It was pretty much established at that time that the way to drive a linear accelerator with sufficient microwave power, you needed a klystron. A klystron can be made into a high power microwave tube, and these tubes in those days were also designed in the Microwave Lab by a Dr. Jean Lebacqz. By 1959, the project that was being planned needed about 240 klystrons. This array of klystron tubes would be installed in a gallery above the accelerator and connected to the sections below by rigid copper waveguides. OK, so now the next question was how were we going to drive and phase this whole array? And since by that time I was pretty much done with the design of the accelerator itself, I was asked to design these next systems called the drive and phasing systems. If you look at the so-called SLAC Blue Book mentioned earlier, the next chapters after the accelerator design and fabrication, have to do with the drive and phasing of these klystrons. This was a good job for me because it involved technology with which I was already familiar. To proceed I had to hire about ten new people. Our group was at first called the Microwave Department., Four or five engineers helped me with the drive system to provide the microwave power to drive the 240 klystrons and another four people helped me design the phasing system which had to make sure each of the 240 klystrons would be phased so it would contribute the maximum energy to the beam. This system involved a lot of modern electronics.

Shortly thereafter, I was also asked to incorporate the Injector Group led by Roger Miller into my department, as well as the Instrumentation and Control Group under Ken Mallory. My department then became the Accelerator Physics Department, approaching 50 people.

JMD [00:40:49] OK. And what was the entire electric power situation for SLAC? I know quite a bit from talking to Pief [Panofsky] and working with him on his memoir about the controversies around bringing power from Skyline Boulevard down to SLAC. Were you involved in any of that?

GL [00:41:18] No, at the time I was not involved directly, but everybody at SLAC was affected by this problem. By 1964 it was calculated that the total AC power that would be needed by the lab could eventually reach 85 MW. This power could be tapped off the large 220 KV power lines running along Skyline Boulevard, and the easiest way was to build similar large towers all the way down to SLAC. However, when the City of Woodside found out that this was our plan, they got very upset because they thought it would ruin their entire landscape. We tried to explore if we could put the lines underground but that was technically impossible because of the steep downhill grade. The City of Woodside complained all the way to our Congressman, Pete McCloskey, and to Lady Bird Johnson, wife of LBJ, in the White House, trying to block our project This led to a serious crisis but fortunately the day was saved by an esthetic compromise: design smaller towers that could still carry 220 KV, paint them in green so they would blend into the landscape, and lower them into place with helicopters so the trees along the way would not be damaged!

JMD [00:45:55] So everyone was holding their breath to see if that would work out, and then it did.

GL [00:46:03] Exactly.

JMD [00:46:57] Were you very busy in those days?

GL [00:47:02] Oh, yes. It was an amazingly intense period of my life, every day was exciting! As a matter of fact, I have to say that my whole 50 years at SLAC have been exciting because I always felt that we doing something new. It was an adventure from A to Z.

JMD [00:47:29] Wow, That's wonderful, although I bet some days the adventure was maybe a little more stressful than other days?

GL [00:47:40] Yeah. But you know, I was a lot younger then and I could take it. I never experienced being very tired or anything like that. The people who worked with me were all exceptional people, unfortunately not too many of them are around anymore.. I recently met with Martin Lee, long retired, who was one of the first engineers I hired in 1962, I believe. Martin Lee's daughter. Melinda Lee is now head of the SLAC Communications Department. I haven't seen Ken Crook of I & C for a while but I think he is still around.

JMD [00:48:45] That's great. Was Dieter Walz in your department? He's still around.

GL [00:48:51] No, Dieter Walz was not in my department. He is still around fortunately. I saw him not too long ago. Several years later we did a great experiment together which I will describe later.

JMD [00:49:26] Okay. What happened then?

GL [00:50:05] In early 1965 we tested the first two hundred meters (two out of 30 sectors) of the accelerator with a beam. Those tests had two purposes, one to see if all the systems worked correctly, and two if we had to make some last minute changes. And indeed, we found that we had to make one important change in the way the rectangular waveguides were attached to the accelerator couplers. These couplers suffered from a field asymmetry which deflected the beam sideways like the rf separator described earlier. To compensate for this asymmetry, we had to alternate the rectangular waveguide feeds from left to right. This modification kept the mechanical engineers busy for a while!
Then, slowly, the rest of the accelerator was installed until the beginning of 1966. It was a huge push to fabricate all of this equipment, install all the klystrons, install all the modulators, the vacuum systems and so on, including the Switchyard. I think you probably heard about the discovery of the Paleoparadoxia[1]?

JMD [00:51:48] Yes...yes

GL [00:51:49] This was a complete fossil of a 14 MY old semi-aquatic mammal that was discovered during the excavation of the Switchyard. The excavation was stopped for several days to give time to geologists to figure out how to extract the bones without damaging them, Later on, Adele Panofsky who was very interested in paleontology, spent several years reconstructing the entire animal.

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[1] Later reclassified as *Neoparadoxia Repenningi* (JMD)

GL [00:52:01] When the spring of 1966 came along, at first we had two-thirds of the linac ready and by May the whole machine was completed. After checking out all the equipment, May 20th was scheduled for trying to turn on the beam. It was a glorious day for me and for Vic Waithman, a member of the Operations Department under Vernon Price because we were put in charge of the entire operation. All the tunnels were closed and we started around 4:00 PM to take our positions in CCR , the Central Control Room in Building 3. We had a joint staff of about 30 people. Standing behind us for most of the evening and night were Pief Panofsky, Dick Neal, Matt Sands (Deputy Director), Ken Crook, Dieter Walz and a few others. The turn-on proceeded in a number of steps as the beam coming out of the injector got stuck in Sector 10, then was steered further down to Sector 20, and so on. Finally, at 6:31 AM on May 21st, the beam finally made it to a profile monitor at the beginning of the Beam Switchyard. It was almost incredible!!!!

Nothing like that had ever been done before, and even though we were fairly confident, there was always some doubt that something could go wrong. Realize that in those days there were no computers in the control system of the linear accelerator. There are a few pictures in the SLAC Book taken by Walter Zawojski, which memorialize the event. Note, however, that soon thereafter, we made a major discovery.

JMD [00:56:28] What was the discovery?

GL [00:56:37] What we discovered was this. It had been known for several years that if you increased the beam current to about 800 ma in a one-section linac, an instability would set in that would turn the round beam spot at the end of the machine into a big horizontal or vertical stripe. We understood that this was due to the fact that the beam excited a parasitic mode in the accelerator structure, again similar to the mode in an rf separator That mode, within each pulse, tore the beam apart.

Since the maximum design current for the SLAC linac was only 50 ma, we decided that this effect would not appear. What we did not expect was that the same parasitic mode would raise its ugly head along the three-kilometer machine, through a cumulative amplification process .When we first turned the machine on, our beam had a peak intensity of 10 ma and everything looked fine, the spot at the end was round and small. But when we increased the intensity to above 15 ma, the effect appeared cumulatively along the machine's length and the spot went from a vertical stripe to a horizontal stripe and eventually to a big unstable round spot, unusable for physics experiments. At first we suspected that maybe our focusing quadrupoles along the linac were at fault but this did not turn out to be the case.

JMD [00:58:21] So then what?

GL [00:58:21] After some further experiments, it became very probable that we were dealing with a microwave instability. But how exactly did it work? A couple of weeks later, Pief Panofsky had to go to Washington and on the plane, being a very good mathematician, he figured out the first model for what we eventually called "cumulative beam breakup (BBU). The model predicted that the growth of the instability was a function of length of the accelerator, length of the beam pulse, and beam intensity (i.e. current). After Pief came back and we discussed his theory, a huge two-pronged effort was set in motion. Richard Helm who was one of our best beam dynamicists, began to make very careful computer calculations of the effect and I, together with another ten people in my department, was put in charge of a long series of experiments to study what was going on. These experiments actually went on for almost three years during which we eventually understood exactly how the BBU instability grew and how it might be controlled. The linac accelerating frequency was 2856 MHz and the BBU resonant frequency induced by the beam appeared at 4140 MHz in the first six cavities of **every** three-meter accelerator section. Part of an entire chapter on beam dynamics in the SLAC Blue Book is devoted to this work.

JMD [01:03:55] OK.

GL [01:03:56] The good news by 1967 was that the physicists could easily begin to do excellent experiments with the 15 ma beam at about 16 GeV. The bad news was that if they wanted to get to 50 ma, the original specification, we couldn't do it.

JMD [01:04:57] So what did you do next?

GL [01:04:58] First, Dick Helm used all our experimental results on the machine to adjust the parameters in his model. Once we had a coherent and predictive theory of BBU, we tried to figure out what remedies would be the most effective. We came up with two answers. One was to improve the focusing along the machine by never letting the beam cross-section grow too much. This could be achieved by redeploying the 90 or so quadrupoles along the 3 km accelerator. We embarked on this approach but it took several months to achieve it since one had to stop operations to enter the tunnel. Eventually this redeployment allowed us to increase the current to about 25 ma. The second remedy was to detune the first six cavities of two-thirds of the accelerator sections so that one third would resonate at 4140 MHz, one third at 4142 MHz and one third at 4144 MHz. This detuning would significantly reduce the cumulative nature of the BBU effect. Dick Helm calculated the best algorithm to achieve this effect, and we then had to put it into practice. It was very tricky. We ended up making a “dimpling” tool which could apply a slight pressure on the relevant six cavities so as to change their resonance by the desired amount from the outside without ever losing the accelerator vacuum. The leader of the “dimpling” operation was Harry Hoag who painstakingly, with technician Ernie Frey, worked his way along the three kilometers during down-times of the machine. This operation took more than two years or so. By the time it was completed, the current in the accelerator could reach 80 ma without BBU! It was a true victory of how the scientific method works by a combination of theory and experiment!!!

JMD: You mentioned Dieter Walz earlier. How was he involved in this saga?

GL: What happened was that with these much higher currents, we wanted to know if the beam could damage protective collimators and stoppers in the End-Stations. Dieter, who had designed many of these devices, built a robust meter-long tungsten rod and installed it in one of the beam lines. We then produced an 80 ma beam with an power approaching 1 MW and bombarded the tungsten rod. Within a five-minute exposure, the beam had bored a hole through the entire stopper. This alerted us to the radiation danger of such electron beams and caused us to design an entirely new beam containment safety system to protect personnel and equipment. Dieter became instantly famous!

JMD [01:09:11] Oh, wow!.

GL [01:10:13] By around 1969-1970, Dick Taylor and his group's experiments in End-Station A was well on his way to the discovery of the quarks. Meanwhile, always planning ahead, Pief Panofsky was asking himself and us what the next step should be to upgrade the SLAC accelerator. It so happens that at that time a new accelerator technology using " rf superconductivity" was being developed at HEPL, Stanford, and it appeared that if it progressed enough, one could convert the entire copper pulsed SLAC linac to a continuous-wave 100 GeV electron accelerator made out of niobium. Not obvious, but it was a gleam in the eye. Perry Wilson who was then working at HEPL transferred to my department at SLAC to work on this new project.

Also, in August 1969, an important event took place in my life: I got married to Gilda!