Jean Deken: [00:00:00] OK. This is Jean Deken speaking to Gregory Loew on Friday, November 22nd, 2019. This is part two of our oral history interview.

DEKEN: Now, while you were fixing the BBU problem, what else was happening?

LOEW: One of the major activities we embarked on in 1967 was to create a record of what we had just built, the book with which you are well familiar on "The SLAC Two-Mile Linear Accelerator." For this purpose we regularly met every morning at eight o'clock with Dr. Neal, myself, Harry Hoag and Doug Dupen, We first outlined the table of contents of the book and assigned each chapter to their presumptive authors, about 30 of them, giving them a timetable to accomplish their tasks. As the chapters came back to our editorial group, we reviewed them carefully and sent them back to the respective authors with suggested changes and corrections. From start to finish, this activity took about one year!

Also during 1967-1970, we began to deliver beams to all the SLAC experimenters. Thanks to switching magnets in the beam switchyard we could distribute 360 pulses per second to various experiments. The LBNL bubble chamber in the C-line could accept one pulse per second, Group A under Richard Taylor and Group C under Burt Richter in End Station A could accept as many as 180 pulses per second, and the rest could go to End Station B to Marty Perl and the Streamer Chamber in End Station C under Bob Mozley. Both of these beams used my RF separators to select the beams. This was a challenging exercise that required many operational refinements.

Another important thing that happened in 1970 was that Burt Richter finally got the agreement from Pief Panofsky to build the SPEAR electron-positron "asymmetric" storage ring, a project that he had been proposing for seven or eight years to the AEC without success. Pief finally decided that we wouldn't need the approval from the AEC if we built it with equipment funds, which the lab already had accumulated. Over a period of two years or so, we spent about five million dollars of this equipment money, and we turned on that machine in 1972. This placed another difficult demand on the linear accelerator that now had to deliver both electron and positron beams to the storage ring.

LOEW: Finally, in 1970, I got involved in the first major VIP visit at SLAC. This happened because I spoke French fluently and had established a steady relationship with the French Consul in SF, Monsieur Batault. He had sent me many French visitors and scientists over the years to show them the lab. One fine day in January 1970, Monsieur Batault called me and said that 'this time he had a different type of visitor to offer: President Georges Pompidou of the French Republic, who wanted to visit SLAC. Would you be willing to take part in that visit and help us organize it?' I of course felt very honored by this proposal and immediately discussed it with Pief who enthusiastically approved the idea and put me in charge of its implementation.

DEKEN: [00:29:40] So, what was Pompidou's interest in SLAC? What was his visit?

LOEW: [00:29:49] He wanted to come because, as the new president of the French Republic, he was very interested in science and industry, and he wanted to promote science in France. As a result, he was invited to NASA, and he was also invited to SLAC. The preparation of the visit, set for February 27th, took about a month. During most of January, we met with people, either at SLAC or in San Francisco, who were working for the French Consulate. Ronald Reagan was the governor of California at the time and although he wasn't planning to come to SLAC, he was part of the visit in San Francisco with the French president. The chief of protocol in San Francisco had to work with me to make sure that everything would go smoothly. To get the president from San Francisco to SLAC, a bus or cars would be too slow, so it was decided to bring the President from the Presidio, which had a heliport at the time, and get him and his whole delegation of ten other people, including the French Foreign Minister Maurice Schuman, to SLAC by helicopter. I still have a brochure which I prepared for the event in both English and French. I don't know if you have ever seen it?

DEKEN: [00:31:48] I don't think I've ever seen it.

LOEW: [00:31:52] I'll show it to you some day, it's a little part of our history. Anyway, I finally went to sleep in San Francisco the night before at a hotel close to the Fairmont, where he was staying, and where I was supposed to meet him. There was a slight confusion caused in his delegation because of the multiple choice of elevators he could use to meet me, but in the end, everything worked out. We made it to the heliport where two helicopters were waiting for us. I got on the first helicopter and sat next to President Pompidou. During the flight we chatted and I explained to him what he was going to see at SLAC. When we landed on the green at SLAC, there were all kinds of visitors, including Vicky Weisskopf who had come from CERN to participate in the visit, and other people like Pete McCloskey, our local Congressman at the time. There were also invited students from Stanford; among them the student body president. Anyway, we started the tour of the accelerator which went very smoothly. Dr. Neal, Pief and I accompanied the whole delegation. I was the interpreter. We first met them in CCR, then the Central Control Room. Did you ever see the pictures of that?

DEKEN: [00:33:44] Yes. Yes, I've seen the photos

LOEW: [00:33:47] Well, Panofsky gave a lecture on how the accelerator worked in English, and I translated it into French for all these people: it was glorious. Such an honor to do this! We then went down to the Klystron Gallery and from there to End Station A. The visit ended in the Orange Room, which was our main conference room at the time. We had a good conversation with the President and we gave him a Beam Tree and a piece of the accelerator.

At the end of this visit, I went back by helicopter with President Pompidou to San Francisco, and I attended a lunch at the Fairmont Hotel, where he gave a speech, and he and Madame Pompidou met with Governor Reagan and his wife Nancy.

This could have been the end of the story except that about six months later, I went to Paris on vacation and the Consul arranged for me to again meet President Pompidou at the Elysée Palace. As a result I had a 15 minute conference with him there! What a memorable experience! It was a sideline to our technical work but it was also the first time I had ever met with the president of any country.

DEKEN: [00:35:55] Right.

LOEW: One avenue we explored involved the idea of superconductivity and, a few years down the road, of converting the entire copper accelerator structure to a superconducting material .This would have allowed us to do two things: one, to increase the 1% duty cycle of the existing 360 pps machine to almost 100%, and the other, to perhaps increase the total energy of the beam from 25 GeV to 100 GeV within the same two-mile site.

For this purpose, a group was formed within my department to study superconducting materials and to see if we could ever design a structure capable of supporting a gradient of 30 megavolts per meter. People at HEPL at Stanford under the leadership of Alan Schwetman were studying the same idea, optimistically, hoping to convert the Mark III linac to a superconducting machine. Perry Wilson who was working with them was very interested in coming to SLAC, so we hired him into my department. Harry Hoag and a whole bunch of other engineers joined the group. Perry Wilson had started to study superconducting lead on Campus but by that time it had become clear that pure niobium was superior so we switched to studying niobium cavities at various frequencies, various sizes, and various methods of machining and forming the niobium. For about two years or so, we tried to get much higher field gradients in test cavities than what we had achieved in the existing linac but we completely failed. There were too many technological obstacles! We got to one or two megavolts per meter, which ironically was less than in the current linac, so the hope of getting a higher energy superconducting machine was completely dashed. And so finally we had two or three meetings with Pief Panofsky at which we showed him our results. and he said: “I give you another two months, and if you don't have better results, we are going to drop that program because it's too expensive.” And that's exactly what happened. After about two months or so, in 1971 or 1972, the program was discontinued. BTW, the people on campus were not making much progress either, and groups in Germany and at CERN were also trying but making very slow progress.

LOEW: Our hope of increasing the energy of our machine in some way had not gone away. After a while, we came up with another idea which was called the "Recirculating Linear Accelerator" or RLA for short. The idea was to bend the beam which came out of the linear accelerator at about 20 GeV, bring it all the way back to the beginning of the machine through a separate line to re-inject it a second time and accelerate it to 40 GeV. The idea was not simple in that it required a lot of beam and RF manipulation. After almost two years, we finally came up with a design, but it was very expensive, probably close to $100 M. When we proposed it to the AEC, they turned us down, so we were stuck! We had no way of increasing the beam energy except by increasing the number of klystrons (also very costly!) or increasing their output power by some kind of RF pulse compression. I talked to my colleagues Perry Wilson, Harry Hoag and David Farkas about this, but I admit I had absolutely no idea of how to do it. At that point I went on vacation to Europe for a month. When I came back, these three guys said to me, ”Hey, Greg, it looks like we have a new idea, which we want to propose and see if we can make it work.” I was very excited. They explained to me that the idea was to increase the power of the klystrons, not by increasing their output but by compressing their pulses in downstream resonant cavities and making the pulses shorter but taller. The system was baptized as SLED, "SLAC Energy Development".

DEKEN: [00:15:05] Oh, okay.

LOEW: [00:15:13] We first built and installed a couple of large test cavities at the output of one klystron and using an extra 180 degree phasing trick to charge them and then discharged them into the accelerator, we were able to increase its effective power by ~70%, albeit with a shorter (compressed) pulse. Once we convinced ourselves that this single prototype worked soundly, we decided to install it on all 240 klystrons of the machine. Each pair of cavities cost more than $10,000 and it took five years to complete the project with operating funds. Herman Zaiss, the then-head of the Fabrication Department, was put in charge of the manufacturing. There was one extra complication with this system in that we wanted occasionally to be able to detune the cavities to operate the accelerator without SLED, and that required a trick we called “acupuncture.

DEKEN: [00:17:51] Acupuncture?

LOEW: [00:17:53] Yes. We invented a system whereby, upon command, a long tungsten needle could be inserted temporarily with a piston (in vacuum) into each cavity. As a result of the insertion, the cavities were completely detuned and the system was bypassed (so-to-speak “unsledded”), causing no compression nor amplification.
Harry Hoag, David Farkas and I spent many nights in the Main Control Room (MCC) as the installation of the cavities progressed and we could gradually increase the energy of the machine. During this period, the Klystron Department was also able to increase the power output of their tubes. Combined with SLED, by about 1979, we had gone from 21 GeV to about 30 GeV, which enabled several particle physicists to extend the reach of their experiments. Superconductivity had not worked, but necessity had been the mother of invention, SLED!

LOEW: By any measure, these were the heydays of SLAC physics. As I mentioned earlier, five or six experiments were running in parallel. In End Station A, Richard Taylor and his group were concentrating on their inelastic scattering experiments of electrons on a liquid hydrogen target and three analyzing spectrometers. By 1972 they had plotted their scattering data with help from theorist Jim Bjorken and determined that protons inside the hydrogen contained three quarks. This confirmed the image of “scattering seeds inside the raspberry jam of the nucleus” and the physical reality of the theoretical quark model predicted by Murray Gell-Mann a few years earlier. For this pioneering work, Richard Taylor, Henry Kendall and Jerome Friedman received the Physics Nobel Prize in 1990.
Meanwhile, another spectacular discovery was made at the SPEAR electron-positron storage ring built under the leadership of Burton Richter. SPEAR had been operating for about two years without any major surprises, until one night in November 1974 they hit the jackpot. Tuning the beams to a joint energy of 3.1 GeV recommended by Marty Breidenbach and Roy Schwitters on the suspicion of a statistical bump in the data six months earlier, they hit upon a gigantic resonance of particles, soon to be recognized as a pair of charmed quarks and anti-quarks. The particle was subsequently called J/Psi, the J stemming from a quasi-simultaneous discovery by Sam Ting with protons at Brookhaven National Laboratory, and the Psi because the signal on the SLAC computer screen looked like the letter Psi. As you probably know, two years later in 1976, Burton Richter and Sam Ting jointly received the Physics Nobel Prize for this discovery of the fourth quark.

DEKEN: [00:23:59] Yes.

LOEW: [00:23:59] But this was not the end of discoveries at SPEAR. By 1973 it was realized that the X-rays being radiated by the stored beams as they circulated around the machine might possibly be used for another type of physics. Herman Winick, Gerry Fisher and Ed Garwin suggested that if a tangential port could be installed at the ring, these X-rays could be extracted and sent to an experimental target.[1] The idea took off like wildfire and it was the birth of the exploitation of synchrotron radiation, a technology that has now

[1] Suggestion was actually made by Gerhard Fischer, Ed Garwin, Bill Spicer and Seb Doniach. See Richter, “Colliding Beams at Stanford” See also Doniach to Panofsky 6/18/68, Subject: Use of Cyclotron Radiation from Storage Ring for Solid State Studies (SSRL-Winick document).
Also: Panofsky Papers, Series II, Subseries E (box 14), Synchrotron Radiation file for 1961 HEPL memos and papers on same topic by Panofsky and O’Neill. Also, email correspondence A. Bienenstock et al. to J. M .Deken 2011; and Hallonsten, Olof, Small Science on Big Machines 2009 p. 112-115.
See also: http://www.slac.stanford.edu/history/ssrp.shtml

spread to labs all over the world And then, in 1975-1976, after painstakingly analyzing his colliding beam data, Martin Perl discovered the Tau lepton, somewhat like a needle in a haystack. The Tau meson was the third in the existing family of the electron and the muon, for which Perl received the Physics Nobel Prize in 1995. This amazing series of experiments and discoveries encouraged SLAC to design and build the higher energy (30 GeV in the center-of-mass) electron-positron storage ring PEP, which stood for Positron-Electron-Proton. The idea was to eventually add a ring to collide the electrons with protons like DESY did in Germany, but in the end this plan never materialized

LOEW: In the late 1970's Burt Richter spent a sabbatical year at CERN and with his success at SPEAR, he convinced CERN management to build a large electron-positron storage ring to produce and study Z particles at about 90 GeV. This suggestion gave birth to the LEP (Large Electron-Positron) storage ring at CERN. However, when Burt came home, he felt that SLAC should not let CERN win this battle alone.

For a year or so, Burt thought that to compete with CERN, we would have to build similar large electron-positron storage rings that unfortunately would not fit on the existing SLAC site, and that they would require superconducting cavities to keep the beams accelerated inside. For a couple of years, Burt asked me to resume the superconducting cavity research we had abandoned seven years earlier, but this still did not look very promising. After 1979, Burt gave up this plan and came up with the idea of the SLAC Linear Collider (SLC).