Jean Deken [00:00:00] This is Tuesday, November 26, 2019, and this is Jean Deken talking to Gregory Loew…

Gregory Loew [00:00:30] Our next topic is the SLC project but before we discuss it, I want to cover a few other subjects.

Deken [00:00:56] Okay…

Loew [00:00:57] In the operation of linear accelerators, one thing which I told you about is that the bunched beam generates a field in the structure and if the beam is intense enough, this field can deflect the particles sideways.

Deken [00:01:16] Yes.

Loew [00:01:17] And that is the beam breakup mode. But long before this happens, any electron linear accelerator has another property, that with a multi-bunch beam of maybe a hundred or a thousand bunches, it takes a while for the beam energy to stabilize because the first bunches that go through also create a field in the direction against the acceleration called the beam loading field. Dr. Neal had written one seminal report on the subject and I also wrote a couple of articles later. For example, if the klystron's energy that is injected into the accelerator sets up an accelerating field, say of 10 megavolts per meter, once the beam is established, it may be reduced by10 percent or nine megavolts per meter. What we had never studied or measured, however, was the transient beam loading effect of a single bunch. One of the questions I then got involved in was to try to see if we could measure this effect in the three-kilometer linac. So, we figured out an experiment, and in this experiment, I was helped by Ron Koontz and Roger Miller in my department. They set up the injector of the machine so that instead of spewing out a whole series of bunches in the pulse, they would only launch one at a time. We would look at the energy of the bunch at the end of the three kilometers and see by how much it had decreased. And that took quite a bit of doing. I think it was probably one of the most beautiful experiments we ever did with the linac! Perry Wilson and later Karl Bane theoretically corroborated our results.

During the years between 1975 and 1979, I also got involved in another project triggered by Henry Kaplan, Professor of Radiology at the Stanford Medical School. His interest was cancer therapy. It turns out that at the time, some scientists at Los Alamos National Laboratory had noticed that instead of doing this kind of therapy with electrons or x-rays, there was one other technique using pi mesons. The property of pi mesons was that if you directed a pi meson at the body of a person with a tumor somewhere, you could adjust its energy so that it would not damage any tissue on the way into the body except where the tumor was located. Kaplan got hold of me and others at SLAC and asked us if there was any way that we could help him generate these pi mesons.

Now, by not a total coincidence, there was also a group at HEPL at Stanford that had designed a magnet capable of taking a pi meson beam from an accelerator and focusing it on a patient. The project at SLAC was finally assigned to me. I designed an electron linear accelerator that could generate this pi-meson beam. Dieter Walz whom I knew well at this point helped me with the interface between the linac and the pi mesons entering the focusing magnet leading to the patient. We spent about nine months designing the whole system, in constant contact with Dr. Kaplan. Note that in those days many linear accelerators were being used to treat patients with x-rays and electrons, but these beams were not as good because they always damaged intervening tissue on the way to the tumor. But these machines such as the Clinac 4 and the Clinac 6 produced by Varian cost only one or two million dollars. In contrast our project using pi mesons cost on the order of $14 Million. Dr. Kaplan took our elaborate proposal to the National Cancer Institute where it was well received. Unfortunately, in the end they decided that the country could not afford this machine, given that many of them would have to be produced and installed.in various hospitals. This decision put an end to our project.

Around that time, in addition to me running the Accelerator Physics Department, Dr. Neal also asked me to become Deputy Director of the Technical Division, a position that broadened my responsibilities and horizons considerably. I actually stayed in that position for the next twenty years.

In early 1979, I travelled to Charlottesville, Virginia for a UVA Conference and presented a proposal for an electron pulse stretcher ring to serve as a machine for precision nuclear physics experiments. I will have more to say about this a little later. During the same trip I stopped in Washington DC to see their accelerator exhibit at the Smithsonian to which I had contributed.

Perhaps my most extraordinary experience of 1979 occurred when I was invited with a Stanford/SLAC group to teach a summer accelerator school in Hofei, China. The group consisted of Art Bienenstock, Herman Winick and Ben Salzberg from SSRL, and Phil Morton, all with their wives, and me from SLAC. Mao Zedong had died in 1976, the Cultural Revolution was coming to an end, and China was beginning to open up to the West. Our trip took us into the PRC from Hong Kong to Guangzhou and Beijing, the Great Wall, the Ming Tombs. In Beijing, in addition to lecturing at the Nuclear Physics Institute, I was taken to see the Democracy Wall where, in opposition to the rule of the Gang of Four, the Chinese people were expressing their opposition to authoritarian rule. In Hofei we stayed and taught classes for a full week. The students were incredibly attentive and eager to learn. Among others, this is how I became acquainted with Wang Juwen who a year later got a Chinese scholarship to work with me at SLAC. Outside the Hofei institute, hundreds of workers on the street pulled primitive carts and building materials with their bare hands like in the Middle Ages. In retrospect, this is how China pulled itself up by its bootstraps during the subsequent twenty years. Our trip ended in the totally overpopulated Shanghai and exit via Hong Kong back to the States. An unforgettable experience!

This trip marked the beginning of my frequent participation in scientific collaborations all over the world, the Soviet Union, the PRC, Japan, CERN, DESY, Saclay, Orsay, Mexico, Brazil, Argentina and eventually South Korea. The only time I refused to attend a meeting in China was in 1989, right after the Tiananmen massacre.

Loew [00:13:23] We are now approaching 1980 and the birth of the SLC, the Stanford Linear Collider. By then Dr. Richter had given up on the idea of building two large electron positron colliding beam rings under Menlo Park as I described earlier. Yet he still wanted SLAC to compete with CERN in the research on the Z boson

And so, he came up with this idea of having an electron beam and a positron beam generated and coming out of the linear accelerator, running along two arcs in the form of a tennis racket, and meeting at the top of the racket in a detector. This idea was very daring, and some of us had serious doubts that it would ever be funded because it was so difficult to realize. But Richter had enormous influence with the DOE, and he eventually convinced them to give us over a hundred million dollars to realize it.

Now, to build this project, all kinds of new things had to be done. One issue was that to reach the mass of the Z predicted and later measured at CERN around 91 GeV, one needed to collide bunches of electrons and positrons of almost 50 GeV each. At the time the SLAC linac energy with the inclusion of the SLED system was just over 30 GeV. It forced us to build 240 new klystrons with peak output power of 64 MW, with twice the output and pulse length of the existing klystrons. This was a herculean effort for us. Second, the collisions had to be planned, one bunch at a time, an echo of the experiment we had done with electrons only, about 6 years earlier. Another feature that was necessary for the SLC to work was that the bunches had to be very, very much smaller in diameter than the bunches that would normally come out of the linac. And the only way we knew how to do it at the time was to build two damping rings, one for positrons and one for electrons at the beginning of the linac, where we could decrease the so-called emittance of the bunches. If the bunches were too big, then the probability of creating Zs at the collision point would be too low…

Deken [00:17:39] Okay.

Loew [00:17:40] Another problem was that the positron beam we had at the time was much too low in energy and intensity, and once created in Sector 19, it had to be returned to the front end of the linac to get damped in its damping ring. When the two bunches finally got accelerated to the end of the three kilometers, one had to go one way along the tennis racket, and the other the other way. The ground in that area of the SLAC site was not flat, so we had to build bending magnets that would follow the terrain, which was a very daring idea. Finally, you had to build a huge new experimental hall to house the final focus system and detector where the two bunches would collide head-on. Two detectors were used in succession: the Mark 2 coming from PEP and the brand new SLD shepherded along by Marty Breidenbach.

Anyway, all these innovations were part of the SLC and every one of them required a huge effort. Nothing worked correctly the first time around. Every morning at 8:00AM, including weekends, we held a one-hour meeting in the Main Control conference room with about 40 participants to review our progress (or lack thereof) during the preceding 24 hours. Many able scientists from other countries came to help us, such as Jean-Pierre Delahaye and Witold Kosanesky from CERN, Jacques Haissinsky from Orsay and Nobu Toge from KEK, Japan.

Deken [00:22:50] Okay…

Loew [00:22:51] I believe that in your Archives you have a complete chronology of all the work we did. It took about ten years to produce the first Z particles. The final battles were fought in the non-planar arcs and in the final focus. Alignment of the bunches at the collision point was essential because if not perfect, the bunches deflected each other instead of colliding. After colliding they blew each other up because of a phenomenon called beamstrahlung.

Anyway. before I finish with the SLC story, I want to backtrack to some independent events that I was involved in during the 1980's.

Deken [00:25:36] Okay.

Loew [00:25:37] Right at the beginning of this project, around 1982, Dr. Neal turned 65, an age at which many people retired. Although he was not obliged to do so, he decided to step down from his position as Director of the Technical Division, and a year later retired from SLAC. Pief Panofsky, at that point as well, realized that his retirement age would come in the next few years, and figured out that he would designate Burt Richter as his successor. So around late 1982 or early 1983, when Richard Neal stepped down after 25 years, Burt Richter was first appointed to replace him, and he became my boss as I was the Deputy Head of the Division.

Meanwhile, as already mentioned, I had helped a group of nuclear physicists back East with a proposal for a new accelerator project capable of producing a continuous electron beam (i.e. not pulsed like SLAC) for nuclear physics studies in the range of 2-3 GeV. My idea consisted of an electron linac and a storage ring from which stored electrons would be peeled off in a continuous way. Two other additional ideas were proposed but mine turned out to be the preferred one. When the lab to build the project was created, I was interviewed to head it but luckily, I was not selected because my life at SLAC was much more interesting. In the end Herman Grunder from Argonne National Laboratory became director of CEBAF (Continuous Electron Beam Accelerator Facility), based in Newport News, eventually renamed Thomas Jefferson Lab. At first, he was going to proceed with my approach but then, he got wind of the gradual progress of rf superconductivity and chose to plan on using this technology to generate the continuous beam in a ring. This was a daring idea that took more time to develop but eventually paid off. Ten years later, the continuous beam became a reality!

It is worth mentioning at this juncture that another much larger project began to gain life: the Superconducting Super Collider, or SSC. LBNL had surfaced the idea in 1976, later on Maury Tigner from Cornell took it up, and eventually the project was moved to Waxahachie, Texas under the leadership of Roy Schwitters, originally from SLAC. The ten-year history of the SSC until it was rescinded by Congress in 1993 is well documented elsewhere. After the demise of the SSC, CERN decided to build the Large Hadron Collider or LHC.

I was involved with the SSC project twice. On the first occasion, sometime around the early 1990s they invited me to Waxahachie to share with them my experience with the procurement and consumption of electric power for SLAC and our consortium with LBNL and LLNL (more on this later). The second occasion became much more time consuming and took place for a couple of years between 1993 and 1995. When the SSC was cancelled, the lab had acquired a couple of billion dollars in all sorts of equipment, instruments, machine tools and supplies that now were no longer needed. The DOE was asked to dispose of all this equipment by distributing it in a free-of-charge and fair way to the other national labs and government institutions. They put one of their respected staff, Earle Fowler, in charge of a committee of about ten people to perform this task. I was chosen as the SLAC representative. We first had to go to Waxahachie several times to make a comprehensive list of the disposable equipment, and then travel to Washington about 6 or 8 times to figure out how to distribute it as fairly and equitably as possible. I remember that at the last meeting SLAC was given all the SSC's cafeteria furniture and LBNL received their large Espresso machine!

Meanwhile, another noteworthy event had taken place at SLAC. In 1983 during the Cold War, it so happened that Edward Teller at LLNL promoted his Star Wars Program and convinced President Reagan that his nuclear-generated x-ray laser could destroy incoming Soviet ICBMs in outer space. Teller's proposal created a huge controversy. Two or three LLNL scientists came out in the open and claimed that the whole idea was totally flawed. One of them lost his security clearance in the process. However, LLNL management nevertheless came to Stanford and SSRL (not organizationally part of SLAC at the time) to propose testing some instruments necessary for the development of the weapon with the SPEAR synchrotron radiation beams. When Stanford said that no classified work was permitted at the University, LLNL changed its approach and said that the instrument tests per se would not be classified.

When our SLAC staff learned of this, a huge uproar ensued, and more than 600 members signed a petition saying that they did not want to participate in delivering beams to SSRL for these experiments. I was one of them. The objection was based on "involuntary servitude" forcing us to work on something to which we deeply objected, on moral grounds. But Donald Kennedy, President of the University, and Art Bienenstock, head of SSRL, said that they could not turn the work down since it would not be classified. Panofsky, Director of SLAC, could not intervene in our favor, even though he sympathized on principle with our position. So SSRL accepted to do the work. We were all very upset.

Several weeks or months later, for some reason unknown to me, LLNL decided not to do the tests at SSRL. Somehow, we lucked out by default. It was quite an experience!

By 1984 the SLC was well on its way, Panofsky stepped down, and Burt Richter became Director of SLAC. Following this, Gustav-Adolph Voss from DESY and Maury Tigner from Cornell were successively offered the job to replace him as head of the Technical Division, but both turned it down. In the end, Kaye Lathrop, who made his reputation as an able computer code developer at Los Alamos National Lab, was hired for the job.

Unfortunately, Lathrop had no accelerator experience, and he had a very authoritarian management style. He turned out to be a poor match for the job, and for the nine years he stayed at SLAC, he was never technically very effective. When he left, Ewan Patterson replaced him.

Now, coming back to the SLC, the Mark II detector from PEP was moved to the Collider Hall under the direction of Jonathan Dorfan. The SLC produced its first Z in April 1989 and during the summer produced about one Z per hour. In October of 1989 the Loma Prieta earthquake caused some misalignment of the accelerator. Fortunately, I had requested a complete recalibration of the 300 Fresnel alignment lenses along the linac just before the quake and we were able to correct the misalignments in less than two months. Meanwhile, the Mark II vertex detector had been replaced and various other improvements had been made to the entire SLC. The run from June to October 1990 logged a total of 298 Z decays, which produced two seminal results: the mass of the Z was determined to be 91.0 + or – 0.3 GeV, and the energy width of the decay indicated that between 2 and 3 and certainly less than 4 neutrinos were involved. These two results were announced just a few days before similar results came out of the LEP machine at CERN. It was a close race but it vindicated Burt Richter's plans for the SLC. By that time, Nan Phinney was put in charge of the running and development of the entire machine. At some point during winter we had an unusual cold spell and many of the glass water flow monitors froze and broke, causing a miserable situation along the entire accelerator gallery. It took a huge effort to recover from what we called the Big Freeze!

Between February and March of 1991, the SLAC Large Detector or SLD built under the direction of Marty Breidenbach and waiting in the Collider Hall pit, was inched into the SLC Final Focus beamline, replacing the Mark II. The SLD logged about 750,000 Z's in the next seven or so years. One of its major achievements was the measurement of the so-called Weinberg angle, of key importance in the theory of Weak Interactions. This measurement was made possible by a system invented by Charlie Prescott and Charlie Sinclair to polarize the electrons coming out of the SLAC injector and comparing the Z cross-sections as the polarization was flipped by 180 degrees.

The gradual success of the SLC not only vindicated Burt Richter's idea at SLAC but over a period of ten years encouraged other labs in the world to come up with truly linear collider projects to discover the Higgs particle. DESY in Hamburg, Germany came up with two competing proposals, one using conventional S-Band technology and the other, pioneered by Bjorn Wiik, using superconducting RF sections. CERN came up with the two-beam CLIC system, BINP in Novosibirsk, KEK in Tsukuba, Japan and SLAC came up with room-temperature X-Band technology. These efforts led to annual international linear collider workshops all over the world. I attended all of them and became a very frequent flyer on United Airlines.

Meanwhile, another series of events had taken place at SLAC that also kept me very busy.

On June 27,1989, Secretary of Energy James D. Watkins had announced a ten-point initiative to strengthen safety, environmental protection, and waste management at Department of Energy (DOE) facilities. To support this initiative, the Secretary established independent "Tiger Teams" to assess environmental, safety, and health (ES&H) programs. Such a Tiger Team of about 50 members came to SLAC from October 7 through November 5,199l and as a result drafted a list of about 200 concerns regarding the conduct of ES&H and management procedures at the lab. Although none of these concerns were considered life-threatening, they required a meticulous response in the form of a corrective plan outlining the specific strategies and actions to be taken by SLAC /SSRL, DOE-SF, and DOE-ER in response to the Tiger Team findings and concerns. Burt Richter asked me to coordinate the SLAC/SSRL part of the response, which took over my life for several months. The complete report was eventually published in October 1992 as the SLAC Corrective Action Plan. It committed to remedy the lab's deficiencies in a series of steps spanning five years at a cost of about $19 million.

Coming back to our linear collider preoccupations, our effort at SLAC prompted me to encourage my PhD student, Juwen Wang, to make a careful R&D study of high-gradient copper structures the central theme of his thesis research. Some of our seminal results are fully documented in his PhD thesis document.

The Berlin Wall had fallen on November 9th, 1989, and a Linear Collider Workshop was scheduled to take place in Serpukhov/Protvino, Russia in August 1991. The meeting had to be postponed at the last minute because of the turmoil caused in Russia by the coup against Gorbachev. The meeting was rescheduled for September after the coup failed. It was another one of these unforgettable trips, both technically and politically. While visiting one of the large labs in Protvino on a Friday afternoon, I was very surprised to see that this place was practically deserted. My guide, an engineer, when asked where all the workers had gone, told me that on Fridays they went home to make some money on the side. He explained to me that Communism failed because people were tired of working "for the collective system" for menial salaries when they could earn much more money by working for themselves! When I got to Moscow after the workshop, an old Russian friend of mine, Nicholas Sobenin, took me to a park where Muscovites had dumped dozens of statues of Lenin, Stalin, Dzerzhinsky and other Soviet "fallen heroes" for exhibit in a corral. Near Red Square, a statue of Karl Marx had been left standing, but he had a sign hanging around his neck saying: "Workers of the world, forgive me."

I then travelled to Leningrad because Burt Richter wanted me to visit a large magnet factory. It was a very interesting occasion because the next day, the official name of the city reverted to St Petersburg!

In Summer of 1993, I returned to Russia twice, once for a visit to Dubna, where they were interested in buying a SLAC klystron for their linac Irene, and a second time for a collaboration meeting under the JCC-FPM accord. By this time, the Soviet Union had been dissolved and Yeltsin was in power in Russia. As I arrived at the airport, the city was in a state of emergency because of a rebellion in the parliament and the occupation by force of the Ostankino TV station by a group of rebels. I was taken to the ITEP guest house and asked to hunker down inside. The next day, matters calmed down and the JCC-FPM joint meeting was able to take place.

By this time, our collaboration with the Japanese scientists at KEK prompted the Japanese government to propose a visit of the Japanese Emperor and Empress to SLAC. As it turned out, given my experience with the Pompidou visit at SLAC in 1970, I was put in charge of organizing this entire event on June 23rd of 1994. Hirotaka Sugawara, director of KEK after Professor Nishikawa, also attended the event. Again, this was an amazing and unforgettable experience! The event was a complete success except for a glitch with the elevator in the Collider Hall which prevented us from taking our guests down to the SLD floor. We had to quickly reorganize the visit at the top floor, but everything went so smoothly that our guests probably never realized what had happened.

Loew [00:52:56] In 1993 there were about seven different electron-positron linear collider proposals on the scene in the world and it was hard to figure out how they compared with each other. At this point I was asked by Burt Richter in conjunction with the other lab directors to form an international committee to conduct a comparative review of all these projects. The committee was called the International Linear Collider Technical Review Committee.

Deken [00:53:29] Okay.

Loew [00:53:31] It probably turned out to be one of the biggest challenges of my career.

Deken [00:53:39] So for the review, the review committee, did you put the people together who were on it?

Loew There were about thirty people on my committee: some were in Japan, some in France, some in Germany, some in Russia, some at CERN and some in the US. Once the committee was formed, we set up meetings all over the world and I traveled to each location for over a year to assess the various projects. By early 1995 we came up with the first ILC TRC report. The cost of each proposal was very high, and it became obvious that no single country would be able to afford its project by itself. So, collaborations formed around the world but the overall competition continued...

Meanwhile, back at SLAC, Jonathan Dorfan and Marty Perl had conducted competing research on smaller projects for the lab: a B-Factory and a Tau-Charm Factory. By about 1992 Dorfan's B-Factory won the competition and the DOE eventually approved the project for construction in the PEP tunnel for a cost of about $100 Million

I didn't work on the design of the B-Factory per se, but I did get involved with Elliott Bloom in the design of the injection system for both the electrons and the positrons, using many of the elements originally designed for the SLC. In the process I got to know Jonathan Dorfan quite well.

Eventually in 1999, Burt Richter stepped down as SLAC director, and Jonathan became director of the lab at the same time as the B-Factory was beginning to function, replacing the SLC as the main project on site. At this point, Jonathan thought of hiring Stewart Smith, a Princeton professor and collaborator on the B-Factory, as his Deputy, but when the latter turned him down, Dorfan decided to offer me the job after a trial period. And that is how in 2000, after 42 years at SLAC I became Deputy Director of the lab.

On a personal basis, the first year was quite difficult for me, because it coincided with my wife Gilda's very serious struggle with breast cancer. In early January 2001 Gilda died from complications with the disease. We had been married for 31 years.

Jonathan was very understanding of my situation and he kept me on the job. It turned out to be a wonderful collaboration for five productive years. Aside from helping him with the day-to-day activities of running the lab, Jonathan gave me the gigantic assignment of reconvening a second International Linear Collider Technical Review Committee. Carrying out this task was necessary because since 1995, the various international projects had made considerable progress and a new comparative study was in order. Of the six or seven original projects, three of them were still competitive: the TESLA superconducting approach led by DESY, the X-Band room-temperature approach led by the collaboration between SLAC and KEK, and the more futuristic CLIC project led by CERN. The first two approaches both proposed to start at center-of-mass energies of 500 GeV later upgradable to 1 TeV, aiming to discover the Higgs particle. For 500 GeV they were fairly mature, but they both had some unfinished business to attend. After making careful comparisons, our committee undertook the difficult task of ranking their various states of readiness for construction. This was the most difficult step, but I think we did a pretty good job. The work again took a lot of traveling all over the world and we published our report before the end of 2003.

One conclusion that resulted from the cost estimates in the report was that the international particle physics community could not actually afford to build more than one such linear collider in the world. As a consequence, ICFA, the International Committee for Future Accelerators, decided to form an Ad Hoc committee headed by highly respected Professor Barry Barish from Caltech to make a choice between the TESLA project and the SLAC-KEK X-Band project. That committee met for about six months and in August 2004 announced its final recommendation that the TESLA project be chosen. This was a big disappointment for SLAC and KEK but eventually, everybody accepted the decision and all the labs agreed to collaborate on the TESLA proposal, which was renamed the ILC. Over a number of years, the ILC design was considerably improved and the Japanese community recommended that it be built in Japan. Ironically, 16 years later, the ILC is still in limbo and has not been funded. In the meantime, in 2012 the Higgs particle was discovered at the LHC, partially depriving the ILC of one of its original purposes.

Several very important things happened during Jonathan's directorship. The most important was probably the successful 10-year run of the B-Factory with the BABAR detector. that in competition with KEK-B and the detector Belle confirmed the theoretical predictions of the Cabibbo-Kobayashi-Maskawa or CKM model.

Another important project that made great progress was the GLAST detector that was going to be mounted on a satellite in collaboration with NASA to serve as a gamma-ray telescope. The idea was originally proposed by Bill Atwood at SLAC. In addition to DOE and NASA, GLAST was supported by institutes in France, Germany, Japan, Sweden and Italy. General Dynamics was chosen to build the spacecraft. During its construction at SLAC, GLAST encountered some difficulties and Persis Drell, who by then was Associate Director of the Research Division, undertook the management of the construction effort until the problems were corrected. The full telescope, renamed the Fermi Telescope, was launched on June 11, 2008 when Persis Drell was already Director of SLAC. As of 2020, the telescope is still doing outstanding research.

Another major innovation that was launched by Jonathan was to broaden our research into Astrophysics and Cosmology. This step took some major effort to convince the University to let us hire two eminent scientists to lead the effort, Roger Blandford from Caltech and Steve Kahn from Columbia University. To house this enterprise, we were fortunate to get the financial support from Fred Kavli who paid for two buildings, one at Stanford and one at SLAC for the joint Kavli Institute. This move opened up entirely new opportunities for SLAC in the areas of the Cosmic Microwave Background (CMB), Dark Matter, Dark Energy and the construction of the Large Synoptic Survey Telescope (LSST).

In the area of Photon Science, the SPEAR storage ring was entirely rebuilt to produce lower emittance beams. The upgrade was called SPEAR 3, qualifying SSRL to become a 3rd generation synchrotron light facility. Moreover, SLAC greatly benefited from the idea proposed by UCLA Professor Claudio Pellegrini that the SLAC electron beam could be used to generate very powerful x-rays by passing the beam through a downstream long array of undulator magnets. This led to the birth of the Linear Coherent Light Source or LCLS, using the last third of the three-km linac.

The project was approved in 2002, and groundbreaking took place in 2006. The LCLS began to operate very successfully in 2009.

Other highlights of lab activities included a draconian review of our Environmental, Safety and Health (ES&H) practices. This followed a very unfortunate accident in 2004 in the Klystron Gallery during which an electrician from a contract agency suffered serious burns while installing a circuit breaker in an electrically energized panel.

Finally, in the area of communications, we were extremely lucky to attract the very capable Neil Calder, then Communications Director at CERN, to come and lead that effort at SLAC. His role at the lab made a huge difference in our internal and external relations. Jonathan, in this area, was able to enact a new behavioral code of conduct inside SLAC called the “Respectful Workplace.”

Aside from all these activities, there is something else I would like to report on. One of the matters in which I had gained some expertise during all my years in the Technical Division was budgets like the cost of Accelerator Improvements, Infrastructure Improvements and Operating costs such as Electric Power for the lab. In these areas, I worked closely with Larry Kral in the Division, and Eugene Rickansrud and Mimi Chang in the Business Division. In 1982 when Dick Neal retired, I took on all the responsibility of calculating the SLAC projected power needs and got involved with the consulting firm of Exeter Associates back East that helped with the procurement of this power. Note that at the inception of SLAC, Pief Panofsky had had the good judgment of contracting with the Western Area Power Administration (WAPA) for all our power. WAPA got most of its electricity from hydropower generated by the Central Valley Project (CVP), i.e. green power. Panofsky called it "socialist power." They also had an agreement with PG&E that in case they couldn't meet their obligations, PG&E would "firm up" their needs with fairly inexpensive supplements. Altogether, this was an excellent deal that meant that SLAC originally paid only a fraction of a penny per KwH. If the site was consuming about 40 MW, the annual budget was on the order of $1 Million per year.

Matters began to change during the two energy crises in 1973 and 1979. By the time I took over this activity in the Eighties, the SLC was going to require close to 65 MW, WAPA could no longer supply us enough power, and the PG&E prices had gone up considerably. At this point, Dale Swan and his group at Exeter Associates suggested that SLAC, LBNL and LLNL form a Consortium for the joint procurement of electric power. This was a very good idea because the needs of the three labs together exceeded 100 MW, which increased our bargaining power with the potential suppliers and qualified us as wholesale customers. At the same time, the Department of Energy and WAPA decided to participate in the construction of a third 220 KV Intertie transmission line from the Northwest, opening the door to bring cheaper electricity to California from Washington and Oregon. Exeter managed to land us some very favorable contracts spread over four or five years in the future. Our power budget at SLAC that during the SLC days could have been as high as $10Million/year sometimes dropped down to about half of that. Burt Richter, who thought it was my own doing, was always very grateful for that. Pacificorp, one of the companies in the Northwest with which the Consortium had signed a very long-term contract, benefitted our situation enormously during the 2003 California Energy Crisis, caused in part by the collapse of the Texas power company Enron and the sudden shortage of power in California. Even though SLAC, during the operation of the B-Factory, was using close to 60MW, the Consortium had some extra power to spare to sell on the open market at a good profit, which lowered our total budget for the labs. Even though we were not technically allowed to make a profit; the matter was eventually settled to the advantage of the labs. Another thing that happened during this period was that PG&E decided to sell all of its generating plants, including its nuclear reactors, and become strictly a power distribution business through its privately owned grid. After the B-Factory was closed down in 2008, the only large consumer of electricity at SLAC was the LCLS which together with SPEAR consumes only20-30 MW. In 2008, after Persis Drell became Director of SLAC, Roger Erickson took over my electric power responsibilities, but the set-up with the Consortium and Exeter Associates continues to this day. The only major difference is that, because of the long droughts in California, WAPA power is now as costly as market power.

During my last few years at SLAC I still worked on some ILC matters and travelled abroad quite a bit. One of my very interesting trips took me to a meeting in Bangalore, India headed by Barry Barish at the Indian Institute of Science where we tried to encourage the Indian Particle Physics community to join the world ILC collaboration. I also travelled to China a couple of times. The last time I went there was on the occasion of the 70th birthday celebration of Juwen Wang at his alma mater, Tsinghua University.

In April 2007, Panofsky encouraged me to organize a celebration of the 60th anniversary of the first acceleration of electrons in 1947 by W.W. Hansen at Stanford. This turned out to be a big affair, including a few scientists who were active that far back in time. I spent many hours at the Stanford Green Library which kept a huge collection of documents about Hansen's life and work at Stanford, as well as back East during WW2. I am very proud of the talk I gave at this symposium which memorializes the origins of klystrons and electron linear accelerators at Stanford.

Dramatically, Pief Panofsky, our hero, died of a heart attack on September 24th, 2007. Until the last day of his life, he came to SLAC and worked on Arms Control problems with Richard Garwin. In April 2008, SLAC organized a large memorial service for our beloved Pief at which I also gave a talk, remembering his work at LBNL, Stanford's HEPL laboratory, and the foundation of SLAC.

I had stepped down as Deputy Director in mid-2005 after Jonathan announced that he would reorganize the management of the lab with two Deputy Directors, one for Particle Physics and one for Photon Physics. After exactly 50 years at SLAC since 1958, I officially retired. Jonathan gave me a great party at my house. However, I kept an office at the lab, actually Joe Ballam's old office in the Central Lab Annex.

As Emeritus Professor, I continued to be active, ran the Monday SLAC Colloquium for several years, attended all the Faculty meetings, interviewed new Faculty members and created a power point talk about "Big Machines and Big Science: 80 years of Accelerators at Stanford."

Deken [01:29:06] Okay, so why don't we talk about your book, which you've done in retirement? … [1:33:39] What was the impetus behind writing the book?[1]

Loew [01:33:34] OK. The impetus came from several aspects of my life. One was that while I was involved in science for so many years, I often asked myself about how we actually knew what we were finding out, i.e. the domain of epistemology. Where does our sense of reality come from? What are waves, particles, where do we get the sense of color, and so on? How do the sciences actually work? What about language? What about mathematics?

Quite independently, around 1970, I was approached by my ex-professor Marvin Chodorow at Stanford. He was setting up a new series of seminars for freshmen and sophomores on a wide variety of subjects and he asked me if I wanted to participate in the program. Intrigued by the opportunity, I could have chosen a subject close to my activities at SLAC, but this was during the height of the Vietnam War that seemed to concern the entire student body. So, I took a chance and I proposed to teach a seminar on the causes of war. To my surprise, even though I was not known as an expert on the subject, my proposal was accepted. I began to study all kinds of subjects on the topic, including psychology, anthropology, the history of war, international relations, arms control, and so on. It was a tremendous education, and my seminars were so successful that I taught them for over ten years. In the process, I accumulated a huge amount of relevant knowledge but, I never had time to consolidate it in one place. Now, with the perspective of time, I could do that.

I started out by writing an article about science and epistemology, but I didn't know where to publish it. I talked to some people at MIT who got interested, but eventually I realized that I would do better with a book. Finding an agent was a losing proposition, so I finally had a very good discussion with Jane Friedman, an expert on publishing, who recommended that I talk to Mascot Books. So that is what I did, which was much better than self-publishing. They were very helpful. (And you, also, were very helpful with all of the copy-editing you provided.) Since I also wanted to have nice illustrations, I made a wide selection and Greg Stewart from SLAC, whom you know, helped me greatly to avoid copyright problems. All this final publishing effort took almost a year. But I finally made it! The title is: The Human Condition: Reality, Science and History, published by Mascot Books in 2019.

My career of 50 years at SLAC has been an amazing ride. The science was of course a major component, but none of this would have been as satisfying without the camaraderie with so many bright and wonderful human beings. Many of these people appear in the SLAC Blue Book. Dick Neal, Doug Dupen, Harry Hoag. and I were the editors. But I want to make sure that when this interview is read by future generations, they realize that SLAC was built with the brains and energy of many of my early colleagues such as Ken Mallory, Roger Miller, Dick Helm, Bill Hermannsfeldt, Otto Altenmueller, Bill Gallagher, Karl Brown, Martin Lee, Roger McConnell, David Farkas, Alan Wilmunder, Perry Wilson, Ron Koontz, Ken Crook, TV Huang, Dieter Walz, Jean Lebacqz, Arnold Eldridge, Al Lisin, Vernon Price and somewhat later, Ewan Paterson, Tor Raubenheimer, Marc Ross, Ron Ruth, John Seeman, Nan Phinney and Juwen Wang. I also had great help from my secretaries and administrative assistants like Bette-Jane Ferandin and Eleanor Mitchell. To all of these and all my wonderful colleagues at SLAC, thank you!

And also, thank you to all the great colleagues and friends with whom I worked during my frequent travels abroad to France, Germany, CERN, Italy, Russia, Japan, China, Korea and Latin America!