Vera Lüth
Video Conference

by David Zierler
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ZIERLER: This is David Zierler, oral historian for the American Institute of Physics. It is September 17th, 2020. I am greatly honored to be here with Dr. Vera Lüth. Vera, thank you so much for joining me today.

LÜTH: Thank you. I'm looking forward to this conversation.

ZIERLER: To start, Vera, would you please tell me your most recent title and institutional affiliation?

LÜTH: I am Professor Emerita at SLAC, Stanford University. SLAC is a DOE funded national laboratory with very close relations to Stanford University. In 2011 I decided to retire, one of the youngest retirees among my colleagues.

ZIERLER: [laugh]

LÜTH: It was the right time for me. We had completed a large and very successful experiment at SLAC; starting a new project didn't make sense for me. Several colleagues asked me to join their collaborations. My response "Not at my age!" [laugh]

ZIERLER: Vera, in what ways have you remained connected with SLAC since your retirement?

LÜTH: I still have an office, which I have not been able to access for many months. Because of COVID-19, only essential staff receives permission to enter the SLAC site, and I cannot claim such priority. For many years - when I was still active and leading a research group, and more so after my retirement – I have dedicated a considerable fraction of my time to various scientific advisory committees and editorial work for a number of physics journals.

ZIERLER: Well, Vera, let's go all the way back to the beginning in Lithuania. First, let's start with your parents. Where are your parents from?

LÜTH: My parents were both German citizens. My father grew up in Riga in Latvia, a city which for centuries had a considerable German minority. My father's grandfather had immigrated to Riga in 1875 to set up his own business which two of his sons expanded over the years. The family retained their German citizenship. Early in World War, my grandfather was taken prisoner by the Bolsheviks and sent to Siberia, while his wife and their five children were evacuated and spent four years in Breslau (Silesia), her home town. My mother grew in Bremen a city in Northern Germany where she has lived most of her life.

ZIERLER: So, your father's upbringing, culturally was German?

LÜTH: That's right. He attended a high school where all classes were taught in German, though some students were Russian, Latvian, or Jewish. He and his siblings learned to speak Russian from a woman who took care of the children in their home. Riga has been and still is multi-lingual, even now street signs are in two or three languages, though in recent years most of the Russian signs have been removed or painted over.

ZIERLER: [laugh] And where did your parents meet?

LÜTH: My mother was born and lived in Bremen. She had been married to an officer of the German Army, who was killed in 1941 in the early phase of the war in Russia. Thus, at the age of 28 she was a widow with two young sons. My father worked at the cotton exchange in Bremen after he had graduated from a trade school with the intent to join his family's business in Riga a few years later. He never returned to Latvia. My mother had known my father for several years and they got married in 1943 in Bremen. Soon after, my father was assigned to serve the German administration in occupied Lithuania, because he spoke Russian and was familiar with life in the Baltic states. I spent the first three months of my life in Kaunas, until the Russian troops advance, and the Germans retreated.

ZIERLER: And where did your family go soon after you were born?

LÜTH: It was the middle of the World War II. My father had enlisted in the German army, and my mother and her three children were invited to stay on a large estate owned by relatives in Pomeránia. As the war came to an end in the East, the family stayed briefly in Flensburg with one of my father's brothers, and soon after found refuge in a farmhouse in a small village, not far from Bremen. At this time, Germany was occupied by the Allied Forces and cities like Bremen were heavily damaged. My mother and her three children lived in a single room, we all slept on the floor, but she had access to the kitchen. There was no currency, so my mother used her linens and other household goods and jewelry (that were saved) as barter to get food for us. For us children, she also sowed winter coats made from army uniforms. The situation improved after my mother was hired as a secretary by the British army stationed nearby.

ZIERLER: Do you have any memories of this period or you were too young?

LÜTH: Only very vague memories, mostly from a few photos and some stories told by my brothers who were almost ten years older. The local farmers were quite friendly to us refugees, I even learned to speak the local dialect. The environment was safe, and life was very simple, but not easy! My parents decided to file for divorce when I was four years old.

ZIERLER: And when your parents decided to divorce, was there a question about who would gain custody?

LÜTH: My father's condition for accepting the divorce was that I would stay with him and my mother would take care of her two sons.

ZIERLER: So you grew up with your father?

LÜTH: Not really. My father arranged for me to stay with a foster family headed by a school friend of his from Riga. They lived in a small village, in a farmhouse with adequate space for the family with two children and me. My first two years at school were in a small building, with two grades in one classroom. So, from the start, I was able to follow second grade math! In 1952 my grandmother moved to Hannover where my father lived, and I stayed with her for the next five years. After she passed away, I basically was living alone for more than a year. At some point it became obvious that this situation was somewhat difficult for a 14-year old. My parents agreed that I move to Bremen and stay with my mother for the last five years of school. Having a stable home was very important and entering the local gymnasium (high school) was a new beginning.

ZIERLER: Vera, if I may ask, why would your father have put this condition to have you if he wasn't really in a position to raise you?

LÜTH: That was never discussed. I had no contact with my mother or brothers for eleven years, and did not see much of my father either.

ZIERLER: You must have learned to take care of yourself from a very young age, and I bet that served you well in many ways over the course of your life.

LÜTH: It did indeed! I started observing the world around me, trying to assess what was happening, good and not so good, and identifying persons whose behavior I liked. Fortunately, I spent summer vacations with relatives, an aunt and her family whom I really got to love. The all-girls school in Bremen was excellent. The science teachers were all women, whereas languages and history were taught by men! I focused on my studies because I realized that doing well at school would open opportunities later. That wasn't hard for me, I graduated top of the class! As a result, I was offered a national merit scholarship which paid for my cost of living as a university student. There were no fees for tuition. As a result, I was financially independent from my parents at the age of nineteen.

ZIERLER: Vera, was it difficult adjusting to when you went back to your mother and brothers?

LÜTH: Well, not being on my own anymore was a change for the better! Of course, the relation with my mother was different from the one most teenagers would have. She took care of my needs in many ways and we got along well. We often spent the fall vacation on one of the North Sea islands which we both enjoyed. One of my brothers lived in the same apartment, while he attended engineering college. The other brother was in the merchant navy, so he travelled all over the world and was not home much.

ZIERLER: Now, were you a strong student across the boards or did you particularly distinguish yourself in math and science?

LÜTH: The school curriculum was very broad, three foreign languages, math and sciences, including laboratory courses, and of course literature and history, and also art and classical music, taught at all grades. I spent the last three summers with families in England, so I was fluent in English by the time I left school. I wrote my doctoral thesis and all scientific publications in English, no problem!

ZIERLER: Were you thinking specifically about pursuing a degree in science even from high school?

LÜTH: I was interested in a lot of things, science and history, especially the German history, which was taught in great detail, with focus on the 19th and the first half of the 20th century. During the

Nazi years, lots of documentary films were produced, so we listened to many of Hitler's speeches and propaganda to learn about the collapse of the first German democracy and what followed. This was the beginning of the Federal Republic of Germany, a democracy, and the forming of a new society, the rebuilding of the cities and the economy. The schools were reasonably well equipped, but they not as well as the schools in California now. Life was simple. We didn't ask for things we didn't need.

ZIERLER: How broad were your options in terms of choosing college?

LÜTH: In Germany, once students have passed the abitur (the equivalent of the baccalaureate in France), the final exam at the gymnasium, written and oral, they are qualified to enter any university - with a few exceptions, - and can choose whatever subject they want to study. The gymnasium is quite different from American high schools. Students enter at fifth grade, and the curriculum is largely fixed, except for a partition during the last three years, one more focused on math and sciences and the other on languages and literature. Still, all students take science classes (two lessons each per week) for many years, and calculus is part of the final exam. Classes are small and the students stay together for many years. But many students choose to leave after tenth grade to take a job or an apprenticeship which includes special classes in technical schools. So, this is very different from the huge American high schools with typically 2,000 students, all of which stay through to twelfth grade with a very large choice of curricula.

ZIERLER: Vera, I'd like to ask, in the early 1960s in Germany and as a woman, pursuing a career in science. Was this something very unique?

LÜTH: All through my studies and career I was one of very few women students or scientists. In the early semesters at Mainz University, courses in math and different fields of science were attended by about 90% male students, and all faculty members were men. At the faculty office of Heidelberg University there was no record of any other women who had received a DSc in physics, though at the time, the president of the university was a woman, a professor of chemistry. In the 1970s, during my years at CERN, there were only three women scientists, all of them married to CERN staff members. Upon my arrival at SLAC, there was one other women scientist, a recently hired postdoc in a different research group.

ZIERLER: So, were you never made to feel discouraged from pursuing these interests?

LÜTH: Not really. Initially, my father was somewhat skeptical. He did not really know what physicists do and he wondered whether I should study pharmacology!

ZIERLER: Vera, at what point did you settle on physics during your undergraduate career?

LÜTH: In Germany, on the day you enter the university, you begin your studies in the selected field and sign up for the lectures as required. If I had wanted to study law, I would have entered the law school on my first day.

ZIERLER: It's so different than in the American system where you have time to be exposed to a broad array of disciplines before you choose. And so that begs the question, how did you know that it was physics, even before you were learning physics at the collegiate level?

LÜTH: In Germany and most other European countries, during the last two or three years of high school the teaching is at a relatively high level, similar to what most Americans encounter in their first two years at college. We had courses in philosophy, world history and literature, more than one foreign language, as well as math and science. In physics we used calculus to understand and calculate certain processes and we also attended lab courses. If a university student is unable to follow the lectures or is not interested, he or she can easily sign up for different classes. I remember that a couple of my fellow students decided to change their career goals and lectures to become high school teachers, rather than to complete the more challenging physics curriculum.

ZIERLER: So, on that note of limiting your discipline, what about within physics; when were you expected to decide whether to focus on experimentation or theory or applied physics? When did that happen?

LÜTH: That usually happens after you finished the basic course work in your third or fourth year of study and you start looking for a topic for a master's thesis. After five semesters, I passed the compulsory preliminary exam in physics and decided to leave the University of Mainz, where I had chosen to start my studies because it was one of the few campus universities in Germany. It was very nice. I could live on campus, walk to the lectures and also enjoy one of the best sports department. I switched to the University of Heidelberg which was a better known traditional university, and I signed up for a set of mostly theoretical physics lectures. I also started some laboratory work at the new Institute for High Energy Physics (IHEP). This turned out to be a preparation for my master thesis:

analyzing particle interaction that were recorded at CERN, the European Center for Particle Physics in Geneva.

ZIERLER: And at what point did you settle on the kind of physics that you wanted to pursue professionally?

LÜTH: At IHEP, photos of interaction of elementary particles in a so-called bubble chamber were analyzed to understand the fundamental rules that governed these interactions. After I had learned the basic analysis techniques, I was sent for several weeks to CERN to assist with the recording of pion-proton interactions in a bubble chamber, filled with liquid hydrogen. The analysis of these interactions recorded on film were to become the topic of my diploma (MSc) thesis, supervised by Professor Siegmund Brandt.

These visits to CERN as a student were truly formative! It was a privilege to meet and work with outstanding scientists and engineers from around the world, who led by example, and were open to answer questions. I was learning about sophisticated instrumentation, their design and operation, the physics and analysis. The international flair at CERN and the city of Geneva were very attractive, three hours by train to Paris and five hours by car to the Province!

I returned to Heidelberg to get the bubble chamber tracks measured and kinematically analyzed using computer programs developed at Berkeley! About two years later, I had almost finished the analysis and had prepared my first publication in a scientific journal, when the IHEP director sent me to CERN for the summer. He had been contacted by Jack Steinberger who had recently joined CERN after a long successful career at Columbia University. He asked for a young scientist to join him on a special short term project. At that time, CERN was constructing a new circular machine to collide protons on protons at high energy. There were plans for a large general purpose detector with a magnetic field to measure the momenta of the charged particles that were produced in these interactions. Three designs for a large magnet structure had been proposed, and for each of them a scale model had been built and the magnetic fields had been measured. I was asked to assist Jack Steinberger to simulate and reconstruct tracks of different angles and energies and evaluate the precision achievable with each of these three proposed magnets. By the end of the 1968, we completed the analysis and on the basis of the written report the so-called split-field magnet was chosen. This visit was a great experience for me, working with Jack Steinberger and many other senior scientists for five months at full salary!

Jack Steinberger was born in Bad Kissingen in Northern Bavaria where his father was the cantor and religious teacher at the small synagogue. He and his older brother had the chance to leave Germany with support by American Jewish charities and arrived in Chicago in late 1934. His parents and youngest brother followed four years later and thus were also able to escape the holocaust. So, Jack grew up in Chicago. He finished his undergraduate degree at the University of Chicago in Chemistry in 1942. He completed his doctorate in 1948 with a thesis on decays of muons from cosmic rays, as a student of Enrico Fermi, also at the University of Chicago.

ZIERLER: So, Steinberger reached out to Heidelberg and that's how you got connected to Jack?

LÜTH: Exactly! This was a very fortunate event with important consequences!! Just before I returned to Heidelberg to finish writing my master's thesis, Jack took me aside and told me "I just proposed an experiment to study neutral K mesons and I am considering a collaboration with the Heidelberg group. Would you like to join us for your doctoral thesis?"

What a surprise! In fact, while I was finishing my master's degree, I was wondering whether I wanted to stay in particle physics or whether I could work in medical research using my knowledge in physics, some chemistry, experiments and data handling. I had taken some classes in anatomy and physiology at Heidelberg University, which I found very interesting, and other students appreciated my skills to fix their equipment! Unfortunately these kinds of graduate programs had not yet been established in Germany. Also, my scholarship was to be terminated once I completed my master's degree! Then the letter from IHEP in Heidelberg arrived, offering me support as a graduate student working with Jack Steinberger at CERN! Knowing Jack and having spent time at CERN, this was a very attractive offer I could not turn down!

ZIERLER: What was Jack's research when you met him? What was he working on at that juncture?

LÜTH: After his PhD at Chicago and two short postdoc appointments at Princeton and Berkeley, Jack had joined Columbia University and engaged in a series of experiments measuring the fundamental properties and interactions of various new particles, first at a small cyclotron at Columbia’s Nevis Laboratory and later at the new large accelerator at Brookhaven Laboratory, using different particles beams and novel detector techniques, including bubble chambers. In the early 1960s he joined Leon Lederman and his graduate student Melvin Schwartz to study neutrinos from pion decay. Their measurements established that these neutrinos interacted differently from neutrinos produced in nucleon β decay. A really surprising result, which changed our understanding of weak interactions, and was awarded the Nobel Prize 26 years later!

In the decade that followed, many other fundamental measurements were performed to study properties of various new particles and their interactions, among them tests of fundamental conservations laws and symmetries. The big surprise was the observation of the violation of CP invariance, the combination of charge conjugation and parity. In other words, a violation of the assumption that the laws of physics should be the same if a particle is interchanged with its antiparticle and the spatial coordinates are inverted. The discovery of CP violation in 1964 in the decays of neutral K mesons could not be explained by any theoretical model. While on sabbatical leave at CERN that year, Jack participated in an experiment at CERN, which resulted in a number of new measurements of neutral K mesons, unfortunately with substantial statistical and systematic uncertainties.

After Jack moved to Europe in 1968, he started thinking about a new experiment, designed to detect the decay of the K^0 to two oppositely charged particles, $K^0 \rightarrow \pi^+\pi^-$ or $K^0 \rightarrow \pi^+\lambda^-\nu$, here the charged lepton λ refers to an electron or muon. The detector was to be placed close to the production target to enable detection of the K^0 decays with decay times in the range of (3 to 40) 10^{-10} sec. The novelty was the use of very large proportional wire chambers recently invented by George Charpak. Their excellent time resolution and zero dead-time combined with a fast selective read-out allowed considerably higher data acquisition rates than earlier experiments. The momenta of the two charged particles from the K^0 decay were to be measured in a spectrometer consisting of three very large multiwire proportional chambers, divided into a left and a right half, each equipped with a horizontal and a vertical signal wire plane. The wire spacing was 2 mm.

When I arrived at CERN the proposed experiment was approved, and the CERN Heidelberg collaboration had been formed. Apart from Jack, the group had ten members, among them three graduate students. By far the biggest challenge was the design and fabrication of the large proportional wire chambers, 2.6 m wide and 1m high, with more than thousand wires each, mounted with a considerable tension to balance the electrostatic forces between neighboring wires. Starting out with a few small chambers (10x10 cm^2) to be used for beam tests, it took us about two years to arrive at a suitable design and to build these chambers with assistance from experts in the Charpak's group and top engineers from the CERN mechanical shop. Once completed and installed, we collected data at the CERN PS accelerator. With a read-out system selecting events with two charged particles in each of the three wire planes, we recorded more than 1000 events per beam pulse, resulting in a total sample of more than 10^9 events (128 bits in length) recorded on tape, typically one tape every 10 minutes!

ZIERLER: This must have been so extraordinarily exciting.

LÜTH: Absolutely! A very simple spectrometer layout, only three layers of wire chambers, one in front and two behind dipole magnet. Almost all components were beyond the state-of-the-art. This included the first microchip front-end electronics with full online calibrations and a very smart trigger system to select truly two-track events. Much of the electronics were developed by Bill Sippach, an engineer at Nevis Laboratory, assisted by engineers from CERN and Heidelberg.

ZIERLER: What was Jack's style as a mentor?

LÜTH: I would characterize it as very informal! The group was small, and our offices were all on the same hallway, so you'd bump into him frequently. The group meetings were ad-hoc, whenever there was a problem, or some good news had arrived. Frequently we had lunch together in one of the CERN cafeterias. On occasions he would invite a colleague or friend to join us. I remember the day T.D. Lee visited, the famous theorist from Columbia University. Jack introduced us graduate students to T.D. as

Professor Lüth and Professor Vannucci! Many years later, I worked T.D. Lee in connection with the Super Collider Project in Texas. This included a visit to Vice-President Gore at the White House.

ZIERLER: [laugh]

LÜTH: Every year, our group arranged a number of outings in the beautiful areas around Geneva, family picnics in the summer, ski excursions in the winter, and classical chamber music at Jack's home. Many of the friends he went skiing and hiking with were German, among them experienced mountaineers. On one excursion to the Argentiere Glacier above Chamonix, Jack fell into a crevasse, fortunately he was saved by the guardian of the nearby alpine hut!

ZIERLER: Did you speak with Jack in English or German?

LÜTH: As Jack studied and did most of his research in the USA, he preferred to discuss science in English. Also, there was one person in the group who did not speak German, otherwise, we might have spoken German more often! In private, he would often change to German and one could still recognize his Franconian accent.

Years later, when he received the Nobel Prize, the city of Bad Kissingen invited Jack for a visit, his first after he had immigrated to the USA. Since then he has frequently visited the town, made personal friends, and developed a close relationship with some teachers and students at the gymnasium, which is recognized nationally for its outstanding science education. He volunteered to give lectures on a broad range of current and past physics topics. On the occasion of his 80th birthday in 2001, the town of Bad Kissingen arranged a special celebration and renamed the school the Jack Steinberger Gymnasium.

ZIERLER: What was the arrangement between CERN and Heidelberg in terms of your degree? You were a Heidelberg student, but you spent all of your time and research at CERN, is that how that worked out?

LÜTH: Yes, that was the case! Once or twice a year I drove to Heidelberg, for private or other reasons. While I enjoyed being at CERN, the center of the action, I missed some of the advanced classes which one usually takes as a graduate student. At CERN, there were many very interesting lectures and seminars, but not the in-depth presentations on theoretical methods which I would have liked.

ZIERLER: So, Jack was your advisor as a Heidelberg professor?

LÜTH: Yes, he was! The University honored him with the title "Honorar Professor". That did not mean that he received an honorarium, but it allowed him to officially supervise students and give lectures.

ZIERLER: And what about the rest of your committee, were they at CERN as well?

LÜTH: No, all participants of the oral exam at Heidelberg were members of the faculty, one mathematician and three physicists, which I was allowed to select. Jack had read my thesis a few days before I had it printed and submitted to the university. Fortunately, he was very impressed and recommended the grade "summa cum laude"!

I wrote my thesis with pencil on paper and had it typed by a CERN secretary. It was published as a CERN report. While I was writing, I consulted with Heiner Wahl, one of the CERN staff members who had a leading role in the experiment. Whenever I finished a new chapter, I handed it to Heiner for comments and corrections - very few - which I implemented right away.

ZIERLER: So you essentially developed your dissertation independent of Jack's own research?

LÜTH: No – this is not how the group worked! All members of the collaboration were engaged in the building and the operation of the detector, hardware and software, and of course in data analyses. While the graduate students were assigned a specific measurement, more senior scientists were free to choose their research topics. While a given analysis was usually taken on by one person, we all worked as the team, sharing responsibilities and consulting on problems that occurred, related to the detector and/or the data analysis. The group was small, we met frequently, especially when new results came out and everybody wanted to understand the details. I remember one day, when Jack looked at some of my notes and told me that there was a mistake. The next day he came to me and apologized, because he had recognized that there was no mistake!

Under Jack Steinberger's leadership this small group built an amazing detector and made a number of measurements that were unique and or far exceeded earlier ones in precision! As the first results came out, there was a surprise regarding our measured value of the ratio of two complex decay amplitudes, referred to as η_{+-}. Our measurement resulted in a value of $(2.30\pm0.035)\ 10^{-3}$ compared to the average of earlier measurements of $(1.91\pm 0.09)\ 10^{-3}$, a difference of about 4 standard deviation! The same analysis also indicated large deviations for two other parameters characterizing the K^0 decay. The probability that the original measurements and our new ones were both correct was very small! Given our large data sample and the simplicity of the detector, we considered it very unlikely that our results were wrong. Nevertheless, the graduate student working on this started checking every detail of his analysis, and at one of our meetings he announced with a great smile on his face, "Jack, I am now getting the right result!!" Jack jumped up. "What do you mean by the right result? The right result is the one you understand. It doesn't matter what anybody else measured." It took several days for Jack to calm down! This has been one of the most important lesson I learned from Jack and will never forget! Nowadays, most experiments do blind analyses for precision measurements. They mask the measured value in the analysis by a certain numerical offset, so that the person doing the analysis cannot compare the result with the values expected from theoretical prediction or earlier measurements. The offset, which is only known by one person, will be removed after all uncertainties have been carefully studied and the work is ready for publication. Since then, we observe a significantly larger spread of measured values obtained from different measurements.

The title of my thesis was “Measurement of the Charge Asymmetry in the decays $K^0 \rightarrow \pi^- e^+ \nu$ and $K^0 \rightarrow \pi^+ e^- \nu$ as a function of the K^0 decay time”. Earlier measurements of this asymmetry were only available for long K^0 decay times. This was new territory! The work on my thesis proceeded well. I found copies of several PhD theses from students at Columbia University on a variety of different measurements, and I derived some very useful guidance on how to structure and plan the important steps of the analysis and how to document them in my thesis. The signal decays involved two charged particles, a charged pion and an electron of opposite charge and a neutrino which could not be detected directly, but kinematically identified by the missing energy and momentum. A special procedure had to be developed to determine the K^0 momentum and decay time. Of course, I had to very carefully test every step in the analysis and the simulations of the signal and backgrounds which were needed to determine their detection efficiencies and to perform detailed cross checks.

In the end, the real challenge was the enormous data set that was recorded on more than 1,000 magnetic tapes and had to be processed. I spent my Christmas holidays at the CERN computing center to get all data processed with the help of one operator on duty. This resulted in a signal sample of 40 million events which were stored in a multi-dimensional histogram for further analysis. The final task was to extract the charge asymmetry and determine some important parameters of the neutral kaon related to CP violation. This charge asymmetry indicated that the rate of positrons in the decay is slightly larger than for electrons in our world where the nucleons are positive. In the anti-world, there would be an excess of electrons, with the opposite charge of the nuclei.

ZIERLER: Vera, of course, this is an extraordinarily exciting time in particle physics generally. I wonder if you can comment on, looking back or even at the time, where you saw your research fitting in with those broader research endeavors in experimental particle physics?

LÜTH: Neutral K Mesons were known as "strange" particles because of some of unusual properties. There were two neutral particles with same mass but very different lifetimes, referred to as K_{Short} and K_{Long}, with decay products of different parity. This was very puzzling. In 1964 Jim Cronin and Val Fitch observed a small CP violation in the decay of neutral kaons. In the following years, several experimental studies were performed to understand this very unusual feature. The results of the CERN-Heidelberg experiment were the most precise measurements, for both the two-pion and semi-leptonic decays. At the time, there was no fundamental understanding of the origin of CP violation which was observed only in decays of the K^0 meson. In 1973, two young Japanese theorists, M.

Kobayashi and T. Maskawa had proposed a six-quark model to explain CP violation in K^0 and also B^0 mesons (which had not been discovered) decays. Their paper did not receive much attention, because at that time all known hadronic particles could be understood in terms of only three quarks, referred to as up, down, and strange! Following the discovery of CP violation in neutral B mesons in experiments at SLAC (Stanford) and at KEK (Tsukuba); the theoretical prediction was honored by the Nobel Prize in 2008.

While I was working on my thesis, I met a young scientist who was spending a year at CERN after he had finished his PhD on an experiment at SLAC. He asked me "What do you want to do when you're done with your thesis?" My response was "Oh, I don't know, spending a couple of years somewhere in the US might be interesting." A few months later, he informed me that there was a postdoc position open at SLAC and asked if I wanted to apply. I knew very little about SLAC and its research program, except for recent news from the Vienna Conference on deep inelastic electron-proton scattering at the SLAC linear accelerator, revealing evidence for substructure of the proton. Soon after, I met one the leading scientists of this experiment who happened to be visiting CERN. I introduced myself, quizzed him about the experiment, and mentioned that I was considering to apply for a job at SLAC. His response: "Oh, we only take the very best!" Nevertheless, I applied for the job. It looked like a very interesting option and a beautiful place.

ZIERLER: Was this your first time to the United States?

LÜTH: Just after I applied for the position at SLAC, the annual APS Meeting on particle physics took place in Berkeley, and Jack had asked me to present the very first, still unpublished results from the K^0 experiment. He had submitted four abstracts, but the organizers did not know the status of these measurements and offered me a single brief talk. After I completed my presentation, they had learned that there were other new and interesting results, and offered me almost an hour to present them. I had prepared four short talks, so there was plenty of material.

A few days later, I visited SLAC to give a seminar on my thesis topic and be available for interviews by many SLAC scientists. A few days later I received the job offer to become the second woman scientist at SLAC. On the way back to Europe, I stopped in Chicago to visit the Enrico Fermi Institute. Jim Cronin invited me to give a talk on my thesis and related topics. This turned out to be a very interesting visit! A small group led by my friend Bruce Winstein was beginning to plan the next generation of K^0 experiments in a new beamline at Argonne National Laboratory. We recapped all related measurements and their interpretation and possible future improvements. I am proud to mention that in his 1980 Nobel Lecture on CP violation Jim Cronin included the figure with the results of my analysis with reference to my thesis rather than the publication in Physics Letters.

ZIERLER: That must've been so gratifying for you.

LÜTH: Jim Cronin was really special person, extremely modest about his accomplishments. At a conference in London celebrating the 50th anniversary of the discovery of CP violation, he gave a talk, deemphasizing his contributions. "I really didn't do anything, I just contributed the detector. Others focused on the difficult analyses."

ZIERLER: Vera, for any other year it might not be such an important detail, but given that this is SLAC in 1974, it's important to know exactly when in 1974 you got there as a way to build up the narrative for where you were during "the November Revolution" as we call it.

LÜTH: I remember exactly! My oral thesis exam at Heidelberg University took place at the end of January, and I landed in San Francisco on March 1st. At that time, the SPEAR storage ring had been operating for about one year. This e+e- storage ring was first proposed in 1965 by Burton Richter and David Ritson, but it took almost five years until funding could be obtained. During this time, the detailed design was further developed, based on experience from two low-energy interconnected electron storage rings at Stanford, including the design and construction of all components, specifically dipole and quadrupole magnets, RF cavities, vacuum systems, and electronics and operations controls. Construction finally started in 1970. All components were technically at the forefront, for instance, ultra-high vacuum to achieve long beam lifetime, small beams at the collision point to enhance the luminosity and thereby the e+e– interaction rate, at beam energies ranging from 1.4 GeV to 4.0 GeV. This wonderful machine was installed on an asphalt parking lot, with components enclosed by concrete shielding blocks. At the cost of about $6 million, this turned out to be a fantastic facility, with first colliding beams in April 1972. The detector was of cylindrical geometry, centered on the beam pipe, with electronic tracking devices in a coaxial magnetic field. This design configuration became the prototype for all future detectors at colliding beams. At the time, Burt Richter realized that his group at SLAC, with experts on the storage ring and detector systems, was not diverse and large enough to prepare for data taking, the data analysis, simulation software, and many other tasks. So, he reached out to three professors at Berkeley, Willy Chinowsky, George Trilling, and Gerson Goldhaber, and also one of our colleagues at SLAC, Martin Perl. This became known as the SLAC-LBL collaboration! While the SLAC team designed and built the inner detector components, the electronics, trigger, and online software, Berkeley built the photon detector, and developed the offline analysis software. Starting in early 1973, the first data were recorded in the center of mass energy range from 2.4 to 5 GeV, in steps to 0.2 GeV. As we learned later, these were rather large steps!

Burton Richter presented the first results on the production of hadronic final states at the conference on high energy physics in London in July 1974. The SPEAR results and earlier measurements from ADONE in Frascati and CEA at Cambridge were in good agreement, given their rather large uncertainties, but the data did not agree with theoretical expectations. Specifically, the ratio of the production rates of hadronic final states compared to muon pairs was expected to be independent of the energy, while the data indicated a linear rise!

This was very puzzling! In June 1974, just before a shutdown to upgrade SPEAR to higher energies, it was decided to take more data with smaller energy intervals, at 3.1, 3.2, 3.3 GeV, and 4.1, 4.2, 4.3 GeV. At each energy, the data set was composed of several runs; a run typically ended by some interruption, either a misfunction of the detector and a beam dump which was followed by a refill of the beams. We subsequently analyzed and checked the data sets recorded under the same beam and detector conditions. To our surprise, for the data set recorded at a nominal energy of 3.1 GeV, two of the eight runs indicated hadronic event rates high by a factor 3 and 5! We had no idea why these rates were higher. Suspecting a problem with our software, a few of us used the new video display connected to the IBM mainframe to examine and classify each of the reconstructed event pictures, trying to find some difference for the runs with higher rates. This was a very cumbersome procedure compared to what we do today. We could find absolutely nothing wrong with the event reconstruction or analysis or the detector performance! In parallel, our LBL colleagues did their scan of the same events, and they also could not find any explanation for these higher event rates.

Then in early November, two scientists, one at SLAC and one at LBL, reported an enhanced rate of K^0 mesons in the runs with higher events rates - it later was explained as a fluctuation, but at the time it had a great impact! Namely, a group of us younger scientists were able to convince Burton Richter to defer the planned SPEAR operation at higher energy and allowed us "to waste a weekend" to find the reason for these anomalies! This was the start of what turned out to be a very exciting three-day weekend! Under the leadership of Harvey Lynch and Roy Schwitters, we prepared the plan for the weekend, to make sure we wouldn't waste it! Specifically, we had to make sure that the detector and the data recording were working flawlessly, by recording test data at 2.4 GeV and 3.0 GeV, before we would proceed with the search at the critical energy of 3.1 GeV and above. In parallel, with the help of the operators, we paid special attention to the energy setting of the SPEAR storage ring. This involved precision setting and monitoring of the field of the ring magnets, and fine tuning of the beam energy by adjusting the RF phase and checking the beam orbits.

It took a while to get all this going, for both the storage ring and the detector. On Saturday morning, at a collision energy of 3.12 GeV, there were short periods when the hadronic rate was about a factor of 3 larger than expected! This was observed on the online display of a small subsample of recorded events showing the reconstructed charge particle tracks. Under normal condition most of the events have two relatively high momentum tracks, easily identifiable as cosmic rays crossing the detector or e+e- elastic scattering. Most hadronic final states have more than two charged tracks and their rate is typically a factor of 5 smaller than the rate of e+e- elastic scattering. This on-line display provided sufficient information to distinguish these three classes of events, and we kept score of the number of events for each. The observation of an enhanced rate of multi-track events near 3.1GeV indicated that we were able to observe the kind of anomalous rate we had discovered in the earlier data!

Unfortunately, on Saturday we encountered a variety of technical problems that limited the data taking, primarily due to recent changes to the SPEAR operation. Still, all the data were recorded on tape and the tapes were sent to the SLAC computing center for full reconstruction and analysis. I was on swing shift and the storage ring wasn't working well. Still, I was pretty sure something interesting was about to be discovered, so at 9 pm I decided to go and purchase a magnum of champagne. A few hours later, still nothing was working, so by 2 am I put the bottle in the refrigerator and went home.

ZIERLER: [laugh]

LÜTH: At about noon on Sunday, I showed up on the control room and Marty Breidenbach asked me, "Vera, you have three guesses on how high the peak rate is." [laugh] By that time, the SPEAR energy setting had been fine-tuned, and both the e+e- pair and hadron rates had grown tremendously near 3.105 GeV, by up to about 100 times for hadronic and 10 times for e+e- final states, so the ratio of the two rates was about 10! Nobody had ever seen an event rate rise by more than a factor of 10 above background! Also, we could ask the operator to slightly change the beam energy, either up or down, and the rate would drop dramatically, but could be easily restored by changing the setting back to the initial value. This was definitely a large resonance, not a technical problem! Soon it got crowded in the control room and the first corks of champagne bottles popped. There were smiles on everybody's face and a general feeling of euphoria. I remember the SLAC director Panofsky pacing back and forth with his hands on his head, uttering "Oh my God! Oh my God!" Gerson Goldhaber and others started drafting the Physical Review Letters (PRL) paper. While data taking continued, many of us were speculating what kind of new resonance this was? It decays to hadrons, what is the decay rate? What are its quantum numbers? What inhibits its decays and makes it so narrow? And how should we name this particle? We checked the

Greek alphabet, but there were not too many choices, iota was not considered because it implies insignificance, so we quickly agreed to call it Ψ, even though some theorists objected, "No, that's for the wave function. You can't use it."

As usual, I carried my compact camera in my hand-bag and had the chance to take a series of photos on the activities and my colleagues in the SPEAR control room on this eventful Sunday afternoon.
At LBL, our colleagues were checking the data for consistency and potential changes in efficiencies. David Jackson requested and received by phone the measured values of the hadronic rates as a function of the energy, many of them preliminary. He spent most of the night to derive the decay width of this narrow resonance, which is closely related to its lifetime, which at 7.2 10^{-22} sec is impossible to be measured directly. The primary challenge of these calculations were the radiative corrections, accounting for energy losses of the electrons which result in a deformation of the true resonance shape. Dave Jackson visited SLAC the next day to present the result of his calculations, in the hope that they could be included in our first publication. However, it was decided to defer this, because none of the other theorists had a chance to cross check these rather challenging calculations.

In many ways, we were lucky that the mass of the giant resonance was close to 3.1 GeV, at an energy we had taken data. Many theorists interpreted this resonance as a bound state of a charm quark and a charm anti-quark, referred to as charmonium (in analogy to positronium for the e+e- bound state). This would prove the existence of the fourth quark, which had been predicted by some theorists more than a decade ago.

And then came the big surprise on Monday morning! Samuel Ting, a professor at MIT, arrived on Sunday afternoon to attend a review meeting at SLAC on proposals for future experiments. When he arrived at San Francisco airport, he apparently received an urgent call from his MIT colleagues who had heard about the news from SPEAR. Ting's group had built and operated a two-particle spectrometer in a proton beam at Brookhaven National Laboratory, searching for resonances decaying to e+e- pairs. Unbeknownst to us at SLAC, his group had detected a very narrow resonance which he named J, which strongly resembles the Chinese character for Ting's name; J is also the first letter of Sam's oldest daughter's name.

Sam Ting had recently completed a draft of a paper on this discovery. After he arrived at his hotel in Palo Alto, he spent the rest of the day and most of the night on the phone with colleagues at MIT, reviewing the paper draft. He also contacted various laboratories in Europe, among them DESY and Frascati which had operating e+e- storage rings and thus were in a position to confirm the SLAC observation. The next morning, he went to show his results to the SLAC director, W.K.H. Panofsky, and said, "Pief, I've got something interesting to tell you." Panofsky's response was "We, too." At this time, Roy Schwitters, Marty Breidenbach, and I were editing the latest paper draft and checked the figures which I had prepared the old fashioned way with pencil on log paper, to be redrawn with black ink on white paper. Suddenly Pief summoned Roy to his office. He returned a few minutes later, white as snow, and announced "Sam has discovered the same thing."

At an ad-hoc seminar, Roy described our experiment and showed the data and Sam Ting sketched the experiment at BNL and their results on the black board, showing a narrow peak centered at a mass of 3.1 GeV. The news of this unusual event had spread, and the auditorium was packed, not only with physicists, experimenters and theorists, but also technicians and engineers, secretaries and machinists. I was not sure if they all understood in detail what was reported, but they clearly realized the enormous excitement among the physics community! So sudden and dramatic was this discovery, which within months led to breakthroughs in our understanding of particles and forces, that this discovery became known in physics folklore as the "November Revolution"!

Sam Ting returned to the East Coast the same evening and the next day he submitted the paper entitled "Experimental Observation of a Heavy Particle J" to Physical Review Letters (PRL). Our paper, with the title "Discovery of a Narrow Resonance in e+e- Annihilation," arrived at PRL a day later. The mass was quoted as 3.105 ± 0.003 GeV and the upper limit on the width was estimated to be 1.9 MeV. At Frascati, they were able to extend the energy of ADONE, their small e+e- storage ring, slightly beyond the limit of 3.0 GeV, and within a few days they observed the resonance, and published their result in the same issue of PRL.

ZIERLER: [laugh]

LÜTH: Now, the obvious question was: How many of these resonances had we missed because of the large energy spacing of the measurements? How many narrow ones are there? It was decided to run the storage ring in a scanning mode, i.e., to record a series of small data samples, and increase the total energy in steps of 2 MeV every 3 minutes. To do this, we had to change the SPEAR controls to allow smaller steps in the beam energy. Furthermore, to process the data at each energy online, we sent the data directly to the IBM 360 mainframe. Based on a simple theoretical calculation Marty Breidenbach, with some assistance from Terry Goldman, predicted the mass of the first radial excitation above the Ψ resonance to be about 3.7 GeV. Thus, on November 21, we started the scan at 3.6 GeV. Within less than three hours another resonance was discovered which we named Ψ'. The scan was stopped, and we switched to the normal SPEAR operating mode to record data at a selected set of energies. This resulted in a mass measurement of 3.695 ± 0.004 GeV, and an upper limit on the width of 2.7 MeV! We published the result in PRL with the title "Discovery of the 2nd Narrow Resonance in e+e- Annihilation". A continuation of the scan up to 5.9 GeV did not reveal any other narrow states.

Unfortunately, the sensitivity of the scan was limited to narrow resonances like the Ψ, so we missed the Ψ(3777) resonance and other states just above 4 GeV. A couple of weeks later, the MIT group reported the result of their search for other narrow resonances under the title, "The Non-Observation Heavier J Particles"!

ZIERLER: Ahh.

LÜTH: A few months later, there was another pleasant surprise! As we started to look for various decay modes of the Ψ and Ψ', we found the decay Ψ' → Ψ $\pi^+\pi^-$ with Ψ → e+e- The four tracks formed the letter Ψ! As someone stated, "This particle is literate, it can sign its name, a property it shares with the Λ hyperon!!" We had chosen the right name for these resonances! The one-event display was pretty, and with the help some friends we produced T-shirts. They were very popular! I still have mine, though it is a little tight!!

ZIERLER: [laugh]

LÜTH: In June of 1974, the largest European conference on particle physics took place in Palermo on the island of Sicily. A couple of months before, Burt Richter asked me, "Vera, do you want to go to Palermo?" I responded, "What? Me?" After all, I was the youngest postdoc in the group. Burt's response "Why not?"

This would be my first invited talk at a major conference, so I spent considerable effort to study in full detail the large variety of recent analyses carried out by the SLAC-LBL collaboration. The focus was on measurements of the properties of the charmonium states and their decays. I prepared not only the transparencies to be presented, but also the written contribution to the proceedings, including many figures. I was the only female speaker at the conference and wanted to make sure that my presentation was well received.

The first two talks of the Palermo conference were on the results from the MIT and SLAC experiments. Sam Ting was invited to give a 60 minute talk, the first at this conference. I was offered the second talk, 30 minutes long, and I had many more results to report. On one of the excursions arranged for the conference participants, I wore my blue T-shirt with the Ψ' decay in white on my back. It attracted the attention of Sam Ting and others!

Following this conference in the ancient city of Palermo, I spent a few days in Geneva. I was invited to give a seminar at CERN on our recent results, only 15 months had passed since I left CERN. The CERN auditorium was filled to the last seat. Based on the many questions after my presentation and the following days in the CERN cafeteria I concluded that there was great interest in our results, both by experimenters and theorists. This was a great experience which I will never forget.

Years later, when I was leading a research group at SLAC, I made sure that my students and postdocs could attend conferences and workshops, and like me, would receive invitations to give plenary talks at those events. Unfortunately, this was not always easy to arrange in large collaborations. Of course, we made sure that the talks are well prepared, so the audience would have a very positive impression of the speaker.

ZIERLER: Vera, can you talk about some of the theorists who may have been involved in Burt's group and what they might've contributed

LÜTH: Burt Richter's group did not include any theorists, but SLAC and Stanford always had a very strong theory department, and so did Berkeley. The first theorist who had access to the Ψ data was Dave Jackson of LBL who calculated the decay width of the Ψ resonance overnight.

Dave Jackson was an iconic figure. Like many physicists, I had my first encounter with him as the author of the two-semester coarse on electro-magnetism, as a 3rd year student at Heidelberg University. Maxwell's elegant equations are a challenge to most physics students, and so was this text-book. But like no other course, I enjoyed the experience of solving the problem sets, gradually understanding more and more. Over the years, I have consulted this textbook from time to time, often finding a problem sets rather than answers to my questions!

More recently, I had the pleasure of serving as associate editor for Annual Reviews of Nuclear and Particle Physics under Dave Jackson's leadership for several years. The annual editorial sessions were a unique experience, with lots of give and take arguments, resulting in an impressive list of topics and authors to be recruited. Citing Persis Drell "J. D. Jackson will be remembered as a highly respected scientist and teacher, revered citizen in the HEP community, and last but not least, as an avid supporter of women scientists! "

At SLAC, there have always been close contacts between experimentalists and theorists, for instance, James Bjorken had predicted that the observation of large angle scattering of high energy electrons on protons would indicate the presence of hard cores inside the proton. He and Fred Gilman were regular visitors in the SPEAR control room to get first-hand information on detailed plans for the data analyses, while we experimenters were able to learn about theoretical concepts and calculations first hand.

ZIERLER: Vera, my question was not so much if theorists were part of the group, it was, was there a theoretical basis that the charm quark should exist?

LÜTH: A couple of weeks after the Ψ and Ψ' discoveries, a flood of theoretical papers appeared on the preprint shelves presenting various ideas by scientists worldwide. At SLAC, James Bjorken organized a workshop to study every aspect of the experimental data and compare them with different theoretical models, before the charm hypotheses would be accepted. The workshop was centered at SLAC, first in person and later through various other means of communication, mostly by phone calls. Contributions to this workshop were published under the name PSI-chology! Many years earlier, J. Bjorken and S. Glashow who at the time were post-docs at the Niels Bohr Institute in Copenhagen, postulated the existence of a 4th quark, based on SU(4) symmetry. In 1964 they predicted meson multiplets and their decay modes.

In 1975, after the "November Revolution," studies of the decays of the Ψ'(3586) involving one or more photons resulted in the detection of three neutral particles with masses between the Ψ and Ψ' and there was a first indication of a fourth neutral state, the Ψ(3085). If these states were charmonium states, referred to as "hidden charm states", then "open charm states" should also exist.

In the following year, Mary K. Gaillard, Ben W. Lee and Jonathan Rosner published a detailed paper with a systematic discussion of the phenomenology of charmed particles and an eye on experimental searches for these states, including precise estimates of the charm meson masses, decay modes, and lifetimes. There was still no evidence for these particles, even though several searches had been performed based on SPEAR data! At a conference in Madison in April 1976, Sheldon Glashow urged Gerson Goldhaber to reexamine his earlier search for the decay $D^0 \rightarrow K^- \pi^+$ which had not revealed a signal. Soon after this conversation, it was realized that the identification of charged pions and kaons was rather poor and had led to low detection efficiencies and high backgrounds. Gerson Goldhaber and Francois Pierre (a postdoc from France) studied the decays of the neutral charm meson, $D^0 \rightarrow K^- \pi^+$ and $D^0 \rightarrow K^- \pi^+ \pi^- \pi^+$ and introduced weights to suppress contributions from poorly identified tracks. This suppression of backgrounds revealed clear evidence for both decays and a measurement of the D^0 meson mass of 1865 ± 15 MeV. A similar approach was used by Ida Peruzzi and Marcello Piccolo to look for the charged charm meson D^+ in the decay $D^+ \rightarrow K^- \pi^+ \pi^+$. They observed a clear signal for this decay, and as expected none for decay $D^+ \rightarrow K^+ \pi^+ \pi^-$. The measured D meson masses were fully consistent with the theoretical predictions. Finally, "open charm" mesons had been observed!

ZIERLER: Can you talk about Burt's style as a scientist and as a leader?

LÜTH: I have always had the highest regard for Burt Richter. He was very supportive and fair to me, the only female physicist in his group. He was a superb physicist, well versed in particle and accelerator physics which allowed him to pursue novel approaches to fundamental physic topics. He attracted outstanding scientists to design and build e+e- colliders, the storage rings SPEAR and PEP, and at much higher energy, the SLAC Linear Collider, the only collider of this kind, allowing precision measurements of fundamental importance for our understanding of the standard model of particle physics. Burton was always open to new ideas, no matter who originated them. He would argue strongly with the goal of understanding a problem and getting to the correct solutions, and expected others to do the same!

He succeeded Panofsky as SLAC director. Based on his broad understanding of the science and accelerator technologies, he expanded and opened new areas of research at SLAC with unique facilities for photon science and astrophysics. After stepping down as laboratory director in 1999, Burton was heavily involved with energy, environment, and sustainability issues, particularly those involving new energy sources free of green-house emissions. He served on the board of Advisors of Scientists and Engineers for America, promoting sound science in government. He published a book entitled "Beyond Smoke and Mirrors" on climate change and energy sources in the 21st century.

ZIERLER: Vera, I'm curious, how closely did you work with Martin Perl on his research?

LÜTH: Just in my first couple of years at SLAC. Apart from the discovery of charmonium and other particles carrying the charm quark, the most surprising and important result based on SPEAR data was the discovery of the third charged lepton by Martin Perl who had been searching for such a particle for many years. In fact, he joined SLAC to study charged leptons. His hope was to find some difference between electrons and muons, except for their difference in mass. When SPEAR started running at energies above 4 GeV after the Ψ discovery, he found 38 events with two charged particles, an electron and a muon of opposite charge. Initially there was no explanation for these rare events, except that the properties of a higher mass charged lepton had been anticipated in 1971 by Yung-su Tsai, a theorist at SLAC! Most of us did not know about this paper, and were quite skeptical when this anomalous signature was found. Some of us asked Martin to allow us to have a look at the events. This was easier said than done, because Martin had processed all the data, but he had not copied the electron-muon candidates on a separate file. Instead, for each of these events he calculated a number of kinematic variables and printed each set of numbers on a sheet of the large blue and white IBM paper. He then made histograms of a few variables by hand. I remember that he got a little annoyed when I asked him for plot of another variable, because he would have had to use his HP calculator or write a special program after copying the kinematic variable for each event.

When I returned to my office, I suggested to Chuck Morehouse that we should try to find these events! We processed all the available data to verify the event selection. To our surprise we found almost all of the 38 two-track events Martin had selected! We also checked the kinematics and scanned the candidates on a video screen, and could not find anything wrong! We then showed Martin the event displays!

ZIERLER: Yeah.

LÜTH: In his paper, Yung-su Tsai (we called him Paul) had assumed three different masses for this hypothetical heavy lepton, and predicted the decay modes and calculated their decay rates. They were verified by further analysis of our data sample. I personally think Paul should have received more recognition for his work.

ZIERLER: Vera, I'm curious, when you were thinking about heading to SLAC, did you have any idea that you would make a life for yourself there and in the United States? Did you think you'd go back to Europe?

LÜTH: Initially, I was thinking of staying for a couple of years, one year was too short and I would have had to find my own funding. After about two years at SLAC, I applied for positions at DESY in Hamburg and at CERN, and promptly received an offer from both laboratories. In response, I was promoted to a staff position at SLAC. At some point, Burt talked to me about a future junior faculty position, but when I decided to get married in the US, he knew that I was not going to leave. About ten years later I became permanent staff scientist, somewhat earlier than most of my colleagues.

ZIERLER: And so what was next for you after the 1974 discovery? What did you do next?

LÜTH: Immediately following the discovery of the Ψ and Ψ' I joined a small group of colleagues at SLAC who were engaged in precision measurements of the masses and decay widths, as well as searches for their principal decay modes. The observation of other charmonium states and the determination of their spin, parity, and quantum numbers resulted in the pursuit of the whole spectroscopy of such states, extending up to masses of 4.5 GeV. As SPEAR was accumulating more data at energies up to 7.5 GeV, some of us, under the leadership of Roy Schwitters, revisited data on the inclusive production of hadronic final states. The new data clearly indicated a complex structure just above 4 GeV, whereas the ratio of hadronic versus muon-pair rates now showed the expected energy independence below 3 GeV and also above 5 GeV, though inexplicably high! I also analyzed the inclusive production of strange particles, K^0 mesons and Λ hyperons. The observed enhanced rates above 4.5 GeV confirmed that they originated from charm particle decays. Overall, the number outstanding results from SPEAR and also from DORIS, the storage ring at DESY, was impressive!

At SLAC, in parallel to the many physics analyses, design studies for new detector were underway. Even though the SLAC-LBL detector was a unique and clever design and had operated extremely well, it had a number of shortcomings. The spark chambers did not allow higher data rates and the time-of-flight system for charged particle identification had poor resolution resulting in large misidentifications. The shower counter made of scintillators-steel stacks was not a great design. The new detector, named Mark II following the Stanford tradition, was built under the joint leadership of LBL and SLAC. The

Mark II was installed in 1977, replacing the original detector, from now on referred as Mark I. The principal components were the large cylindrical drift chamber, a novel liquid argon calorimeter to detect electromagnetic showers from photons and electrons, a high resolution time-of-flight system for charged particle identification, and a solenoidal magnet, enclosed by a flux return embedded with large area sensors to identify muons. These new capabilities allowed many extensions of earlier measurements, including detailed studies of the τ lepton which was expected to differ from the low-mass electron and muon only by its much larger mass. Of course, the larger mass allows for many different decay modes, most of which were predicted by Paul Tsai. First measurements of the lifetimes of the τ lepton and D mesons were among the important results from Mark II data recorded at SPEAR.

In the meantime, much larger e+e− colliders were under construction at SLAC and at DESY in Hamburg. PEP at SLAC would be operated at a fixed c.m. energy of 29 GeV. Following the surprising discovery of charm in 1974 and of beauty mesons in 1977 at Fermilab, there was expectation that the sixth quark might have a mass of less than 30 GeV. DESY was hoping to discover this new generation of particles. For this reason, the c.m. energy of PETRA at DESY was gradually increased to a maximum of 38 GeV. Unfortunately, the top quark turned out to be much heavier! The most prominent result at PETRA was the discovery of the gluon, the carrier of the strong nuclear force, in 1979.

At PEP we focused on searches for other new particles, including charm hyperons, and performed precision measurements of lifetimes and masses of these new particles, and also searches for new phenomena, exploiting the higher energy and enhanced performance of the tracking system and the calorimeter. The much improved photon detector was extremely important for charmonium spectroscopy and neutral pion reconstruction. Apart from inclusive spectra, I focused on the fragmentation of charm and beauty quarks and also studied the two- and three-jet structure in hadron final states, following the discovery of the gluon at PETRA.

As early as 1975, Burton Richter realized that due to the emission of synchrotron radiation, the size and cost of electron storage rings would increase as the square of the center-of-mass energy. On the other hand, linear colliders have no synchrotron radiation emitted in the acceleration process, and therefore their size and cost scale with the first power of the energy. Many years later, Burt Richter initiated the construction of the SLAC Linear Collider (SLC), which used the 3 km long linear accelerator to accelerate bunches of electrons and positrons to about 50 GeV. At the end of the linac, these high-energy beams were brought into collision with very strong focusing. The challenges for this machine were in the beam dynamics arising from their very high peak currents and from the submicron beam sizes required at the collision point, because this single-pass accelerator had much lower bunch frequency than a circular machine like LEP at CERN. The goal was to produce and study the Z^0, the intermediate vector boson which was first observed at CERN. The construction and operation of the ILC were an enormous challenge, primarily the control of the bunches from the source to the final focus, requiring incredibly complex systems for monitoring and controls.

ZIERLER Vera, how closely involved were you with SLC?

LÜTH: The MARK II detector was upgraded to make use of the advances in electronics, calibrations, and online control and data processing, and was installed in the new collider hall. I had spent the year before at CERN with a group from Munich who developed silicon strip detectors for high precision tracking. Upon my return from CERN, I teamed up with Sherwood Parker of U. Hawaii and Alan Litke of UC Santa Cruz to design and build the first three-layer silicon strip vertex detector (with microchip read-out) to be installed just outside the 25 mm-radius beampipe of the upgraded Mark II detector at the SLC. My primary responsibility was the high-precision support structure and the alignment of the system. SLC started operation in April 1989. Though the event rate was extremely low, first measurements of the mass and width of the Z^0 and constraints on the number of different types of neutrinos were presented on the Lepton-Photon Symposium at Stanford in the summer of 1989. A total of 856 hadronic decays of the Z^0 were recorded. One of my graduate students performed the very first measurement of the branching fraction for Z^0 decaying to beauty particles, a measurement that benefitted from the high precision silicon vertex detector and established the size of the coupling of the b-quarks to the neutral current.

From the point of physics, one of the unique capabilities of the SLC was the polarization of the electrons, which resulted in very high precision of the measurements of the critical Standard Model parameters. It was exploited by the SLD [SLAC Large Detector] experiment, after the Mark II was retired in 1990.

ZIERLER: How was SLAC able to do this while CERN could not?

LÜTH: At the SLAC Linear Collider (SLC) the extent of parity violation in the electroweak interaction could be probed directly in the production and decay of polarized Z^0 bosons. The Z^0 bosons were produced using longitudinally polarized electrons produced at the source. The electron polarization was randomly chosen pulse by pulse to be either parallel or anti-parallel (in other words left or right handed) to the electron momentum. The measured left-right cross-section asymmetry is directly related to the effective electroweak mixing angle $\sin^2\theta_W$, a fundamental parameter of the Standard Model.

At LEP the electron beams were not polarized and measurements of the product of initial and final state asymmetry parameters had to include a separate measurement of polarization of the final state τ leptons. At SLC the beam polarization enabled the direct measurement of the lepton asymmetry. Consequently, with 75% beam polarization, the left-right forward-backward asymmetries yield a statistical precision equivalent to measurements using a 25 times larger event sample of unpolarized Z^0 produced by the unpolarized electron beams.

To produce a longitudinally polarized electron beam for injection into the accelerator, SLAC developed a source consisting of a 3-electrode photocathode and a flashlamp pump dye laser. Longitudinally polarized electrons produced at the source were accelerated and the spin was rotated into the transverse direction prior to the injection into the damping ring and at the exit oriented properly for transport through the linac and the arcs so that the spin was again longitudinal upon arrival at the interaction point. After many years of R&D, polarizations of up to 80% were achieved. The produced Z^0 bosons were fully polarized, either right-handed or left-handed, depending on the randomly chosen spin orientation at the source. This impressive technical project took years to bring to fruition!

ZIERLER: Why did you move to the SSC [Superconducting Super Collider] and what was your role?

LÜTH: After Mark II was replaced by SLD, I started thinking about a possible B factory at SLAC i.e., a facility to search for CP violation in B meson decays which was predicted by Kobayashi and Maskawa in 1973! The concept of symmetries and conservation laws had interested me since my studies of K meson decays as a graduate student

Similar to measurements of CP violation in K^0 decays, tests of CP violation in B meson decays would require measurements of decay time distributions, though with decay lengths of about 0.3 mm. I had spent some time on the design of a precision vertex detector using silicon strip sensors, and I had prepared several reports on layout options, support structures and studies of position resolution based on simulations. As this was not an approved project and I couldn't get any support for R&D. Thus, I accepted an offer to join the SSC laboratory in Dallas as deputy to the Associate Director for Physics Research.

I had served a member of the SSC Program Advisory Committee for two years, and I knew several members of the SSC directorate. The director Roy Schwitters had been an Assistant Professor at SLAC many years before, one of the leaders of the Mark I and Mark II construction, operation and physics program, and later the spokesperson for the large Fermilab detector. Fred Gilman, the Associate Director for Physics Research had been a member of the SLAC theory group for many years, working closely with the experimenters.

ZIERLER: And what was your involvement in the creation of the B factory?

LÜTH: While I was at the SSC, I could not participate in further studies related to a proposal of a B factory. I made my earlier studies and simulations available to the LBL group who was taking over this task. Parallel to the SLAC effort, plans were being developed for an upgrade of the Cornell storage ring! After the SSC was terminated, the SLAC B Factory was recognized as a future DOE project and received some R&D funding to develop the design.

ZIERLER: Vera, besides the lack of knowledge from the Secretary of Energy and his staff, I wonder if you can reflect on perhaps some early warning signs you might have noticed that might have suggested to you that the SSC was not viable?

LÜTH: In many ways, the SSC proposal was somewhat early. CERN had claimed that they would complete a collider of similar capability in the year 1999, but nobody really believed that. There was no design and no realistic schedule for the LHC; it took 10 additional years to be completed, even though the tunnel existed! Furthermore, Fermilab had just upgraded the Tevatron Collider and the two detectors. So hardly any of their scientists with experience in high energy proton interactions or accelerator design and operation were ready to leave and join the SSC project. The DOE insisted to have a nation-wide site selection, rather than trying to fit this large project on the Fermilab site, following the long tradition of the large European laboratories, CERN and DESY, which had expanded beyond their original site, allowing particle beams under private property. The site in Texas had a number of advantages. Real estate was cheap! There was a lot of poorly used space. The soil is about a few inches deep, so one can grow cotton, but not much else. Also, some fraction of the 80 km long tunnel could be drilled in dry chalk, resulting in low cost and fast construction. Furthermore, there was strong support at the time by the Democratic governor and two very good senators, representing the state of Texas. In fact, Texas was offering 2 billion dollars in support of the project. Unfortunately, it turned out to be very difficult to find experienced staff persons because the Dallas area was viewed as not that attractive a place to live and educate their children. My husband and I found a nice apartment close to city center, and we enjoyed exploring Dallas and the local areas. Still, we more or less had decided not to stay, even if the project was to be completed. We preferred California, where we would be closer to our family. Most of the SSC staff members were quite young and they appreciated the low housing costs, typically $120,000, for a house with three bedrooms and a two-car garage! They were excellent young scientists, hard-working and excited to be part of this new project. The problem was that we were not able to hire more experienced senior persons. Among the exceptions was Jim Siegrist who joined us from Berkeley where he had just been promoted to faculty. He grew up in Texas, and had been a graduate student on the MARK I experiment at SLAC.

ZIERLER: Vera, what specifically was your role in SSC, what were you working on?

LÜTH: My prime responsibility was to assist the Laboratory Director and the Associate Director for Physics Research in the planning and oversight of the experimental physics research program, in consultation with the Physics Advisory Board. I maintained close contact with the national and international research community, convened various committees and advisory panels, and organized workshops on special topics, including a neutrino beam and studies of weak interactions. I represented the SSC Laboratory at US and international conferences and collaboration meetings. I shared the responsibility for the recruiting of scientists during the first 18 months, and spent the last 6 months assisting them in their effort to find employment elsewhere. With support from others, I was able to convince the DOE and other US funding agencies and also some companies to open positions for the highly qualified scientists in particle and accelerator physics, as well as engineers and computer specialists.

There were many reports to be written and reviewed. For a while I was assigned to present the laboratory’s monthly report to the DOE representatives at the lab. Quite a few of them had retired from relatively high positions in the military, but most of them had no idea about the science and engineering of this project. They were primarily focused on the schedule and budgets. One day, they didn't realize I was still in the room and one of them commented, “They are finally getting the idea,” referring to me and the lab management!

On one occasion in 1993, Fred Gilman and I went to Washington to visit members of Congress and make the case for the SSC Laboratory, its importance for the science and technology in the USA. We were accompanied seven Nobel laureates and met with staff and members of the advisory committee on science and technology, and also the federal budget office. We arranged for a press conference and some of the Nobel laureates gave talks on the importance of large science facilities serving frontier research at universities and in industry. All of us were invited to the White House to meet with Vice President Gore, presenting the benefits and the issues of the SSC Laboratory and listening to his thoughts about climate change and the environment.

ZIERLER: What was your prospective on the infamous under-budgeting of the SSC?

LÜTH: There were definitely short-comings in the project approval and management process. This was mostly before my time. The SSC was a very large and complex project, planned on a “green site”, with a limited number of experts preparing the various stages of the project planning. While the first two stages were largely conceptual, the preliminary and technical design reports required enormous number of details to be well understood, i.e. technical issues and engineering, staffing, schedule and costs. In regard to the budget planning, it was very important to include contingency funding. This means the risk of each component to the cost and schedule had to be assessed, and for items that were not fully specified and beyond prior experience and fabrication, the contingency funds could be as large as 100%! It was very important that these contingencies were included in the total cost and the funding profile based on detailed schedule planning. The lack of a fully qualified cost-and schedule control system had been cited repeatedly by the DOE, the Congress, and its principal investigative agency, the General Accounting Office. This shortfall could be attributed to several causes, including the failure of Swerdrup, the company tasked to take care of civil construction, and also delays in hiring a capable project manager. Of course, there were cost increases due to necessary changes in the technical design, in some cases the reason for such changes and their impact should have been analyzed more thoroughly to avoid cost increases and delays. The presence of more experienced personal might have allowed more scrutiny.

After the change to the Clinton administration, the annual budget decisions were impacted by new guidelines to reduce the national budget deficit and also by competition from other large science projects. Furthermore, this administration apparently was not too eager to spend billions of dollars a year to rescue a giga-project in Texas. Their focus was on applied science and technology and renewable energy. Efforts to attract foreign funding from Russia and China had not been very successful. Also, there were competing science projects, among them the advanced neutron source at Oakridge, the main injector at Fermilab, and plasma research at Princeton. In the fall of 1993, when it came to the vote in the House and Senate, the only funding approved for the SSC was for dismantling the facilities. Funding was provided for the Tokomak Plasma Experiment at Princeton in New Jersey, the advanced neutron source, and also initial funding for the B-Factory either at Cornell or SLAC.

ZIERLER: How long did you stay with SSC? Was it a sabbatical, so you could always go back to SLAC?

LÜTH: Both of us stayed for two years. Stanford allows senior staff and faculty members a maximum of two years of leave of absence, and I made use of that. My husband also worked at the SSC, but received his regular salary from SLAC. For me, this was a major change in responsibility and promotion.

ZIERLER: And did BaBar happen shortly after your return?

LÜTH: Yes, in December 1993, there was a first BaBar meeting! Anybody who was interested in B physics was invited, and more than 100 scientists from many different counties attended. This was before the detector design was defined, though there were ideas and conceptual designs for various components and people were invited to join the collaboration and participate in the preparation of the experiment. I returned from Texas in early March 1994.

ZIERLER: So planning for BaBar actually started before there was a real understanding of what it would accomplish?

LÜTH: Not really! From the outset, the primary physics goal of the BaBar experiment was the observation and systematic study of CP-violating asymmetries in the decays of neutral B mesons. Secondary goals were precision measurements of the decays of bottom and charm mesons and of the τ lepton, and as always, searches for other rare processes and tests of the Standard Model. The design of the detector was to be optimized for CP violation studies, and also well suited for these other physics topics. The SLAC B Factory was designed as an asymmetric e^+e^- collider to operate at a luminosity of 3×10^{33} $cm^{-2}s^{-1}$ and above, at a center-of-mass energy of 10.58 GeV, the mass of the Υ(4S) resonance. This resonance decays exclusively to B^0B^0 and B^+B^-pairs and thus provides an ideal laboratory for the study of B mesons. In the two-ring collider, electrons of 9.0 GeV collided head-on with positrons of 3.1

GeV, resulting in a Lorentz boost to the Y(4S) resonance of $\beta\gamma = 0.56$. This boost made it possible to reconstruct the decay vertices of the two B mesons, to determine their relative decay times, and thus to measure the time dependence of their decay rates and to uncover CP violation.

The layout of the BaBar detector was quite similar to the Mark II, but the implementation exploited major advances in electronics and detector technology. It was of cylindrical geometry, and centered on the beam pipe: A silicon vertex detector and a large wire chamber for charged particle tracking, a Cerenkov counter system to identify charged particles of different mass, surrounded by a large array of CsI crystals for the detection of electrons and photons, all imbedded in a coaxial magnetic field, and to detect muons, large flat chambers interspersed with the steel plates of the magnet flux return on the outside. For several detector subsystems, small prototypes had been built and used for laboratory tests. There were four different methods proposed for the identification of charged particles of different mass, i.e., pions, kaons, and protons, a feature that would be very important for background suppression. I ended up chairing a committee of four BaBar members to select the best option. Only one of the proposals included results from beam tests and measurements of a full-size prototype with electronic read-out, proving a separation of more than three standard deviations between pions and kaons up to momenta of 4 GeV. The committee's decision was pretty straight forward, and was well received, except that it was not the technology favored by the BaBar spokesperson. This new type of a Cerenkov counter was conceived by Blair Ratcliff and Jaroslav Va'vra at SLAC and became known as DIRC. The concept was relatively simple, but the implementation was very challenging, both the design and the fabrication of the principal components. The acquisition of the radiators made of synthetic quartz turned out to be very difficult! Fortunately, groups at Saclay and Orsay in France and at LBL with very strong engineering support joined this project, and delivered the various components in time for installation.

After a commissioning phase, the performance of this unique system was outstanding! Our Japanese competitors chose different techniques that did not perform that well.

ZIERLER: And how long were you involved with BaBar? What were your primary contributions?

LÜTH: Following the termination of the SSC project I returned to SLAC in March of 1994 and joined the BaBar collaboration. Soon after, I was appointed Technical Coordinator for the BaBar detector with responsibilities for the overall coordination of the design and construction. BaBar was supported by an international collaboration of scientists and engineers from 10 countries who participated in the design and construction, followed by the operation and physics analyses. The construction and installation of the BaBar detector components was completed on budget and on schedule. It operated smoothly for 9 years with a few upgrades, mostly to the electronics. In total, about 700 million Y(4S) decays were recorded. The BaBar experiment was recognized for broad research program, including several discoveries and many important precision measurements, and searches for signatures of processes that are not part of the SM [Standard Model].

As the head of a research group at SLAC, I supervised a large group of excellent students, postdoctoral fellows, staff scientists, and technical staff with major responsibilities for the detector operation, and a strong engagement in a variety of physics analyses. I am proud to recognize that almost all my graduate students and postdocs continue to pursue research in particle physics and have advanced to faculty or senior staff positions at major universities in the USA and in Europe.

At the time when my primary activities switched from detector issues to physics analyses many of the BaBar physicists were engaged in the search for CP violation and studies hadronic B decays, so I decided to focus my research on semi-leptonic, purely leptonic and radiative decays of B and D mesons.

These studies involved both rare and not so rare decays, leading to the first observations of hitherto unknown decay modes as well as precision measurements of decay rates and of the couplings of quarks via weak interactions. Most of these measurements required high precision and absolute normalizations, i.e. accurate understanding of the detector components and their performance, as well as various background components. Much of this work was performed in collaboration with scientists from Germany, Canada, and Russia. For several years I was one of the two coordinators of the BaBar wide analysis working group on these decays.

Much of the theoretical framework for semileptonic decays is based on this early work by Kobayashi and Maskawa. Extending the earlier proposal by Nicola Cabibbo for two generation of quarks, they introduced a matrix that contains information on the strength of the flavor-changing weak decays for three generations of quarks. The values of the elements of this so-called CKM matrix cannot be predicted theoretically, but have to be determined by experiments. Measurements of two of these matrix elements, $|V_{cb}|$ and $|V_{ub}|$, have been an important part of the BaBar physics program, they refer to $b \rightarrow c$ and $b \rightarrow u$ transitions, for instance in decays $B \rightarrow D \lambda \nu$ and $B \rightarrow \pi \lambda \nu$ (here λ refers to a charged lepton e, μ, or τ). Since leptonic and semileptonic decays are well understood in the context of the SM, any results that deviate from expectations would be of special interest. It turned out that the measured value of $|V_{cb}|$, based on these exclusive B decays differed from results based on inclusive decays of the form $B \rightarrow X_c \lambda \nu$, which result in larger event samples with fewer kinematic constraints. This problem still has not been resolved.

In 2009, the very first indication of a larger than expected branching fraction for decays involving the higher mass τ lepton was found based on a partial BaBar data sample. A more elaborate analysis based on the full BaBar data sample for the decays $B \rightarrow D^* \tau \nu$ and $B \rightarrow D \tau \nu$ decays was developed by my last and best graduate student, and in 2012 resulted in the sum of the two decay rates that exceeded the expected value by 4σ. Several years later, the LHCb and Belle experiments confirmed the excess for the decay B →D*τ ν, though with larger statistical and systematic uncertainties. One of the motivations for this study was to test if a hypothetical charged Higgs boson might result in an enhancement of the branching fraction due to the higher mass of the τ lepton, whereas its impact on decay rate involving the low mass electron or muon is expected to be very much smaller. At present this interpretation of the observed enhancement has not been confirmed. We are all awaiting measurements based on larger data samples and with smaller systematic uncertainties from current LHCb data or based on future data from the Belle II experiment. I consider this one the most interesting measurements I have been involved with for many years. We need a confirmation with an enhancement of five standard deviations. Currently a number of other decay rates are being studied by various experiments.

While I will continue to follow future studies of these phenomena, I have asked to have my name removed from the BABAR authors list and thereby ended my membership in the BaBar collaboration after more than 20 years.

ZIERLER: What were some of the central research questions that BaBar was designed to answer, and how well did it answer those original questions and perhaps others that came up along the way?

LÜTH: The principal goal of the B factories was to search for CP violation, the combination of charge conjugation and parity, in B meson decays. As mentioned earlier, the observation of CP violation was the first important result by BaBar, in parallel with measurements by Belle at the Japanese B Factory. In 2008 Kobayashi and Maskawa received the Nobel Prize for their groundbreaking explanation of CP violation in the quark sector. Several years later, BaBar published the first clear evidence for the violation of time-reversal invariance in B meson decays. Given that in the SM, the CPT invariance is assumed and has been confirmed, this violation of time reversal (T) is expected for processes that violate CP invariance.

Conservation laws are concepts that have been fascinating me for a long time. As early as 1918, the mathematician Emmy Noether proved a theorem relating symmetries with conservation laws. Specifically, due to the invariance of the laws of physics under spatial transformations, the momenta are conserved, due to time translational invariance, energy is conserved, and due to the invariance under a change in the phase of the wave functions of charged particles, the electric charge is conserved. For every transformation of a physical system that acts the same way everywhere and at all times, there exists an associated independent quantity! However, discrete symmetries, such as time reversal invariance or mirror reflection, do not lead to new conserved quantities.

Allow me to divert a little from physics: Emmy Noether was born to a Jewish family in Erlangen where she studied mathematics at the university. As a woman she was not able to attend classes, and therefore she was tutored by her father who was a professor in the department of mathematics. She completed her dissertation in 1907 under the supervision of Paul Gordon, who at the time was referred to as “the king of invariant theory”. As a woman she was excluded from any academic position and worked at the Mathematical Institute of Erlangen University without pay for seven years. In 1915, she was invited by David Hilbert and Felix Klein to join the mathematics department at the University of Göttingen, a world-renowned center of mathematical and physics research at the time. However, the Faculty of Philosophy objected to her joining the faculty, so for four years, Dr. Noether gave lectures under Professor Hilbert's name. She made a variety of contributions to the theories of algebraic invariants and numbers, and her work on differential invariants in calculus has been referred to as "one of the most important mathematical theorems ever proven in guiding the development of modern physics". This is very abstract math, and there is no way I will ever understand this work. These publications qualified her for the habilitation in 1919, followed by the promotion to the rank of "Privatdozent". At the International Congress of Mathematicians in 1932, her algebraic acumen was recognized worldwide. The following year, Germany's Nazi government dismissed all Jews from university positions, and she emigrated to the United States to take up a position at Bryn Mawr College. She was considered to join Princeton Institute of Advanced Study, but unfortunately, she died two years later after a very simple surgical operation.

Many years later, the Deutsche Forschungsgemeinschaft (DFG) initiated the Emmy Noether Programme to support outstanding junior scientists with substantial international experience who are qualified to lead an independent research group. I must admit, I had never heard of Emmy Noether in prior years, but since then, several of my postdocs were awarded the Emmy Noether six-year grant and have since been promoted to professorships.

Years after the premature termination of the BaBar experiment, physics studies based on BaBar data are still being performed, primarily measurements of decays of B and D mesons and τ lepton decays, and a variety of searches for processes that violate SM rules, or might be signatures for dark matter or new other particles. These results are presented at conferences and published in journals.

The spectrum of physics programs carried out by the two B factories over period of more than ten years, far exceeded the very broad scope of measurements originally foreseen. Both collaborations formed about a dozen physics analysis groups, each focusing on a related set of topics. There were close contacts with theorists and dedicated workshops to exchange ideas and develop an in-depth understanding of the theoretical work and the isolation of interesting signatures in data, and improvements of simulations of various signal and backgrounds samples.

In recent years, at KEK in Japan the storage rings and beam injection system, and the detector have undergone major upgrades, replacements of many components, electronics, and controls, the online and offline software, and complete replacements of computer systems. This unique facility has attracted many new collaborators from around the world. Over the next decade much larger data sets will become available.

Many of the previous topics will be readdressed allowing more precise measurements of critical parameters of the SM, like the CKM matrix elements and other parameters related to weak or strong decays, searches for new types interactions or particles resulting in deviations from the SM predictions, discovery of new approaches to analyses, ranging from CP violation to spectroscopy, and new phenomena predicted by beyond SM theories.

ZIERLER: Vera, you mentioned you were relatively young when you decided to retire. What were some of your motivations for that?

LÜTH: Well, I was younger than most retirees! I had spent wonderful years at SLAC, especially the last two decades as a member of the BaBar Collaboration. From the beginning, this organization of roughly 500 scientists and engineers, including many graduate students and postdocs from the USA and Canada, and 8 European countries, was led by an experienced team that set up rules for various activities and phases of the experiment. The detector design and fabrication were well conceived and beyond the state-of-the-art, especially in the areas of electronics and data processing, starting with digitization of the raw signals, transmission via fibers rather than enormous bundles of bulky signal cables, online single channel calibrations of all subdetectors via common read-out modules and FPGAs. Subcommittees on data processing, computing, publications, oversaw progress on various activities. About a dozen physics working groups were led by experienced coordinators developing a broad range of analyses and assuring critical reviews at all stages, in particular the assessments of systematic uncertainties and as well as the proper use of theoretical concepts and Monte Carlo simulations. In total, there were about 700 BaBar publications! As mentioned earlier, the analysis of semileptonic decays of B mesons into final states containing a τ lepton, which resulted in a decay branching fraction exceeding the SM expectation by almost 4 standard deviations. My graduate student and I spent three years to improve every step in the analysis, from the very detailed selection of events with two fully reconstructed B decays in the event, including evidence for a missing neutral particle, assumed to be the neutrino, to effective background rejection, and an unbinned, fully two-dimensional simultaneous fit to many individual subsamples. Once we obtained and understood the results of these fits, a thorough assessment of the systematic uncertainties was performed, checking all the distributions that entered multivariable background subtractions as well signal simulations. We also tried to find the origin of the observed excess of events, testing various theoretical ideas, in particular, the potential contribution by a hypothetical charged Higgs boson. This effort and the preparation of the 40-page article in Physical Reviews took almost another year to complete and to pass the BaBar publications committee! I very much hope that we soon will hear about the new results based on the large LHCb event sample. No matter what the outcome will be, this was a very exiting period, most of the credit should go to my excellent graduate student, who recently became an assistant professor and member of the LHCb Collaboration! So, this is why I decided it was a good time to retire!

These measurements have attracted quite some attention, especially from CERN, but also several journals. Together with colleagues from Belle and LHCb, we were invited to write an article for Nature, entitled “Challenge to Lepton Universality”. This was my very first publication in Nature, because the format and constraints of this journal are generally not well suited to publications of particle physics results, especially complex measurements.

Since then, I have continued to serve on a number of advisory committees for various international research laboratories and funding agencies in Norway, Sweden, Germany, and most recently at KEK in Japan. This gives me insight into the Belle II experiment and physics program at the KEKB collider. While these meetings and the formulation of various reports represent require quite a lot of effort, they allow me to stay current on certain developments and learn about other fields of science. Recently all these meetings were via Zoom, in California this meant several night shifts for me!

ZIERLER: And, Vera, to state the obvious, you've been amazingly productive since you've retired, so I wonder if, in some ways, retirement afforded you the flexibility to pursue the kind of physics that you wanted to do?

LÜTH: I largely withdrew from participation in active research. I occasionally attend some BaBar meetings via phone on topics that are close to my past activities. Until recently I served as Associate Editor for Physical Review Letters. My term ended a few months ago, and that was appropriate. I still check the preprint lists, but seldomly read the whole articles.

ZIERLER: Well, Vera, now that we've worked our way right up basically to the present, I want to ask you, for the last part of our talk, a few broadly retrospective questions, and then we'll look to the future at the end.

LÜTH: OK.

ZIERLER: First, I'd like to ask you, your tenure at SLAC was long enough so you can comment on both how the culture at SLAC and the science at SLAC have changed over the decades. So you can answer those as intertwined concepts because, of course, the culture impacts the science and vice versa, but, generally, I wonder if you can talk about how SLAC has changed over the years?

LÜTH: Wolfgang K.H. Panofsky is remembered as the legendary founder and the first director of SLAC, who for a period of 23 years shaped this laboratory. As stated eloquently by one of my colleagues "To scientists of my generation, Panofsky set the gold standard. His scientific leadership and vision created the wonderful environment at SLAC in which all of us could flourish."

Panofsky's first encounter with accelerators was a 32 MeV proton linear accelerator at the UC Radiation Laboratory in Berkeley. The design of this linac became the basis for future accelerators of increasing energy and intensity. He appreciated the unique and diverse opportunities at UCRL and had envisaged a productive career at Berkeley. However, when during the McCarthy era the UC Board of Regents required all employees to sign the loyalty oath, Panofsky decided to accept an offer for a full professorship at Stanford, even though his colleague Luiz Alvarez warned him, "Oh, Pief, you'll fade away at Stanford. Nothing goes on there! You'll never be able to do any significant research!" Well, Pief proved him wrong!

ZIERLER: [laugh] Right.

LÜTH: At Stanford, Panofsky teamed up with Edward Ginzton, an expert on microwaves, and became involved in all aspects of a new 1 GeV electron linac, the design, fabrication, assembly, and operational tests. One of the spectrometers at this accelerator was used by Bob Hofstadter to study nuclear structure, which earned him the 1961 Nobel Prize.

As a follow-up Panofsky, Ginzton, and other, mostly younger scientists began to develop a conceptual design for a multi-GeV accelerator, commonly referred as the “Monster”. The physics goals were nuclear form factors and tests of Quantum Electrodynamics (QED), electro-production of hadrons and resonance formation. The 25-m-long prototype, referred to as Mark IV, served as a test bed for critical components. It also was used for the treatment of cancer patients.

In 1957, this relatively small team submitted a 64-page proposal. The estimated construction costs for the 3-km long accelerator were $ 114 million, including 25% contingency funds for unforeseen expenditures. Panofsky realized that large experimental facilities had to be designed and built in parallel with the accelerator to reduce undue delays and attract the best scientists. The estimated cost of these facilities added $ 18 million. It took five years to obtain approval and the necessary resources to start construction in 1962. The 3-km long linac was completed as planned in May 1965 and the first beams were available a couple of years later.

This was a huge project, and Panofsky realized early that the establishment of the new laboratory far transcended the construction of the accelerator complex and its research facilities: It required an environment that would enable great scientists to perform outstanding research. This facility was to be open to qualified physicists from the United States and abroad. He formed a Program Advisory Committee to assist him in creating of “a vigorous, forward-looking research program in high-energy physics, with scientific priority determining the allocation of machine time.” As of July 1967, 24 proposals were received, of which 17 were approved for beam time. Approximately half of them were carried out by collaborations involving Stanford or SLAC scientists. Among them was the use of spectrometers measuring high energy electrons scattered on nucleons, uncovering structures which were later interpreted as quarks.

Panofsky was fully aware that the laboratory could survive only through intense accelerator research and innovation. He fostered upgrades to the linac energy, the addition of secondary beams of photons and K mesons, and a variety of unique experimental facilities. The earlier experience at Stanford with the $e^{-}e^{-}$ storage rings was the foundation for SPEAR and later PEP $e^{+}e^{-}$ storage rings, and resulted in many discoveries and new insights into what is now referred to as the Standard Model of electroweak interactions. The discoveries of the charm quark and the heavy lepton by Burton Richter and Martin Perl, and also the observation of “partons” inside the proton by Richard Taylor, Jerome Friedman, and Henry Kendall were awarded separate Nobel Prizes. Panofsky also promoted the use of SPEAR as one of the earliest synchrotron light sources, thereby launching the development of intense photon sources as novel probes to examine microstructures in a wide range of materials.

Panofsky viewed the free flow of knowledge across international borders as an avenue to foster collaboration and peace. He encouraged exchanges with scientists from around the world. Many of the prime movers at laboratories that were planning electron or synchrotron facilities came to work at Stanford or SLAC, and SLAC physicists spent their sabbatical leaves in Europe.

Panofsky had realized early on that to establish intellectual leadership and to take on an educational role, SLAC needed to recruit its own faculty. For the appointment of faculty members, associate directors, and department heads, he selected persons primarily on the basis of their scientific and technical expertise. Richard Neal and Joseph Ballam served as associate directors for the technical and research divisions for many years, as did the deputy director Sidney Drell, who headed the theory group.

Panofsky stepped down as SLAC director in 1984, at a time when the linac was converted into a prototype for the next generation of high-energy e^+e^- linear colliders. Burt Richter was the obvious choice as the 2nd director of SLAC. Up to that time SLAC's research program was accelerator-based particle physics, except for SPEAR being used as an auxiliary synchrotron radiation source. Its full upgrade in 2003 into a modern storage ring and the addition of a dedicated electron injector in 1990 secured the future of photon science at SLAC.

The same year, the Kavli Institute for Particle Astrophysics and Cosmology (KIPAC) was inaugurated to serve as a bridge between the disciplines of astrophysics, cosmology and particle physics. KIPAC's members work in the Physics and Applied Physics Departments on the Stanford campus and at the SLAC National Accelerator Laboratory. KIPAC's mission is to bring the resources of modern computational, experimental, observational and theoretical science to bear on our understanding of the universe at large. Research at the Kavli Institute covers both theoretical studies as well as participation in a variety of experiments, ranging from LIGO to record gravitational waves to dark matter searches by the LZ collaborations with a large detector being assembled in the Sanford Underground Research Facility in South Dakota.

Ranked as the top U.S. ground-based national priority for astronomy, the Vera Rubin Observatory is currently under construction in Chile. During its first 10 years of operation, it will conduct the Legacy Survey of Space and Time (LSST) of the entire visible southern sky. Its vast public archive of data will dramatically advance our knowledge of the dark energy and dark matter, as well as galaxy formation and potentially hazardous asteroids. SLAC has taken on the design and construction of the LSST camera, which started in 2015 in a clean room at SLAC. Weighing more than 3t this 3.2-gigapixel camera will be the largest digital camera ever built for ground-based optical astronomy. Displaying just one of its full-sky images would require more than 1,500 high-definition TV screens.

With the completion of the world's first X-ray free electron laser (FEL), the Linac Coherent Light Source (LCLS) in 2009, photon science became the central experimental research program at SLAC, through a series of changes in the organizational, scientific, and infrastructural set-up and in its science policy. This FEL is a source of ultra-intensive laser radiation suitable for studies of materials very much like those with synchrotron radiation, but with dramatically improved performance in some selected parameters. At present, SLAC is in the midst of a new major construction project that involves the replacement of another part of the original linac with LCLS-II, a superconducting FEL facility with some ground-breaking scientific capabilities.

The premature shutdown of the B-factory in 2007 with no options for a major upgrade to expand its tremendous physics research potential, was the end of on-site accelerator based experimental particle physics at SLAC. SLAC's reorientation unfolded in a science policy context in which funding priorities drifted towards the materials sciences and the life sciences at the expense of nuclear and particle physics.

Since then, several SLAC groups have joined national and international collaborations to participate in experiments at CERN and Fermilab, or have switched to particle astrophysics. These research efforts allowed SLAC to retain engagements in forefront particle physics and also detector developments, electronics, software and data handling, and future quantum computing. This broad know-how will continue to be critical to technology developments, accelerator research, and experiments, including photon science and cosmology.

ZIERLER: So, is it your sense that Panofsky built into his vision of SLAC that it would be conceivable that particle physics would no longer be a large part of the SLAC research agenda?

LÜTH: SLAC, like other labs, large or small, depend on their technical infrastructure and knowhow. Having a strong technical division is very important. Also, it is important that scientists are encouraged to reserve time to work on technical projects. Some years ago, one of my postdocs asked me for a hardware project in addition to his work on BaBar data analyses. A streak camera needed to be installed in the linac to record images of the beam. This was a very interesting task, including work with technicians to install the instrument, preparation of the readout system, design of the software to control the instrument, and analyze the data for use in the main control room.

Over many years, the SLAC linac has been adapted to serve many additional functions, starting with the beams injected into the storage rings or used for fixed target experiments, or special beam lines for the FEL. Its energy was raised to feed the SLC, with damping rings and sources for polarized electrons. Until a few years ago, the main accelerator components have remained unchanged, except for upgrades and higher power. Now, one kilometer section of the original linac has been removed, to provide space for the LCLS II , the new superconducting accelerator with much lower power consumption, allowing much higher bunch frequency and intensity; both badly needed for the free electron laser and other future projects! This was not an easy project, especially as SLAC had very limited expertise in cryogenics. As on other major accelerator and detector projects, like the PEP storage rings and the BaBar detector, collaborations with the large national laboratories at Berkeley and Livermore were formed, with shared responsibilities and funding. Also, in recent years, much closer relations with Stanford were established, mostly in astrophysics and photon science, and more recently in machine learning and quantum computing, areas where SLAC scientists have a leading role. Currently, there are no large particle physics experiments located at SLAC, but there are sizable groups working on ATLAS at CERN or participate in the neutrino experiments at Fermilab, and perform dark matter searches elsewhere. Also, the SLAC theory group remains a very important asset!

ZIERLER: Vera, I'd like to ask you—you were instrumental in the creation of the working group on women scientists and engineers at SLAC in 2015. So first I'd like to ask, what were your motivations in getting this working group up and running, and how might that motivation have been formed by your own experience as a woman over the decades at SLAC?

LÜTH: Prior to the formation of this working group, I had not been engaged in activities to address issues related to women scientists at SLAC or science education and careers for women. There were two exceptions: I participated in a program initiated by San Jose State University by attending a few meetings with parents of girls attending middle school. I was asked to explain that careers in science, engineering, and computing are suitable for their daughters, as long as they do not drop classes in math and sciences. I told them about my career, my family and hobbies, emphasizing that women scientists are not weird, but have lots of opportunities and a good life. The astronaut and physicist Sally Ride was part of this program! In addition, I have been mentoring women graduate students and postdocs at Stanford, typically two or three at a time. I usually meet with them for lunch several times a year. Our conversations have been interesting, welcome and helpful for the young women, most of them not born in this country. On some occasions, I was able to advise, and in one case I helped to address a serious problem with the graduate student's advisor. Stanford assigns mentors to all undergraduate students, most of them meet with the student once per year, if at all. I find this incredible, especially for a private university with extremely high tuition.

A few years ago, I had lunch with JoAnne Hewitt and Persis Drell, who was just about to leave SLAC and return to Stanford to become head of the engineering department, quite unusual for a physicist. Four years later, she was appointed provost of the university. During this lunch, Persis turn to me, "Vera, we really have to do something to get more women into SLAC at all levels." As faculty emerita, I was the only one among the three of us who had some time available!

I created a Working Group on women scientists as suggested by Persis and strongly encouraged by the SLAC Director Chi-Chang Kao and the Director of Science David MacFarlane. The goal was to identify ways to raise the number of women scientists and engineers at SLAC and to improve their career development options. At the time, the fraction of women scientists was about 11%, the fraction of female members of the faculty was about 9% , the lowest of any Stanford University schools or laboratories.

The total list of topics we addressed was very long. They covered two primary areas, recruiting, career development and promotion, and also work-life support (childcare/maternity leave), dual careers, working hours, and support for housing.

I invited eleven persons to join the group, three of them men. We looked at various documents and processes and I learned a lot about what Stanford does, what MIT does, and what other institutions do. I visited childcare centers at Fermilab and at DESY in Hamburg, and also at the NSF in Washington. Fermilab has had a childcare center for decades, located in a barrack near the site entrance and surrounded by playgrounds. A unique feature is that most of the teachers have been at the center for decades! The monthly fee of about $1200 is set by the state of Illinois! At DESY, the bilingual childcare is located just outside the entrance to the lab. It is supported by the city of Hamburg; the monthly fee is EUR 300. There is considerable engagement by the parents.

As a group, we examined many issues related to the recruiting process. For instance, in one of the searches for a senior staff position at SLAC, there was no women candidate, and we were told they could not find suitable candidates. This clearly indicated a lack of effort! We recommended that if there were no women on the short list of typically three top candidates, the best female candidate should be added and invited for a visit, just like the three male candidates. Studies have shown that letters of recommendation for male and female candidates have a largely distinct set of adjectives to describe the person. What's good for a woman is bad for a man and vice versa. Undoubtedly a common, but not so hidden bias!

We documented our findings and distributed the report to SLAC faculty and supervisors. The personnel department claimed that nothing needed to be changed. They stated that a childcare center on the SLAC campus would be too expensive to build and maintain, and anyway, there was no need. Well, we found a survey which indicated that there were about 150 children of preschool age with parents working at SLAC! The monthly cost would probably be in the range of $1500 to $2000 per child. Almost one salary will be used up! What do you do for two children? How do we assist parents with only one salary?

ZIERLER: [laugh]

LÜTH: Jonathan Dorfan, a previous SLAC director, was head of the Institute of Technology in Okinawa in Japan. His wife and a Japanese administrator proposed a childcare center and in three months a temporary building, a playground, and a fence around were completed. Since then, there is a new building with room for more than 100 children! Where there is a will, there is way!

ZIERLER: Vera, I asked you two cultural questions, and I want to ask you a scientific question broadly conceived.

LÜTH: Yeah.

ZIERLER: You've been interested in symmetries and conservation laws throughout your career, and you've explained beautifully what we have discovered in those realms so far. So I want to ask you, what remains unknown or poorly understood even today about symmetries and conservation laws? What work is there to still be done in this area?

LÜTH: More than 70 years of particle physics research have led to an elegant and concise theory of particle interactions at the sub-nuclear level, commonly referred to as the Standard Model (SM). On the basis of information extracted from experiments, theorists have combined the theory of electro-weak interactions with quantum chromodynamics, the theory of strong interactions, and experiments have validated this theory to an extraordinary degree. Any observation that is proven to be inconsistent with SM assumptions would suggest a new type of interaction or new particles.

In the framework of the SM of particle physics, the fundamental building blocks, quarks and leptons, are each grouped in three generations of two members each. The generations are sorted by their masses, but we don’t understand the very large differences of the masses of the three generations, for both quarks and leptons. At present, we believe that there are no more than three generations, because we have determined that there are only three types of neutrinos. In the absence of any direct indication for their mass, the SM assumes that neutrinos are massless fermions. As a consequence, neither mixing nor CP violation are expected in the neutrino sector.

It is obvious that the SM has some shortcomings, for instance, it depends on 19 numerical parameters. Their values are derived from experiments, but their origins are not. There are ad hoc selection rules, for example, the conservation of isospin, lepton and baryon number. Also, the SM is inherently an incomplete theory. It does not include gravity, and there are several unexplained features in cosmology, for instance, the matter-antimatter asymmetry requiring strong CP violation, the existence of dark matter and dark energy. To address some of these deficiencies, theorists have proposed variety of extensions, among them Supersymmetry which postulates that each SM particle has a high mass super-partner whose spin differs by ½.

Recently large underground experiments have revealed neutrino oscillations which indicate that neutrinos are not massless. These oscillations are only sensitive to the squared mass difference, so additional measurements will be needed to determine their masses and search for CP violation. In fact, there are many questions related to neutrinos and their interactions, primarily because precision measurements are so difficult, because neutrinos interact so rarely. Future laboratory experiments at accelerators and reactors, as well as astrophysical and cosmological probes will address many of these open questions, and may or not may not strengthen the evidence for physics beyond the SM.

Most particle physics experiments are designed to challenge to SM prediction by discovering forbidden processes or unpredicted particles, or attempt to improve the measurements of these 19 constants, or precision measurements of couplings to the Higgs boson. Some of these measurements or searches require the highest energies like the LHC at CERN with three experiments. In the recent past, one of their primary goals was the detection of supersymmetric particles. At present, measurements of the coupling to the Higgs boson are of high priority, but the event rates are relatively small to support precision measurements.

CERN is planning to further enhance the luminosity of the LHC and hereby the sensitivity of searches for rare processes and new particles, but this will require further upgrade of the detectors and the LHC. It is not clear how successful this will be and how many years this will take.

Currently, the large neutrino experiments are DUNE and NOvA at Fermilab, Kamiokande in Japan, and IceCube at the South Pole. There is a growing number of dedicated dark matter searches of modest size, as well as special high resolution experiments. The new B Factory at KEK is designed for high precision measurements of heavy flavor interactions and highly sensitive searches for dark matter and other unknown processes.

It is hoped that some questions related to the early universe will be addressed by astrophysical observations, observations of gravitational waves and associated emissions of neutrinos or radiation, or potentially new theories challenging the existence of dark matter or dark energy. Current and future experiments are much larger with novel and more sensitive detectors, and produce enormous volumes of data that are applying novel multi-variable machine learning techniques. These facilities are built and operated by very large, international collaborations of scientists and engineers. The level of sophistication is enormous! The interpretation of measurements also requires in-depth understanding of the signal characteristics and the backgrounds, and a theoretical framework for detailed simulations. The time scales for building and operating large facilities are very long, but the capabilities are very broad and impressive. This is evident, given the large number of publications from each of the four LHC experiments or the two B factories.

Since my thesis experiment at CERN, experiments are orders of magnitude larger and more complex in all aspects! Design it and figure how to build it--that's how Burt Richter started.

ZIERLER: Vera, what a comprehensive and elegant summation of all the physics that remains to be done. And so that begs my last question, I think, which is, for you personally and for the fields that you represent, what are the areas of discovery that you are most excited about as you look ahead to the future? What are the things that are within reach that you think that can be discovered in the next 5 or 10 years that will count as really fundamental discoveries on par with all of the exciting things that you've been involved with over the course of your career?

LÜTH: In some ways I addressed this question in the previous section. I will only comment on a few areas of particle physics that may deliver answers to some of the questions that the SM cannot answer or for which ad hoc rules appear to remain unchallenged. I will comment on three areas of experimental particle physics: accelerators and storage ring experiments at low energy and at the energy frontier, and neutrino experiments.

At low energies, charm and beauty factories have and will cover a wide range of interactions and studies of properties for a variety of particles, hadrons and leptons of moderate masses. Also, there is a growing interest in searches for dark matter, some at very low masses and therefore potentially detectable in rare decays or interactions of known particles.

At the energy frontier, the LHC serves four unique experiments, the very large general purpose detectors ATLAS and CMS, ALICE focusing on heavy ion interactions, and LHCb, a forward spectrometer exploiting the high production rates of beauty mesons and also baryons which are not produced at B Factories. The primary goals of ATLAS and CMS is to search for any new particles or new interactions which might be observed at the LHC's record-breaking high energies. Currently, the unique topics are studies of the Higgs boson's couplings to various particles, and the exploitation of large productions rates of top quarks and the W and Z bosons. LHC is preparing for future operation at much higher beam interaction rates, requiring substantial upgrade and modifications of the current detectors, which will take another decade to get ready for operation.

One area of experimental particle physics that has grown considerable in the past decade is neutrino physics. The great surprise was the observation neutrino oscillation in 2015, over a wide range of neutrino energies and sources with many different detector technologies. The existence of oscillations implies that neutrinos have a non-zero mass, which requires a modification to the SM! These oscillations are of great interest and have established that there are mass differences, but have not yet determined the mass hierarchy. Neutrinos could have other strange properties. They could turn out to be identical to their antineutrinos, and there is speculation that they might violate CP invariance. If this were the case, neutrinos could be related to massive particles that theorists think might have greatly influenced the formation of our universe.

Another area which most of us follow from a distance, is the detection of gravitational waves by the LIGO and Virgo spectrometers. These waves are disturbances in the curvature of spacetime, generated by accelerated masses that propagate outward from their source at the speed of light. It took about 50 years to develop the two-arm 4-km long LIGO interferometer with incredible spatial resolution, quoted to be at the level 1/1,000 of the size of the proton!!! As years go by, many of us will be learning about the strongest cosmological events, colliding black holes, supernovae, and colliding neutron stars and probably other surprising features.

There are so many areas of physics and other sciences that I find fascinating, most of them I know very little about, but am trying to keep track of a few!

ZIERLER: Vera, it's been an absolute delight speaking with you today. I'm so thankful that we were able to do this and for you to share all of your recollections and insights. I'm so happy for this. Thank you so much.

LÜTH: Thank you.

[End]