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My Recollections as a Physicist *

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Introduction

I am honored to be the first speaker of the symposium. I was supposed to talk about my role in the discovery of the τ lepton and my recent work on the investigation of CP violation through decay of τ and the semileptonic decay of t, B, D, K and π. However after reading my draft I felt it was somewhat fragmentary and does not quite convey what I have done since I left this university. We are supposed to talk about things we have done that we are most proud of. I will start out with how I became a physicist.

My birth place: Yuli, Hualian County, Taiwan
Birth date: February 1, 1930

Yuli Elementary School, Yuli, Taiwan (1936 to 1942)

There were only two Taiwanese students in this Japanese school. The other Taiwanese was Mr. Chi-Tang Lo, also an alumni of this university, who is now the chairman of the board of Bank of Taiwan.

Doshisha Middle School, Kyoto, Japan (1942 to 1945)

I went to this school in Kyoto, Japan, by myself during the Second World War. This was one of the best known private schools in Japan built mostly by donations from Christian Missions in New England. Many of my relatives including my father went to this school. Near the end of the war all the students were mobilized to work in the Mitsubishi Electric Company near Kobe to make radars. It was a prime target for the American bombing, so I left Japan and went to Manchuria via Korea three months before the end of the War.

Manchuria (May 1945 to May 1946)

The Americans dropped atomic bombs on Hiroshima and Nagasaki and the Soviet Army marched into Manchuria about one week before the Japanese surrendered. The Soviets took everything valuable in Manchuria back to Siberia and raped all Japanese women they could lay their hands on. All the able-bodied Japanese men were rounded up and taken to Siberia to work as slave laborers. The Chinese Communists marched into Manchuria ahead of the Nationalists and the Soviets handed them all the Japanese weapons they got from disarming the Japanese. The Communists tortured and killed all the landlords and wealthy people and took away all their land and money. The Nationalists came a couple of months later, but they were corrupt and incompetent. Eventually they were defeated by the Communists and chased out of Manchuria in 1948. I left Manchuria in May of 1946 with about 250 other Taiwanese with the help of the United Nations. I learned to speak fluent Chinese Mandarin in Manchuria and learned the horrors of war and the state of anarchy first hand. We went back to Taiwan from Manchuria via the major port cities of China: Huludao, Qinhuangdao, Tianjin, Tsingdao and Shanghai. This was a turbulent period in China in the midst of the civil war between the Nationalists and the Communists. The journey took two months by a Norwegian freighter.

Tainan No. 1 High School, Tainan, Taiwan (1946 to 1949)

I entered Tainan No. 1 (one of the best high schools in Taiwan) after I came back to Taiwan from Manchuria. The three years in this high school were the most valuable years in my life. After surviving the life and death nightmare in Manchuria I felt great confidence in life and a burning determination to do something for the world. I learned to speak English from an American diplomat and a nun. I became a student leader by being the president of the class. I learned calculus by myself and studied college-level physics and chemistry. I also built a physics and chemistry laboratory at home and did most of the experiments covered for college freshman and sophomore. My uncle, Zhu-An Liu, who was a chemist and a high school principal, was shocked to find out that I had a big jar of potassium cyanide for making the electroplating solutions. He begged me to dispose it immediately. He was also shocked and impressed by the fact that I had an elaborate distillation column to synthesize many organic compounds. I was accepted by the chemistry department of Taiwan University, the best university in Taiwan. I passed the entrance examination with the highest score among all the students who entered the university in 1949. During this period in Taiwan most books available on college level mathematics and sciences were in Japanese so it was my major tool of study during my high school years. Only in my senior year at the high school did I use English reference books and this was fortunate because all the textbooks except the analytical mechanics (in Japanese) used at the university were in English. It may sound strange that I never learned to give physics lectures in Chinese until 1978 when I first gave several lectures in Beijing at the Institute of High Energy Physics. Since then my ability to lecture in Chinese has greatly improved.

Chemistry Department, National Taiwan University (1949 to 1950)

I studied chemistry because I liked to deal with natural phenomena from atomic and molecular points of view. There was also some practical consideration involved in this decision. I thought chemistry was more useful than physics! Also I did not know anybody who was a physicist, whereas my father was a physician who knew more about chemistry than physics and I had three uncles who were chemists, one owned a chemical factory, another was the head of Taiwan's Bureau of Standard and the third was Mr. Zhu-An Liu mentioned previously. However by the end of my freshman year, after using Linus Pauling's book on general chemistry as a textbook, I realized that one needed quantum mechanics to understand chemistry and that was impossible at that time in the Chemistry Department of Taiwan University. I switched to the Physics Department. I did not become a chemist but I was glad that a few years I spent studying chemistry made me a more well rounded scientist in dealing with all sorts of chemical compounds that we encounter in our daily life and in our laboratory.

Physics Department, National Taiwan University (1950 to 1953)

I obtained my BS degree in Physics here. At the Taiwan University we studied basic physics and mathematics earnestly. The most memorable professor was Professor Wolfgang Kroll who got a Ph.D. under Heisenberg and left Germany because he was against the Hitler's regime. He taught mathematical physics, statistical mechanics, relativity, quantum mechanics at various times and always with the highest standard. I did my senior thesis with Professor Shengbiao Chung, who had a Ph.D. from the University of Paris, in his optics laboratory doing spectroscopy of some chemical compound.

Reserve Officer's Training (1953 to 1954)

I was commissioned Ensign in the Navy of the Republic of China. All male college graduates had to receive this training at a great expense to the Taiwan government. I never had to serve even one day in the armed forces after the training so it was a waste of money on the government and waste of one year of my valuable time. After one year of officer's training I learned something about the military and the working knowledge of many naval weapons and the navigational systems for ships (these are now mostly obsolete because of the positioning device by satellites used these days). I went to the University of Minnesota immediately after the military training in 1954.

Physics Department, University of Minnesota (1954 to 1958)

I did the *p-p* scattering experiment for my Ph.D. thesis under Professor Lawrence Johnston, who was the only person on board the airplanes on all the three atomic bomb explosions during the War. I always thought that the biggest defect in the intellectual tradition of China was that it never engaged in the experimental science in the Western sense. Since I believe all physical principles must be based on experimental facts, I felt duty bound to know how to engage in the most up-to-date version of experimental science. At that time at Minnesota they were building a 68 MeV proton linear accelerator and I thought that was the best opportunity for me to participate in the research. After finishing writing my Ph.D. thesis I realized that I must become much more familiar with topics such as field theory, scattering theory, symmetry principles etc. to become a first-rate physicist, so in my last year at Minnesota I studied Field Theory and QED from the late Professor Don Yennie. He thought I was good enough to be a theorist and found a job for me at Stanford to work for Professor Pief Panofsky as an in-house theoretician in the new project called Princeton-Stanford Electron Electron Clashing Beam Project, that was the first colliding beam project in the world. I thought I was going to do theoretical physics for two years and then go back to being an experimentalist. I never went back to do experimental physics again except for about half a year when I joined Bjorken and Luke Mo in 1980 to look for axions [1].

Postdoc at the High Energy Physics Lab at Stanford University (September 1958 to December 1958)

• Helped Build the First Colliding Beam Machine in the World

I participated in building the first colliding beam machine in the world. The project was led by Professor Pief Panofsky at Stanford and the late Professor G. K. O'Neill of Princeton. The late Professor Carl Barber was the senior research associate, Burt Richter (now the director of SLAC) and I were the research associates from Stanford and B. Gittelman (now a professor at Cornell) was a research associate from Princeton. I was supposed to spend 50% of my time building the machine and 50% figuring out how to do the radiative corrections. The purpose of the experiment was to measure the radius of an electron but the existing literature on the radiative corrections by an Englishman Redhead [2] and a Russian Polovin [3] showed that the radiative corrections were almost 100% and thus the experiment might not mean anything. It was thus decided that the calculation of the radiative corrections would be the top priority and I moved to the theory group of the Physics Department at Stanford, then headed by Professor Leonard Schiff, to spend 100% of my time finishing up the calculation.

Postdoc in theoretical physics at the Physics Department, Stanford University (January 1959 to August 1961)

• The First Calculation of the Radiative Corrections to the Colliding Beam Experiments and the First Theoretical Calculations of many Electron-Positron Annihilation Cross Sections

I finished my task of calculating the radiative corrections and many cross sections for the colliding beam experiment in about one year and wrote up my first paper on theoretical physics [4]. I made several important and original contributions in this long paper:

• I found that the double log terms of the form $(1/137)[Ln(s,t,u/m^2)]^2$ appearing in Redhead [2] and Polovin's [3] paper were infrared in origin, namely they occur in both elastic and inelastic terms with different signs and thus they cancel each other in the cross section. The reason why these terms appear all over the place in their papers was that they made an unrealistic assumption on the energy resolution. They assumed that the experimental condition is such that the maximum energy of the photon that is emitted is the mass of the electron in the laboratory system. With this unrealistic assumption it was impossible to see the cancellation of the double log terms. If one uses the realistic experimental conditions to define the photon phase space, the double log terms disappear. These double log terms are of order 1 compared with the lowest order cross section and thus the existence of such terms meant the failure of perturbation expansion and it would have made the entire experiment meaningless. I devised a simple procedure to extract these double log terms from both the elastic and inelastic terms and have them cancel out before performing radiative corrections to any problem. This elegant procedure was later applied in Refs. [5, 6].

• In 1959, the calculation of higher order radiative corrections was still considered a novelty. Feynman, Schwinger, and Tomonaga invented the methods of calculating higher order QED corrections only a few years back. People never thought that QED was an engineering problem whose calculation depended upon how the experiment was done. I was the first one to point this out and went ahead to calculate the radiative corrections to many experiments associated with both the colliding beam experiments and the stationary target experiments.

• I was the first one to calculate the hadronic contributions to the vacuum polarization using the dispersion relation relating the cross section for the electron positron annihilation into hadrons to the vacuum polarization [4].

• I wrote one of the earliest papers on electron positron interactions that became the most important industry in high energy physics subsequently [4].

I was invited to attend the 1960 Rochester Conference on High Energy Physics to report my results listed above in the plenary session. My audience included legendary figures such as Lee, Yang, Nambu, Feynman, Gell-Mann, Heisenberg, Oppenheimer, Bethe, Panofsky, Hofstadter, Rabi, C. S. Wu ... as well as young scientists such as Sakurai, Bjorken, Weinberg, Glashow, Salam, Cronin, J. Bell... I maintained friendly relationship with many of these people. My talk was a great success [7].

• Hofstadter's Experiment on Elastic Electron Scattering from Nucleons and Nuclei

At the 1960 Rochester Conference [7] I noticed that Hofstadter *et al.* were using the Schwinger's formula for radiative corrections in their electron scattering on nucleons and nuclei. They were erroneously using the half width of the experimental elastic peak as the energy resolution in the Schwinger's formula. I pointed out to Professor Hofstadter that the width of the elastic peak seen in the experiment was mostly caused by the machine energy width and the slit width and had very little to do with the radiative width in the Schwinger's formula. Also Schwinger's formula was strictly speaking not applicable because it ignored the nucleon recoil that resulted in different kinematics as well as the emission of photons by the target. The latter causes a few percent difference in cross sections between positron and electron scattering from a proton. Hofstadter not only did not get angry at me for telling him these embarrassing facts, we developed a life time friendship because of this. He was awarded the Nobel prize for his work on the electron scattering in 1961. I wrote up the radiative corrections to his work in 1961 [5].

• Deep Inelastic Electron Scattering and quark structure of nucleons

I did two other radiative correction works that resulted in somebody getting a Nobel prize. Hofstadter invited me to give a talk in 1963 at a conference held at Stanford in honor of Hofstadter for his Nobel Prize. My talk was chaired by C. N. Yang and I gave a talk on how to do radiative corrections to inelastic electron scattering [8]. I later wrote a review paper with Luke Mo on this subject [9] and also by myself [10]. These papers were used by the inelastic electron and muon scatterers all over the world including the SLAC-MIT collaboration and in 1990 Friedman, Kendall, and Taylor were awarded Nobel Prizes for their work in the discovery of quarks using the inelastic electron scattering at SLAC.

• Extracting the Width of the ψ

Another significant event was the discovery of the ψ in November 1974. At that time an informal group led by Bjorken [11] was organized at the Theory Group at SLAC immediately after the discovery. Our first task was to investigate whether this particle was a weak or a strong vector boson. For this we needed to know its width and branching fractions. I was almost the right person at the right place at the right time for the job. The problem was to extract an intrinsic width (about 65 keV) that was buried under the much wider machine width (about 3 MeV). I solved the problem in less than one hour [11] by using the area method which I invented in dealing with the machine width in the elastic peak of the Hofstadter's experiment [5]. My experience with Hofstadter's experiment [7] taught me how to deal with the machine width and the radiative width and my experience with the W boson problem [12] taught me how to approximate the Breit-Wigner formula with a δ function that was extremely handy in obtaining the branching fractions and the total width [11]. This was essential in determining that the J/ψ particle was a charmonium and not a weak interacting neutral boson. Burt Richter and Sam Ting got the Nobel Prizes for the discovery of the J/ψ particle in 1976.

Assistant Professor, Physics Department, Stanford University (1961 to 1963)

• Taught Quantum Mechanics and optics at Stanford

In 1961 I became an assistant professor and taught quantum mechanics to about 150 students. I was also in charge of an optics laboratory for sophomores at Stanford. I attacked head on in teaching quantum mechanics because in my youth I changed from a chemist to a physicist because I needed quantum mechanics to understand chemistry! Even today I believe that in order to truly understand quantum mechanics one has to teach it for at least a couple of years. I tried to be the kind of teacher I wish I had when I was in college. I expected a student to acquire the gut level understanding of the subject and the ability to solve physics problems using quantum mechanics.

For this reason I gave homework every week. I had three teaching assistants grading all the weekly home work as well as the mid-quarter and final exams for about 150 students every year. For those students who worked hard I rewarded them with good grades and they gained a solid working knowledge of this important subject. For the optics lab I just designed the experiments and wrote the manual for the lab and supervised about a dozen teaching assistants to take care of the students. At that time the laser just became available commercially and the application of the laser beam in various fields had just started. Also, Stanford had just hired Arthur Schawlow who invented the laser and got a Nobel Prize later in 1981 for it. He was very instrumental in popularizing the usage of the laser and revived general interest in optics and atomic physics at that time.

Stanford Linear Accelerator Center (1963 to present)

- Became a United States Citizen

SLAC started construction in 1962. Professor Pief Panofsky , then the director, offered me a tenured position at SLAC and I accepted it in 1963. I came to the United States as an exchange visitor who must go back to his homeland within two years after he or she gets a Ph.D. I was thus an illegal alien subject to immediate deportation even though I was married to a United States citizen. Pief asked Dr. Harold Brown, then the Chief of R&D at the Department of Defense and later the Secretary of Defense and the President of Caltech, to intervene on my behalf. He wrote a short letter stating that I made indispensable contributions to the projects sponsored by the Office of Naval Research at the High Energy Physics Laboratory and by the Air Force at the Institute of Theoretical Physics at Stanford. Because of his letter I received my Green Card in about two weeks (a record time) and eventually became a United States citizen in 1965.

I wonder sometimes what would have happened to me had I gone back to Taiwan. I would probably be either shot to death or become a very important person in Taiwan. I studied communism for two months at a communist cadre school in Manchuria but they never asked me to become a party member because I was almost impossible to be brainwashed and they did not know how to handle a person like me who had many strong views that were incompatible with their communist dogma. Fortunately all instructors were young idealists and true believers in their cause. I was only 15, too young to be sent to a gulag or shot. They were rather curious about me for I came from a remote place called Taiwan and went to one of the best schools in Japan and knew many things they did not know. In fact they were somewhat awed by me for I knew four languages: Chinese Mandarin, Japanese, Taiwanese and English and was much more urbanized and cosmopolitan than they who were mostly from rural China. We parted amicably. At that time in Taiwan (1963) having attended such a school alone would be enough reason to face a firing squad. This was the reason why I did not dare to go back to Taiwan for 29 years until in 1983 Professor Da You Wu, then the President of the Academy of Sciences, invited me to go back to Taiwan.

As the new laboratory began operating I did the following things:

• Theoretical Estimates of Secondary Beams from SLAC

Pief commissioned me to write the theoretical estimates of the secondary beam yields using the electron and positron beams from the Lineac so that experimenters could have rough ideas about the kind of experiments they could plan using the secondary beams such as photon, muon, π meson, K meson, anti-proton etc. The photon beam [13, 14, 15, 6] and the muon beam [13, 15] were calculated more or less exactly, but the pion and the K meson beams were estimated [13, 16, 17] correctly to within a factor of two compared with the secondary beam survey experiments [16] that were later carried out. Reference [15] is now the world standard on pair production of electron, muon and τ and bremsstrahlung from electrons and muons. The radiation lengths of all the materials listed in the High Energy Physics Data Book are based on my work. This work also contained the most up-to-date elastic and inelastic form factors for all atoms, nuclei and nucleons that were necessary for calculation for all the processes considered. My entire work on the secondary beams is now taken over by the Environment, Health and Safety Division at SLAC for estimating the experimental backgrounds and the hazards to human of the secondary beams at SLAC and elsewhere. It was amusing that in the process of writing Ref. [6], I found that Brown and Feynman [18] were the real culprits for making the unfortunate assumption that the maximum energy of the photon that is emitted is equal to the electron mass in the radiative corrections; Redhead [2] and Polovin [3] merely followed Brown and Feynman's prescription. This shows that one should not blindly follow the work of even the greatest master in the field.

• Quark and Gluon Distribution inside the Nucleons

Bjorken proposed to measure the fundamental constituents of nucleons using the inelastic electron scattering as one of the things SLAC could do after its operation. The idea was similar to the inelastic electron scattering from a nucleus where the shape of the quasi-elastic peak represents the momentum distribution of protons inside the nucleus. He assumed that the constituents be pointlike particles. This was an exciting idea because this was the first time human beings could measure directly the existence of the basic constituents of hadronic matter. For reasons incomprehensible to me, when Bj wrote up his idea in the Physical Review [19] he presented it as the test of local commutation relations of vector and axial currents that is much less appealing to the intuition than the notion of measuring the momentum distribution of the fundamental constituents of matter which he originally proposed. Feynman later popularized the Bj's original idea and called these constituents partons. The radiative corrections to this important experiment fell on my shoulders. Even though I wrote a paper on this subject earlier [8], there were still many practical considerations needed to be ironed out. The purpose was to obtain the theoretical lowest-order cross section from an experimental cross section that contains all the higher-order effects as well as the effect of external Bremsstrahlung due to finite target thickness. The difference between the radiatively corrected cross section and the experimental cross section could be more than 50% especially at low energy end of the electron spectrum where most of the events are the radiative tails from the higher energy events. Since there were millions of points to apply radiative corrections we must find a reasonably accurate approximation formula. The project must be a close collaboration between three kinds of people: (1) A theorist who knows how to derive formulas; (2) An experimenter who must design the experiment from the beginning so that it can be radiatively corrected. This means that we need an experimentalist who could arrange the experimental data in the computer so that it can be radiatively corrected; and (3) We need a computer expert who could program all the formula and apply the radiative corrections to each data point. I collaborated with Professor Luke Mo. I played the role of a theorist and he was responsible for the rest. We finished the project in one year and published the results in the Review of Modern Physics [9]. The paper became the bible of radiative corrections to electron and muon scatterers in the whole world.

- W Pair Production and their Decay Correlation

SLAC was also planning to build an electron-positron colliding machine called

SPEAR so I wrote several papers on production of W pair [20, 12] and the heavy lepton pair [21, 15] via either SPEAR or photo pair-production. The standard theory was not invented yet at that time so I assumed the mass of W to be 2 GeV [20, 12]. Even though the mass of W used was wrong the calculation with Tony Hearn [12] taught me several important lessons: (1) The μ-electron coincidence was for the first time discussed in this paper. Later when the μ-electron events were discovered by Perl *et al.* the most important question to answer was the spin of the parent particles that produced these events. Since I have worked on the production and the subsequent decay of both W pair and heavy lepton pair I knew how to solve the problem, namely from the threshold behavior [22, 23] and the energy spectra [12, 21] of the decay muons or electrons; (2) The phenomenon of decay correlation was for the first time proposed in Ref. [12] to investigate the magnetic dipole moment and the electric quadrupole moment of the W's [24]. The idea was later applied to the τ pair production and was used extensively by LEP people to obtain the Weinberg's angle in the Z^0 decay [25, 26]; (3) My co-author Tony Hearn invented the famous algebraic computer routine [27] called "REDUCE" to calculate Feynman diagrams. This was the first time such program was used to calculate a complicated problem. He convinced me of the value of his program earlier by completely reproducing the result of [20] in a few seconds that took Berman and I half a year to calculate. Now I use REDUCE to obtain covariant expressions for all the cross sections and transition probabilities but I use a program called "Maple" to obtain helicity amplitudes and numerical results. Both of these powerful programs are in my powerbook computer that I often carry with me when I travel.

• Tau Pair Production and their Decay Correlations

This is my most important paper [21] that produced over 600 citations and a Nobel prize for Perl. David Coward did a Ph.D. thesis under Panofsky and Ritson in 1963 to find heavy leptons by Photo pair production using 0.8 GeV photons from Mark III Lineac on Stanford campus. He came to consult with me on theoretical matters; that was the first time I heard about heavy leptons. He did not find anything. It became obvious that pair production had too much background and the colliding beam machine being planned at SLAC was the way to discover the heavy leptons. So as a sequel to my paper on the W boson production by the colliding beam machine I wrote this paper on the production and the subsequent decay of heavy leptons. This paper not only enabled Perl *et al.* to find the τ it also enabled LEP people to measure the Weinberg angle and at this moment it is keeping me busy trying to test the CP violation using the decay of polarized τ leptons. Perl *et al.* made a proposal [28] to detect the heavy lepton using my calculations in [21] for e-μ, e-hadron, and μ-hadron coincidence. I estimated the branching fractions for 1.8 GeV τ that was close enough to the actual mass of 1.777 GeV recently obtained at Beijing. Even though the paper was written 25 years ago and several years before the discovery, it is still regarded as the Bible on the heavy lepton. In this paper I also invented the method of decay correlation to measure the polarization of the τ pair and this was used extensively at LEP [25, 26] to measure the weak angle in competition with SLD at SLAC that uses the polarized incident electron at the Z^0 energy to measure the same quantity. I was invited to give a talk at the Perlfest in honor of Martin Perl's 65th birthday to reminisce why I wrote my τ lepton paper [21]. I said jokingly that because I did not have anything better to do at that time. It brought the whole audience to laughter and a dirty look from Panofsky who was the chairman of the session. My speech was published in Ref. [29]. Martin Perl was awarded the 1995 Nobel Prize for his discovery of τ in 1975. He invited me and my wife to Stockholm to participate in the Nobel Awarding Ceremony.

• Laser-Electron Scattering and Laser + Photon → Electron + Positron as the Sources to produce High Energy Polarized Gamma, Electron and Positron beams

I was always interested in the coherent nature of the laser beam and production of the polarized positron beams for the CP violation experiment to be discussed later. Therefore when Bob Palmer asked me to look into this problem I gladly accepted the challenge. I spent about one year looking into the problem and wrote the most comprehensive treatise on various subjects covered in the title of the paper [30].

My Recent Interests

• Investigation of CP violation through Decays Involving Leptons as a Parent or as a Daughter [31, 32, 33, 34, 35, 36]

I got the idea of doing this research while I was in Taiwan giving a seminar on the Tau Physics at the Academia Sinica in June of 1994. I noticed that I used the CP invariance to relate the polarization dependence of the decay angular distributions of τ^+ to that of τ^- in my 1971 paper [21]. This means CP violation can be tested by checking this relations. The standard model of Kobayashi and Maskawa predicts that there is no CP violation in any decay involving leptons either as a parent or as a daughter. This is because the standard model assumes that only W bosons can transmit the charged current. Thus if CP is found to be violated by any decay involving a lepton it will be equivalent to discovering a new force responsible for CP violation. I prefer this possibility to the one given by Kobayashi and Maskawa because the calculation is much simpler and it will allow leptons to participate fully in CP violation. This mechanism will make CP violations much more prevalent and thus it may be what is needed to explain preponderance of matter over anti-matter in our universe. If such a boson exists we can determine its coupling constants to nine quark vertices and three leptonic vertices. I have written six papers [31, 32, 33, 34, 35, 36] in the past two years and made ten speeches all over the worlds to promote such an investigation. My proposal is already the top priority item on the proposed Beijing Tau Charm Factory. Martin Perl has organized a group of people to do τ physics at CLEO detector at the Cornell machine (CESR). The group seemed to be very much interested in testing my idea and recently (March 7, 1997) invited me to give them one hour lecture.

Other Miscellaneous Activities

- Opening of China to the West through Scientific Exchange.

For the first time since P.R.C. was established China sent about a dozen most senior scientists from the Academy of Sciences to visit the United States at the end of 1972. Panofsky took me along to their hotel in San Francisco to welcome them and I served as their tour guide in the Bay Area for a couple of days. This was the beginning of the 25 years of collaboration between SLAC and China. There must be at least 500 Chinese high ranking officials and scholars who came to visit SLAC. We helped them build BEPC in Beijing and the synchrotron lab at Hofei. The first internet connection to China was through SLAC. I must have entertained several hundred visitors from China at home. I visited PRC for the first time in 1978 and gave several seminars there on the discovery of τ and the experimental evidence of Z^0 exchange in electron scattering using the longitudinally polarized electron beam. I accompanied the five delegates they sent from Beijing to Tokyo to attend the International Conference on High Energy Physics held there in the summer of 1978. This was the first time they sent anybody to attend a conference in the non-Communist world. Since I knew many people who attended the conference and I spoke Japanese fluently they could not have found a better travel companion to attend this conference. They also tried to establish their first contact with Taiwan, but the Taiwanese delegation was ordered to decline the invitation to attend the joint banquet arranged by me. I met Professor Yinchuan Yang who was the head of the Taiwanese delegates at that time in Shantou, China in 1995 and reminisced fondly about the awkward episode of 17 years ago. Scientists from Taiwan and P.R.C. seemed to be getting along fine now but the politicians seemed to be still quarreling with each other. The most important work they did at BEPC was the measurement of the most accurate mass value of the τ lepton. I helped them untangle the effect of the Coulomb wave function and the radiative corrections. Now they want to replace their BEPC machine with the Tau-Charm Factory. My proposal to test the CP violation using the polarized beams is their first priority experiment at the newly proposed machine.

- Physics Interview Project

In 1983 and 1985 I visited 11 countries: Pakistan, Sri Lanka, Bangladesh, Nepal, Singapore, Indonesia, Malaysia, Thailand, Philippines, Hong Kong and South Korea to interview potential physics graduate students coming to study at the United States and Canadian universities. We gave each student 30 minutes interview to assess their abilities in solving physics problem in English in front of us. Each year we interviewed about 220 students at about 25 universities in 50 days. I learned a great deal about these countries because half of our time was spent on sightseeing and having dinners with students, faculty members, the chairman of the department, the dean of the school or government officials.

- Revolt against TIAA-CREF

By 1980 the TIAA-CREF became too complacent in their operation for handling our retirement money because it was the only organization handling the retirement of employees for most universities and research institutions in the United States. I diagnosed that the reason of their complacency was lack of competition and lack of incentive to do better. So I led a revolt by having more than 200 participants sign the petition to the president of the Stanford University asking for a change. Now we have about 200 options for investing our retirement money and a freedom to control our own money after retirement. Most universities and institutions in the United States followed the lead of Stanford and offer similar options.

• Member of SLAC EPAC

I was appointed the member of Experimental Program Advisory Committee that advised the SLAC director on experimental programs in 1981.

Concluding Remarks

Taiwan has progressed a long way both politically and economically since I left here in 1954. Now people can say anything without fear of being persecuted. Its per capita GNP of $13,000 is about half of that in the United States, but an average person in Taiwan is richer than that in the United States because the saving rate in Taiwan is more than 30% whereas it is only about 4% in the United States. Instead of leaving the money to our children to let them squander away, we should use it to build a better society by investing more on education, environment, scientific research and guaranteeing the security of our newly gained freedom. We as physicists must participate in the betterment of this society by engaging in education, scientific research and creation of wealth by becoming entrepreneurs. Physicists must also participate actively in the political process to make sure that it is kept on the right track. I am glad to have met so many illustrious alumni who contributed so much to building Taiwan into such a prosperous society today. I notice that Taiwan has attracted many people from abroad in many fields. In Hsinchu I felt like I was in Silicon Valley, California, because so many leaders there are transplants from the Silicon Valley. I hope Taiwan will keep its environment attractive so that it can induce more excellent people from abroad to participate in the building of this society.

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