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KSU Physics Ernest Fox Nichols Lecture
March 1, 2010

Thank you Dean Zollman, for your kind remarks. I am honored to speak to you today, and I hope that I will be able to say something of interest to each of you.

When I attended school here, it was called Kansas State College.

1) Agriculture and agronomy were strong and I am pleased to introduce my friend during those years here, who later became a professor in Agronomy here at K-State: Dr Warren Prawl, and his wife Nancy.
2) The Physics Dept was in a different location in those days
3) Wildcats did just fine, but Oklahoma won the trophies in those days.
4) And in 1951, there was a terrible flood here in Manhattan.
5) I helped clean up several areas: Penny's, where we took wet clothes and spread them out on a field of alfalfa, to dry out;
6) IGA store where we had to wear gas masks - you cannot imagine the stench!; and
7) a home of an elderly widow, who had lived there for many years, with all possessions covered with mud on the main floor as well as a basement full of former treasures.
8) We are here today, primarily, to celebrate the life of Frederick Reines.

His parents were immigrants from Russia. His childhood was spent in an average community where his father ran an average country store. He first became interested in physics, while he was relieving his boredom of sitting in a classroom. He noticed a "diffraction" pattern as he looked at outside light through a hole in his fist. He was a man of evident intellect who concentrated full attention on the project of the moment. He was very articulate and loved to explain the details and objectives of work at hand. His presence raised an aura of respect and admiration. He spoke with a booming, bass voice - though not as low as Edward Teller! Our paths crossed as we both sang in Choral Societies in Los Alamos, Aiken, SC and Augusta, Georgia. He nearly always was a soloist. Above all, he displayed a high level of enthusiasm for life and his work. He really "LOVED" to be involved in physics, throughout a life-long career, and even in everyday life.

While he was being shaved at the barber shop, he asked, "I was just wondering - How many razor strokes do you make for an average shave? The barber said he didn't know --and they started counting! I have heard Reines and Cowan estimate the number of bits of information comprising all the scenes one views during a lifetime. Reines is credited with a large number of "firsts" in particle physics and was awarded the Nobel Prize, for his discovery of the neutrino. Reines was a giant in the field of particle physics.

In an interview with the New York Times, Reines was asked to describe the significance of his discovery of the neutrino. Reines said "I don't say that the neutrino is going to be a practical thing, but it has been a time-honored pattern that science leads and then technology comes along, and then, put together, these things make an enormous difference in how we live."

References to our experiment, sometimes use the word "discovery" and sometimes, "detection". This title, including the word "discovery" was imposed for my talk at the international conference, '"Neutrino, Santa Fe 2006", which was, to the day, the celebration of the 50th anniversary of the discovery of the neutrino. To be honest, I have some reservations about the word "discovery". That word implies an element of "surprise" at finding something new, perhaps by accident. In this case, there was hardly any surprise. In 1948, some 8 years before our experiment, Crane published a paper saying "Not everyone would be willing to say that he believes in the existence of the neutrino, but it is safe to say there is hardly one of us who is not served by the neutrino hypothesis as an aid in thinking about the beta-decay process.

Reines clearly thought that his earlier experiment at Hanford provided at least a "hint" of a neutrino and our first paper in 1956 was entitled "Detection of the Free Neutrino: a Confirmation" referring to his previous experiment at Hanford. However, this claim regarding the Hanford data was generally dismissed because of poor signal/noise ratio and the sparsity of data. I feel that it is appropriate to consider the Hanford work a success, but only in the sense that it provided enough justification to do a better experiment. I think most experiments turn out that way!

One day, while stalled in an airport, Reines and Clyde Cowan discussed the possibility of detecting the neutrino. The two became collaborators on that project. Cowan was a bit laid-back in demeanor and was experienced in several areas of physics. That combination made possible a successful group effort in carrying out a difficult experiment. Cowan was the Group Leader of the effort and the two worked very closely. They made frequent calls to each other, even late at night, to discuss new ideas. Reines stated "Clyde Cowan became my very stimulating and capable collaborator".

Reines had been given permission to take a year, away from his duties in the Theoretical Division at Los Alamos, in order to think ---about fundamental questions. How many of you would like to be offered that assignment? It represented enormous confidence in Reines' ability on the part of his superiors.

Please realize my difficulty in doing justice, in a short time, to the important but ancient history of events leading up to the neutrino experiment. The plot started roughly 100 years ago when the process of beta decay was studied intensely. The expectation, of course, was that the electron carries away a specific energy to account for the difference in energy states of elements undergoing transmutation, in order to be consistent with the long-held acceptance of conservation of energy and momentum. Measurements of the electron spectrum were very difficult and results were inconsistent. It wasn't until the late 1920s when two experiments provided confirmation that the electron spectrum was indeed--- continuous! Meitner wrote to Ellis, the other experimenter, "We have verified your results completely. But I do not understand this result at all!" It seems to me this result should be considered a genuine discovery - it was not anticipated, not even understood. And no explanation came forth to resolve the dilemma.

A serious debate continued between two points of view: On one hand, Niels Bohr was a proponent that conservation of energy and quantum mechanics were not applicable to the nucleus and that the development of new physics would be required. On the other hand, the possibility of some new particle to carry away some of the energy in beta decay, was completely rejected. At that time, we just had 3 particles: proton, electron and photon and we thought that that's all there were! In short, most physicists were very reluctant to accept either viewpoint - to reject conservation of energy or to accept any new particle.

The debate continued until in1930, Wolfgang Pauli is credited with a suggestion that the missing energy in beta decay was carried away by a neutral particle in the nucleus. He wrote to the Physical Institute in Zurich:

"Dear radioactive ladies and gentlemen. I have hit upon a desperate remedy to save the energy theorem. But I don't feel secure enough to publish anything about this idea, so I first turn confidently to you, dear radioactives. I admit that my remedy may appear to have a small a priori probability because neutrons (later termed neutrinos by Fermi), if they exist, would probably have long ago been seen. However, only those who wager can win, and the seriousness of the situation of the continuous beta spectrum can be made clear by saying, One does best not to think about that at all, like the new taxes."

So this communication hardly sounds like a convincing remedy. And the discovery by Chadwick of a neutron in 1932 did not alleviate the problem either. He said "it is possible to suppose the neutron is an elementary particle, but this view has little to recommend it at present."

The breakthrough finally came in 1934, when Enrico Fermi formulated a theory of beta decay. In his theory, the neutrino is uncharged, has a small mass and carries away some of the decay energy. This theory was widely accepted and carried with it an acceptance of a neutrino as an elementary particle. Notice that nearly everyone believed in a neutrino, based on a theory!

Reines undoubtedly reviewed all of this history and it would not seem surprising that he would choose to attempt detection of the neutrino as the most important goal to undertake. In fact, a publication by Crane in 1948 discussed plausible ways to detect the neutrino and suggested using a chain reacting pile as a neutrino source. However, Reines and Cowan, with their background in nuclear weapon tests, proceeded to develop a plan to use the huge flux of neutrinos associated with fission, in those tests. They later redirected efforts towards a chain reactor, first at Hanford and later at Savannah River Plant.

I was employed at Los Alamos in the summer of 1953 having been recommended by Max Fowler, my graduate professor at K-State. I had been employed in a graduate teaching position at K State and later in a graduate research position working for Fowler. He had been employed at Los Alamos working in the area of magnetic fields in the mega gauss range. You graduate students should take note of this train of events and might decide to be more attentive and respectful to your professors!

I found Los Alamos a delightful place. I learned to love chill peppers, as well as surrounding scenery. My first summer in LA I worked in the cyclotron group. During the noon hour, one day, I took a phone call when most people were absent. The caller asked to speak to John Broley, one of the staff physicists. I said, John is out to lunch right now. Could I take a message? He said," Just have him call me." I asked "who is calling, please? He answered, "Fermi". My jaw dropped and I was almost speechless. Just the thought of my talking to such a giant in physics, was overwhelming.

The next summer I traveled again to Los Alamos and after the paperwork, I learned that I would be in the neutrino group, preparing to discover the neutrino! After the shock, I tried to make a quick recovery, and I remembered especially one phrase from my courses at K-State-" it probably would never be detected!", so I had the sinking feeling that I had joined a losing project. But then I reconsidered and thought it might be a real adventure and possibly, a great opportunity.

This team was formed, which traveled to Savannah River Plant in SC. There were also two other members who did not travel with the team, a chemist and an electronics expert. Notice that the team make up is a trivial number compared to teams since that time that sometimes run into hundreds. Jones and Rice installed detectors and lots of lead shielding, Warren was a gopher, Harrison was expert in the area of large liquid scintillators, the kid in the lower corner looks like I did 55 years ago, and McGuire designed the tank farm containing a scintillator to take across the county.

An incident occurred in Los Alamos after the scintillator had been loaded into the tanks. One day, some radioactive material leaked near the Van DeGraff , located in the Physics building, which also housed the neutrino group. The activity was tracked down the hall as a staff member raced to the division office to report the spill, an action which is difficult to understand! The Neutrino Group reacted immediately. The last thing they wanted was to contaminate the Tank Farm with radioactivity. A decision was made to move the tanks to a remote location, an isolated point on the mesa, above steep cliffs, near a small lab which was reserved for use by Fermi. Later we realized we may have jumped from the kettle into the fire as someone pointed out that just below that cliff, there was a site used for explosive tests containing radioactive Lanthanum. However, we were most relieved that the scintillator tanks escaped contamination.

McGuire was also our entertainer on special occasions in Aiken, where all team members stayed. He played a neat guitar and composed great lyrics. Reflecting on his days as Test Director in the Pacific, he wrote -

> Don't stand under a coconut tree when the bomb goes off out here

And this one.

> "Oh, the little neutrino, of which there may be no,
> Has been merely suspected and not detected at all.
> With no mass and no charge you see, it's not very large,
> It resembles a nothing, in a swift little ball.
>
> Now to further the glory of our great Laboratory
> and achieve some new knowledge of scientific import
> We are bent on detection of this tiny cross section
> the results will be published in a learned report.

We found Aiken to be a delightful place too. It boasted of houses for the polo ponies that seemed to outshine many local residences. The flowers, especially camellias and magnolias, were magnificent. The beach was a welcome change from a hot and humid climate in Aiken. You can probably tell that I never got to first base with this gal because she said to me "You're a Yankee!

Our experiment was set up at reactor P and I noticed recently on TV that reactor P was being decommissioned. And they showed a video of the control room, something I could not photograph when I was there. But I failed to get a picture of that scene on TV.

We traveled from Aiken to the reactor a distance of about 8 miles. Our first day, while driving to the reactor, traveling through a swampy area we were shaken up a bit. If felt like a big speed bump but when we looked back, we saw it was just a big rattlesnake.

Moving closer to the reactor we noticed a sign reading DANGER, DO NOT STAND CLOSE TO FENCE, HIGH NEUTRINO FLUX.

So the scene was set. A long history of published results finally led Reines and Cowan to design a convincing test of the existence of a neutrino. He was aware of a proposal for detecting neutrinos by K capure, 1941, by Wang Ganchang. You can be certain that they agonized about this for a long time. What would constitute convincing evidence? How would you have done it? After all, the weak force introduced by the successful Fermi theory was really weak! Hans Bethe and Rudolf Peirls calculated the cross section for a neutrino interacting with a proton to be less than 10 -44 cm2, corresponding to penetrating distance of a trillion earths. It would take a trillion earths to stop a neutrino! And they stated "It is therefore absolutely impossible to observe processes of this kind, with the neutrinos created in nuclear transformations".

But there were other factors which gave Reines and Cowan a chance at success. First, their experience in conducting nuclear tests in the Pacific had fostered a "can-do" attitude and to "think big" when faced with huge problems. They enjoyed the full support of a large organization which was able to supply needed resources such as electronics, machine shop fabrication, and chemistry development. They also enjoyed access to intense neutrino sources, either from fissions in weapon tests, or from the large reactors, both of which required security clearance. Finally, Reines had outstanding ability indicated by his past experience. His thesis for PhD at New York University was calculations of the liquid drop model of fission fragments, elaborating on Bohr's model. He was hired by the well known physicist, Richard Feynman to work for him at Los Alamos. Cowan had received a Bronze Star for his work on radar at MIT during the war. His PhD thesis at George Washington U was on absorption of gamma radiation.

They finally settled on this particular reaction. ---As viewed 55 years later, this reaction seems so simple, so innocent, as if to say, what's all the fuss about? But in 1956, Fermi's theory was so successful in treating beta decay that it inspired nearly everyone to believe in the neutrino as a real particle, so why bother with a detection experiment? Reines argued that the scientific method requires experimental verification of theory to the extent possible. Of course, there had been numerous experiments attempting to record evidence of the neutrino using any approach that held promise, but those promises had all vanished.

Reines and Cowan had encountered such formidable cosmic backgrounds at Hanford that they were pleased that a detector could be placed near the Savannah River Plant reactor with 12 m of overburden, as well as 11 m of concrete shielding from the reactor core. So we proceeded to set up instrumentation there.

The theoretical expectation of this chosen reaction was a positron which would produce 2 gamma rays in annihilation, in opposite directions, and a neutron which would scatter and then be captured in Cd, resulting in some capture gamma rays. The proton target was created by filling the two small tanks with water. The other 3 tanks contained liquid scintillator, viewed by an array of 5 "photomultipliers. The detector array was surrounded by 4" to 8" lead shield. Without the shield, the total detector weight was about 10 tons.

The signals from banks of scintillators were directed to a nearby trailer, housing power supplies, amplifiers, and cameras able to record oscilloscope traces. These large electronic units contained absolutely no transistors!

This is a typical scope trace that might have been the result of the chosen interaction. The first pulse might represent the positron gamma rays from each of the bottom two scintillator tanks. The second pulse might represent the signals in the same two tanks, from neutron capture gamma rays. Recording both sets of pulses was a critical feature, as a way to discriminate against backgrounds. This was accomplished by demanding a coincidence within a few microseconds for the two sets of pulses. This feature required a signal delay so that the coincidence could be recorded on an oscilloscope. I am uncertain if this delayed coincidence technique was invented by the neutrino team, but this technique was used frequently in subsequent experiments. Our experiment would not have succeeded without this important technique.

When we turned on all units for recording the first neutrino from the reactor, we waited anxiously for the first count. And then we waited some more! We even called the control room to see if the reactor was still on! But soon some clicks indicated possible signals and we all relaxed a little.

It became somewhat comforting to find that our counting rate was about 5 times higher when the reactor was on, and about 4 times larger than other accidental backgrounds. The net counting rate of reactor associated events was about 3/hr for both detector triads taken together but of course the rate varied with many factors: the reactor power level, as well as many parameter settings. So evidently, something was being detected that came from the reactor!

The team felt encouraged by these rates and obtained additional evidence that neutrinos were responsible for the signal. To see if the reactor signal rate was proportional to the protons in the tank, the water target tank was diluted with heavy water to reduce the effective proton content, to about one half of its normal value. The reactor associated signal rate was found to drop also to about one half of normal.

Indication that the first pulse might be due to positrons, was obtained by observing that positrons from Cu64 added to the water target tanks, gave similar amplitude signals, as the first pair of pulses recorded in reactor associated signals.

Indication that the second signal was due to a neutron was obtained by measuring the time-delay spectrum of the second signal, which agreed well with the expected time-delay spectrum for the Cadmium concentration used. Doubling of the Cd concentration produced expected shortening of the time-delay spectrum without increasing the signal rate. Removing the Cd from the tank resulted in disappearance of the reactor-associated signal.

Other tests were performed including one in which additional shielding was placed between the detectors and the reactor.

We wanted to use bags of black-eyed peas, in deference to the South Carolina culture. However, we settled on bags of wet sawdust. This extra shielding should have reduced signals produced by neutrons by a factor of 10 but no decrease in the reactor-associated signal rate was observed.

Even then, the team worried that something might have been missed. In summary, we had a reactor-associated-signal of some kind, interactions with protons, and two unique signatures indicating a positron and a neutron. What else could produce such results, other than a neutrino? ----- There was a fear that we could not claim detection of a neutrino since we claimed consistency with Fermi's theory, and therefore that our claim of detection was dependent on the validity of that theory. ----- Reines countered by arguing that our results confirmed Fermi's theory of inverse beta decay and had the theory not been correct, our results would have been greatly different in some way.

Eventually, Reines and Cowan bit the bullet and started reaping some rewards for their labors. They sent a telegram to Pauli; "We are happy to inform you that we have definitely detected neutrinos from fission fragments by observing inverse beta decay of protons. Observed cross section agrees well with expected six times ten to minus 44 cm2." Pauli received the telegram while at a meeting at CERN where it was reported that a case of champagne was consumed during the celebration. Pauli remarked "Well, there really is a neutrino after all!" Our team celebrated with paper cups of COKE.

Word spread that we had detected the neutrino. One day, I took a call from *Life* magazine. They were interested in our experiment and wanted some pictures. I offered really exciting pictures of some amplifiers or a tank of scintillator. That turned out to be of no interest to *Life*, I don't know why! They wanted some baby pictures of Cowan---- and I responded that I didn't have any of those!

I mentioned the importance of the delayed coincidence earlier. And another delayed coincidence happened as the word of the Nobel prize for neutrino detection reached Savannah River. They were excited of course since they had provided enormous support for the experiment. It happened that my daughter was there at the Savannah River Plant on business from Los Alamos. During a meeting she was attending, an announcement was made about the detection of the neutrino. She stood up and said, "Hey, my Dad was here on that experiment!"

The team broke up after the experiment. I set up one of the detector tanks we had used, in an underground tunnel in Los Alamos, called the ICE HOUSE, in order to record possible evidence of proton decay. This topic was of continuing interest to Reines and we published a paper with a new lower limit for the proton decay. Later he led a giant experiment to determine an even lower limit with the huge IMB detector.

Cowan departed to George Washington U and then to Catholic U. At his suggestion, a bubble chamber was set up as a detector of neutrino events. Unfortunately, Cowan died in 1974 and so was denied sharing the Nobel prize. Reines was eager to pursue other questions that followed the detection of the neutrino. He fielded a follow-on experiment using one of the detector tanks in the Savannah River program, and proceeded to obtain a more precise neutrino cross section. And then there were other burning questions of neutrino mass and possible oscillations and he asked for support from Los Alamos National Lab to proceed. His request was denied, saying that he had enough fun already and it was time to get back to work.

So Reines went to Case Institute of Technology as physics department head to continue experiments. He suggested that I go with him and at times like this, I think perhaps I should have done so. There was one reason that I did not do so, and her name is Peggy Jo. Perhaps she will stand for recognition.

In the following years there were some noteworthy events: In 1956, our first paper on detection of the neutrino, 1960, our more detailed paper on detection, 1959 new cross section result with lower uncertainty which agreed with the two-component-neutrino, parity-non-conserving theory, 1988 Nobel prize (Lederman, Shwartz and Steinberger) for finding the neutrino, --- the muon neutrino, 1995, ----Nobel Prize to Reines for electron neutrino discovery, shared with Perl for discovery of the tau lepton.

This time span of nearly 40 years between the Reines detection paper and his award of the Nobel Prize, is somewhat puzzling, though, of course, we were all thrilled by the award. By the time of the award, Reines was quite feeble; he passed away just two years later. Possibly, the Nobel committee was waiting for more convincing evidence or confirmation of our results. But I understand the delay was due, probably, to a lack of support from the Laboratory Director in promoting a Nobel Prize. My feeling is that Reines fully deserved the award, for his expertise and dedication to the neutrino, throughout his career, and his detection of the neutrino has passed the severe test of time. His accomplishments include many important firsts recorded in the literature. Picking out just one; his observation of supernova 1987A with one detector recording a burst of neutrino events, with simultaneous recording of neutrino events on the opposite side of the planet. This was the unmistakable signature of an exploding star, confirming that the little neutrino could drive the largest explosion ever witnessed by human beings. As I understand, it is the interactions of neutrinos in these explosions, which may have given rise to nearly all elements heavier than iron.

There has been some controversy concerning our detection experiment. Perhaps some of you came to hear more about that! If so, I strongly encourage you to read Dr. Arns paper. He is the only person, other than Reines and Cowan, who has seriously examined our data, recorded in numerous notebooks stored at UC-Irvine. This study must have been enormously painstaking and time consuming. I believe his review was detailed and his judgments were fair. In cases in which experimental results are questioned, it is normal for the experiment to be repeated by another group. No such tests were forthcoming.

Fermi's theory allowed computation of the cross section which could only be deduced, in our experiment, by making several estimates. It would seem understandable that Reines, being a theorist, would stretch the intent of the experiment, i.e. to find evidence for a neutrino, and determine the cross section, if at all possible. As Arns points out, "Reines frequently calculated the neutrino cross section during the Hanford phase to see if they were on the right track, but at Savannah River "cross section checks do not appear to have entered at all".

Reines was well aware of the importance of measuring the neutrino cross section. Alvarez had pointed out in 1949 that "it would be important to know that the cross section is less than ten to minus 41 cm2 but that if the cross section were to be shown to be less than ten to minus 45cm2, then the whole neutrino theory would have to be re-examined critically and quite possibly discarded". Clearly, in the 1956 paper, Reines believed that his approximate value of the cross section was well within these limits. But this issue changed complexion in 1957 with the introduction of parity non-conservation in weak interactions, which changed the theoretical neutrino cross section by a factor of two. So, naturally, theorists were anxious to know if the neutrino cross section agreed with the new two component theory. Consequently, Reines placed high priority on making a new cross section measurement, with lower uncertainty. He made the assumption that the neutrino was a reality, which allowed the experimental configuration to be much simpler. His result agreed well with the two component theory.

In my time at Los Alamos, I was fortunate to be able to plan and participate in many challenging projects. Late in my career, I formed a team and built a large neutrino detector which we used at the Meson Facility (LAMPF) with a collaboration of several universities. I had intended to field it at Nevada Test Site and detect neutrinos there, for one thing, to demonstrate a practical use of neutrinos. At that time, there was a need to find unobtrusive methods to measure fission yield in downhole nuclear tests, in order to ascertain compliance with treaty requirements. I planned to look for neutrino oscillations there, also. We did quite a bit of work in preparing for an NTS test, such as measuring acceleration from shocks in those tests and demonstrating survivability of photomultipliers in such accelerations. We made extensive calculations of the neutrino spectrum from fissions in a nuclear test, and expected to record 100 neutrino events in a 30 second interval.

The Reines-Cowan neutrino team obtained successful results by overcoming a deficit of several orders of magnitude for the cross section, and introduced large scintillators for detection of particles. Reines left us with a legacy of work that has led to vast enrichment of our knowledge of the role of the neutrino not only in elementary particle physics, but in astrophysical processes as well. We are gratified that honors were bestowed on Reines but regret that Cowan could not receive honors as well. Our experiment opened the door for many questions about the neutrino to be raised. Each new finding seemed to trigger a few more important questions. What an intriguing particle it is! ---Our experiment opened the door for each of you and many others to make use of the neutrino as a tool in experiments that further our understanding of particle physics and our universe. It seems fair to say that our experiment was the trigger for a whole new field of particle physics!

I am grateful to the Reines estate who donated $100,000 of Nobel Prize funds to the Los Alamos Historical Museum. Through this donation, and by being privileged to have been a part of the neutrino detection team, I feel 'brushed' by a Nobel prize. I feel it is far better to have been brushed in this way than not to have been brushed at all. I am pleased K-State also has been brushed by a Nobel prize as I have been!

I am grateful for the good fortune of attending graduate school here at K-State and learning from outstanding professors who prepared me for experiences which I will always cherish.

When Reines graduated from high school, he answered the question posed in the year book, of which he was editor, "What will be your goal after graduation". He wrote, “To be a physicist extraordinaire.”

Thank you for your kind attention.