

Wayne Escott Hazen (1914-2010)

This is the report of an interview that I held with Wayne in 2007. The audio recording was transcribed in March of 2013 by Sara Stoutland, and that transcription was first edited by her husband (Wayne's son) Eric Hazen. A group of Wayne's former colleagues then read the transcript: Larry Jones contributed edits and additional information. Comments and corrections came from Jack van der Velde, Tris Coffin, and Mike Sanders. I then made revisions that include some fact-checking, some re-ordering of comments on a given topic, and some edits for style so that the text would better convey the conversational nature of the interview.

*Jens Zorn
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Early years

J: When did you come to Michigan?

W: I think it was in '48

J: You'd been at Berkeley before that?

W: Yes I was an assistant professor at Berkeley, but did not get tenure. So, bravely and innocently, I shopped around and visited Harvard, Princeton, Michigan and maybe MIT. I got offers from Princeton and Michigan. We decided on Michigan for two reasons: Wheeler was my sponsor for the visit to Princeton, and Jean sensed from a lunch with Mrs. Wheeler that social life would be quite different at Princeton than it was at Berkeley and what it probably would be at Michigan. Also, Princeton was known to hire more assistant professors than they planned to promote. So we came to Michigan. Barker was chairman at the time, but Crane and Dennison and Laporte effectively ran the department. So they were the ones with whom I talked.

I had a Guggenheim fellowship when I left Berkeley. We drove through Ann Arbor, paying our respects to the department, on my way to spend a semester with Bruno Rossi at MIT. I built a cloud chamber there and went to Echo Lake in Colorado the first summer, doing an experiment in collaboration (this was in 49, I believe).

So we came back here in fall of 49 (I believe) when Barker was chairman. My first important Michigan association was with Dick Crane; this may have started when I passed through on my way to MIT. Anyway, I'd been talking to Jim Van Allen about sending a cloud chamber up with his technique of lifting a relatively small rocket up through the atmosphere with a balloon and firing it. It was this method that led to discovery of the Van Allen belt.

J: He sent up the balloon and then shot the rocket?

W: Yes, very clever, very clever. During the war, Van Allen had been associated with rockets. So anyway, I was talking with him about building a cloud chamber and I didn't have money. I hadn't brought money from Berkeley with me. I do remember that Dick Crane immediately offered to put up the money to make some castings to begin this cloud chamber but it never materialized.

Undergraduate years

J: Can we step back a minute? You were an undergraduate where?

W: At MIT. And I recall thinking that I would like to see the west coast so I applied for grad school at Cal Tech and at Berkeley. I was pretty innocent. I got offers from both places, but I chose Berkeley because their teaching assistant stipend was a little bit more.

J: In retrospect do you think you would have been better off at Cal Tech?

W: No, I don't think so, no . .

J: What year did you graduate from MIT?

W: That was in 1936, and I then went to Berkeley, towing my Snipe sailboat from the Charles River at Boston/Cambridge all the way to San Francisco Bay. I took along a Dutch lady and her daughter to help pay for the gasoline. They were going to visit relatives in San Francisco. Near the end of the trip we ran out of money, so I left my trumpet as security for the toll to get across the Carquinez Strait bridge. That allowed us to get to Berkeley where the ferry pier extended a mile out toward San Francisco. I drove them out there and put them on the ferry to visit their relatives in San Francisco, but I had to leave my wristwatch as security for their ferry tickets. I then went to LeConte Hall on the Berkeley campus where I met Raymond Birge for the first time. He found a bit of cash to give me as a small advance on my teaching assistant pay, and with that I redeemed my trumpet and my wristwatch.

J: So you were not married at the time?

W: No. . yes. . no. I have to be careful here since my wife isn't here. I was married in 1939 after 3 years at Berkeley.

J: So you were at Berkeley, having done an undergraduate degree at MIT in the depths of the depression . . .

W: Yes, at that time my brother, Harold L. Hazen, was chairman of electrical engineering at MIT and he semi-adopted me; I lived with them for 2 years. Then the depression began to die out a bit, so the second two years, I lived in the dormitory with the other students. I had a tuition waiver, so that is how I got through.

J: Your brother was chair of the electrical engineering department at MIT when you were an undergraduate? Wow!

W: Well he was born in 1901, so he was almost 14 years older than I.

J: Did you enjoy MIT?

W: Yes, very much. They had senior theses. As an experimentalist, I built a vacuum chamber and studied the emission of electrons from thoriated tungsten filaments. In principle this was under the guidance of Professor Wayne Nottingham. But in fact I did this under the guidance of two of Nottingham's graduate students—Ralph Poole Johnson who later went to the Ramo-Woolridge Corporation for his career, and the notorious William Shockley who was one of the three who got the 1956 Nobel Prize for the transistor. Ralph Johnson very diplomatically suggested slight changes in the composition of my MIT senior thesis.

J: So you wanted to go the west coast, so you chose Berkeley and probably had a better time there than you would have had in Pasadena.

W: Oh, I'm sure. First of all, sailing on San Francisco bay was quite different from what it would have been in southern California. And second, the Sierra Nevada is much closer to Berkeley than it is to Cal Tech. So I lucked out in that choice.

Europe trip 1938

W: Then after I had done 2 years of graduate work, my father suggested that we go as a family to Europe. So I dropped out of school for the fall term and toured Europe on a tandem bicycle. Toward the end of that time I unfortunately caught what must have been hepatitis A. I found that the mountains in Switzerland were insurmountable. I was seen by a doctor who suggested that I head for home. So I did. I went back to Berkeley.

J: How long was the break in your studies?

W: We went over in June of 1938 and it was December when we gave up. We were in Bad Godesberg when Chamberlain made his visit to Hitler in September 22-23 of '38. Chamberlain stayed on the far side of the river, took the ferry over each morning, and then was driven in an open, old-fashioned car for a meeting with Hitler. And I remember standing and watching Chamberlain go by. We tried to get permission to go through the gate where Hitler was staying and so have a chance to see him standing on a balcony, but we didn't succeed. Then several weeks later, at the end of September, we were in the Black Forest staying at a *Jugendherberge* (youth hostel). We were confined to the upper floor, because members of the Hitler Youth occupied the bottom floor. And that night, we heard tremendous cheering from the Hitler Youth down as they celebrated the capitulation of Chamberlain at Munich.

J: Who was with you on this bicycle tour?

W: My sister. We had a long coast down from the Schwarzwald [Black Forest] to Basel. It was a wonderful coast. And we did continue our tour in Switzerland until the hepatitis laid me low.

When I got back to California, the Golden Gate exposition was being set up on Treasure Island. Robert Brode of the University of California was in charge of setting up three cloud chambers that would illustrate gamma rays, electrons and alpha particles; I participated in the implementation, on Treasure Island. I spent the summer as a demonstrator on Treasure Island and then, in the fall, got back into the academic world and continued to get my Ph.D.

J: So you had been doing coursework up until that time and after you got off Treasure Island, and you then got into research?

W: Yes, with Brode, since he had been the one who supervised the cloud chamber experiments there.

J: So that was in 1940, -41.

W: Probably.

WWII

J: When the war broke out, you were a graduate student at Berkeley?

W: Yes. I chose to teach in the V-12 program, while most of my friends joined with Ernest Lawrence on the bomb project.

J: Was that a choice? . You were married at the time, so you had an established place to live, and the teaching for V12 allowed you to stay on campus. Was that the main thing governing your choice?

W: Well, I'm not sure. I was perhaps a semi-pacifist . . .there was a touch of that. So during the war, I did the V-12 teaching and worked in the tomato canning factory down in the waterfront in Berkeley, I've forgotten which one . . . it was one of the big companies.

J: The V-12 work wasn't enough to keep bread on the table?

W: Oh sure. Work in the canning factory gave me the belief that I was contributing something. They needed employees.

J: Given your interests and abilities, it's remarkable that you were canning tomatoes. What did you do in the factory?

W: Well I recall that my job was moving pears and tomatoes as they came in on flats over to the women who were putting them into the machines. And since that wasn't a full time occupation, when there was little bit of leeway I learned how to use one of the unoccupied pear machines to pop pears onto the automatic peeler. Keeping up with the peeler was an interesting challenge. That is the only machine that I learned to use.

Postwar trip to Europe, meeting Jean

J: Did you have children at the time?

W: No.

J: What was Jean doing then?

W: Oh, she was an undergrad student. She got her degree as an undergrad, the same time as I got my PhD.

J: How did you meet?

W: Well, my father, an adventurous man, organized this trip to Europe for my sister and me. She had just graduated from Wellesley and I was willing to take time off. We went across on the *Île de France* , this in the days before transatlantic air travel became common. I went aboard ship with a former classmate from high school who had come to see me off. We admired an attractively dressed young lady standing on a cargo hatch. My friend went ashore and the ship sailed. At about 4 o'clock the ship had passed the Statue of Liberty and was heading out into the Atlantic Ocean. I was on an upper deck when I saw this young lady on the port foredeck, leaning over the rail, being befriended by a couple of young men. I guess I considered this a challenge. I went down to the fore deck and became acquainted with Jean Shearer, my wife to be.

We spent the voyage becoming acquainted. It turned out that she was from Grand Ledge, Michigan, near Lansing and only 100 miles from my birthplace, Three Rivers, Michigan. She was traveling as the companion to her high school English teacher who was experimenting with doing bicycle trips around England and who had persuaded Jean to be the sample of future participants for such trips. And by coincidence, that teacher turned out to be the daughter of the minister of our local Methodist church in Three Rivers.

J: So Jean was traveling with this person who was going to set up bicycle tours. She was part of a pilot project?

W: Exactly. Meanwhile my father had rented a Humber, a mid-sized car, in which we toured. He had a general plan for where to drive in England, but details were being arranged day to day. It worked out that our driving tours overlapped her bicycle route to some extent so Jean and I were able to keep in touch.

Then, after that, my parents went home. My sister and I we took off in the rain from in front of the Strand Palace hotel in London to take the ferry to France. Jean and her chaperone left their bicycles to do a railroad-and-boat tour on the continent. And we met again in Paris for about a week.

J: Was that meeting by design or by happenstance?

W: We stayed at the Grand Hotel du Mont Blanc, a hotel which Ted and Beth Hecht found not very desirable when they happened to go there.

J: But that was about 60 years later!

W: Well, yes and by then it had become simply the Hotel De Mont Blanc. I have a photograph of the old Grand Hotel du Mont Blanc—with a sign which no longer exists.

J: So you came back to the U.S. And Jean's home base at that time was?

W: It was still the depression, so she went to work in Lansing to earn a bit of money to pay off the loan she had for travel in Europe. She stayed in Lansing for about a year until I came back from Berkeley to be married.

J: When did you finish your degree in Berkeley?

W: Well, I'm unclear on dates, probably about '41. Jean says—we finished at the same time—and I think she says she was writing a final exam at the time of Pearl Harbor.

J: Where had Jean been student?

W: She had done two years at Michigan State before this trip and then she finished her last two years at Berkeley.

J: You got married after she finished her degree?

W: No, before.

Berkeley: Cloud chambers

W: And then I continued doing several cosmic ray experiments at Berkeley. One was underground in the Broadway, a low-level tunnel in the San Joaquin valley where the muon component of the cosmic rays had higher energy than above ground. We were studying the effect of energy on the interactions of muons.

For the next experiment, I built a portable set-up that I could put on our boat trailer and take to Tioga pass in Yosemite Park where we spent a summer taking pictures of higher energy, higher intensity cosmic rays and their interactions. The last experiments I did when I was on faculty at Berkeley were done by flying cloud chambers in B29s out of a base in southern California.

J: This was when you were an assistant professor?

W: Yes, or while I was becoming an assistant professor.

J: Has a real history of the cloud chamber been written? Many people, myself included, know what the cloud chamber is but not much about its development and refinements.

W: I don't think there is such a history. Descriptions of cloud chambers usually come up incidental to experiments in which they were used to observe cosmic rays and events at accelerators.

J: It was a very important detector for a long time.

W: That's right. My PhD thesis, on a topic suggested by Brode, started out as a study of ionization by fast particles. But it turned out that so little was known about the properties of cloud chambers that Brode told me I had done enough by the time I had shown and explained the details of condensation phenomena within a Wilson cloud chamber.

When a particle goes through it produces path of ions; the condensation efficiency, the fraction of those ions that act as condensation centers for drops, depends on the supersaturation that has been produced which, in turn, is a function of how much the chamber has expanded and (if the expansion was not counter controlled) the time that the particle goes through. So my thesis was on the operating characteristics of the Wilson cloud chamber. I was one of the first to look into the details of cloud chambers, details that were very important when those chambers were used for the instrumentation of accelerators and for cosmic ray experiments

J: Did you ever meet C. T. R. Wilson?

W: No. I had forgotten whether or not he was still in existence when I first went to England.

Leaving Berkeley

- J: So you remained with the V-12 program during most of the war and stayed at Berkeley to finish your degree. You stayed on as an assistant professor doing work that included the flying of cloud chambers in the B-29s. You then decided you would not go to Princeton, but go to Michigan.
- W: I didn't get tenure at Berkeley. The rumor was that Carl Helmholtz, thesis advisor for both Larry Jones and Kent Terwilliger and a very nice guy, had proposed that they keep me, but Louis Alverez didn't think so.
- J: Well in those days, and even much later, there was a strong prejudice against people becoming faculty members by just staying in the university where they earned their degrees. But not getting tenure at Berkeley brought you back to your home state, back to Michigan where you and Jean had family.
- W: Well, that's where the jobs were. And I've often said that Ann Arbor is farther than any place in the United States from real mountains or the ocean --- I liked the mountains and ocean! But it wasn't that bad.

Michigan early years

- J: Barker was the chair when you came to Michigan, but you said that Crane, Dennison and Laporte were the ones actually running the show.
- W: That's right. With failing memory I'm trying to remember our nuclear physicist who had a very many graduate students . . . Oh yes. . . Marc Wiedenbeck! He and I came at the same time. And later we heard the story that the dean had been recalcitrant . . . the two of us would not have been hired except that Dennison, Uhlenbeck, Laporte and Crane said that they would resign unless Marc and I were appointed to assistant professorships.
- J: I know that Michigan's physics department hiring was a very low rate during the depression. Dick Crane and Robert Thornton were the only professorial hires over the years 1932-1942.
- W: Thornton came here from Berkeley to build our cyclotron. He came from Lawrence's group where they knew how to make cyclotrons.
- J: I talked once to John Kraus who was a radio astronomer down at Ohio State. He had gone to high school, college and graduate school in Ann Arbor; he got his PhD in physics from our department in 1933. His father, Edward Kraus, had joined the Michigan faculty 1904 and was dean of LSA from 1933 to 1945. Having finished his degree, John Kraus stayed at Michigan during the time when our cyclotron was being built, and because advice from Berkeley was often needed but long distance calls were expensive, John used his amateur radio setup to provide the communication link between Berkeley and Ann Arbor.

On leave from Michigan to MIT and Europe

- W: Not long after I came to Michigan I went on to MIT for a semester. I had a Guggenheim fellowship, and the generosity of Henry Allen Moe, head of the Guggenheim Foundation, made it possible for me to combine that fellowship with a Fulbright award.

So in principle I did the Fulbright in Paris with the Leprince-Ringuet group who were doing a cloud chamber study of amazing new particles produced by cosmic rays. I then went to London to take the second half of the Guggenheim with P.M.S. Blackett. He had done very fine work in Manchester where he, Butler, and Rochester had found the V particles. But when accelerators began to take over, Blackett got out of the particle game and went to the University of London where he switched to studying the earth's magnetism by looking at rock samples that provide evidence for continental drift and for the long-term reversal of the earth's magnetic field. But even though Blackett was doing this when I came to London, I nevertheless proposed to do a cloud chamber experiment with him.

J: With regard to Blackett ... In 1961 I had just finished my thesis at Yale and was staying on as a junior faculty member to finish some other experiments, and for fun I was attending a small history of science seminar taught by Derek deSolla Price. One day we had the good fortune of having C.P. Snow as a visitor. He told the story that in 1932 Blackett had seen particle tracks that indicated the existence of a particle with the electron's mass but with positive charge. But just at that time he was being nominated to fellowship in the Royal Society and he did not want to jeopardize his FRS by publishing something foolish. Anderson, working at Cal Tech, had no such inhibitions, so he did not have to share his Nobel Prize for discovery of the positron.

W: That's interesting. Well one of the things that we hear about Blackett is that his Nobel Prize was one of the three in which Occhialini might have had a share ... in this instance because it was his bright idea to have a counter control the cloud chamber.

J: Was he also the one who felt left out of the Nobel Prize for discovery of the anti-proton?

W: No, that was Piccioni, a man I got to know back in 1948 when he and I were both in Rossi's lab at MIT.

J: So you got to work with Blackett and Rossi and Leprince-Ringuet and the big players in the cosmic ray business!

W: Sort of, sort of.

Fred Hendel

J: Where did you meet Fred Hendel? In Paris?

W: I came to the École Polytechnique in Paris to work in the Leprince-Ringuet lab just when they were analyzing the pictures they had taken at the Pic du Midi de Bigorre. They were looking at V particles and similar things that had been produced by cosmic rays. The analysis was all done in Paris. Those were the days when pictures were projected onto a horizontal table and then people looked at the pictures and put curves down to fit the track curvatures. Either Bernard Gregory or his co-boss, whose name I don't remember, was at the scanning table. I was in the scanning room watching when a man appeared at the door. And he just stood there and watched for about an hour while I was there. I remember thinking that this is probably someone who would just stand and watch things going on. . . and that is about all he would do. But just a week thereafter this man was doing the top work in the scanning—this was Fred Hendel.

J: So he was involved in the scanning and interpretation of the photographs?

W: He had been a member of the faculty at University of La Paz, Bolivia but didn't have a degree. To get that degree he had gotten a small grant that enabled him to study at the École Polytechnique in Paris.

J: So Fred was from Austria and then he had gone to La Paz because of the . . .

W: Yes, well he was from Vienna. He got his bachelor's degree at Vienna in two years instead of three, I believe. Their courses were set up in such a way that they weren't taught every year. And the only possible way to get a degree was in three years. Fred and one of his brilliant friends decided that one of them would go to one course and the other would go to the other course and they would share their notes and discuss them and by doing this, they finished in two years. Typical of Fred, probably with honors.

But this was the time of the German occupation, so he escaped, via France I believe, using a Bolivian passport because they were available under the table. And there were already quite a few German refugees in La Paz, so he decided to go to La Paz.

J: So you met him when he was back there getting his degree at the Polytechnique.

W: Yes, my first meeting with him was standing in the door of the scanning room, watching and digesting.

J: I've heard somewhere that the members of the committee on Fred's thesis defense included Louis deBroglie and Marie Curie.

W: Yes two Nobel Prizewinners! . . . I don't recall the name of the third examiner. I think the French academic policy at that time was such that only a French citizen could get the equivalent of a full Ph.D., the degree necessary for one to become a permanent member of the French university system. I think Fred's degree was a doctor of science. Somewhat later he got a postdoc at Princeton where he was talked into doing a cloud chamber experiment in Colorado; I believe it was in a trailer set up at one of the passes, which might have been a smidgen higher than Echo Lake. I've forgotten the name of the man with whom he was working under at Princeton.

W: And then, I was headed away. . . maybe it was 1958 when we went to Beirut.

J: Did you go to Beirut on sabbatical?

W: Well, it was not a university sabbatical but a leave funded by a Smith-Mundt grant and a Fulbright. You see the Fulbright program was funded indirectly by the sale of our military supplies abroad. Fulbright had the bright idea that it was ridiculous to ship all those things back to this country; they should be given to our allies who could sell them, the money then being held in escrow for the Fulbright scholarship program. I remember that in France the local governments had to decide what was an appropriate remuneration. The first Fulbright scholar who went to France was a student of Bill Fretter (who later became acting president of Berkeley). That student was married but they had no children. He discovered that the grant was such that they had to eat in a 3 or 4 star restaurants almost every meal in order use it.

I think I may have been the third one to go on that program.

J: I was in Tübingen, Germany in 1955-56. I didn't have a Fulbright—Fran and I were there on money I had saved from my four years in the Navy. But Ken Evenson and his wife Vera were there, both with Fulbright support; they could afford a Volkswagen. Of all those working in the Physics Institute in Tübingen, only the Evensons and the chairman Professor Möllenstedt had automobiles. We had a Messerschmitt 3-wheel motorscooter. Everyone else was riding bicycles, mopeds and ordinary motorscooters.

W: Apparently the Fulbright grants were generous not just in France, but in Germany too.

J: So you came to Michigan, and got things rolling here, and then you took a lot of time out? How did you keep an experimental group running here while you were off enjoying yourself in Beirut?

W: Well now we're back to Fred Hendel. I had quite a sizable group at Michigan when I decided to go to Beirut. Fred was then still at Princeton but not a staff member. So I contacted him and asked whether he was willing to come here and take over the research group while I was away for the year. So that is how he came here. At that time Dennison was our chairman. And Dennison made decisions. He would listen to the executive committee—I was a member—but the decision was his. And I remember in the case of Fred, his consulting me briefly and I was just sort of neutral, but Dennison approved the appointment.

J: Department chairs had more control in those days.

W: Fred was brilliant, is brilliant, was brilliant. He had some eccentricities but he was always very helpful. Later he became very involved in teaching, particularly with the Keller self-paced course that became his baby. Anyway, he took over when I went to Beirut.

Beirut and Microbus adventures

J: Did you enjoy Beirut?

W: Oh, yes! Very much. We had bought a VW microbus, taking delivery in Hamburg and were going to drive with the whole family to Beirut.

You need to know that the whole large area, including Beirut, was under French mandate after WWI. The French made Lebanon and Syria into two separate countries because Lebanon was then at least half Christian. The Muslims in Lebanon were always a little unhappy with that. At one time, Syria had effectively invaded Lebanon to stand up for the Muslims, and in 1958 they did it again. So the U.S sent a military contingent to warn the Syrian army to go home. This resulted in travel prohibitions that affected us: When we arrived in Istanbul, we found that orders from our State Department prevented us from driving beyond Istanbul on our journey to Beirut. This kept us in Istanbul for a few days and it ended up that we had make the last leg of our trip by sea, with our bus as cargo, on a Russian ship.

J: Was that your first microbus?

W: We've taken European delivery on two microbuses that we bought in the US. One in Hamburg for the Beirut trip, and later, another in Leeds.

W: I went back to the Pic du Midi for a sabbatical a second time, when our son was a year or two old, so that would be 40 year ago. That was when we took delivery of a microbus in Leeds, England. That microbus had undergone a clever conversion to semi-camper. The first one I converted a little bit, in the Polytechnic shop when we were headed to Beirut. But the second one already had a stove and things like that in it. So we had two micro-buses. Our first time in Paris in 53, we bought a VW beetle from an American. Both the French and the Germans had a system where a car could be bought by a foreigner without paying taxes. And if sold to another foreigner, it could spend its entire life avoiding taxes. We sold that beetle to another American when we came home.

J: I had a bit of an adventure like that in the spring of 1956. We had sold our Messerschmitt and I took the train to Milan to get a 125cc Lambretta Model D motorscooter to use for the rest of our time in Germany and then take back to the States. I took the train from Tübingen to Milan and bought the Lambretta from the Innocenti factory at a special export price (a little more than \$200, as I recall) and started my trip back to Germany. When I went over the border leaving Italy, the Italian customs officials were all smiles as they tore tags from my temporary registration papers and sent me on my way. I then rode about 100 meters to the Swiss border and they looked at the registration and said “there is no way you can bring this machine into Switzerland, you don’t have the appropriate papers.” I turned and went back to the Italian customs and they said “there is no way you can bring this machine back to Italy; it has been permanently exported.” So I went back to the Swiss side and, after much pleading, finally convinced them that I was just going to ride the Lambretta over the Alps (albeit via the St. Gotthard tunnel to avoid the highest regions) to Germany as fast as I could.

W: You were very fortunate! Swiss customs officials can be very strict.

J: You were in Beirut at the American University for a year. How do you feel about the way things are in Beirut now?

W: When we arrived there by boat, Soua Nasar (??), chair of the American University physics department who had been my first first PhD student in Berkeley, had arranged for us to have a separate house on the campus. This was very nice; indeed that was one of the reasons we took the Smith-Mundt award.

Well, every morning we awoke to the sounds of gunfire. We were told that in order to keep things stirred up a bit, unhappy Muslims went down the streets in Beirut firing shots in the air, so the retail stores would leave the barricades up on their stores and so commerce was shut down. And every morning we would see barges, bringing soldiers from their barracks aboard US ships berthed in the Mediterranean close to the airport. The soldiers landed and kept order during the day. Eventually the Syrians withdrew and the American ships left.

Well in answer to your question. Since we had our VW bus, we did lots of travelling in Beirut and the countryside in Lebanon. We saw the refugee camps and they looked pretty grim. They still have them of course. . . .

W: Did you know Ernie McCarus? He was the leader in Michigan's Arabic language program for many years. He and his wife were in Beirut the same time as we were.

J: His son Peter McCarus and my son Eric were very good friends.

W: That may be the son for whom I am, in principle, the godfather.

J: I believe that Peter McCarus took Catholic orders after having been a college student here for a few years. I think he is now in a monastery in the Boston area. For a long time, Peter was not allowed to communicate with anyone on the outside, but now he can. Eric has been in touch with him from time to time, and in fact visited him about a year ago.

I knew Ernie McCarus from being on several committees with him here at the University. And his wife Adele was my daughter Karen's teacher in junior high school.

W: Adele McCarus is French. We took a beginning course in Arabic before we went to Beirut and Ernie taught it. And Adele was the spokesperson in his course because he didn't have the ability or the confidence to do the vocal part.

J: Really? I thought he was an expert in Arabic.

W: He is, but apparently he doesn't have the best pronunciation. I believe we visited her parents in Beirut. They were upper class Palestinians who had been chased out when the English mandate ended. In principle the English were to have arranged free elections, but all that went astray. There was much disorder and confusion, and our feelings ended up pretty much on the Arab side.

J: I've had had similar feelings. Many years ago I spent five days exploring Dubrovnik, an historic walled city on the Adriatic. In my memory it is such a romantic, charming place. And in the mid 1990s the news broadcasts about the Bosnian uprisings showed the bombings and artillery barrages on Dubrovnik. The walls are still standing but many of the buildings within are reduced to rubble. It made me very sad to see this.

W: We were in that area back then. When crossing the border on our drive to Istanbul we had asked for advice on the best route... whether to go inland via the capital or along the coast. The man in customs was very enthusiastic about their wonderful new coastal highway. So we took his advice. It was indeed a beautiful highway along the coast, until we got to Split, the city that has Diocletian's palace.

I remember having lunch in a courtyard where there was also a local wedding going on. And when we were deciding what we would like to eat, the waitress made the friendly suggestion that we should come to the kitchen and there make our choices. It was wonderful.

After lunch we left the city and started on our way south, but the brand-new coastal highway had come to an end. A detour arrow directed us inland, and we made many stops to ask for directions, mainly using sign language. In general people could show us the way to the next village but most of them seemed to know nothing about Dubrovnik. I seem to recall that we drove for a long while along an inland road but then emerged again on a winding coastal road for the rest of that leg of our trip.

J: I remember driving our microbus in Mexico, along roads with steep dropoffs and it was scary driving a microbus.

W: You shouldn't have said that! We were driving along this winding coast road, headed towards Dubrovnik, when we saw a tortoise on the road. I drove on, but the kids insisted that we rescue it, so I had to back up. And I backed up using the rearview mirror. My family remembers to this day that our right rear wheel was tipped off the edge towards the Adriatic Sea. Fortunately, it wasn't tipped very much.

J: In the microbus, if I recall correctly, the exit for passengers in the back was only on the right hand side.... The only door on the left was for the driver.

W: That's right. But people were very helpful. We put some rocks under the other wheels to keep the bus from rolling, I got out the jack and jacked up the car bit by bit, far enough so I could pull the microbus back onto the highway. But the rim had been damaged; it was severely bent. Fortunately the tire still held enough air so that we could continue to a repair place where they took the wheel off and used hammers and an anvil to beat the rim back into shape. We then continued on our way to Istanbul.

J; You can drive with a damaged rim if the tire uses an innertube, but had they been tubeless tires, you would have been in trouble.

W: Oh, don't you mention that to me! Jean's ancient Lexus had aluminum wheels and they hadn't learned to coat the aluminum. So after the salt has corroded the aluminum a bit, you take it to a tire dealer and they sort of clean the rims so they don't leak. But I've found that you still need to pump them up every week or so.

Now we have a new car that has steel wheels, since it's a cheapie car, but I'm very pleased with it. I actually date back to the days of demountable rims. They come apart in a way that makes it very easy to take tires on and off. Did you know those?

J: Yes, but I've only seen them used in trucks and tractors with very large tires. I've never seen them used in an ordinary car.

Well, its 2:30 . . . do you need to let your wife know that you haven't fallen into the ditch somewhere?

The spectrum of cosmic ray energies

W: We can continue, if you wish. Where was I?

J: We were talking about your overland trip to Lebanon.

W: So that was about the end of our travels. Oh, I should say also that I did an experiment in which I took over the double cloud chamber system that Bernard Gregory and Charles Peyrou (the two senior leaders of the cloud chamber group) had set up at the Pic du Midi de Bigorre. I did an experiment to study the electromagnetic cascade that arises when a high energy electron hits a bunch of lead plates on a cloud chamber. The motivation was that one of my students in Berkeley had what we thought was a picture of a negative proton being captured; the evidence was a tremendous electromagnetic shower coming out that that looked like the energy corresponding to the annihilation of a negative proton.

J: In retrospect, do you think that was really an antiproton? [*the recognized discovery of the antiproton was in October of 1955 at the Bevatron in Berkeley*]

W: Yes, but there was only one event. So I was calibrating that by doing an experiment by which the upper cloud chamber would give the energy of the incoming electron and the lower multi-plate chamber would develop the shower. Fred Hendel and I must have been doing that for a year. And then Fred went over for one semester and then I did the other semester there.

J: In the 1960s, you were doing some work with radio showers as well.

W: Yes, that was an idea Fred picked up at an international conference that I didn't go to. The cosmic ray laboratory on Mt. Chacaltaya, near La Paz in Bolivia, is 5200 meters above sea level, exactly half way through the atmosphere. There physicists from MIT and Japan had set up one of the world's biggest scintillator arrays to study cosmic rays showers.

J: Those were standard scintillators, and you were going to supplement that with signals from an array of radiofrequency antennas?

W: Yes exactly. We used their information to know what had hit and then we looked at the radiofrequency energy coming from it.

J: I remember that in the 1960's you had what seemed to be the world's supply of T.V. antennas stacked in the upper corridors of Randall

W: Mike Longo always points this out! He took one home and I believe he still uses it.

W: And another thing I did was to augment those Yagi antennas by adding longer elements to get more sensitivity. We were able to detect radiofrequency pulses correlated with the scintillator counts, but the rf pulses by themselves did not give us new information.

Larry Jones says that there is interest in doing the rf work again in the hope that modern technology will enable useful data to emerge.

So that was one of the last things that Fred and I did down at Chacaltaya.

W: Did you ever know Herb Bridge at MIT? He joined Rossi's group in 1946 and did a lot of work with cosmic rays. In 1961 his group's research on interplanetary plasmas provided the first quantitative measurements on the solar wind.

...I'm not sure why I brought his name up.

J: I still find cosmic rays to be quite amazing. I find it hard to imagine that the highest energy cosmic rays, as individual particles with many Joules, have an energy comparable to that of a fast-pitched baseball.

W: And you see the big new array down in the Argentine is to try to settle the question of whether there is a cutoff at high energies due to the Greisen-Zatsepin-Kuzmin effect. In this, the universal black body radiation looks like very energetic gamma rays in the frame of reference of a cosmic ray. So that should provide a cutoff. I think Larry says that they

have good evidence one way or the other. And . . . this is terrible . . . I've forgotten whether they have evidence for a cut-off or not. . . . Perhaps out in Utah?

J: My personal feeling is if they find a cut-off, they should remember Van Allen. His detectors found a decreasing count rate with increasing altitude, not because of reduced flux but because the flux had gone so high that the counters were saturating.

W: I particularly recall hearing Fermi give a remarkable lecture on the possible mechanism for acceleration for cosmic rays. Space is filled with electromagnetic fields that are dynamic. A cosmic ray going through these fields will be deflected. This can be described as a collision between a particle and a moving field. And to this day, I can remember Fermi, in his wonderfully lucid way, saying that when you're walking down a crowded street, you have more collisions with people coming toward you than with the people who are going in your direction. Right? So there is an energy gain by the cosmic ray when it makes a head on collision with the field, and there is a bit of loss of energy in the other case . . . but the head on collisions occur more frequently.

J: Forgive me, but this reminds me of times when I have heard talks by smart, articulate physicists — I haven't heard Fermi talk, but I've heard Robert Dicke, Willis Lamb, T.D. Lee and people at that level.

W: Not Sagan?

J: Not Sagan. But these physicists give good talks and the material sounds quite plausible as they are talking. But then I come out of the lecture room and a friend comes up to me and asks "could you explain to me what Dicke said?" And I start the explanation but suddenly realize that there is some critical point that passed me by . . . I hadn't really grasped the content — I seem to have been mesmerized by the elegance of the presentation.

W: Well anyway, Fermi later did the analysis and his calculations didn't result in the observed spectrum of primary cosmic rays. His explanation was that some of them must have escaped.

Quark search

J: When you came back from Beirut you were doing experiments based here. What kind of things did you do after the radio work?

W: Well, do you remember the tent on the roof of Randall?

J: I remember that there was one but I'm not sure I ever knew whether that was just Ned Dickman's hideout or what. . . .

W: Anyway, I commuted to Leeds for 5 or 10 years and that was the result of Brian McCusker's lecture here when he was preparing us for his Nobel Prize. Using cloud chambers, he had observed things from cosmic rays with a charge of one third. I quickly sensed it was because he didn't know anything about cloud chambers. What he was seeing was particles that went through when the condensation efficiency was one third. Probably went through later than the other ones when the super saturation had diminished.

J: What time scale is involved when one is expanding a cloud chamber?

W: Oh, hundredths of a second. The only sure fire way to get good results is to delay the expansion. A particle goes through and produces both plus and minus ions. And you have an electric clearing field that separates the two columns and you delay the expansion until they've separated enough so you can see them separately. Then, it depends upon the liquid that you're using. But usually, if the thinner column is thirty percent of the density of droplets of the other column, the condensation efficiency has been 99.9%. So that is the only sure fire way to know that you're OK. So then you would separate tracks like that. You could look for ones where the denser column--

J: So was McCusker based in Leeds?

W: Oh no. He's a mad Australian. I think he was Irish maybe.

J: He had presented this evidence, so how did that tie in with your work at Leeds?

W: I first got interested in doing the fractional charge search here at Michigan. We had a chamber that we had used in the salt mine and in Colorado, so I set it up in my lab in 4036 Randall and had a student photograph showers of cosmic rays to look for the McCusker effect, but we never found it. Then I got in touch with Herb Bridge at MIT who had built a big cloud chamber for use with accelerators; I didn't think he was using it because bubble chambers had almost completely taken over. I went to MIT and had it packed up and shipped to Randall Lab where Fred Hendel and I worked on it for a while. We then shipped that cloud chamber to South America and set it up in the Chacaltaya Lab. We had two or three, or maybe four graduate students do experiments with it.

J: Who were the graduate students? Do you remember any?

W: Cock (??) He was unhappy working with the cyclotron here so he shopped around for another group. He joined us and went to South America. Paul Barker, as a post-doc, worked for us for a while. Who else got their degrees? One or two others. . . .

J: I know that the Michigan cyclotron was a tough place to work during the 50's and 60's. Parkinson ran a tight ship.

W: Some of our work was funded by the Air Force and I remember that in 1969 when McCusker announced that he had discovered free quarks, the head of the AFOSR in Washington gave us an excited ring and asked couldn't we look at those pictures from Xacochia (??), but we didn't have any way of really adequately seeing them. But then I remembered that the world biggest cloud chamber was at Leeds, England. I telephoned a man I knew at Leeds and his wife answered. She was quite excited; we knew each other a bit. She said that her husband was in the bath, but would soon be out. He did call back and gave me information about this cloud chamber that had been built by another man in his department. It had been made with stainless steel so it could be used in the magnetic field environment of an accelerator, but with the advent of the bubble chamber, it had never been put to that use.

So that's how I ended up looking for quarks with that huge cloud chamber at Leeds. I commuted back and forth. Have you been in an English lab?

J: Only briefly to visit. I've not worked in one.

W: Things go at a modest pace over there. We had a little trouble, differences of opinion, now and then because I introduced the American way of doing things: You didn't shut

down the cloud chamber just to show visitors or for other trivial reasons. You kept the damn thing going.

W: At any rate, we never found any evidence for quarks either at 4036 Randall or at Leeds.

J: Did you interact with Peter Franken? He was also looking for quarks.

W: No, I'd forgotten that he did.

J: He and Earl Johnston, his graduate student, were looking for fractional charges with a modified oil drop of experiment.

W: You mean, charges just floating around?

J: Yes. He had the thought that quarks might be found in sea water, since there is so much of it being exposed to cosmic rays. He said (this is typical Franken) that you needn't limit yourself just to a jug of sea water, you need to do measurements on something that concentrates the minerals in sea water. So he bought oysters on a research contract!

W: And then ate them!

J: He probably did.

On the same principal that an ocean-feeding animal would concentrate quarks, Peter and his students also did oil drop measurements using sperm whale oil. For quite a while several gallons of left-over sperm whale oil were still kicking around in the Randall lab basement. Actually, this oil is particularly valued for lubrication of delicate instruments, so we finally gave it to a clockmaker who was delighted to get it.

Last Experiments

J. We've been talking for quite a while but we haven't come to the end of the story.

W: No, and I should tell you about my penultimate experiment that started under the tent on the roof of Randall Lab. After some preliminary work here, we packed up this equipment and took it to a high altitude lab to look for structure in cosmic rays air showers, structures that calculations had indicated should be considerably greater up at 10,000 feet than at sea level. But unfortunately they weren't.

J: And after that?

W: About 3 or 4 years after retirement, I did my last experiment in Japan at Akino Highland.

I was planning to go to Beijing. But then when I stopped off in Tokyo and talked to my friends in cosmic ray work, they reported that four of the students in Beijing would be able PhD candidates, but that one of them was particularly good. So I chose him, sight unseen, and he turned out to be a brilliant man who came over and worked with me for a year, getting his Ph.D. on that experiment.

We were looking for a way to characterize cosmic rays from measurements on the shower of secondary particles that they create. A very smart guy here in the U.S. who did lots of shower work had the bright idea that the thickness of the shower would show up as a difference in time of signals from the electrons produced near

the center as compared to the electrons on the periphery. And by being a bit clever in the setup, you would get the direction also. So you didn't have to have a huge array of scintillators to look at the whole shower.

J: Did that work as you had expected?

W: Well. . . We had gone to the Akino Highlands because that is where one of the worlds' two biggest air showers arrays had been installed. We also appreciated that the Japanese are exceedingly cooperative. I got permission to use a setup of 50 scintillators that had been used by a Japanese group ten years before. These were quite far from the main building, but at my request the Japanese very thoughtfully moved those scintillators to the top of the laboratory in which all the electronics were installed. Data from their scintillators combined with data from our array told us all about the shower. We tried to extract a measure of thickness from these measurements, but the statistical fluctuations in the data kept us from getting a useful result.

So that was the last experiment.

J: Do you keep in touch with any of your graduate students?

W: Well, there was a recent occasion. . . Do you remember the group of Chinese women that Bing Zhou brought in to build the tubes for the Atlas detectors? One of those women had been a classmate, in Beijing, of Dai Hong-Yue's wife. He had been my student and I last saw him when he had come here for a cosmic ray conference. I helped him get a job at Utah's big air shower array. This young woman knew that he was no longer at Utah . . . maybe he didn't get tenure, or perhaps he just decided to make more money, so he went to work for a Seattle company.

Memorable Experiment: Mass of the Muon

W: In some ways I consider that measuring the mass of the muon to be the most interesting experiment that I've ever done. At that time, just after WWII, there were about a half-dozen measurements of the muon mass, but the uncertainties were substantial –indeed there was a hypothesis that muon did not have a unique mass. So I set up the experiment and Bill Fretter did the measurements to show that the muon mass is indeed unique and well-defined. We found it to be equal to 202 electron masses, a value that is within one percent of the currently accepted value. [*Phys Rev* **70**, 230 (1946); *Phys Rev* **76**, 51 (1949)]

J: That was when you were in Berkeley.

W: Yes, I had set up too many experiments, including some that involved flights in B-29s, so I was delighted when Bill Fretter came back from the war and I was able to turn that experiment over to him.

J: Well, we've been at this interview for quite a while; perhaps we should wrap up.

W: You're very patient.

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