Dr. Harold Leinbach  
3 October 2000  

Brian Shoemaker  
Interviewer

(Begin Tape 1 - Side A)

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BS: This is an oral interview with Dr. Harold Leinbach taken as part of the Polar Oral History Project, conducted by the American Polar Society and the Byrd Archival Program of the Ohio State University on a grant provided by the National Science Foundation. The interview was conducted at Dr. Leinbach's home in Boulder, Colorado, by Brian Shoemaker on the 3rd of October 2000.

Dr. Leinbach, for your interview we're interested in you as a person. You certainly were a part of the International Geophysical Year in the Polar Regions, but you grew up in an environment that influenced you. You had mentors. You had done research certainly before the International Geophysical Year and because of that research; you were selected for the IGY. Since then, your work in the Arctic and the Antarctic is important. The Geophysical Year influenced you and your future work, too. So, the interview is yours and we can get started on a real interview this time.

HL: Well, I trace the whole of my professional career basically back to a childhood interest in astronomy and that goes way back. I remember in the second grade, my father taking me to visit a friend of his who had a telescope. This was well after midnight one night and we looked at the moon and that was an absolute thrill for me. I got through grade school and high school in a
variety of places and ended up in Maryland where I finished high school and started college at the University of Maryland.

BS: Your father was a . . .

HL: He was on the faculty at the University of Maryland and his field was animal husbandry.

BS: Research? Research scientist?

HL: He did do research, mostly with cattle and I think mostly with feeding - feeding of cattle. The sun was very active in 1947. It was the peak of that sun spot cycle and there were lots of big flares and lots of subsequent magnetic activity that drove the aurora, for example, way far south in some magnetic storms. We saw overhead aurora in Washington, DC, which happens a few times during very active sun spot cycles. Now, after finishing my freshman year at the University of Maryland, I moved to South Dakota State College in Brooking, South Dakota. My father had gone there the previous year to become President of South Dakota State. I've often wondered, not seriously, sometimes wondered in hindsight how different life would have been had I not followed them to South Dakota and taken up a more independent life at that stage in my career. But, that's all pure speculation, of course.

BS: When you look back, it doesn't seem at all like we're in control of our lives, does it? Everything's happenstance.

HL: So much is happenstance. When I got to South Dakota State, I found that on top of the chemistry building was a dome that immediately peaked my interest. I inquired and found out that there was a five inch diameter refractor sitting in this dome and got to look at it and was sort of given license to do whatever I wanted with it, because it was totally unused and in bad state of
repair. A buddy of mine and I fixed it up, cleaned it, got it working again, and then I used that telescope to make observations of sunspots. This was after the peak of the sun spot cycle, but the sun stays active for several years after it's peak, so there were still plenty of sunspots. And I started making these daily drawings of sun spot activity.

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About that time, I learned of the existence of what's called the Solar Division of the American Association of Variable Star Observers. That's a very active amateur organization. I joined the solar division.

BS: *This is the AAVSO?*

HL: Yeah. And a fellow named Neil Hines was Chairman - an amateur - was Chairman of the Solar Division and he was kind of a mentor for me in those days, albeit by mail. I never met him personally.

BS: *What was his name again?*

HL: Neil Hines. I sort of had a fast track at South Dakota State in the sense that I went to summer school so that I, indeed, graduated ahead of my nominal class. I'm called the class of 1950, but I was out of there at the end of the summer of 1949. I needed one credit to graduate from South Dakota State. I was a credit shy, and so I had this idea that I'd build a little spectroscope - a visual spectroscope and hang it on the back end of this five inch telescope and see if I could see solar prominences. And, indeed, it worked just fine. This is sort of a repeat of old experiments that had been done way back in the late 1800s by at least two famous solar astronomers in different countries in Europe.
BS: *Who were those?*

HL: I was afraid you might ask me that question. You know, I'd have to . . . it's not at the top of my head. So, I built this little spectrohelioscope. I got my one credit, and graduated at the end of the summer school in 1949. I later published a little paper in a popular magazine called *Sky and Telescope* about this solar spectrohelioscope. It wasn't a spectrohelioscope. It was just a plain spectroscope, there being a fine difference, which I'll get into perhaps later. I was a little at odds with what I was going to do. I had written to Neil J. Hines, because I thought I wanted to go to graduate school. I didn't have much of an idea where would be an appropriate place to go with my interests in the sun, and he wrote back and suggested that I apply to both Harvard and to Cal Tech. I never heard back from Harvard. Not even a rejection. But, Cal Tech accepted me, which I thought was kind of nice of them.

So, I headed off for Cal Tech in the beginning of the 1949-50 school year. In their Astronomy Department, there were, of course, lots of famous astronomers associated with Cal Tech. Jessie Greenstein was Head of the Department of Astronomy at that time. But, there were lots of Mt. Wilson astronomers, some of them very famous solar astronomers who were connected with Cal Tech as well. I wouldn't say I had the easiest year at Cal Tech, but I don't think I would be alone in saying that. I did get by and I got my Master's degree at the graduation in 1950. Sort of a terminal Master's degree, I think, to put it frankly.

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There was one interesting happenstance while I was at Cal Tech that totally changed my future life professionally and it was the outcome of just a one evening bull session in one of the astronomy grad student's office. There were a number of us there, including a fellow named Pierre St. Armand. Pierre was a geophysics graduate student at Cal Tech, but he was taking an
elementary astronomy course - the same course I was taking from Dr. Joy - and so he knew the astronomy graduate students and paled around with them a bit. So, in this particular bull session, he was talking about the University of Alaska and the Geophysical Observatory at the University of Alaska. It turned out that in a nutshell, they had just built a new Geophysical Institute building complete with six or seven residence houses and they were all set to take off on a big project. Something happened between the staff of the old Geophysical Observatory and the President of the University of Alaska, who at that time was Terris Moore. You know Terris?

BS: *Know of him.*

HL: I think he then went to Boston and one of the museums - one of the Boston museums of natural history or whatever. Well, anyway, the outcome of that dispute, I don't know the details, was that the old Geophysical Observatory staff, almost to a man and there were probably 20 members at that time - something like that - resigned. And here the Geophysical Institute was left as a new organization with a nice building and nice residence houses and essentially zero staff. So, the Head of the Chemistry Department, Bill Wilson, was elected to be interim director of the Geophysical Institute and was tasked with quickly building up a staff for this new Geophysical Institute.

One of his recruits was a fellow graduate student from Cal Tech named Jim Heppner who also played, certainly played a role in my early professional career. At any rate, the summer of 1950 came. I had an MS from Cal Tech. I didn't have a lead on a job, so I decided, remembering this bit from the bull session about the Geophysical Institute, to apply to the Geophysical Institute, and I think as fast as my letter arrived on their desk, Bill Wilson must have written his offer to me and I got it back. So within a couple of weeks, I had a job at the Geophysical Institute paying a handsome sum of $4800 a year. It sounded OK to me. So, I left for Alaska. I arrived there about the 12th of September of 1950. And my very first job there was to work with Jim Heppner on observing the aurora. For example, one of the things he was doing was to
photograph the aurora with vertically pointing K-24 aerial cameras. I don't know if you ever ran across the old aerial cameras from World War II days.

BS: *The mapping cameras?*

HL: Yeah. K-24 was fairly big lens at 2.5.

BS: *Big heavy thing with two handles on it.*

HL: Right. You had to use roll film in it. And he had set up - this was before I arrived - he had set up two field sites and they weren't very far apart.

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They were probably not more than about 10 kilometers apart. And so, what he was trying to show was that . . . or ask the question actually, was it possible to measure the height of the bottom of auroral displays from a short baseline pair of cameras? In fact, we had some success in doing that and verified the results that had already come out of the Norwegian auroral studies about auroral height - like the bottom of aurora might be 60 kilometers.

BS: *So you did it stereoscopically.*

HL: We did it stereoscopically by comparing the position of auroral patches on each photograph using the star background, of course, as the reference point to deduce angular changes. I think that was a Bureau of Standards sponsored contract that we were working on. Jim also started a very interesting observational program the winter of 1950, and we continued it into the next year of making visual observations - not photographic - just visual observations of the aurora from the
roof of the Institute building, but in a very systematic way. By that, I mean we had a strict regime of how we were going to observe the aurora and of how we would identify the different forms of the aurora. The Norwegians, under Störmer, had come up with a classification scheme for the different forms that auroras can take, so we worked on using their classification scheme, and every ten minutes, I believe it was ten minutes, we would step outside of the little dimly lit penthouse room on the Institute building, go out, make a visual observation of the aurora across the sky, note the observation on a prepared paper form, drawing the position and identifying the type of aurora and other features of interest. The interesting thing about this project was that Jim left after 1951 to go back to Cal Tech to work on his Ph.D., and used these visual observations as the basis of a marvelously done thesis on morphology of the aurora. I'm sure, even at that time, that there were not very many theses in physical sciences, which were based strictly on visual observations of the phenomenon. Later on, just before the beginning of the IGY, the DTRE - what does DTRE stand for? Defense . . . I'm missing the T. Defense Research Establishment.

BS: *Technical?*

HL: I don't think it's Technical. Isn't that funny. It has slipped me. Well, I have to check that out. At any rate, DTRE of Canada was sort of, I guess, their equivalent of NSF in handling the Canadian IGY project. So, that may ring a bell with you.

BS: *So, the Canadians used his thesis.*

HL: So, they republished his thesis as "A Manual for Visual Observations of the Aurora," during the IGY. And Jim went on, after he got his degree to join Goddard Space Flight Center at NASA and had a long career as a scientist in NASA.

We did other interesting things. For one season we had a French astronomer named Dr. Louis Hermann visit us and he brought a huge astronomical spectrograph with him which was
designed to go on the back end of one of their large telescopes. He wanted to try it out and see if he could get a spectrum of the aurora with that instrument.

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We never were terribly successful in doing that. But, we did other studies of the aurora, including comparing aurora as seen in infra-red, using infra-red sensitive film, to compare with what aurora it looks like in the visual end of the spectrum with ordinary black and white film. We published a little paper on that.

There was another auroral project under Dr. Chris Elvey. Chris was the Director of the Geophysical Institute shortly after I arrived. I don't remember the exact time at which Elvey arrived. But, it was not very long after 1950. This program made visual observations of the aurora from several sites including Barrow, College, and Nome, Alaska, and a little place called Sheep Mountain that, as I remember, was sort of halfway between Anchorage and Fairbanks. These were all Signal Corps sites and the enlisted men at these sites made the observations - visual observations. So, this was sort of an extension of the kind of observations that Jim Heppner had established earlier, except now done from widely separated sites.

Well, the Army reached out. I thought I almost had the draft beat, but the Army reached out in 1953 and said, "Why don't you come and join us for a couple of years?" At that time, I had been to enough field sites, Army field sites in Alaska, that I decided I'm not so sure I really want to spend two years out in the field in Alaska. That was an option. I could have signed up and enlisted and been assigned to sites in Alaska. I had just gotten married. I decided I’d take my chances going back through my draft board that was in Brookings, South Dakota, and see where they send me. We had a snow delayed trip, so I had to delay by a month my induction date. I guess the draft board had no recourse but to do that since we were stuck up in Montana or someplace by a snowstorm.
I went to Kansas for my basic training, then got assigned to the Rocky Mountain Arsenal in Denver, and I think it's coincidental, but it wasn't a matter of a few days after I arrived at Rocky Mountain Arsenal that they made the first public announcement of what they did out there which was to manufacture nerve gas. I spent my Army career essentially working with a bunch of civilians and their job was to x-ray the heights of fill of these little containers, and my job was to take the data and do a small sample statistical analysis and say "this lot passes or fails." They always passed. I'm not sure I ever had to reject a single lot.

Well, I got a slight early-out from the Army and was discharged in January of 1956, and of course, at that time, probably true now too, you could go back to your point of departure to the Army. You were assured of that job. Is that still true in the military?

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BS: You get preference in hiring in federal sponsored jobs back wherever you left from. For instance, a guy can go back and first of all, if they wanted to work for the post office, they moved to the top of the list. So, you get preference on that. If you've been drafted out of a job, which of course, nobody's drafted now, you can go back to that job. They can't turn you down.

HL: Yeah. Well, at any rate.

BS: Couldn't.

HL: Couldn't at that time. Lots of things had happened at the Geophysical Institute in those two years that I was gone. Oh, I should back up just a notch. Sometime back in the early '50s, besides getting Dr. Chris Elvey as Director that was a tremendous step forward for the Institute - he was a well-known scientist. He had come from Naval . . . what's the Naval test site?

[I think, looking at a California map, that it was the China Lake Naval Weapons Center.]
BS: *The Missile Test Center. It's the Naval Missile Rocket Center... or...*

HL: They had also latched onto, as it were, Professor Sydney Chapman, who had left England after his mandatory retirement and came to the States and established a liaison with several universities which included Cal Tech and the University of Michigan, as I recall, and certainly the Geophysical Institute. He started a program of spending, would you believe, winter months at the Geophysical Institute - not summer months, but the winter months. And he was with the Geophysical Institute in that capacity for many, many years. At any rate in 1954, C. Gordon Little, who was a radio astronomer from Manchester, England, I believe I'm correct that that's where he got his degree, had joined the Geophysical Institute staff, and one of the programs that he started shortly after his arrival was to measure the absorption of radio waves in the lower ionosphere. He did this with a technique that is called cosmic noise absorption. You probably wonder just how this technique worked. It is a really simple technique. At high frequencies, we worked it around 30 megahertz, but in that frequency range, the background sky is very noisy due to all kinds of radio sources. The sun is a radio source. So are all stars. Gas clouds. Nebulae, galaxies, are all relatively strong emitters of radio noise. And in collection, if you looked at the sky with a broad beamed antenna so that you didn't pick out individual sources, this gives you a time variable background noise and this is called cosmic radio noise.

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Now, in order for you to receive it, the cosmic radio noise obviously has to penetrate through the ionosphere. What Gordon Little proposed doing was setting up some cosmic noise receivers, look at the cosmic noise intensity and then, if the lower ionosphere became perturbed as it does during aurora displays, it should take a bite out of the strength of the cosmic noise signal. In other words, you should see less cosmic noise because it's been absorbed in the lower ionosphere.
and this would show up on the records as a decrease in the signal strength relative to what you would normally get on a quiet day. So, this is a relative measure of the ionospheric absorption. The technique is, as I said, a very simple one and very effective. When I got back to the Institute in late January of 1956, this program was already underway. Gordon needed somebody to run this program on an operational basis, so he said, "How'd you like to do that?" I didn't know a thing about radio astronomy. I knew a little bit about auroral absorption, so I sort of said yes. I did have a background as a ham radio operator, so that electronic equipment itself wasn't totally foreign.

So, it was at that point that I got involved in the cosmic radio noise studies. Things were happening fast because the IGY programs were already being planned at that time and I'm afraid I don't know the details and the history and it's not in my notebooks, but Gordon Little and Chris Elvey had already proposed to NSF to support several IGY projects at the Institute. One of them was this cosmic noise absorption project. Another was the rather famous all-sky camera program that Neil Davis ended up being in charge of. Another was an auroral radar program and there may have been a couple of others. Gordon Little realized that there were probably better techniques for measuring cosmic radio noise than the simple receiver technique that we were using because simple receivers, for example, are subject to gain drift and problems like this. There's a way to beat it that had been devised by English radio astronomers, where you essentially measure the radio noise and compare the signal coming in through the antenna with a standard noise source. There's a little diode tube - those were all vacuum tubes in those days - called a noise diode that has very specific characteristics of the radio noise that it emits as a function of the current that's going through the tube.

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And furthermore, you could make this system a little more sophisticated by turning it into a servo system so that you switched on a very rapid basis, between the antenna and the noise
source. You used the difference signal to drive a servo loop to bring the noise diode up to the level or down to the level of the cosmic noise, and then just record the diode current as your measurement of the cosmic noise level. It worked very successfully. It also had the big advantage that now the system was somewhat, to a first order, independent of gain drifts in the receiver system, so it was very stable.

So, in the summer of 1956, we started work on the new instrument based on the English work. During that same summer, George Swenson who had just recently joined the University of Illinois as a radio astronomer, spent the summer in Alaska. I have to take a slight detour here and tell an interesting little story about George. He was quite active. He liked to do things like canoeing. He was out on one of the rivers, I believe, near Fairbanks and flipped his canoe over and lost his wallet and all of his ID cards. Credit cards, everything was gone. He found out that it didn't take anything at all to get an Alaska driver's license. So he went down and applied for an Alaska driver's license and got that. And then from that, was able to build back his whole history of things that had fallen out of his wallet.

BS: *George Swenson?*

HL: Yeah. He's still around. I suspect he's retired now from the University of Illinois. One of George Swenson's contributions to the project was actually to come up with the name riometer for this new instrument that we were in the process of prototyping. And I just found a piece of paper last night - a carbon copy of a letter that George had written to somebody. And I flipped it over and on the back it had, I'm sure in George's writing - it's certainly not my writing - it had the word iometer, `i-o-m-e-t-e-r`, iometer.
And I suspect that that was George's original cut on what to name this instrument. At any rate, he broached the subject at coffee hour one day and at the end of the coffee hour, everyone had agreed that the proper name for this instrument was going to be riometer. That stands for relative ionospheric opacity measurements; we could actually stretch it out and put words to each of the letters in 'meter,' but it's much too long an acronym if you think of it that way, so riometer it became.

We worked hard that summer on the design of the riometer.

BS: *You and Swenson?*

HL: Yeah. While he was there . . . George Swenson . . . I was just reviewing some of his notes and he had some ideas that were electronic design ideas that we didn't, in fact, adopt. So, he was thinking, though, in detail very hard about what kind of electronics were going to be proper for the riometer. He left at the end of the summer. We continued, of course, the prototype development. Other major players in building and designing the riometer were Bob Merritt who was on the faculty of the University and the Geophysical Institute. An electronics engineer and our technician at that time was a fellow named Nate Warman who was a good guy. Nate had come from a background of doing TV set repairs, for example. He knew a lot about electronic circuits and was very instrumental in some of the circuitry design for the riometer. Gordon Little had taken a leave of absence that summer, but he was still involved deeply in the project. He was head of the project.

So, let me switch gears here and go to my other notes. Some interesting things. Now I'll proceed chronologically through the years for a little ways here. The first piece of equipment that I inherited when I returned to the Geophysical Institute in early '56 was what was called the total power receiver - just a straight receiver - no sophistication to it and you just record the signal strength of the radio noise received.
On February 23rd, 1956, there was sort of a pivotal geophysical event. There was a monstrous solar flare on the sun that was extremely energetic and in fact, associated with that flare was one of the largest increases of solar cosmic rays at ground level that had been recorded ever. I think that's still probably the record so-called ground level event. I wouldn't be surprised if Marty [Pomerantz] wasn't involved in cosmic ray observations of that event. You ever talk to him about it again, you might ask him. Associated with that flare, we found a different kind of absorption event. One that was not familiar to us at all. We had been mostly gearing the program up to study the cosmic radio noise absorption that's associated with auroral displays. Aurora, you may remember, is basically caused by electrons - energetic electrons - plowing into the upper atmosphere and ionizing and exciting the molecules and atoms in the air to emit light and that was well known.

BS: *At which level? Did they do it at all levels? Ionosphere? Stratosphere?*

HL: The electrons penetrate down to oh, 70 or 80 kilometers above the surface of the earth - that's sort of the lower height of the aurora. Auroras, for example, auroral rays can extend up to several hundred kilometers in height, but the lower boundary of aurora is 70 or 80 kilometers. And that's also the region of the ionosphere that absorbs radio waves very well if it's disturbed as during auroras. So, auroral absorption was well known as a polar phenomenon. Many decades of studies had shown that. So, all we were doing was trying to study it using a different technique. It wasn't a new discovery. What was new, certainly to us and others, was this absorption associated with the solar protons.
At any rate, the February 23rd, 1956, flare had a big magnetic storm with it. As I remember, we made an observation of red aurora during that storm, and I published a little letter to *Sky and Telescope* about that. So, we were continuing to make our measurements of cosmic radio noise at the same time that the riometer itself was being developed.

We started field testing the new version of the riometer alongside the old total power equipment some time in late ’56 and decided that by February of 1957, that the riometer was in good enough shape that we could put it out for bid to have some commercial units built. The plan was for the Geophysical Institute to procure 13 of these riometers and 7 of them were to be operated directly by the Institute or in one case, indirectly by the Institute, and the other 6 were to be distributed, 3 to Stanford University, 2 to Dartmouth College, and one was going to go to the brand new Kiruna Geophysical Observatory in Sweden.

BS: *Who built these?*

HL: These were built on a lowest bid basis. They were built by the Virginia Electronics Company in Washington, DC, who did, basically, a good job with them. There were some problems in the electronic design for the receiver, for which they used their own receiver design, and some problems were associated with the prototype specs that we supplied them. The biggest problems were sort of a lack of quality control because I remember getting back, particularly the first couple of riometers they built for us, and they weren't working very well.

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And we went through them with a fine-toothed comb and found all kinds of unsoldered joints - poorly soldered joints. So, I don't think they probably had a NASA school for soldering at that time as they do now. But at any rate, that all got straightened out. And the riometers then worked just fine. They were really quite remarkable instruments. But, because of these initial problems,
the delivery of the riometers was somewhat delayed. We had hoped to get delivery of all the units by the beginning of the IGY that was July 1957, of course. We got a first unit at the Institute on the 10th of July, '57, and a second unit on August 12 of '57, and after debugging these and going back to Virginia Electronics and suggesting some changes - incidentally some of which were their responsibility, some of which were our responsibility, for which we paid for the changes. The rest of the riometers began to be delivered. For example, in late September and October, early October 1957, we made our first field trip and that was down to King Salmon, Alaska, and Unalaska, better known to many people as Dutch Harbor - just an island not too far off in the Aleutians. Have you ever been to King Salmon and Unalaska?

BS:  *Um-hum.*

HL:  Interesting places. We stopped first in King Salmon and then went on to Unalaska to install both an IGY auroral radar and a riometer. The Unalaska trip was a tragedy that I'll come back to in just a moment. It was also a very interesting place because Dutch Harbor, as I understand it, was the only place in the Continental United States that was attacked by the Japanese during the War.

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A small contingent of Japanese planes came over and strafed Dutch Harbor one day and apparently the commander decided, well, they'll be back, so they quickly put up a bunch of dummy decoys up on the side of the hills. . .

(End of Tape 1 - Side A)
BS: OK. Repeat what the commander decided to do.

HL: The commander, base commander, decided that he'd have to put up decoys to try to attract the Japanese away from more important targets, I suppose. So, he put up a number of these crosses, wooden crosses, on the side of the hill dressed in GI clothing with boards or tree limbs, something like that, to try to convince them that these were machine gun nests. And indeed, the Japanese came back the following day and made another run at it. That was the last Japanese attack on Dutch Harbor, as I remember the story. At any rate, when we were there in October of 1957, that's quite a few years after the end of the War, you could still see these stakes and remnants of the clothing up on the hillside that hadn't been disturbed. This was the hill that was just above the town. You know, there was a river that ran between the town and the hill, and then you climbed the hill. And there were bunkers, old bunkers that had been set into the ground up there, which, as I remember, were just the old Quonset huts. And that's where we elected to install the riometer and the auroral radar equipment.

And the fellow that we had recruited earlier to run the station was Reverend Keith Whitem. He was a Methodist missionary at Unalaska. I think Unalaska also had an orthodox Russian church - a very beautiful building. And he was also sort of the town handyman, as it were. He took care of the power system for the town and did other things like that. And he ran, with our help, an electrical line across from one of the power lines going through town, up the side of the hill, to give us power to run the equipment. We had a relatively easy time installing the equipment and checking it out.

One of the early days that we were at Unalaska, the weather was absolutely incredibly beautiful. There were no clouds in the sky. This was early October. Very warm weather, and Keith Whitem offered to take us - there were three of us who were doing this fieldwork, myself, and Norm Sanders - a young man from California who was at the Geophysical Institute at the...
time. He was a handyman sort of fellow. And another technician named Dick Shoup. And so Keith said, "Well, why don't we go for a boat ride. I'll just show you some of the inlets and the islands around here." So, we had a beautiful day. Even saw a whale - I've forgotten what kind of whale it was. I have a picture of it someplace that I snapped. And we spent several hours out on this beautiful boat trip. Went to one place and saw an old World War II fortification where there had been a Naval gun set up which could sweep the whole harbor. The gun was long since gone, but the concrete fortification was still there. And we came back that afternoon and then Keith realized that on one of our several stops, he had left something behind on a beach - I've forgotten. It might have been clothing, jackets or something because it was so warm I don't think we were wearing jackets by that time. And so the next day, he said he wanted to go out and retrieve that. And this was the day of the great tragedy, unfortunately.

Dick Shoup and I wanted to get on with the work, so we begged off on that trip, but Norm Sanders went with Keith and Keith's youngest son. Keith had several children, but this boy was probably 6 or 8 years old. And they left early in the morning. I expected them to come right back and by late morning, nothing had shown up. They had not returned and I began to get worried and eventually I went down into the village and mentioned my concern to some of the natives and they said, "Oh, don't worry about this. He's often gone for a day at a time. Not to worry about it." But, I became increasingly concerned and I even walked up to the top of the hill that the riometer was located on and looked out over the bay and in hindsight, I realized what I had seen was maybe a foot or two feet of the bow of their boat sticking out of the water. And it was several miles away. I just saw this little white speck on the water. But at any rate, we did mount a search party by afternoon, which was too late, as it turned out. We found Norm sacked out on a beach probably three-quarters of a mile from where they lost the boat. He was a young strapping fellow from California with a lot of scuba diving experience and so on behind him.
And he told us later that when the boat rolled over, they all got out, got life jackets on and he tried to convince Keith Whitern who was holding his boy, to swim for it. Norm had dived down to try to release the engine on the boat and he couldn't release the engine. So there the boat was, bow out of the water - couple feet of the bow. And he finally said, "I've just got to go for it." And so he swam, like I say, half a mile to three-quarters of a mile, I'm not sure quite how far. By the time he got to the beach, he was so exhausted that he said he crawled as far away from the water as he could, which was 50 feet or more up from the beach and he just simply conked out and he was apparently out for 5 or 6 hours from total exhaustion.

Well, Keith and his son didn't make it. Keith's body was recovered in a day or two and I'm not sure they ever recovered his son's body. So this was, for us, and for the community of Unalaska, really a terrible tragedy. It was probably one of the few IGY tragedy's that occurred. We eventually recruited another person at Unalaska to run the riometer and we had several months of successful operation of the Unalaska riometer, although it wasn't the best of our sites, by any means. King Salmon worked well for us and we used the data from King Salmon in several papers. The Thule riometer was one that we supplied to the Air Force and I think they called it the geo-pole station. Does that ring a bell?

BS: I don't know on that. Was Martin involved? He had something up there.

(100)

HL: Yeah. There was a neutron monitor at Thule. And there was our riometer and I'm not sure what other IGY programs the Air Force had going on there. But at any rate, it was manned by the Air Force and I'm pretty sure it was called the geo-pole station, although I might be confused on that point. We ran the Thule riometer up there. None of us from the Institute ever traveled to Thule, which, in itself, turns out to make sort of an interesting little story. How do you set up equipment remotely? There was a Navy supply plane. I think it actually was an airborne
ionosonde plane that was going to fly via Fairbanks up to Thule. We learned about the flight, and
we took the riometer out - at that time they were landing - I don't think we went all the way to
Eileson Air Force Base. I think they were landing at Ladd, you know, right next door to
Fairbanks. And we hauled the riometer out there and loaded it onto this airplane and they tied it
down, of course, and took off. And that's how the Thule riometer got there to Thule. I don't think
it was a very formal arrangement, but it worked.

Now, once the Thule riometer got there, the Air Force people had trouble setting it up.
And this is where my ham radio experience came in very handy. We had, in the Institute, an old
Army surplus ham radio transmitter that we used for long-range communications and I found out
that we could actually reach Thule - this was during the peak of a sun spot cycle. The ionosphere
was supportive of 30 megacycle transmissions, so we could actually reach Thule on 20 or 30
megahertz and we just talked them through setting up this riometer. I remember spending hours
doing that, asking them to make an adjustment here, an adjustment there. Give me a voltage
reading from some spot or another, then going through my manual that I had written up on the
operation of the riometer. And they had a copy, obviously, also. And we just talked our way
through to the successful operation of the Thule riometer. Which was really a critical station
from the point of view of what later became called polar cap absorption - the ionospheric
absorption associated with these solar cosmic rays. It's the result of the bombardment by solar
protons over the polar cap. That's why we called it polar cap absorption. And to really study it
well, you would like a station to be deep inside the polar cap, as close to the magnetic pole as
possible and Thule fit that bill quite nicely. So that was a crucial station from the point of view of
studying polar cap absorption.

BS: Where else did you install riometers?

HL: Well, our next installations . . . we had now taken care of King Salmon, Unalaska, Thule.
We installed at a site called Fairwell. I don't know if you ever ran across Fairwell.
BS: *Cape Fairwell?*

HL: Fairwell was a little . . .

BS: *Was it at Cape Fairwell?*

HL: No, no. Oh, I think that there may be a Cape Fairwell, but this was inland, again, between Anchorage and Fairbanks. I think it was, at that time, it was called CAA - not FAA - a CAA site at Fairwell and I'm not sure there was much else there. There was a little airfield.

BS: *So it was near Fairbanks, then.*

HL: It's southwest of Fairbanks. And that was a successful installation. We installed a riometer at Barrow, Alaska, in mid-April of 1958, and that riometer ran successfully.

(150)

We installed a riometer at Fort Yukon, which is just above the Arctic Circle, north of Fairbanks - about 100 miles north of Fairbanks, in February of 1958. We had troubles with that equipment, primarily because of a severe interference problem from a local source - one that we never successfully resolved. So, we had not too much useful data from the Fort Yukon riometer, unfortunately. Then the riometer that went to Sweden, the Kiruna riometer, was shipped directly to them by Virginia Electronics, and it was successfully installed in about April of 1958. And the Swedish went on to make significant contributions to the whole field of auroral and polar cap absorption using their riometer data. They did a really tremendous job.
See, I have here that it was actually 1958, when Gordon Little took leave of absence to join the Bureau of Standards here in Boulder, and he later decided that he wanted to take a job full time, and left the Institute. George Reid, who also lives here in Boulder now, came from Canada and joined the Geophysical Institute staff, and he became the second of my three graduate advisors. Gordon was the first. I never did mention the fact that, when I got back to the University of Alaska in early 1956, they had just started a Ph.D. program in geophysics jointly between the Institute and the University of Alaska. The Institute is part of the University of Alaska, of course. And I was strongly encouraged, rather urged might be a better word, to join this graduate program, which I did. It was an interesting program because they allowed us graduate students to work two-thirds time at the Institute and take half-time course work - a deal that you could hardly pass up. There were several graduate students. Neil Davis was a graduate student in this program. Syun Akasofu was a student of Sydney Chapman's in this program. And Bob Leonard of auroral radar - several of us.

So, at any rate, how'd I get off onto that? George Reid being my second graduate advisor. That's how that came up. And he had already done some work with polar cap absorption. In fact, he and a buddy of his in Canada had written quite a definitive paper before he came to Alaska about polar cap absorption. There's another name that deserves mention. Unfortunately, a gentleman who is gone now was Dana Bailey. Did you ever run across Dana Bailey's name?

BS: *No sir.*

HL: Dana Bailey was an ionospheric physicist by profession and one of his studies while he was still back east - I think he worked for the Bureau of Standards and also for a private company back in Washington - was to explore the Arctic ionosphere using a technique called forward scatter. It's essentially a technique where you beam out high frequency radio waves and then pick up the forward scattered radiation from them - now forward scatter being different than the direct wave which would reflect off the ionosphere from station to station.
But, off the paths from the direct wave, you'll still get some noise if you have sensitive enough equipment that is associated with the scatter radio waves. What scatters it? Well, ionization associated with aurora will side scatter radiation. That's why sometimes during big magnetic storms right here in Boulder, you might be able to hear a Texas TV station because it goes up, hits the aurora and gets scattered back down. That's rare, but it does happen.

BS: *Do you still do ham radio work?*

HL: No. And the reason I don't do ham radio work is that I got so engrossed in doing my thesis that I forgot that my ham license was going to expire and it lapsed and I just, by that time, had so many other irons in the fire, that I never went back and picked it up.

BS: *Gordon Barnes, who was second Chief Scientist at Byrd Station during IGY lives out through that end of town and he was also the Chief ham operator and had been before that. He was the weather observer, sent by the Weather Bureau and was made Chief Scientist and Chief ham operator. Visit him. He's got quite a ham set. He's big time.*

HL: Oh, I've seen some big antennas out there.

BS: *Those are his. Yeah.*

HL: That's great.

BS: *You should meet him. You might get your license renewed.*
HL: Yeah. I got too many other hobbies now.

BS: He's 84.

HL: 84! Well, that's great. So, in the meantime, while we're doing all this work with field stations and setting up riometers, we're also trying to do a little bit of science. So, in January of 1958, Gordon Little and I published what was really the first major paper out of the Geophysical Institute absorption project called, "Some Measurements of High Latitude Ionospheric Absorption Using Extraterrestrial Radio Waves." I don't know why we couldn't have made the title a bit longer than that. And that was published in the proceedings of the Institute of Radio Engineers in early '58. After George Reid came at the end of . . .

BS: You were the Principle Investigator for all these studies.

HL: I was the Principle Scientist on the IGY, NSF contracts, yes.

BS: You hadn't gotten your Ph.D. yet.

HL: No, I didn't have my Ph.D. I was working on my Ph.D. Then, the sun was extremely active during 1958, and we observed a lot of solar proton induced absorption events, or polar cap absorption, as we called them. And in early 1959, George Reid and I published a little letter in Physical Review Letters on just one small PCA event in July, 1958, which contained all of the essence of what these things looked like as a function of observing them from Thule down to the edge of the polar cap, or into the auroral zone as College is. And that's one paper - it's interesting in hind sight, I think, because that is one paper I wrote where I had this tremendous urge that this
paper must get out and must get out, if not today, it must get out tomorrow. Why? Because this was - we knew by this time - that this was a hot field.

(250)

The study of solar proton events was catching everyone's interest. For example, never before was it realized that the sun put out such copious amounts of moderate energy protons. A few ground level events certainly had been recorded such as the one in '56 and even prior to that, by neutron monitors. But, this regime of lower energy protons was essentially unknown. This, you remember, was very early in the satellite era. Some of these measurements preceded satellites, like the February '56 event. There hadn't even been the launch of a satellite yet.

So, this letter in Physical Review Letters served the purpose of getting more of the physics community knowledgeable about solar proton events and this method of measuring them. And, as an outgrowth of that, in a meeting at about the same time at the University of Iowa of cosmic ray physicists - we were invited to participate in that meeting - a number of people began to say, "Well, can you provide us warnings of the occurrence of these events by the riometer data?" So, we ended up supporting a little experiment in Fort Churchill. NASA called it their solar beam experiment and I honestly don't recollect exactly the details of what they were doing there. It might have been rocket launches. The University of Chicago group was under John Simpson, who just recently died, I learned from reading EOS. University of Minnesota balloon group was under John Winckler. Those were all balloon flying groups. The University of Iowa - of course, Van Allen's group, were all interested in these events. And so we started providing warnings of these events. I just cite one example of such an event that occurred in May 10th of 1959 and this is an embarrassment to me to this day. I was in charge of daily operation of the riometer at College and I hardly missed a day of passing by the field site and just checking to make sure that the equipment was operational. Well, it must have been a long week because I decided I was going to take a weekend off from this business of working and looking at my
equipment. So, I didn't go in on the weekend. I've forgotten what day of the week the 10th was. This was probably either local time, late Saturday or maybe early Sunday. Monday morning, I went in to look at the riometer chart record at the field site and the first thing I noticed, much to my horror, was that the level of the recorder was clear down at zero. There was no record of noise from the noise diode. Now we knew enough about cosmic ray events at the time to recognize that this is one symptom of very large cosmic noise absorption events, but to be sure, I unrolled the chart recorder back far enough that I could see the onset of this event and verify that, indeed, it was due to a huge polar cap absorption event.

(300)

So, I quickly went back to the office and got on the telephone and one of the first groups I called was the University of Minnesota group. Now at that time, they were on a stand-by situation with their high altitude balloons out of Minneapolis. These were balloons that were outfitted with cosmic ray detectors and something called nuclear emulsions that are stacks of a special kind of photographic emulsion, not exposed to light, through which cosmic rays penetrate and leave a track. I'll show you a photograph of this event in just a second. At any rate, I called them up. It, fortunately, was just at the beginning of the magnetic storm associated with this event that means like 24-36 hours after the beginning of the event. And, they got a balloon up and they got it up to altitude and ran into a huge flock of solar protons. This is at 100,000 feet to 120,000 feet above Minnesota. They made a great deal about their observations of this event and they got wide publicity. Here's a reproduction of a segment of the nuclear emulsion obtained during this event. Each of these black-segmented stripes on here is a solar proton plowing through the emulsion and some actually stopped in the emulsion. When they lose a lot of energy, then they are deflected in the magnetic field and the trail turns. But, that's what these streaks all represent and they're quite a few of them here, obviously. And on the caption, on the back of it, it says, "This solar micrograph is a 2000 enlargement of the photographic flight flown on May 14th, 1959, in
an intense beam of solar protons. It was exposed in a balloon flight at 100,000 feet.” A normal cosmic ray exposure would have had approximately 1/10th of a track in this area, so I don't know how many tracks you can count here all together, but it's certainly dozens so that the flux of solar protons was like, let's say, 20 to 40 times higher than the background flux of cosmic rays.

BS: *Those protons have mass.*

HL: Protons have mass, yes.

BS: *And they slow down satellites.*

HL: Yeah. Probably that's a minor effect as far as satellites are concerned because the total number of these things is not all that high. They do have mass. What's more of a problem, and NASA has recognized this for years is that, if they ever fly astronauts into one of these events of this strength, they're in for trouble from radiation. And so, warning of solar proton events has been part of the NASA strategy for years and years now.

(350)

BS: *Who has the - I'm curious - who has the patents on your riometer?*

HL: It's un-patented. It belongs to the public.

BS: *It belongs to the public.*

HL: It's in the public domain. In fact, at least two commercial companies later built more sophisticated solid-state versions of the riometer. One was out of Golden and I've forgotten their
name and another back East which was actually started by one of the Air Force fellows who had run a riometer at Thule during the IGY - post-IGY period.

So, at any rate, these protons, which were recorded above Minneapolis, in order to reach 100,000 feet altitude, had an initial energy of 100 mev or greater - 100 million electron volt energies. And to put that in some kind of perspective, as I remember, a 100 mev proton will go through about - well, it will go through several centimeter thickness of aluminum. So, these are highly penetrative energetic particles.

BS: *Through the space shuttle.*

HL: They'd go through the space shuttle, yes.

BS: *Let me put this event, this occurrence that you discussed - try to put it in perspective and I may be wrong. But, you discovered the radio signals by accident on a week-end in Alaska and you phoned the University of Minnesota and said, "Get something up to observe these things. They're coming."*

HL: Yeah.

BS: *"Protons are coming."*

HL: Yeah.

BS: *And they put a balloon up to 100,000 feet over the University, I take it.*

HL: Yeah.
BS: And that's where they first observed these. Now, is that the first time they got an event like this with a warning in advance?

HL: I believe this was the first occasion that they got a solar proton event with balloons.

BS: You get stronger radio signals in Polar Regions than you do in the mid-latitudes?

HL: You mean as far as the cosmic noise is concerned?

BS: Yes.

HL: Not really. That's a somewhat deep question. You're essentially asking for, one thing, is the absorption of these medium frequency radio waves - 30 megacycle radio waves - stronger . . . does it vary as a function of latitude? Yeah, to a certain degree, but a minor sort of degree, it would. The cosmic noise itself has a diurnal variation in it because, for example, the Milky Way is a very strong source of radio noise. So, [the cosmic noise is strongest] whenever the Milky Way is in the antenna beam - we always beamed our antennas vertically.

(400)

We were looking at a slice of they sky with a vertically directed antenna. So, whenever the Milky Way goes across the antenna, you get much more cosmic noise than if you're looking away from the Milky Way.

BS: So, that's why you did your studies with the riometer in the Polar Regions, to coordinate it with the observations of auroras.
HL: Yeah, we started out basically motivated by the knowledge that auroras were a great perturbation in the polar ionosphere and then stumbled on these. Why did we stumble on these? We lucked out and hit the biggest sun spot cycle that has ever been recorded, before or since, and so we just fell into a situation where we couldn't lose. The sun was just pumping out these solar flares right one after another. George Reid and I published a little paper in late 1959, just summarizing and tabulating our observations of polar cap absorption events. We had seen 24 identified polar cap absorption events from May 1957 through July of 1959. So, in two years, we had seen 24 events. That may not seem like a big number, but considering the fact that no one had ever seen these before, and the fact that we did see that many was certainly due to that solar cycle. For example, in this solar cycle that we're currently in which is about reaching it's peak now, there have been, I think, two significant solar proton events so far this cycle.

BS: *That's the 2000 solar cycle? Or, they call it the 2000 solar cycle?*

HL: No, they actually number cycles. So, we are in cycle number 23. The numbering of the cycles goes back to sun spot observations in the 1700s and it's roughly 11 years per cycle, so multiply 11 times 23, that's like 250 years ago that the observations became good enough that someone later could systematize them and discover the sun spot cycle - the variation of the number of sun spots.

BS: *So, this was in 1958.*

HL: That event was in May of 1959.

BS: *May '59. OK.*

(450)
HL: Yeah. Now, like I say, it got a considerable amount of press, including a write up in *Time* magazine, and Walter Sullivan, who was the famous science writer for the *New York Times*, did an interview with the Minnesota people on this event and I think, in both cases, they were kind enough to mention that I had given them warning of the presence of this proton event. So, as far as I can remember, that's the one and only time that my name appeared in either *Time* magazine or the New York Times. But once is probably enough.

BS: *Walter Sullivan was a good friend of mine.*

HL: Was he?

BS: *He died, you know, three years ago. Right after Larry Gould. He was writing Larry's obit and then I had to take over and write it for The Polar Times.*

HL: Oh, is that right?

BS: *And he went into the hospital because he had pains and he had pancreatic cancer. He didn't live too long. He proofread what I wrote from his hospital bed before he died. He wanted to write it. Larry was a very good friend from way back in the '40s. Anyway, a lot of connections there.*

HL: Yes, there are. Well, let's see. That gets us pretty well through happenings in 1959. The whole business was becoming more and more interesting. By this time, satellites were up and satellites had begun to record solar cosmic rays then, so the general interest in solar proton events was certainly increasing all the time. Continuing on with my small part of that history, by 1961, when I was hard at work on my Ph.D. thesis again, I think, having been given an edict that
it really was time I finished and got out of there, I think it was late 1960 and it might have been early 1961, that the University of Alaska procured it's first computer, which was an IBM Model 1620. This was a small computer, I suspect, compared with the power that you and I have on our desktop computers these days. Of course, it amounted to nothing, but it was a marvelous machine for its day and age and it was probably the first small computer that IBM put out that could sort of be bought by the man on the street, as it were, at least by universities for a modest sum of money - probably a few tens of thousands of dollars.

(500)

There was, at that time, on the University of Alaska faculty, a fellow named Alfred Bork who was in the Physics Department. He's a good friend of ours. And he was very much interested in computers - these early computers. And he decided to teach a course on the philosophy of computing and also the practicalities of writing programs in the FORTRAN programming language, which is what we used in those days. It's still widely used for scientific programming. So, I took his course along with several others and started to write a program, the result of which I incorporated into my thesis, which essentially calculated how an energetic proton loses energy as it plows into the atmosphere. That was sort of a fundamental bit of information one needed to know to come up with quantitative models of where the absorption layer was, what it's height profile was and things of that nature. Well, I finally finished my thesis in time to make the 1962 graduation at the University of Alaska. I stayed there until late in the summer of ’62. By that time, I had applied for and received a job offer from the University of Iowa, and I went from the University of Alaska in late ’62, to join Van Allen's group at the University of Iowa.

I stayed at the University of Iowa, doing several projects involving satellite data and some of my own data from Alaska that I brought along with me on the polar cap absorption, and started a little radio astronomy program there. I stayed there four years and then moved to
Boulder, Colorado, in June of 1966, and I finished out my career here in Boulder, working for what ended up being called the Space Environment Laboratory of NOAA [The National Oceanic and Atmospheric Administration].

(550)

So, in a nutshell, that's a little bit of the development the riometer project and the outcome of it.

BS: *And your IGY work.*

HL: *And my IGY work. I never really got back into polar work, *per se*, after that time directly. I did maintain my interest in polar cap absorption and looked at a lot of polar data, including satellite data, after that time. But, as far as going back to Fairbanks, my old home stomping ground, we did make a couple field trips after I joined the predecessor of NOAA, which was - I've even forgotten the acronym by now - not important. Gordon Little was here in Boulder, and had the idea that you ought to be able to measure the, as it were, the increase of temperature of the electrons in the ionosphere by using cosmic noise technique, so another friend of mine and I ran a little experiment out of Fairbanks, using one of their radio telescopes.

BS: *When was that?*

HL: It was 1970, when we did that work.

BS: *So, you returned to . . .*

HL: That was the first time I had been back.
BS: *Institute* . . .


BS: *Just for a short project.*

HL: Yeah. Just for a short project, and I haven't been back since.

(End of Tape 1 - Side B)

(Begin Tape 2 - Side A)

(000)

BS: *This is Tape 2, Side A, of the interview with Dr. Harold Leinbach on the 3rd of October 2000. To follow on to what we were talking about as we changed tapes, I asked if you'd been back and you said you went back in 1970. Have you been back since?*

HL: I have not been back to Alaska or the Geophysical Institute since that field trip.

BS: *They changed it.*

HL: Oh, yeah.

BS: *Have you coordinated any work with them while you were down here?*
HL: Yes, and in an entirely different field. Let me preface that by saying that after I joined the Space Environment Laboratory in Boulder, I did many things over the years. I was with them from the summer of 1966 until I retired in 1984, so almost 20 years.

Wife of HL: You retired when?


Wife of HL: '93.

HL: '93? We'll get it. It's been so long ago; I've forgotten when I retired. At any rate, yes, because I got married in '94, so I retired in '93. I worked on many projects of different nature during my career with the Department of Commerce, one of which was a very interesting project and it bears on the aurora, as a matter of fact. Artificial aurora in the laboratory, and the history in a nutshell goes like this. Way back in some earlier plasma physics - laboratory plasma physics studies associated with nuclear fusion work, people had made a discovery of what was called a beam plasma discharge. If you sent a beam of very energetic electrons through a rarefied atmosphere, you would excite or ionize some of that residual atmosphere and strike what really is best called a discharge phenomenon. And a fellow I was working with named . . . oh dear . . . I'll get it . . . let me come back to that. I'm thinking of two different Bills. First name was Bill, at any rate. And he started a program to do beam studies inside a big vacuum tank. NASA has a big vacuum tank. Bernstein is his name. Bill Bernstein. Unfortunately he's not with us any longer either. And the first thing we did was to go to the NASA site at Plum Brook, Illinois, which is on the south end of the lake. Is it Illinois? Ohio. It was Ohio. Plum Brook, Ohio. It was an old NASA facility that was set up by NASA to support the development of nuclear rockets and so as part of the facility; they had built this gigantic, thick aluminum vacuum tank. I think the vacuum
tank stood about 33 meters high from the floor to the top of the dome of the tank and was 30 meters across. So, it was a huge tank which could be pulled down by a large number of oil diffusion vacuum pumps that were under the floor - could be pulled down to pressures which were equivalent to the atmosphere at about 60 or 80 miles high. And we set up an electron gun on the floor of this chamber and directed it along the direction of the local magnetic field, up towards the target that was at the top of the tank. And we couldn't see into this tank, but we had indirect means of observing what was going on inside the tank. We couldn't see visually into the tank. We rediscovered the beam plasma discharge. Now this was under totally different physical conditions than the old nuclear fusion work.

Here we were working with just the strength of the Earth's magnetic field, which was low, and with electron beams of a few hundred kiloelectron volts that were about the same energy as the electrons that are emitted in your TV set and strike the TV screen and create the picture. Plum Brook was closed down by NASA - mothballed by NASA after a while. So, we had to search for another chamber and we found one down at Johnson Space Center in the form of a slightly smaller chamber that had been used and was still used for testing gear that was going up into space on the, well this work was begun in the '70s, so pre-shuttle spacecraft stuff. And we continued to do our work there. But, the neat thing about the Houston tank was that it actually had portholes in it. So, you could look in the window into the tank and see the luminosity of the electron beam which is the direct excitation of the air by the electron beam just like happens in an auroral display and then all of the sudden, under the right conditions, out to a distance of a couple of meters away from the electron beam which was really quite a narrow streak of light going up through the tank, you would suddenly get a big illumination which was beam plasma discharge.
BS: *Now, you say you discovered the beam plasma discharge at Plum Brook.*

HL: Um-hum.

BS: *Now, you say you discovered it as a phenomenon or you discovered it for the first time in an artificial situation? Both.*

HL: It was the first time the beam plasma discharge had been found to exist under these conditions which were so vastly different than the high energy beams - high magnetic field strength, small chambers that had been used in the fusion work where it was really first discovered. It was like so many phenomena. It actually occurs over a wide range of variation of the system parameters.

BS: *I see. But, basically you're in these low-pressure tanks, you're duplicating the situation over the poles where auroras occur.*

HL: That's right. It's a good simulation.

BS: *And that was the first time that artificial auroras were observed?*

HL: No. I don't think that's the first time that artificial auroras had been done. Certainly on the scale we did it, in the big tank, it was the first time. I think artificial auroras had been produced in laboratories. Norwegians did some work with what they called terellas which were magnetized spheres that they bombarded with electrons. This was done way back. I've forgotten - '20s or '30s. That's when it was discovered that electron beams could come from a distant source and be focused into auroral zones. And I think that was a sort of an underlying basis for, or support for the hypothesis that what caused the aurora were particles coming from the sun.
Now, that simple idea turned out, of course, on further study, to be just skimming the surface. It's really more complicated than that. But, certainly the fundamental driving energy for auroral displays is energy derived from the solar wind variations.

BS: *Um-hum. OK. So, this helps understand auroras then, being able to observe them when you want.*

HL: It does help. One of the later projects that the University of Minnesota people did and others have done it also, was to carry electron guns up by rockets into the upper atmosphere and fire the electron guns from the rockets and see what happened. And I believe, if I remember correctly, that they found, when they had done this, that they could also strike a beam plasma discharge under appropriate conditions.

(100)

BS: *OK, I'm going to ask you a couple of personal questions. The people that influenced you - you've certainly listed them very nicely here - changed your life. Who was the most influential?*

HL: Who was the most influential? Gosh.

BS: *Gordon Little?*

HL: Well, certainly Gordon and George Reid as far as my work in radio noise absorption. After all, Gordon introduced me to the whole field. If it hadn't been for Gordon, I probably would never have gotten off into this field. So, in that sense, he was most influential and an awfully nice guy, to work for . . . work with. And then George Reid, who is here in Boulder, as I mentioned. We became close friends from that time. In fact, he's the fellow who was
instrumental in getting me to move to Boulder. He had already been in Boulder for a few years, after he left the Institute.

BS:  *But, he was at the Institute during IGY and was part of all . . .*

HL:  At the tail end, yes. At the tail end of IGY. Yes, that's correct.

BS:  *And what was his position then?*

HL:  Well, he was a senior scientist. I don't know his official title. Gordon Little had become Deputy Director of the Geophysical Institute. I do not believe George ever had an administrative role of that nature at the Geophysical Institute. Certainly he was one of the senior scientists at the Institute. So, those two probably had the most direct impact on the development of my scientific career after I got it launched in Alaska.

BS:  *You must have had some young men and women working for you that you sent on their way.*

HL:  Yes. Now, I did have a couple of graduate students when I was at the University of Iowa. I was brand new to teaching when I went to the University of Iowa. I went as an Assistant Professor, so I didn't have a big cadre of graduate students. Getting back though - backing up from that just a bit in time, much to my surprise and happiness too, several people, both in this country and abroad, sort of picked up on my work that I published in my thesis and took off from that point and one of the best known in this country is Jack Gosling whose been down at Los Alamos in the Space Physics work at Los Alamos for years and years. He did his thesis work under Bob Brown, who was then at the University of California in Berkeley. He used some of our results in his thesis work.
BS:  *Was that Jack Brown?*

HL:  Oh, Jack Gosling was the student and Bob Brown, who was Professor of Physics. Bob Brown visited the Institute back in the IGY days and also was flying balloons, not with nuclear emulsions, but with Geiger counters, and ran into a couple of the July, 1959, big solar proton events.

(150)

He had balloons up and recorded the cosmic ray increase at . . . his balloons went to about 100,000 feet, too. They weren't the real giant balloons that the Minnesota people . . . they were intermediate size.

BS:  *Where were his balloons launched?*

HL:  Right up by the old magnetic observatory at the top of the hill, above and slightly to the west of the new Institute building. And he also was studying solar auroral x-rays. He would record the ionization in his detectors by x-rays that are emitted during the auroras. Bob and I and a number of other people, too, collaborated over the years on several different studies connected with aurora. One of the things we found early on, for example, and I think this was a joint discovery - a lot of people were involved in this. Have you heard of sudden commencements of magnetic storms?

BS:  *Yes.*

HL:  When the solar plasma runs into the magnetosphere on the sunward side of the Earth, it sets up shock waves, among other things, and perturbs the ionosphere and you can see this in the
magnetic records as what's called the sudden commencement. The magnetic field measured at ground level just suddenly shifts in all its components associated with this compression of the magnetic field by the incoming solar plasma. And, we found associated with the onset of sudden commencement that we would see in the auroral zone, a sudden increase of cosmic noise absorption. Bob Brown found from his balloon flights that he would also see a sudden increase of x-rays at the time. So, something was shaking out electrons that precipitated into the atmosphere at just the time of sudden commencement. And that was, as far as I know, quite a new discovery. And, as I say, it was made jointly. In fact, if you looked at the early papers that were written about this, it had Bob Brown's group and our group and the Swedish group - a stack of authors goes on and on for half the page.

BS: *Yeah. Would you do it all again?*

HL: Sure, I'd do it all again.

BS: *You liked your life.*

HL: I did. I went to Alaska totally ignorant of what I was getting into. I didn't know anything about Alaska. I really hadn't done my homework about Alaska. I just said, "You know, I'm going to go and get this job." And I'll relate one little story that can be edited out of the tape if it's not worth anything. But, it was a strange experience for me. We were in the old Geophysical Institute building. Do you know which building that is? It's . . . .

BS: *I've been there, but it's been so long. '80s, no '70s.*

HL: At any rate, it was a building that was situated on the southwest side of the campus and it essentially was on the side of the brow of the hill and it overlooked the Tanama River Valley to
the south and the Alaska Range. You could even see Mt. McKinley down in the notch between the Alaska Range and the nearby hills. You could see Mt. McKinley which was what? 250 miles away or something like this. And I arrived on the 12th of September 1950, and I was assigned an office on the third floor. There was no problem in assigning offices because there wasn't any staff. There were more empty offices than occupied offices.

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BS: You were there on the ground floor, weren't you?

HL: Yeah, as far as the beginning of the new Institute. And I remember looking out the window across the Tanama River Valley out to the Alaska Range and thinking now this is really a lovely sight. It's beautiful to be able to sit here and look out over this scenery. And the trees in the Tanama River Valley were a mixture of sort of scrub pine trees, Jack pines or something, and Aspen trees. And the Aspen trees had all turned yellow. They had not dropped their leaves, but they were all yellow. I looked out over this and every day for two or three days as I looked out over this, I'd get a queasier and queasier feeling in my stomach that I could not figure out. What is going on here? This is beautiful. Why am I getting this queasy feeling in my stomach? Then one day, I looked out and it hit me just like that. That's exactly the color of the succotash that my Mother used to make. She made succotash by mixing cans of green string beans with cans of corn. A dish I absolutely detested because I hated stringy string beans. As soon as I realized that, I had no more trouble looking out the window.

BS: Yeah. That's funny. Well, you were there basically at the founding of the Institute. It's certainly grown. Do you know Sidney left all of his papers at the University of Alaska? I was just thinking as we went along, we talked about archiving stuff. That's an appropriate place for your papers probably, of that era. Because you really, you made history.
HL: Well, it was fun. It was fun. That's the biggest part of it.

BS: Yeah, who wants to make history that's not fun?

HL: Yeah. Thanks to the sun, we really lucked out.

BS: Oh yeah. Well, James Van Allen said he didn't anticipate that in 1951, at that dinner at his house. I bet he didn't, but he gets credit for it. So, well I think that pretty well does it unless you can think of anything else.

(End of Interview)