

Dr. Norbert Untersteiner

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Brian Shoemaker

Interviewer

BS: This is an oral interview conducted with Dr. Norbert Untersteiner 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 in the Regal Harvest Hotel, Boulder, Colorado by Brian Shoemaker on the 7th of October, in the year 2000.

Dr. Untersteiner, you played a pivotal role in polar research during the IGY and that's certainly the beginning of a long career of research, including the polar regions and which, with the MEDEA work which you do today, still continues. So we're interested in you, your background, what brought you to it, and the events that happened along the way. And, looking back, what you think of it when we're kind of wrapping this up.

NU: Well, if you ask me what brought me to the polar regions, my answer is very clearly, it was my father. He . . . my father, was a medical doctor. He was not really crazy about that as a profession. He was always interested more in science. He took me out to the mountains when I was a small boy and in 1931 - we were living in Salzburg, Austria - he organized some kind of a gentlemen's expedition at that time, to Spitsbergen. There was a museum director in Salzburg who wanted specimens. There were two of his buddies - they wanted to make first ascents on those little mountains in the Reuss(?) Peninsula, and then they had a friend who had just gone through some family tragedy and

they wanted to cheer him up, so they got the support from some publishing house and they made one of those summer trips - today we would call this a summer vacation. But in those days, it was a little expedition. They went up by boat and they spent the summer of 1931 on the Reuss Peninsula in the northwestern-most corner of Spitsbergen. I was five at that time and, of course, everything associated with that - the maps, the stories, the pictures, the lectures - left an indelible impression on me. In fact, my father really then wanted me to do the real thing in life later on. So this is how I got interested. I read all the classics - Shackleton, Scott, Mawson, Amundsen, Manson, Svytrop? - in my teens.

BS: _____? *Boys of the _____?*

NU: No, well, that was a little too sophisticated. I was just interested in the stories, and all the exploration.

BS: *The Germans did make it kind of dry.*

NU: Yeah. And so when I finally became a scientist with an interest in polar stuff, well, the closest thing to polar stuff is glaciers - that's at least snow and ice.

BS: *I'd better ask you a pregnant question here - where did you live and grow up?*

NU: I was born in south Tyrol which ethnically was Austrian, but became Italy after World War I in 1918. My parents lived there. And it was a very difficult time apparently for my parents because my father taught at the University of Innsbruck. And he had a practice in Merano, so he went by train every week to Innsbruck.

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It's not a long train ride, but it was crossing the border and to both the Italian and the Austrian authorities, somebody who crosses the border once a week was eminently suspect and he was perpetually detained and searched and questioned because they thought he must be up to something bad. Of which there was a lot going on in those days. So they finally decided they had enough of that and they moved to Austria. And they moved to Salzburg when I was, I believe, four, in 1930 or 1929, and that's where he continued his medical practice and that's where I grew up. That's where I went to elementary school and to high school.

Well I think I'm one of those people who can say with a great deal of justification, I was really programmed by my father. I believe I did pretty much everything that he sort of hoped I would do. Unfortunately he did not live to witness it because he died very young. But in any case, I was programmed clearly, and I'm very happy that I was because I think from a very early stage on, I had a sense of direction. I had an idea of what I wanted. I wanted to be a scientist, I wanted to be an academic, and I wanted to leave Europe. And I did all of that in spades, I guess. So, back to my school days - this was the time of the, in Austria, the Austro-Fascists Regime with this very powerful, politicized Catholic church and Dolfuss, the Chancellor of, the infamous Chancellor Dolfuss and all this illegal Nazi activity. Anyway, all these people, of course they were all still mired in the Austro-Hungarian monarchy and they had very little idea - they were politically babes in the woods. They were extremely naive. And so when I was twelve in March of 1938, Austria was taken over by the Germans, by Hitler. And that profoundly changed everyone's life. I was immediately told that I had to march and sing these idiotic songs. I have fulfilled my lifetime allowance of marching right then and there and since then I have not even marched in academic processions when I was Dean because I told the President of the university that I have a medical condition that prevents me from marching, no matter what the cause. So, 1939, the war came and it became increasingly

clear that this whole operation was really doomed. I was drafted to this labor service that preceded the military duty. I was in 12th grade in the gymnasium, in this type school where we had Latin for eight years and I was sent to a German air base outside of Munich and we had to shovel the snow and keep the runway free, even though there was no planes flying because there was no fuel to make the planes fly. So we did that. And then in May of 1945, I was drafted to the infantry and I was sent to what was then Yugoslavia, is now Slovenia, just south of the border in Austria. That's where I did my basic training, and then I went to another course. There was a lot of . . . down in Slovenia there was a lot of sort of land mines, of mortar fire, and surprise attacks, and futile little dinky military operations - everything by foot. It was the end of the war. Everything was falling apart. And I managed to get through all of this without getting shot or killed or anything. My generation was pretty badly decimated in these last years of the war.

(100)

BS: *You started very young - very young.*

NU: I was 19. When the war ended, I was 19. So, then I hired out to the American Occupation Troops right away and became an errand boy taking uniforms to the dry cleaner and going to the PX and picking up cartons of cigarettes and driving this headquarters commandant, Captain Cooper, around. And, uh. . .

BS: *Where was this?*

NU: This was at first in Salzburg and then this unit, this American unit moved to Vienna to become part of the Four Power Occupation of Vienna. In Vienna, the first district, the center of town, was administered by all four Allies - American, English, French, Russian

- and the rest of the city was divided into sectors that were occupied by the four powers and outside, surrounding Vienna, was all of the Russian occupation zone because the Russians occupied all of lower Austria and this district that borders on Hungary all the way to Linz, which is about half way to the West, no a third of the way to the West in Austria. The British troops were in the south. The Americans were in the center. Salzburg, for instance, Linz and Salzburg were American occupied, and the French occupied Tyrol - the western most part. So, to go by train from Vienna to Salzburg was quite an operation and you had to pass many passport controls and identifications and they would dust us with DDT to uh. . .

BS: *I did that with my wife in Holland.*

NU: Yeah?

BS: *They all had lice.*

NU: Yeah. So anyway, in the fall of 1945, I registered at the University of Vienna just to do the basic stuff - physics, math, chemistry. But life was very tough. There was not enough to eat. We had ridiculous rations of coal briquettes. I was sharing a room with a friend of mine and we had some kind of a tent inside our room because it was easier to heat the air just in the tent. Then we would sit there and do our math problems and that got pretty bad. And then, it turned out that the famous professor, Albert Defant, one of the giants of oceanography, returned from Berlin to Innsbruck. The Chair in meteorology and geophysics at Innsbruck was vacant and Professor Defant's institute in Germany ended up in East Berlin, in East Germany, so he, of course, didn't want to go there. So he came back to Austria. He was, in fact, Austrian. In fact, he was from the same south Tyrol region where I was from and he just came back to Innsbruck and said, "Well, do

you have a job for me?" and they said, "Well fancy that. We just have a vacant Chair for you." So in late 1945, Defant was back in Innsbruck, and my father said. . . called me in Vienna and said, "Look, move to Innsbruck because they've got a famous professor there and that's a much better place and it's easier to live there anyway." So, I - for the spring semester of 1946, I started in Innsbruck. And this was a very - it was an inspired time. Life was still tough, you know. There were these - this situation where one week there was unlimited amounts of tomato paste, but no meat, no cheese, no butter.

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BS: *No spaghetti.*

NU: But, uh. . . no spaghetti. The next week was only spaghetti but no tomato paste. The next week there was only Danish ham but no bread. It was not easy to . . .

BS: *Sort of like living on an ice station.*

NU: . . .to get by, but it was, you know, everybody worked very hard. It was all forward-looking, so to speak. And what I really, in retrospect, when I watch people today, what I appreciate in retrospect is that we had no choices. It was not a problem to make up your mind what you might do and worry what you are really interested in or whether you should maybe do this or maybe go part to the university and work for a while. Work - no way. There was no work. There was only plowing ahead, straight ahead as fast as possible to get through school and to get a job. Getting a job was the one looming problem because in a country that had just lost a big war that was - where a lot of damage, war damage, was

done - science was not really on their minds much. They wanted, first of all to eat. They wanted to repair the roads. They wanted to repair the postal system and the trains and get enough food for people to eat. Science was way down on the list of priorities.

BS: *Joke tells the same story as you.*

NU: But, on the other hand, it was spirited because it was clear that we had to perform and we had to hustle and we had to work hard to just get somewhere. Well, the first thing I did, sort of in the direction of my ultimate professional choices was that I signed up to help with surveying glaciers. This was something that the Austrian Alpine Club and the Austrian Academy of Sciences had supported way back. From way back, there were these classic glaciers like the Hinter Ice Werner, for instance, that is now famous that has one of the long series of mass balance studies. And I signed up to simply help carry the tripod and the _____ light? and to do triangulation in the summer. And each summer I spent several weeks working up on the glaciers and then doing, of course, mountain climbing on the side because in those days, I was an avid mountain climber. You know . . .

BS: *Who did you work for when you were surveying?*

NU: These were faculty members. There was a professor - two, in fact, two professors who were both in the math department who happened to like this work very much and they did these survey. There was the famous Richard Fensterwalter at the Technical University in Munich. He was sort of the boss of the surveying and map-making for the Alpine Club. And there was Professor Viatorres, and Professor Schotts - they were both in the math department. They were our leaders, and told us where to go and told us where to put the numbered rocks for velocity profile studies and so forth. It was all strictly

volunteer work and they just paid our expenses and sometimes not even the expenses. We had to bring our own food and our own stuff. But it was interesting and I learned a lot.

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And I might throw in as a little side remark that I probably walked over this ___Ixie?___guy - the Stone Age, many times, because that location up there on the Italian border was one that we visited at least twice each summer because it was a standard point to triangulate the points of the glacier. Except when I was there, there was probably ten meters of snow over this guy, Ixie? But it was fascinating when they found this and I saw the first pictures and I looked at this and I thought, "My God, I must have walked over this guy many times."

So, in 1950, I received my doctorate nominally in geophysics and astronomy. You had to make a, take a big exam in your main topic and then in a secondary topic for which I chose astronomy because it was something that also interested me and I had learned a little bit about it. By present day standards, what I learned was really quite modest, but anyway, it was enough to get through. And then the question came of a job. What, where was a job? And in the Austrian academic hierarchy, the first thing you are striving for is to become an "assistent". In our system here this would be something a little bit shy of an assistant professor, but it is somebody who is clearly on the academic track. So there were no assistantships available. There were only three universities. There was Innsbruck, there was Vienna and there was Graz and that was it. And if there wasn't an assistantship available at any of those, then you were just out of luck.

By that time, another famous person was director of the Central Institute for Meteorology and Geodynamics in Vienna - Professor Heinz von Ficker. He was probably the only other really eminent person around at that time. He made a very famous expedition to the ___Palmiers___? He became a member of the Russian Academy of

Science before World War I. And at the time of the Russian occupation after 1945, he was very effective in protecting the Austrian Weather Service and getting help from the Russians and getting the teletypes back up and so forth. Because he was well known to the Russian authorities from his work in pre-Bolshevik Russia and in the Palmiers and in Caucasus. So he said to me, "Why don't you come to Vienna? Maybe I will have an assistantship." So I went to Vienna. So he said there might be an assistantship become available later on, but for the time being, I would have to go to the weather department and make weather forecasts.

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This was a kind of an apprenticeship position that was funded by a radio station. So all of a sudden, I became a radio weather forecaster. I had drawn and analyzed exactly one weather map during my course of study so I really had only the most vague idea of what I was doing. So I tagged along with the other people and I gradually learned how to draw isobars and you know you have to remember in those days, those were radio people with headsets on who were receiving the weather messages from the international radio broadcasts. They were by hand entering those into the weather maps and then when a block was covered with the station entries, the meteorologist was sort of looking over the shoulder of the technician and beginning to plot down the isobars and then look at the pressure tendencies and look at the cloud and precipitation, and figure out what was happening in the weather. And of course, if you have done this only once before, it takes an eternity to try to remember - this is all coded stuff - it takes a very long time to remember what you're looking at because you have to, you frequently have to look up in your code book what this symbol means and what that symbol means. So it was excruciatingly hard work. But I finally sort of learned how to wing it, and I made weather forecasts for the radio - there was no television in those days, of course - for a year. And

then the assistantship became available. Then the going got better because then I was able to start research. I didn't have anything in particular in mind except that I did want to do work that takes me outside and takes me to the mountains, if at all possible.

So with my friend, Herfried Hoinkes, who was at that time an associate professor in Innsbruck - he will come back in my story because he had other significance. So he and I went to a glacier in Tyrol and made a heat balance study patterned after what Hans Almann had done, what Harold _____? had started in Spitsbergen. Almann and his students had done in northern Sweden, particularly Karl Christian Wallen. And nobody had done this kind of study in the Alps. So we set up, we man-hauled all our equipment up to the glacier.

(300)

We lived in a tent and we set up our little micro-meteorological masts with anemometers and psychrometers and radiometers and we measured the components of the energy balance that caused the glacier to melt. It was, of course, not the whole picture. Only what happened in the summer. But it was very interesting and it got some note in the scientific literature.

Hoinkes and I did two of these field studies and published them and it sort of really convinced me that this is something that I want to do. I want to go out. I want to do end to end science. I want to go identify my own problem. I want to go out and measure stuff. I want to analyze the results and if at all possible, I want to make some theory to go along with this. Now, I can say this in retrospect, I'm sure. At the time I was not thinking this clearly about it.

BS: *That's important.*

NU: But in any case, that's sort of what I was striving for.

BS: *What year was this, Norbert?*

NU: This was in the early '50s. I believe it was 1951 and '52. Then in '54, I started something else. Another friend of mine, Walter Schwarzacher - he and I - he was also, he had been a student at Innsbruck and his expertise was in mineralogy and especially in petrographic analysis, so we got interested in looking at the structure of ice, especially the foliation in ice, which is a feature inherited from the deformation of the ice while the glacier is moving along. And we went to another glacier in the Salzburg area, one of the largest glaciers in Austria, and we climbed down in crevasses and got samples and we did fabric analyses. We did this for two summers. I believe it was the summers of '53 and '54 if I'm not mistaken. But anyway, it was all pre-IGY and published three or four papers about this stuff. How, especially how the foliation that we see with the naked eye and that we see in the axial distribution of the grains in the glacier, how that relates to the deformation field in the glacier. Well, it was published and that was that.

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Then came along my sojourn to the Karakoram. The city of Frankfurt and some people in Germany got support to visit a region in Kashmir. It's in the Pakistani part of Kashmir in the general area that is still, ever since the partition of India and Pakistan in 1948, is a region of a great deal of unrest and fighting, even. They had identified a region in Kashmir that was first visited by an American couple of wealthy mountaineers. Their name was Bullock Workman. I believe this was before World War I in the early part of the 20th century. The region was glaciologically of interest and there were lots of unclaimed peaks that were not too big. It was not like Kato(? K-2?) or Mt. Everest. And

they decided to have an expedition - a climbing expedition - but they needed some scientists because they wanted additional support from the German Science Foundation.

BS: *What was the group that organized this?*

NU: It was a group, I think they coagulated together from, uh. . . simply from climbers.

There was a . . .

BS: *Germans?*

NU: German. They were all from the Frankfurt region, and the leader - the organizer and leader of the expedition was a person who at that time was politically active and was friends with the mayor of Frankfurt. The mayor of Frankfurt in those days was a very influential person and Frankfurt was, de facto, the capital of Germany. So it was beginning to be a wealthy town. So they decided to fund something like that. But they also wanted additional support from the German Science Foundation, so they were looking for a research program. And what they, I'm sure, with the advice of somebody at the time - I don't know - what they zeroed in on is human physiology and then this kind of glacial meteorology that I was doing.

(400)

BS: *Had Heinrich Herard returned from Tibet by then, or was he still lost over there?*

NU: I think he was probably . . .

BS: *Still there.*

NU: Still there, but . . . you know, I . . .

BS: *Got involved in _____mountain . . . organizing. . .*

NU: Yes, but I think that was really later. Anyway, so . . . they recruited a young man by the name of Walter Brendel, and he was a physiologist at the Max Planck Institute for Physiology in Baden-Nauheim. And his plan was to conduct studies of our moisture balance which turned out, I guess scientifically, very interesting and it turned into something of a pain in the neck for us, the climbers because if you're climbing in high altitude and if you have to measure every bit of water that you drink or that you pee, it gets a little - the logistics of that gets a little cumbersome. But, anyway, in the end, he got very interesting results.

And the other discipline to be supported was this kind of heat balance and micro-meteorology study that I knew how to do. The expedition first contacted my friend, Alfred Hoikes and asked him if he wanted to go and, as I recall it, he really didn't want to do because he was married, he had two smallish children. He was also, I think, in the throes of getting tenure or something. So he didn't want to go, but he said to them to take me. You know, take him instead. So, this is how I got on that expedition. And in April of 1955, we set off - Oh, I should say that before that, we did some very intense training and conditioning in the Swiss Alps. Whatever crazy stuff we did there was far more strenuous and risky than anything we did in the Karakoram later on. But anyway, that gets into too much detail. Anyway, we set off in April of 1955 on a ship from Genoa, going down past Crete in the Mediterranean. The Swiss canal was still operating at the time. Later on, you know, in the war, it got blocked. But we went through the Swiss canal, down the Red Sea. We visited Aydin and then we went to Karachi.

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Then from Karachi, we went by train up to Peshawar and then we flew to Saidu(?). This is the capital of East Pakistan. It had one house and the rest of it was essentially furniture crates - maybe 2000 people or so. There we hired 200 local guys - porters - to carry our five tons of stuff up to this glacier. And anyway, this is all written up in a book. It was a fabulous experience. I made these heat balance measurements at two levels. One at 4,000 meters and the other one at 5,000 meters. I made some radiation measurements up to 66,400 meters - that's about 20,000 feet. Three people in our group made - scaled the summit of this mountain that they were after. It's not one of the biggest ones. It's not even 8,000 meters. It's 7,800. But it was a wonderful experience. And we came back in October of that year and - okay.

In the meantime, this is 1955, all through these years, I was writing to people, I was going to international conferences to the extent that I could because travel money was very scarce, and I was looking for contacts because I wanted to leave. I wanted to leave because it was, it was . . . it was clear that I would have to wait 25 years for a professorship. It was not . . . it was . . . and I think still is, a system that these chairs are very, very few - these so-called chairs. And if somebody doesn't die, there is no vacancy. I think, in the meantime, they have ameliorated the system a little bit, but it's still essentially the same.

BS: *Well, Colin Bull and David Elliott tell you the same story in England.*

NU: Yeah. Same thing. Same idea. Well. . . so. . . Out of the blue, I get a letter from Robert Sharp at Cal Tech asking, saying that he had read this paper that I wrote - co-authored with Walter Schwarzacher about the foliation in the petrofabric analysis. Would

I be interested in something like a post-doc at Cal Tech? So I said, "Well, of course. Funny you should ask."

(500)

BS: *And who offered this?*

NU: Sharp. Robert Sharp. He's well . . . I think there's not a glaciologist, or a snow and ice person or a geophysicist alive in the country who hasn't had some contact with Bob Sharp. He's still around and he was a great guy who moved the careers of many people. So, I said, "Yes, of course. Just tell me what to do." Then a few weeks went by, and I got an extremely embarrassed letter from Bob Sharp saying that he's terribly sorry but they didn't get the grant that they were going to fund my post-doc with, and he had to - most regretfully - had to renege on this. Now, to me, this was nothing. This was just another of my three dozen attempts that didn't pan out. I . . . it never bothered me, but it bothered Bob Sharp a lot that he had to renege on this. And apparently he called all kinds of people saying that we gotta get a job for this Untersteiner guy. I told him, you know . . . I invited him, then I had to dis-invite him. And I'm so embarrassed and find a job for this guy.

BS: *And you saw his influence produce(?) Right?*

NU: And it so happened that they were just ramping up for IGY. And the next I heard was from Bert Crary. "Would I like to go to the South Pole?" And I said, "Sure, I go any place you want to send me." A few weeks later, I hear from Harry Wexler who says, "Guess what? We had a little breakdown in communication here. We're not doing this kind of work at the South Pole and also, we already have somebody for Little America.

How 'bout the North Pole?" And so I said, "Fine. Hey. Any Pole you want to send me to."
And this is how I came over here.

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Then there was correspondence back and forth. I had to send them all my reprints. It turned out that Richard Hubley, who was the, I believe, the second Ph.D. that my department at the University of Washington ever turned out, had become the coordinator of the US Northern Hemisphere Glaciology Program. So, my further contacts were with Dick Hubley. Dick Hubley was detailed from Seattle to Washington, to IGY headquarters, and I corresponded with Hubley. I told him what I wanted to do. It was a great experience because they only vaguely told me what they wanted me to do.

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(Begin Tape 1, Side B)

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NU: So, all I knew was that I was going to do something measuring the mass changes of the ice and measuring everything else that's causing the mass changes. Energy balance at the bottom and top. But as it turned out, the Russians had done something similar, but I didn't know about it at the time because Russian literature was very hard to find in those days. So they essentially said, "So well, so what do you need?" When I came - when they said I should first come to Washington, I was shown around by Harry Wexler, by Hugh

Audeshaw(?) and by Dick Hubble. I talked to Bert Crary on the radio because he was in Little America at the time. This was February of 1957. And then I got on a train and went to Madison, Wisconsin, to talk to Vern Sumi(?) and to Heinz Lethow(?). And they gave me a lot of good advice. I don't remember if our friend, Chuck Sterns, was already there. But he might have been. Maybe he was a graduate student.

BS: *Bentley was already on the Ice.*

NU: He was already on the Ice. Yeah. John ____bach was not there yet. I think it was the guys - you know, the old guys. And so they gave me some very, very useful hints because Lethow was one of the big people developing boundary layer theory, and then I went to Seattle. The trip from Madison to Seattle was very bad because I missed the flight that I was booked on and I had to go on some all night milk run that was flying at 1000 meters, or 2000 meters, just at the base of the cumulus clouds. There was some cold air moving in from Canada and it was extremely turbulent. We landed about five times between Chicago and Seattle and I got sicker every time. I had and still have a very low tolerance for having my stomach jostled around. So I arrived in Seattle really beat. Then I went to the Meany Hotel, checked in the Meany hotel which they told me to do. It was in the wee hours of the morning and there was, of course, nobody there. I'm glad nobody saw me because I must have been green in the face. So I went to this hotel and the guy checked me in and I went to my room and there was no bed in my room. So I called the reception and said, "You gave me a room that has no bed." And so the guy said, "Excuse me sir, but we don't have any rooms with no beds." And I was so sick and so exhausted that I got mad and I told him to get up here and I will show him that there is no bed in my room. So he came up and opened the door and folded down the bed. And I had never seen a hide-a-bed in my life. So it was one of my numerous early American experiences. S

So, and then we spent a few weeks with Phil Church who was the Chairman of the department at the time. He was extremely generous along with everybody else. They just ran around and procured all the equipment that I had told them I need including three or four water level recorders, like regular tide gauges you know with a float and a pulley and a recording pen. And they had, in fact, not got those because they must have thought - they must have had their doubts about me, you know - what kind of idiot did we hire here? He wants to measure tides on a floating cake of ice with the tide level recorder. Maybe we made a mistake here.

(50)

But, as it turned out, it was, in fact, a moderately clever idea because the tide gauge allows you to measure how much melt water is running off the ice floe if the tide gauge is fixed in the ice and the water runs off. The ice comes up relative to the freeboard level, and this is an integrating measurement that is far better than measuring ten stakes or holes in the ice. But anyway, after I had explained to them that I'm not going to measure tides but the vertical motion of the ice floe relative to the sea level, Phil Church jumped in his car and drove down to Portland - there was a company there that made these things. And hauled them back. And so it was all very exciting, of course, obviously. And I fully expected to go out there and sit on this cake of ice for a full year. So in early March, I proceeded to Fairbanks to, at that time, Ladd Air Force Base just outside of Fairbanks. There was an aeromedical research laboratory that gave us all kind of health checks and psychological checks. And my field assistant arrived, Arne Hansen. And with a huge full leg cast and I thought that someone else is crazy because they're sending me a field assistant who can't walk, who had to walk on crutches. It turned out that Arne Hansen wanted a quick skiing fling before he spends a year on a cake of ice and he went to Stowe

or someplace and broke his leg. But as it turned out, they got the cast off before we went out on the ice.

So, the whole first crew for Ice Station Alpha, as it was called, was assembled in Fairbanks at Ladd Air Force Base waiting for the station to be built to the extent that we could go out there. That we had a place to sleep and a place to work and a place to eat.

BS: *Who were the people?*

NU: Let me preface this. The two people who were instrumental in finding a suitable ice floe for Ice Station Alpha and putting all that stuff out there were none other than Joe Fletcher, who flew along with Father Tom Cunningham - Father Tom of the Arctic who spent his entire life as a Jesuit priest up on the north slope. They were the experts who would select the floe, flying in a ski equipped R4D - Select the floe that eventually was chosen to build the camp. And the head of the construction team that actually built the camp was the same Fritz Awe that Mildred Crary mentioned in her lecture a couple of days ago. He was a monstrous guy of 300 pounds of pure muscle and four or five equally staunch characters who knew how to level a hummock? and run a road grader on this extremely bumpy - to make a flat surface running over bumpy ground. Anyway, this was all happening before the scientific crew went there. I crossed paths with Joe Fletcher a couple of weeks before when Joe came back after he finished his assignment with this weather squadron in Alaska and I believe at the time he was on his way to his next assignment that was to be Air Attache at the Embassy in Oslo.

(100)

Joe and I walked up and down University Avenue in Seattle and he gave me a download of what he knew about living on the ice. Which, at that time, was, of course, biased by his

time of T3 which had no pressure ridges and it had all kinds of other problems, but not pressures, but it was very, very useful what - the briefing that I got from Joe. So anyway, we were there in Fairbanks waiting. Let me see if I can remember what the team was. The team was a guy by the name of Morris Davidson, and Ken Hunkins, who was then a graduate student at Stanford. That was the team from Lamont. Then there were four people, four technicians from the US Weather Bureau who were doing the radiosondes. There was my assistant, Arne Hensen and I. There was another person by the name of Terrence McDonald - also Weather Bureau. I think he was sent there to supervise the establishment of this weather station and the radiosonde station. Lieutenant Colonel Stromquist, third Joseph Bilotta, and in the end, for the very short time, they had a fourth guy that I didn't get to know because I left.

BS: *Smith. Joe Smith.*

NU: Smith. Air Force?

BS: *Yes.*

NU: Yeah.

BS: *He's a retired Colonel, lives in Washington now.*

NU: Beside, now - here I want to ask you this, Brian.

BS: *Freeman was the first though.*

NU: Freeman was the first. Freeman was the first. Freeman. Freeman with all of this was so funny. With all of this pushing oil drums and pushing gasoline drums, he developed this hernia and he and Tom English, the biologist - this was a few months later - Tom English had a whole case of Drambuie and Major Freeman was addicted to Drambuie and he would come in. We were already sacked up, you know, four people in the corner. And Freeman would come in holding his hernia, begging for sympathy and Tom English would say, "Major Freeman, the fuckin' Drambuie is under my bed." He would drink a water glass of Drambuie, but he said it was so good for his hernia. How should I do this? The first senior scientist out on a camp was this guy Davidson and he crumbled.

BS: *You mean was scared?*

NU: Well, he did . . . he did. He's dead now. I want to ask you how to do. He did this well documented, well known detachment. He said, "You know, it's crowded in the lab when everyone works during the day. I can work better if I sleep in the day and work at night." And in the meantime, I have learned after many years of work that this is the first signal that the guy is falling apart. He can't take the pressure, he can't take the presence of the other people. He wants to . . .

(150)

BS: *He's worried about the Ice Station falling apart and breaking up.*

NU: Maybe he's worried . . . I don't think he was worried so much. I think he, or maybe he was worried. But anyway, he disengaged. And, of course, when they go on this famous night schedule, they have dinner. They have drinks with everybody else. They

have dinner, they sit through the movie and the bull-shitting session in the mess hall in the evening. People go to bed at 11 o'clock. He goes to the lab and he goes to bed at 3 o'clock. So we worked 10, 12, 14 hour days. He worked 3, 4, 5 hour days. That's one thing. And then, the next phase is that these people spend enormous lengths sleeping. Hunkins, bless his soul, picked up the whole slack with all he did. He did all the work and finally I had to send a confidential message to Phil Church and Phil Church contacted headquarters and they took this guy out and substituted me. So I don't know how to - why don't we just drop . . .

BS: *What was Phil Church?*

NU: Phil Church was the Chairman of the department at the University of Washington. He was my boss.

BS: *Tell me who Phil Church was.*

NU: Phil Church was the founder and Chairman of the Department of Atmospheric Science at the University of Washington. He came from the University of Chicago during World War II and I believe his expertise was boundary layers and they hired him because they were building a nuclear plant in Richmond in eastern Washington, and they needed him out there. And he was a lecturer in the Geography Department. And then in 1946, the University decided to start a separate, it was called Meteorology and Climatology and he was the first chairman and I think he was chairman until the '70s. How do you want this?

BS: *Well, you became the Chief Scientist.*

NU: OK. Well, it didn't change. Oh, but excuse me. Now we have to - all right. So, the first scientific team was assembled in Fairbanks, but the station was not ready. and Fritz Awe and his wild guys were still working out there. They were as I learned later - the main thing was that they wanted to make a runway for a C-124 to land on because they felt they had to have a road grader, they had to have a D6 Cat, and all they had was a D4 Cat that was parachuted in and it was quite slow and inefficient. So we were just waiting. Now, in the meantime, Dick Hubley had completed his job of planning the US glaciology program for the northern hemisphere, and he proceeded to do his own project in the Brookes Range on the McCall Glacier. This was a study - a comprehensive study of the energy and mass balance of a small, high latitude glacier. They had planned a two - an all-year camp in the upper part of the glacier staffed by two people and also an all-year camp in the somewhat lower down in the middle part of the glacier all year. And they were in the process of setting this up.

(200)

And I, you know, I was - they knew that I have a lot of experience in the mountains and working around glaciers, so it was a natural that I said, "Well, can I help you set up these camps because I have nothing to do. I'm just playing billiards in the BOQ." So, I flew with them. We dropped many loads with the C-119s flying out of Ladd Air Force Base. We loaded them up there.

BS: *These are to the Brookes.*

NU: To the Brookes. To the McCall Glacier. The upper camp was already in sort of an advanced state. But the lower camp we had to start from scratch. So we dropped boxes with tents and parts of . . . Jamesway parts. We dropped them from the flying boxcar out

the back door and then we flew over to Barter Island and with a small plane - I can't remember what it was. It was one of the bush pilots who flew. I think we landed on the glacier. And spent a week or so setting up the camp. The persons there were John Sader, whose name must have come up, and Charlie Keeler, who I believe worked for CREL at the time. And then a person whose name I can't remember. He was from Martin-Marietta and I think he was looking at possibilities of logistic requirements. He was clearly looking for some business for Martin-Marietta, possibly. So we set up this camp. And then I returned to Ladd Air Force Base and then it came time to occupy Station Alpha. It was much farther north than anybody had expected, really. But it was on a big, huge multi-ice floe and the first thing that I saw as I stepped off the airplane was one of these construction guys running around a power drill like crazy because his glove was caught in the mechanism and he couldn't kill the motor so he had already broken his thumb and he was running around, yelling for help for somebody to turn the So it was kind of an eerie thing to see, to step out of this airplane, expecting dramatic action of a different kind and there you see a guy yelling at the top of his voice, running in a small circle. Somebody rescued him. So, we occupied these Jamesways that were already there and started mapping out my radiometer farm and making sure that nobody walks out there and contaminates the snow and all of that. And it was very exciting.

BS: *What did you call the camp? What was the camp?*

NU: Alpha.

BS: *Camp Alpha?*

NU: Station Alpha.

BS: *In the Brookes?*

NU: In the Brookes? No, no sorry. That was just McCall Glacier. So the thing about out there on the ice, on the pack ice, that one tends to forget was that this heightened,

(250)

you know, almost bizarre ability to work long hours and to do this multitude of things, was powered or fueled by the anticipation that this is the one chance in a lifetime to do this. We had no idea, of course, that this would become, more or less, routine. And drifting stations would become the order of the day. There would be another one every year. We thought, this is it and anything I don't get done on this trip, I will never get done. So people were, you know, working maniacs. Just a shut-eye, a few hours sleep and up installing stuff, but, of course, it was very nice because we got an enormous amount of work done. So, we went through the first summer with learning about all the problems about the soot contamination around the buildings and learning to put out ablation shields, to put out skirts with whatever we had, insulating material to prevent this formation of these pedestals that were so small and so high that the Jamesways, the first ones we didn't watch. They just fell off these pedestals. And we learned all this stuff. We were, I don't know if we can say we were lucky. There was no accident, no air crash of any kind. We had one tragic accident with a guy walking into a propeller backwards and getting his head smashed in.

BS: *Killed?*

NU: Killed. And we had, in the winter, well I'm not there, I'm not there yet in the winter. That was in the summer. So, we went through the summer of 1957 very nicely. We

learned how to make a water supply. We had a clothes washer and dryer going. We had a shower. We had a big vat with a 2000 watt immersion heater. We had a good water supply. So, and then other programs would come. There were people with heat conduction probes measuring geothermal heat flux in the sediment. Tom English came with his biological program.

BS: *I've got a question to ask you. They put you down prior to the summer melt.*

NU: Yes.

BS: *So it must have been April, May. Yeah.*

NU: In early A. . . . We flew out in I think it was the 6th of April and the IGY, as you know, officially started on the 1st of June,

BS: *First of July.*

NU: First of July, so by the first of July everybody was fully operational. The Weather Bureau, the surface weather, the radiosondes, the oceanography, the hydrocasts? my radiation measurements, my mass balance measurements - everything was in place on time. So we got through the summer okay.

(300)

BS: *Any air drops during the summer?*

NU: We had a couple of incidents. In those days, running out of cigarettes, for instance, was a severe life threatening thing. We had, I'm not sure now whether this was in the first or in the second summer, but it was in the summer, two R4Ds flying out to land. We still . . . it was still barely landable. The snow was getting soft, but it was possible to land. The planes were maybe an hour out and the fog moved in. The typical summertime thick haze. They could never see the landing strip. They circled around for quite a while and had to decide to abort the mission and return. On the return, it turned out that they had maybe miscalculated a little bit the fuel or got a little stronger headwinds than they were bargaining for and at some point they decided they have to jettison everything. All the stuff that they were going to bring us - thermal probes, current meters, Nansen bottles, a lot of stuff had to be thrown out. And in their, maybe. . . I never. . . heard the follow-up - maybe in their slight panic they also threw out the US Mail and that was the end of these pilots. Their careers as pilots. Apparently this is a . . .

BS: *Air Force?*

NU: Air Force. Yep. This is . . .

BS: *Never throw the mail out.*

NU: This is a total no-no. Anybody - it was two bags of letters and a couple of packages. It wasn't . . . it couldn't have possibly have been anything that made a difference in the cargo of that plane. It was just something that they got so excited, that they threw out everything and they didn't realize that this was the mail bag. They tossed that out too. So, backing up a little, they did manage to build a runway long enough for the C-124. There were some interesting learning experiences. For instance, they had put teeth on the blade of the D4, scraped up the ice and removed one long hummock that had bothered

them. And working day and night, day and night, they scraped the ice and then dozed away the chips and dozed away the chips. And they flattened that hummock enough so they thought, well now the C-124 will be able to land there. What they did not do is every day check the level out there on the ice.

(350)

What apparently happened was that they flattened the sail of a huge pressure ridge that had a humongous keel underneath with a lot of buoyancy and after a few days, the hummock kind of came back and it was so gentle because of course it was so evened out. It was so gentle that you had to look pretty hard to see it. But they did see it when the C-124 finally landed, it made some tremendous porpoising motion. It pushed that nose gear down to the stop, I'm sure. And maybe that stop was bent a little bit. I have a movie of this. But nothing happened. And then they got the road grader and the D6, i believe, even in one load. You should know that. Is that possible?

BS: *With a 124, yes.*

NU: I think they had the road grader and the D6 and then Fritz Awe was really happy and then they could go to town and they made a nice, they made a secondary runway and - well okay. So, we got through the summer okay. I was spelled by Frank Badgley who was on the faculty in my department in Seattle. I was spelled in September of 1957 and I had a big good vacation. I worked on some data and I went back in around Christmas time of 1957. I think it was between Christmas and New Years. Directly I was on a vacation in San Diego without stopping hardly any at all from San Diego to 86 North in 48 hours. It was quite a shock. Into the pitch darkness. But the program was working fine. We were in there. By that time Franz Wanderhoven had spelled Ken Hunkins. The

Lamont Program was down to one person. I had a new field assistant - Bill Campbell who then went to the Antarctic after that and worked for USGS. So we went nicely through the winter. There was one bad thing that happened. A fellow working for the Weather Bureau came up with me. He had, he had in fact, spent a year in the Antarctic, only went home to New York for a brief vacation and then came out to the ice and in the course of about 4 to 6 weeks, he sort of gradually died. And we were never, on the station, we were never told exactly - of course, I wasn't particularly interested either. It was probably methal alcohol poisoning. He brought some.. .

(400).

BS: *What was his name?*

NU: I don't remember. He brought from his home, from his relatives, some home brewed booze. And he had - he was a very quiet, very good worker, very quiet, friendly, peaceful and every day before dinner, he had a couple of drinks by himself, pretty much. And then he'd go over to the mess hall. And he got weaker and weaker. He was bed ridden in the end. We had a medic, an Air Force sergeant who had some medical training. He was on the radio all the time to the aeromedical lab in Fairbanks getting instructions. And they told him what to do which was essentially nothing he could do. What puzzles me is that after he got so weak, he stopped taking these drinks. So maybe he should have recovered. But he didn't recover. He just died. Maybe he had something that aggravated this in addition. So, it was a little unpleasant to know that, you know we had to put the corpse in a bag and put him in a cold storage thing. We had no idea what this guy died from and there is this secluded group of people and once in a while somebody said, "You know my neck has been kind of stiff."

BS: *We all do that now, you know. You hear about the effects of the various diseases and what have you and you've got it.*

NU: Yeah. Well, I'm a little worried. I can't sleep and I wake up every night and, but anyway. . . .it passed. And well when the first plane came, we loaded him up and . . .

BS: *This is in the dark?*

NU: In the dark. We, I think this is perhaps also something that even the pilots didn't quite realize is that many times it's a lot easier to fly in the dark than it is during the day.

BS: *They knew that. Up in the north, we'd rather fly in the winter than in the summer with the fog and ice.*

(450)

NU: Yeah. The weather is much better. We had a fancy runway lighting system that was methodically destroyed by a bear. A bear must have gone down the whole runway and he smashed every light. Every single light was broken. And then we had flare pots. So it was kind of a pain in the neck to haul in all these flare pots every time, fill them, light them and then on the way out, half of them got blown out by the wind and we had to bring them back and light them in a wind shelter. It was a little clumsy. The logistics of it was. But apparently it's a lot easier to fly in the dark. It is, of course, it is a lot colder. It was the time of the propeller airplanes. Landing meant immediately firing up the Herman Nelson(?) Hauling out the . . . keeping the engines warm. Everybody was very nervous about quickly unloading because the pilots did not like to spend any time on the ice. It was later on in AIDJEX with the triple props, it was a lot easier.

So, I don't have a good record of how many flights we had, but it was not many because the plane that did our normal supply - maybe once a month or so - that was the personal plane, the personal C-54 - what's that a DC4? - of the theater commander for the Alaska Air Command. And of course that plane was not available a lot. We were pretty much out of reach of the C-47, because I think we went up to - in the wintertime we were up there at 87 North and that was a little too far, so it was the DC4 on wheels that did much of our flying people and flying food. We had some air drops out of, I don't know what. Maybe a Globemaster, but I'm not sure. This went on, and then I got back around Christmas and New Years I went back and then Phil Church urgently needed me to go to a conference in Washington.

(500)

BS: *When did you get there?*

NU: This was after Christmas of 1957.

BS: *So it was early '58.*

NU: In February of '58, he suddenly said I have to come out and go to a conference in Washington and that was the only time . . .

BS: *Washington state?*

NU: Washington, DC. And so, and he wanted me to see the country because I had not seen anything of the States. So he put me on a train and I went to Washington DC from Seattle by train. Which was very nice. And that meeting resulted in a publication that is

now well known. This is Academy of Science publication No. 598 that has a lot of the early. You know this is all brand new stuff. There was a lot of the early stuff in there.

And so, but then I immediately went back on the ice and then the real problems started. In April of 1958, we started breaking up. First a few cracks through the camp. They were small enough so that some of them healed back because the ice was still very cold and if the crack is only a few inches wide and if nothing moves for a day or two, it heals. But some of them didn't heal. And the main problem was a big crack between the camp and the runway. The road grader was over on the other side. About 400 drums of fuel were over on the other side. And it was a real mess. It started to get into a real mess. We decided to evacuate about half of the people. To bring it down to one plane load of a C-123. That was, at the time that was the plane that we thought would be the most likely to do an evacuation.

(550)

It had the range and it had the capacity to haul 15 people or something. We kept a very basic scientific program going and we started moving the whole camp. We had scouted out before that there was another good piece of ice nearby a mile or so. We were planning to mobilize the scientific lab - that had a lot of stuff in it. The key buildings were the mess hall and the science lab. And we decided that we would . . . they were, in fact, built to be moved in one piece. They were double - the mess hall, I think I recall, was a Jamesway and a half - 24 feet.

(End of Tape 1 - Side B)

(Begin Tape 2 - Side A)

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NU: The plan in building the camp was that these Jamesways should be moveable and they were all on 8 x 8 beams. The floors were nail to 8 x 8 beams with the ends a little slanted cut so that with a Caterpillar you could haul the building. Now the problem was that the mess hall was two Jamesways and the lab was also two Jamesways and very long 8 x 8 beams. We were thinking of moving the mess hall first, because if we can't eat, we can't do any science. And then Nature kind of took care of that problem because a pressure ridge came up right behind the mess hall and started grinding it down. We tried to move it in a hurry. We didn't think about the mechanics exactly of this stuff and what happened was that it was a little windy and it was snowing and everybody was exhausted. We had this powerful D6 Cat hooked up to it and the mess hall traveled okay for a while and then one of the runners got stuck on a little hummock and the Cat started, just continued to pull and it pulled the other one. One runner was stuck and the other one was moving and it was windy. We yelled to the Cat driver, but he couldn't hear us and he moved along for another few yards and the result was that these big nails that were pounded through the floor of the Jamesway into these 8 x 8s - these nails plowed up the floor of the Jamesway and the whole thing began to crack and pop and these arches were breaking and everything spilled out. There were probably 2000 cans of food in there and all, and the kitchen equipment. And we totally wrecked this thing in only a few yards. And that was the end of that. We decided that this is a total loss and everybody went to sleep wherever they could find a protected hole to sleep in. The next day we established a kind of an emergency mess hall in a small hut and we moved the lab. The lab was also very big - was also a double Jamesway. I think they were 32' long - twice 16', and that traveled okay. We moved that to the other floe, maybe a mile away or so, and we moved the smaller huts and we established by about early May, before the melt season, we had

the other camp, more or less working. But the camp on the ice never came back to quiet. We continued to lose runway. We continued to have to bulldoze. I remember . . .

BS: *Was this the same old runway or was it a new runway.*

NU: Oh no, the runway was all gone.

BS: *So you had a new runway at the second camp.*

NU: We had a new runway and at the second camp we desperately tried to get one last flight in because we had people coming in and we had people leaving. And remember it was still the C-54 on wheels.

(50)

The snow started getting soft and all of a sudden we decided we can't land that. The snow was too soft. We can't land. We dragged the snow with a drag frame, but it was getting too soft. So the only answer was to bulldoze the snow away. They spent a day and a night and maybe another day continuously bulldozing the snow off to the side and that made such a big two ridges of snow along the runway. The load was so great from this slightly soft snow that it buckled the runway up and it broke the runway down the middle. Then they quickly worked some more and they got another runway going. and we got this one last flight in successfully. But this was the beginning of a lot of troubles. There was a huge piece rafted right outside of our washroom. That's where we had the water supply and every once and a while this thing would make "uugggh" and that twelve foot slab of ice that was resting there moved another few inches toward our wash house. We spent a lot of time fixing things, splicing cables where the floor had moved and it never got quite

back to normal. And then I left in September and I got spelled by George Cvijanovich. The change of crew was quite bad. We had to slob over some very loose recently moved ice. A lot of people fell in the water. It was not good.

BS: *You left what?*

NU: I left in early September, 1958, and George Cvijanovich took over for me. My assistant, Arne Hansen, stayed and on the 7th of November the situation had become completely disintegrated and they were all evacuated by a C-123 which I believe evacuated them to Thule.

BS: *And that was the end of Ice Station Alpha.*

NU: That was the end of Ice Station Alpha.

BS: *I have questions.*

NU: Please.

BS: *OK. You . . . I want to talk about your research and probably others.. I think your research is an important part of this too. You became, by default, Chief Scientist and a little bit about what the others were doing. One sort of pregnant question - you had these interruptions in your stay there when you had to go to Washington and off the ice. Did your research continue when you were gone with your assistants and then when the Ice Station started moving and you started moving to the other new Ice Station Alpha II or whatever, did it interrupt the research?*

NU: No. Not at all. I had two assistants. The first assistant that I had was Arne Hansen. He had a Master's degree in geology. He didn't have much experience in this kind of work we were doing, but he was a very dedicated and competent guy. Actually you know what we were doing was not all that complicated. But he was a resourceful fellow and learned the few skills that were needed, he picked up very quickly. My next assistant was Bill Campbell who at that point was a graduate student in the department working for Dick Reid. He had already done a study about ice drift, so he understood the science and he was also a handy person - a resourceful guy. So I had good assistants. And then the long time I was not out there in the fall, I was spelled by another faculty member from the department who knew some of these things a lot better than I did. So we had, except for these logistical interruptions that were actually worked out not that seriously, we had a continuing capable staff and we got 18 months worth of really good data.

(100)

BS: *Tell me about the work. What were you trying to do?*

NU: What we were trying to do is to establish the physical fate - a one year cycle with an overlap, a one year cycle in the life of a slab of sea ice measuring it's temperature, measuring it's salinity, measuring the melting, the ablation at the top - in the summertime there is also some ablation from the bottom and then in the wintertime, if the ice is in equilibrium thickness, in the wintertime the amount of ice that was lost in the summer on the surface gets replaced by the accretion at the bottom in the winter and we wanted to not only measure all of these things, but also relate them to the forcing functions from the outside - from the heat flux and from the ocean and the radiation, both short wave radiation and long wave radiation, and turbulent flux. And we did all that. And, you know, it's very similar to what they did in Sheba except in Sheba they had far more

sophisticated instrumentation. As we saw the other day, they had the cloud radar and the (light r__?). They could make measurements, remote measurements in the atmosphere that we were not able to make. They could make much better measurements in the ocean boundary layer, but . . .

BS: *This was the first.*

NU: But this was the first and this was the concept that was established at the time. We looked at the role that the melt water plays on the surface because the melt water is really important because it darkens the ice and you get this famous ice (albedo?) feedback. The more ice melts, the more water stands on the top, and the darker it gets, the more it melts. So there's a positive feedback in this. So this was the essential purpose of my study.

Hunkins and Tom English did all kinds. For instance, the bathymetry alone was new because we discovered what later became the Alpha Rise. It was named after the station. Nobody knew that it was there.

BS: *Who did that?*

NU: Well the Lamont program. They . . .

BS: *Ken. . . Ken and . . .*

NU: Ken and Vanderhoven . . . we didn't have a Fathometer of the kind that is now commonplace so we just fired a blasting cap every day and recorded the travel time of the sound with the seismometers. Now Hunkins was able to do some refraction shots for looking at the sediments on the ocean floor. Vanderhoven was the guy who first got the

bottom camera going and they took the first pictures of the ocean floor in the Arctic basin and realized that it is strewn with pebbles and rocks. And the only way that these rocks can get up there is by ice rafting. So from that and later studies of the sediment course, they determined that the ice cover must have been out there at least 300,000-400,000 years.

BS: *300,000 years*

NU So you want more science?

BS: *Yeah. I'd like to know just in general all the science that was going on. I know a lot of papers have been published and these can be keyed to those later.*

(150)

NU: Well, another thing that just came up recently again in a lecture when Ken Hunkins was a guest of the Chapman Chair in Fairbanks. When Nansen? came back from his famous drift, they had taken wire angle measurements with a drogue and they found that there is this peculiar turning of the relative motion between the ice and the water. And then they gave this data to a young student by the name of Ekman in Oslo. And Ekman looked at this and went home and solved the pertinent equation and that has become the Ekman Spiral. This is one of the fundamental things in dynamic oceanography which is extremely important because the mass transport in Ekman drift is to the right of the wind velocity which is a little bit (color?) intuitive that the water is not being blown down wind.

BS: *The Ekman Spiral was discovered on Ice Station Alpha.*

NU: No. The Ekman Spiral was discovered from the data of Nansen, but ever since that time, every textbook of physical oceanography shows you they have this picture of the Ekman Spiral. Except since Nansen, nobody has seen it until Alpha because in the open ocean, the turbulence is not isotropic. The boundary layer is not as clean. The forcing is a little violent because you've got the surface waves, you've got swell messing up the picture. The ice stresses at lower, the changes are not so rapid, the boundary layer is very nicely mixed and in the winter time it's almost completely homogeneous and that's where you see the Ekman Spiral the way it looks in a textbook. And it wasn't until Alpha that . . . and it was Hunkins who actually plotted many of those things that look beautiful just like you're supposed to - just what they're supposed to look like.

BS: *Ekman was a Norwegian in the 1950s, is that correct?*

NU: No. In the 18 . . . well, around the turn of the century. Walfried Ekman.

BS: *I know the spiral from my . . .*

NU: He was a student of Wieland Bieckness(?), for instance, you know. Ekman probably lived until 1930.

BS: *He more? _____accurately measured this .with . .*

NU: Yeah. And of course, Hunkins did not have - he wasn't using a drogue. He had current meters that measured velocity and direction of the current and he raised them up and down and he had them mounted at different levels, so, you know a lot of stuff was found for the first time.

BS: *He redefined it more accurately or he defined it more accurately?*

NU: He documented that this thing is really there the way it's supposed to - and that it obeys the laws that Ekman wrote down and obeyed these laws to some quite high degree of approximation. They also, of course they took deep casts with Nansen bottles, there were no CTDs at the time, it was all Nansen bottles. Tom English did some fundamental work about primary production in the summertime. So there is - the Air Force Cambridge, in fact, printed a volume of single papers under one cover that is probably hard to find now anymore, but there is a lot of basic stuff that came out of Alpha.

BS: *It might be at Ohio State, and if it is, I'll get it tagged to this. You know what the number is?*

NU: I don't know the number and it's edited by Cabaniss and Hunkins and me. And it's Technical Report No. something or other from the '60s.

(200)

BS: *Who's Cabaniss?*

NU: I have no idea.

BS: *Probably the publisher.*

NU: Um. . . it was Cambridge, but he's the first one that would be listed. You look for Cabaniss. So, well . . . we mentioned the irreparable break-up of Alpha in early

November of 1958 and then everybody went home. I had already gone. I left the States in October of '58 to go home. You know I was on leave from the University of Vienna and I had to get back to my job. And I did all kinds of other things. I worked in the seismology department in Vienna, but I continued, of course, working up the data. And in 1961, I published a comprehensive paper about the heat and mass balance of multi(?) ice. That was sort of the cream of what we found. There were a few other papers about special topics later on.

So then I was back in Vienna. I was teaching at the university. I was . . . my research was mainly now about the Arctic. About, you know, broadening out into a little bit into ice dynamics and Phil Church, the Chairman of the Department in Seattle kept saying well, wouldn't I like to come back for good. And in 1962, I left Austria and I joined the faculty at the University of Washington. At first they offered me a tenured associate professorship and being completely ignorant of the value of tenure, I said, "No thank you. I don't want to teach because I have so many research projects in mind. I want to spend 100% research."

BS: *But you liked teaching.*

NU: Yeah. Well, I didn't really know that at the time. So they were probably - they were very polite about it, but they were probably, they probably thought that I had a screw loose or something, that I didn't want a tenured faculty position. But I said, "No, I'm too busy, I'm sorry, I can't teach." And it was 5 years later, so I don't know what happened in between. Maybe Phil Church sort of poked me a little, but finally in 1967, by that time - No, I think it was still Phil Church was the Chair. . .

(250)

So uh, by 1967 - they said, "Okay, now you have to become a member of the regular faculty." And it was pure serendipity because in these five years I had published so much that they had to make me a full professor off the bat.

BS: *You didn't do the Assistant and all that?*

NU: No. So even though I was . . . I lost some years and I was not the same age with the other people, I sort of caught up with them because more or less inadvertently I said, "No I don't want to teach, I want to do only research."

BS: *Did you make trips to the Arctic, into the field?*

NU: Yes. I did, and in fact, I did a fairly long field trip on ARLIS II. Immediately after I came back to join the department in '62, I went to Barrow to look at the damage that the storm surge, the famous storm surge of 1961. . .

BS: *Um-hum.*

NU: Had done. It had practically washed our whole station into Ellesmere Lagoon.

BS: *What station?*

NU: You see, I was going to back up a little bit. After I left in October of 1958, the program that I had started didn't end. They hired other people and it got sort of, it got a little changed and it evolved in different directions, but nevertheless, it continued. And it was called Project Huskey. The funding shifted over from Arctic Institute to ONR. I think it was maybe even as early as 1959 with Phil Church as the Principle Investigator. This

was funded by ONR. They hired an oceanographer to do physical oceanography from ARLIS I. They hired, his name was Hal Brayton - did a very nice data report from oceanography of the ARLIS I. They had - they hired Ken Bennington who was a mineralogist who did extremely interesting work on physical properties of ice and drainage channels and salt segregation in the freezing process. My former field assistant, Arne Hensen, went . . . I believe he went to every drifting station that followed Alpha. He was on ARLIS I, he was on Charlie, he was on ARLIS II, ARLIS III, ARLIS IV and later on on the ADIJEX camps.

BS: Now you say your work went to Barrow in 1961. I'm a little confused here. '61, you're still in Vienna, though.

(300)

NU: '61 yes. I am in Vienna, but, starting in '59 already, they continue Arne Hensen, they hire Ken Bennington, they took Brayton, they branch out more into ice physics and to physical oceanography. Arne Hensen continues more my line of work from IGY, so that was out on the drifting camps. And in Barrow, they establish Project Huskie. They had up to four people, technicians mostly, doing heat balance of the tundra - radiation climatology of Barrow. There are many technical reports published. They had a station out on the tundra and they had one on the Sand Spit with an arm with a radiometer swinging out over the lagoon to measure radiation over the water surface. And that was the one that I was referring to that got washed into the lagoon by the storm surge of 1961. It was a freak storm and also a freak open water fetch. It was just a very unfortunate coincidence that there was about 100 miles of open water and a very small, very strong low exactly on-shore winds that caused a lot of damage. And it flooded, it spilled over the spit, it cut a breach in the spit even. I think it did damage to these earth cliffs in the

Barrow village and various other problems. The lab - there was a foot of water in the buildings.

BS: *In the old Jamesway ones.*

NU: In the Jamesway ones, yes. And I think they had the - the animals had to be let loose or something. All the wolves that Max Brewer had up there. So . . . but anyway, I came back in December of 1962 to stay and well, I sort of jumped ahead a little bit here. So in '67 I joined the regular faculty. This is what I want to say.

BS: *In '62 you came back. In '67 you're a full professor when ARLIS II came about.*

NU: Yeah. ARLIS II. Before that, I really don't remember the year. I spent a total of several months on ARLIS II. And they did some other things. I did some blowing snow measurements. I did my dry coefficient roughness measurements from ARLIS II. ARLIS II was quite different. As you know, ARLIS II was an ice island from an area similar to the one where T3 came from, but it was not the same because ARLIS II was not as flat as T3. It had more topography. It had large amounts of moraine material on top. There was gravel and sand and rocks.

(350)

BS: *Which ice shelf did it come from? Did they figure it out?*

NU: Well. No. But I think it's also Ward Hunt. But I think it may be off to the side. More off to the side. And ARLIS II lasted a long time. Unlike - well we could look it up when it was first occupied. But T3 made at least one loop around in the Beaufort Sea gyra(?)

ARLIS II never made the loop. ARLIS II came down the islands - the Canadian Archipelago Islands closer to shore than T3. It came closer to Barrow and swung out to the west and got in one turn into the transpolar drift stream. The longest time I spent on ARLIS II was when it was in the northern Greenland Sea which was quite an experience because the weather conditions over there are totally different from the central Arctic. It's not as cold and it's much windier and much more vigorous. We had the whole camp at ARLIS II was totally covered by snow at the roof level. You had to get in and out of the buildings through vertical shafts, almost like in the Antarctic. And sometimes, in poor light, the wind was really strong and we had ropes and flags marking all the frequently visited spots because we were afraid that people might get lost because the blowing snow was ferocious there sometimes. I have a - I have a one case of one of these polar lows that went right over us with extreme drop of atmospheric pressure. I think we had a 40 millibar drop in an hour and a half, or something like that.

So, then Max Brewer insisted to keep supplying ARLIS II from Barrow which meant flying with the R4, flying to T3 which was still in the Beaufort Sea at the time. . Fueling up at T3, they flying over to Alert, fueling up in Alert and then flying from Alert over to the Northern Greenland Sea for a total of something like 16 hours of pure flying time when the ARLIS II was, in fact, only 2 or 3 hours from Keflavik.

BS: *Didn't go into Thule at all?*

NU: They went to Alert.

BS: *Alert then? OK.*

(400)

NU: Alert and over Greenland, over into the western Greenland Sea. I don't know why they didn't go to Thule, but apparently it was easier to use Alert. And, you know, we were rapidly approaching Iceland and they could have stationed the plane in Keflavik easily. But there was some reason, I don't know what. There was . . . well, Bo Boxgroup(?) was up there and ARLIS II and also Kusunoki from the . . .

BS: *Japanese.*

NU: Japanese. He did all the physical oceanography that was done from ARLIS II. There was a really interesting project done by fellows from USGS working for Art Lackenbrook. They stuck these heat flux probes into the sediment in the Greenland Sea and measured the temperature gradient. Got some very interesting results. Maybe remnants or manifestations of cold water outbreaks because the temperature profiles in the ocean sediment showed some peculiar wiggles that Art Lackenbrook interprets as gushes of colder water coming out of the Arctic Basin associated with some climate changes. So it was a stable platform. It never broke or anything. But on the other hand, it was not so easy to work from because it was so thick you couldn't work through it, so all the oceanography had to be done from off the side which meant having huts out there at the edge with the main camp a kilometer away which means a lot of traveling on foot. So it had both advantages and disadvantages. But it was an important station and a lot of work got done. I don't know about ARLIS III and ARLIS IV, but I think they went up to IV. I was not on them and I'm sure Arne Hansen was because he was on all of them.

BS: *You've mentioned the science. You got on it in - I don't have the dates.*

NU: Well, because I don't know them. This is something I have to . . .

BS: *OK.*

NU: And I know because I wrote a couple of papers and my work that will show when I got on, when I made my measurements.

BS: *Were you evacuated by ice breakers?*

NU: Not me. I left before that. But they were. Yes. They were evacuated by ice breaker. And they sort of salvaged whatever. I don't know which ice breaker it was. But I'm sure Lawson Brigham(?) knows their exact. . .

BS: *Well I. . . . I've got it too because I've got the Arctic Drifting Stations book. I just wondered if you . . .*

NU: I know nothing about ARLIS III and ARLIS IV.

(450)

BS: *I was ARLIS V.*

NU: Ditto. ARLIS V.

BS: *ARLIS VI and VII followed. I didn't do much with those. ARLIS V was a great station. I could have stayed on it but . . .*

NU: What was it, was it an ice island, ice floe?

BS: *Ice floe. It was twenty feet thick. I could see the different - two year - year by year things. On ARLIS II, I've got to back up to ARLIS I because . . . not ARLIS I. Ice Station Alpha because there was an incident out there that you were involved with with the submarine and the Skipper and could you just enlighten us on that?*

NU: Yeah. They were in the second summer - in the summer before the summer of '58, there were two people from some Navy lab that didn't mean anything to - maybe they were NCEL - who came up and they were sort of a little cagey. They kept to themselves. They set up a little hut. I don't know. They told us some story about what they are measuring. They said that, I believe they said that they have to be away from the generator because of radio interference and they are making some ionospheric measurements which was, of course, not true. They were doing acoustics and they were - I think they were recording ambient noise and they were listening for, I bet they were listening for possible Russian submarines that got wind of maybe something. I don't know. But this was all very classified, obviously. And they did not want visitors out in their hut. They had a little Jamesway or maybe it was one of those plywood huts. Maybe 500 yards away from everybody else. And then in August, there was a noticeable frequency with which Major Joe Belota went over to the radio shack and he always looked extremely concerned and worried and very, very busy. And I think he didn't get much sleep and as we learned later, the reason was that he had to talk to the submarine on the radio because they had come up through Bering Straits and they had surfaced and every time they surfaced, they called us on the radio.

(500)

BS: *But you didn't know that at the time.*

NU: I did not know that, no. And then finally, and I believe it was on the 17th of August.

BS: '57?

NU: '58. No, sorry. '57. It was the 8th or the 18th, in the middle of the night, I had to get up in the middle of the night. I look out and of course, it was broad daylight and I see Joe Belota like a maniac running up and down this wide lead next to our camp with the outboard motor and I thought, you know Joe Belota was a little different. I mean, he marched to a slightly different drummer. I can tell you some other incidents. He finally was called back. He was a very nice guy and extremely hard working, but he was a little flaky. So I thought that he's just doing another little flaky job here with the motorboat. But then I started thinking about it and it was pretty clear that he's sending some kind of an acoustic signal and it turned out that the outboard motor provided the signal for the submarine to home in the last few miles to the camp and then I think he also outlined the big wake. There was a lead that was at least several hundred yards wide and maybe a mile and a half long. It was very nice. And behold, in the morning, in this light fog, there was the *Skate*, surfaced. And a lot of excitement and some people came ashore and I remember that Jim Calvert who, at that time, was a probably a lieutenant.

BS: *Commander.*

NU: Commander. Commander Calvert and his Executive Officer and a third officer came and made an extremely polite, courteous visit to my lab. And formally apologized that he's not allowed - that he would really like to invite me on board the ship, but because of security restrictions, he was not allowed to do this.

BS: *Well you're a foreigner and . . .*

(550)

NU: And I was still a foreigner, yeah. Still a foreigner. And he was very interested in this, and I guess he wrote some complimentary stuff about me in his book. And well, the next thing that happened was that they drank all our beer and all our booze because the ship, of course, was dry and I think maybe the visit to the camp was considered like shore leave.

BS: *So they rotated everybody through the camp?*

NU: They rotated everybody until our camp was completely depleted. But I think the galley gave our galley some nice supplies and stuff and they baked a beautiful cake. I have a picture of the cake. By the way, Brian. I have a movie that Franz Vanderhoven and I made of Alpha that never got - it's not professional quality, and it has no sound track. But it has some awfully nice footage and some dramatic footage of us moving this - the 32 ft. lab building, and stuff, with the D6 Cat. I would be happy, you know - Andy Hyberg - we could arrange to have just a video copy made of this.

BS: *If you'd do that, it would be great. I'd file it with this.*

NU: File it with this.

BS: *Take it down and have somebody do it. It'll cost you \$20 bucks and they do it better than trying to do it video.*

NU: Oh no. I would never do that. They do . . . it's from a 16 mm film.

BS: *You've already made it into a video.*

NU: I already made it into a video. Now, I don't know how good the quality was.

(End of Tape 2 - Side A)

(Begin Tape 2 - Side B)

(000)

NU: Well, and then I believe, as I recall, they were there maybe less than a day and the ice began to move and this wide wake started closing in a little bit. And all of a sudden all of the guys were back on the submarine. And they departed. It was impressive. It was an impressive thing. I guess then they went up to the North Pole and went out into the Atlantic.

BS: *Yes. They didn't surface that time at the North Pole. It went back.*

NU: Straight shot out?

BS: *But they went to the North Pole, but went under it and they came back and surfaced the next time. They had two trips.*

NU: In that year?

BS: *Um-hum. No. Next spring, they went and surfaced at the North Pole. He went home, he got Sir Hubert Wilkins to visit, Wilkins died, he gets his ashes, takes them to the North Pole. That wasn't the purpose. It was all coincidental.*

NU: Well, you know, maybe I should insert a little story with McMillan and Lowell Thomas and Wilkins at Alpha.

BS: *Yeah.*

NU: We can do this on the second round.

BS: *Yeah. Let's do it.*

This is the second round of interviews with Dr. Norbert Untersteiner. The date is the 14th of November 2000. The interview is being conducted by telephone with Dr. Untersteiner from his home in Seattle by the interviewer, myself - Brian Shoemaker - at my home in North Bend, Oregon. Dr. Untersteiner, we left off with you describing visits by Sir Hubert Wilkins and others to Ice Station Alpha in 1958 and we'll take it from there.

NU: This was in the late summer of 1957. We had a visit from some very interesting and distinguished people. It was a small group led by Lowell Thomas, Sr. of Lawrence of Arabia fame and I believe his son was there also along with him, just to help his father then. In this group was McMillan, the famous Captain of one of Perry's, Admiral Perry's ships and North Pole expedition and then there was Sir Hubert Wilkins and also in the party had been Peter Froekin, you know, famous publicizer of all things Arctic except

that Peter Froekin - they were all kind of old guys. They were probably some of them as old as I am now. Peter Froekin, I'm told, by Max Brewer, insisted at the Anchorage Airport to carry someone else's suitcase - I believe it was Lowell Thomas' suitcase, up some steps. And Peter Froekin took the suitcase and carried it up the steps and at the top of the stairs, collapsed and died. And so they were short one person and it was - Oh, there was also Bert Balkin(?) - famous Norwegian pilot.

(50)

He was a Norwegian who fled when Hitler occupied Norway and then became one of the people to pioneer the air supply route flying over Greenland, supplying the Russian army with trucks and other supplies from the Western Allies - from America, mostly. Well, anyway, Bernt Balchen marched on up and down our little runway there that was - we uh, we never really didn't know much about aircraft operations on sea ice and he was marching up and down this runway and was declaring that he could land a locomotive on this plane, because he knew a lot more than we did about the bearing capacity of floating plates of ice. Anyway, it turned out that Lowell Thomas of course with his incredible memory for places and people, knew several people in Salzburg, Austria, and had, in fact, skied with one person that I knew very well in the Swiss Alps. And so this was a very animated and quite, I guess, friendly and interesting evening that we spent together. Later on, the other guys went to bed and Sir Hubert Wilkins sat in our lab and we had a bottle of scotch and of course, for me, this was a unique opportunity. I had known about Wilkins and his attempt, first of all his flight with Eilsen from Barrow to Spitsbergen via the Central Arctic . . . I guess I had read about Wilkins when I was a teenager - the flight from Barrow to Spitsbergen and then also his attempt to use a submarine - the first submarine named Nautilus - I believe it was in 1929 or 1930, to do some oceanography over in the Greenland Sea. And the oceanographer on that vessel was none other than

Harold _____? Now it turned out that the World War I submarine, of course diesel powered submarine, had some mechanical problems and after a few attempts turned out to be incapable of diving under the ice and Sir Hubert was telling me that this was, in fact, caused by one of the crew members on the submarine was incredibly scared of the dive and sabotaged the depth rudder. So anyway, this took a whole bottle of scotch to tell the whole story.

BS: That's interesting because he was pretty quiet about it. The accusations of sabotage came from others in the crew in the stories that came to the press. He was a gentleman about it.

NU: He was quite unequivocal about it. He said that this was somebody he might even have said what the function of that person was on the ship, but I forgot that. So, he clearly didn't want this publicized or anything and it was all, you know . . . it was all decades earlier, so he probably felt that it was okay to say this. Well and so they stuck around for about a day and a half and then the whole group with Lowell Thomas and Balchen and McMillan and Sir Hubert left. It was quite an event.

(100)

So a few years after I had come back to the University of Washington to the Department of Atmospheric Science, about 1965 we started working to establish a new interdisciplinary program in certain areas of geophysics. We had in mind to do cur_____ternery? research and snow and ice research and upper atmosphere research, and air sea ice direction and various special branches and this was a new academic degree granting graduates only program and it started operating in 1967 and as part of that program, we established for the first time a set of graduate level courses in snow and ice

research. And it became the beginning of long-term and systematic _____ to do Arctic work.

BS: What did you call the degree? Was there a specific degree or . . . ?

NU: Geophysics. It was called geophysics. It was jointly designed by faculty members in atmospheric science, in geophysics, in geology, in oceanography, in electrical engineering and even in chemistry because in those days, environmental chemistry was a specialty that was sort of coming on line and people came to realize how much chemistry, important chemistry is going on in the environment. Anyway, as part of that program, we established these graduate courses. It also happened that fortunately the National Science Foundation funded Centers of Excellence - under the title Centers of Excellence, made large grants to institutions and in an internal competition at the University of Washington, we won a place in that program and then subsequently the university as a whole won I believe it was a seven or eight million dollar grant to establish four specialties and one of them was snow and ice research and the other that is also related here is certernery(?) research. It was very timely and fortunately we managed to hire Professor Charles Raymond from Cal Tech who was a rising, already a rising star at that time and we set up these programs. And the other good thing that happened was that this program attracted the first batch of graduate students who all turned out to be extremely successful. I can only say Allen Thorndike and Gary Baker and Sam Kolbeck who sat in the first graduate course that I ever taught at the University of Washington, then a you will know what - how far these guys went in the meantime. Leading research in Arctic _____?

Well, so . . . this went along nicely. And then more or less out of the blue, I get a phone call from Walt Whitman. Walt Whitman was a person who told me many years later that he was actually by profession a haberdasher. But due to the vagaries of World

War II, he was drafted and got involved somehow in Naval Oceanographic Office activities and he became interested and expert in sea ice and a few years after the end of World War II, he found himself pretty much as the national expert on sea ice research and monitoring base at the Naval Oceanographic Office.

(150)

This guy told me and said, "Look, you have been on our case over the years, saying that one station observations are intrinsically not adequate because all the fields we are interested in are two and three dimensional fields and if you just float around in that medium making measurements at one point, you're never really going to learn the essence of things. So why don't you - I have a little money left over. Why don't you guys do something - work out a plan how you would go about a multi-station approach. So I called up my old friend, Ken Hunkins who was at the Lamont _____? Geological Observatory, Columbia University, and said, "Ken, we have a few thousand dollars to spend on writing some kind of a plan. Why don't you come to Seattle and we sit down and write something?" We had advice and help, of course, from a number of people, and in a few months, we turned out a thin little report that explains how wonderful it would be to have two-dimensional observations. To have triangle stations with the station at each corner to make continuous measurements of the deformation of the ice and also for us to measure the forces that cause that deformation - the wind stress and the water stress and the other stresses that are involved here. And it was - this was the beginning of AIDJEX, the Arctic Ice Dynamics Joint Experiment. We probably did this in, I would guess, 1979. I can look this up.

BS: '69.

NU: 1969, Hunkins and I published in some informal cover, I believe it was Arctic Institute of North America, the first sort of embryonic plan for AIDJEX. It so happened that Joe Fletcher, at that time, was at the Rand Corporation in Santa Monica and he was looking to move away from Southern California and at the Founding Father's Workshop which I believe was held at _____ of _____ North America _____ Washington headquarters office we more or less crowned Joe Fletcher to be the person to spearhead AIDJEX. We managed to get him an appointment at the University of Washington. Joe Fletcher came to Seattle in 1971 with his family and we got busy writing plans, working _____ plans and writing _____ launching the whole idea. It turned out, though, that as soon as Joe Fletcher was out of the Rand Corporation and as soon as this Arctic Initiative became known to the people in the National Science Foundation, they got the idea that maybe they need Joe Fletcher in Washington and it was Admiral Bowen(?) who was the head of the Earth Science Division at the time at NSF, called Joe and convinced him that he has to come to Washington to head the Office of Polar Programs. Well, in a way, this was unfortunate in that Joe never really got to sink his feet into establishing the program.

(200)

On the other hand, of course, it was of considerable advantage to have him in the role of Director for the Programs that would be the first office that would have to approve this kind of enterprise. So if anybody wants to see conflict of interest in this, they should feel free to do so. In any case, . . .

BS: Incidentally, I heard that from other program managers who worked for Joe - that he redirected money.

NU: Yeah. Well, but on the other hand, of course. . .

BS: *There's a bit of jealousy involved there.*

NU: On the other hand, the enterprise was large enough and expensive enough that it required the approval of the National Science Board so this was, in fact, even out of the hands of the Director of NSF and here is where we lucked out again because the National Research Council which is the operating arm of the National Academy of Science, sent up a joint committee or panel, I believe it was at the time, to advise the planning of AIDJEX and also supervise this. And this panel was chaired by Richard Goodie of Harvard University. And it turned out that Richard was not only interested in this project, but that he also thought we were doing a decent job and we were all for real and we were motivated and we had a good chance to actually bring this off. So he really put his shoulder to the wheel and it was, in the end, it was crucial to have him make our case to the National Science Board, which he did. NSF set up a special advisory - an agency advisory committee which included Suki Manave and Brooks Brian and Ned Ostenso and Bill Swenbeck - an Australian boundary expert to advise us scientifically, so this was all very forward looking and really went extremely well. So, in 1971, there was a small pilot study that was also connected to other activity that wasn't really that important. The main part of the study we had in 1973. We hired for the first time, actually, three manned stations. There was a main camp and two really small ones. But there were people there, they lived there. There was a springtime only experiment - we set it up early March.

BS: *What year was this?*

NU: '73. And it was to try out certain things to try our oceanographic gear, to try atmospheric science gear, to do captive balloon measurements in the boundary layer and

also to try automatic data buoys who were just beginning to be developed at those times. They were called IRLS buoys and I forget what the acronym was. They had to be hand deployed. They were spar buoys. They were essentially a long tube maybe 8 inches in diameter. They were stuck in the ice.

(250)

The batteries were down in the water in a relatively warm environment and the transmitter, the satellite transmitter was up on top of it - this tube was about two meters high sticking out of the ice. And they had pressure sensors and temperature sensors. And they worked reasonably well. Their location _____ navigation was by means of the Argos System based in Toulouse, France, and we got deformation measurements and air stress measurements. We had anemometers at these locations and it was a try out period for many people. There were I don't know how many, but there were at least two dozen principle investigators - potential participants in the main experiment. This '73 one was billed as the main pilot study and it _____ for the main experiment that belongs to 1975, we had to scale back and we had to, so to speak, dis-invite some of the principle investigators.

Well, it turned out to be, in a few cases a little . . . but eventually, it all worked out in the end. And in 1975, our steering committee had a compact program. Dick Waters and the project director _____ I had _____ extremely competent administrator _____ President _____ during the difficult times of the campus unrest in '69 and '70. She was a very decent and circumspect person who turned out to be invaluable for the project. We also retained the services of two people who were laid off from Boeing during the bad times of the aerospace industry - Ross Burent and Andy Highburg and. . . well Andy Highburg is still there and doing logistics. Ross went out to the field. Andy Highburg was what we called then base manager of operations.

BS: *And that's how his career in the Arctic got started?*

NU: That's how his career in the Arctic got started.

BS: *He was laid off from Boeing, what, as an engineer?*

NU: He has a degree in engineering from _____ University in Switzerland and he went - he and Monica emigrated _____ he was _____ probably in the early '60s when they were hiring people left and right, and he was - Andy was, in fact, involved in designing some kind of a gravel deflecting device for the 737. The 737 has the low hanging engines.

BS: *Yes.*

NU: I think in some areas they had a problem with sucking in gravel.

BS: *Gravel, dust. Dust will eventually chew your engine to pieces.*

NU: Yep. And there was something that they designed and Andy was involved in that. And then when the big downturn came, of course, he didn't have much seniority, so he was laid off and we picked him up. In fact, I think it was Joe Fletcher who found both Ross and Andy. And they were in from the very start and Andy still is.

(300)

So anyway, in '75, we launched the big, the main, what we called the main experiment and it had been accepted that we would need a little redundancy in the station design, so we had a triangle of satellite camps with a main camp in the middle. The satellite camps were essentially two buildings and three people and in the main camp, we had, depending on what season it was, between 20 and 50 people.

BS: *How far were the satellite camps from the main camp?*

NU: They were about 100 kilometers from the main camp. Within easy helicopter distance. And so, by that time the observing program was all carefully studied and re-studied and designed and we had a strict observing program that was carried out at all four camps. There was relatively extra stuff. Now looking back . . .

BS: *How many unmanned camps? Automated stations?*

NU: They were on a ring of about 300 kilometers and I believe we had eight. It was not easy because we had to find, we had four homing beacons that could be turned on _____ Otter as it was flying toward the buoy and I believe in a few cases, we lost them because the beacons wouldn't turn on or the beacon in the twin Otter wasn't functioning right. It was all early technology. This buoy program was hanging on by . . . barely by . . . so to speak and it was really not until after AIDJEX that that buoy technology became as - approached the level of reliability that we have now. We were, I think, even during the main experiment, we were pushing the limits of the technology that was available at the time. But, in any case, the observing program was really focused on this question of the constitutive law, the rheology, this relationship of the strain, rate, and stress in the ice. And we were so preoccupied with that question that looking back

now, I have to honestly say we missed valuable opportunities to observe other things that we could have observed with very little additional effort and very little additional money.

(350)

BS: *Like what?*

NU: Like, for instance, what is going on at the underside of the ice. What . . . are pressurage keels melting preferentially or not and at what a rate and how, did it depend on the season? Which is something that wasn't observed systematically until a couple of years ago in SHEBA and even they did not do as much as they could have done. And we would have really liked_____There was a very sketchy program - solar radiation measurements that we could have done a lot more with a little effort. But everybody was convinced that the next big step in _____was going to come from a knowledge of this reology that we simply, kind of, brushed the other stuff aside.

Well, looking back at AIDJEX, there was one experience that really stands out and that is in a nutshell, that we went out, focused on one particular scientific problem and as it turned out, we didn't do so well with that one, but we did very well with another one that was, in fact, given much less of a chance initially. I remember very clearly a meeting we had in Washington of our Special Advisory Committee consisting of Suki Manave and Kurt Bryan, Ned Ostenso, and Bill Swinback, told us that the idea of dividing surface stress by the wind observations, by pressure observations, in fact, probably wasn't going to work. They told us now at this late stage we're not going to try to rain on your parade, but don't hold your breath on that one. That is probably not going to work. But as it turned out, this worked like a charm. We had a reliable pressure - a grid pressure measurement on a grid of about 200-400 kilometers. From that, we derived a geotrophic wind field that is a frictionless wind field. Then, with the help of two rather

simple models with different constants for winter time and summer time, we got down to a surface stress field that explained about 90% of all the variance.

(400)

And that - well, we are doing that to this day and many modelers have done it to this day and this, I believe, was probably the central success of AIDJEX. And, of course, it did another thing too, and that is it really stimulated broadly based research in the Arctic. We have to remember that, especially this early part of AIDJEX, Arctic research was considered the domain of a few people, mostly driven by some sense of adventure or of being out there and then it was not - Arctic research was not really in the mainstream of serious earth science. Many of my older colleagues will remember that this was sometimes a struggle and it was that big climate conference organized by the Global Atmospheric Research Program in Stockholm in 1971 before the first time people began to really pay attention to the role of sea ice in ocean circulation and things like that and the role of the large ice sheets along the trans_____.

BS: Can you tell me the name of that conference again, Norbert, in Stockholm?

NU: It was called The Physical Basis of Climate and Climate Modeling and it was held in a small resort outside of Stockholm, Sweden, and it was chaired by Bert Boleen, who then became the international coordinator of the international biosphere IGBP - International Geosphere Biosphere Program and John Kutzba of the University of Wisconsin and they had - we turned out a volume that was, you know, probably a bit of a milestone. All of the heavy hitters were there. Joe Smagerinski and Jule Charney and Bert Boleen and Kirk Bryan and Phil Thompson and Francis Breverton and it was, for me, I was kind of an outsider a little bit being in charge of the snow and ice part of the

section. It was an eye-opener to live at close quarters for almost two weeks with all of these guys. I learned a tremendous lot during that time. We were practically locked up. It was extremely inconvenient to get - to travel to Stockholm, so everybody was putting in 10, 12, 14 hour working days.

(450)

So anyway, that is published as a GARP - Global Atmospheric Research Program - GARP publication, Series No. 16 and I believe the year of publication is 1971. Well, so back, to AIDJEX - the main experiment started in 1975, planned for a little more than a year. We deployed in March of '75 and we went home in May of '76. Now, maybe I said this before, but AIDJEX was jointly funded by the National Science Foundation and by ONR. There were other agencies that contributed - NASA for instance, made a series of flights with their specially equipped aircraft - remote sensing aircraft. The Canadian Polar Continental Shelf Project cooperated with scientific teams that made certain specialized measurements but they also gave to us, if I remember correctly, \$50,000 worth of twin Otter flying time. The main bulk of the money, though, came from the National Science Foundation.

BS: And this was a change in Arctic Funding, wasn't it?

NU: Absolutely, it was a, if I remember correctly, the authorization was for eleven and a half million dollars - nobody had ever gotten that much money in one package. We probably used a similar money in kind from ONR. The deal was that for the duration of AIDJEX, we had the sole use of the Naval Arctic Research Laboratory in Barrow. Brian Shoemaker was Commanding Officer at the time, and even though AIDJEX as a project put very large burden practically monopolized all of the resources of NARL, we had

excellent support and enjoyed the full sympathy and support of everyone at NARL. So this was really very excellent. And it was so many many people were not crazy about the tight organization of AIDJEX.

(500)

That kind of a field project simply required a fairly hierarchical structure. During the pilot studies, we had far more principle investigators, far more diversified work was going on, but once the tests and the pilot studies were done, we focused in on the main plan as it was agreed. Of course the plan was agreed in a very collective fashion with inputs from many people, but once that plan was set, we had to proceed in a very orderly and structured way. I'm pretty sure one of the reasons why we did it that way, even though this was for an academic university based organization _____unusual - the reason why we did it this way was because the person who was ultimately responsible for the _____was a retired Admiral - Admiral Tom Owens was the head of the first science section in the Navy. I remember Owens coming to Washington. We gave our song and dance. This was early on - about 1972 - and the first question he asked when we explained all the things we had in mind and we were going to do and the people we were going to hire and how this would all be structured. The first question he asked me was, "And when are you going to shut this whole thing down?" So he wanted to see a beginning a middle and an end. Like in a military operation. When do you win, and then go home and call it quits?

BS: I think he was thinking of funding at the time. That's why he had to ask those questions.

NU: And should. And surely they did not want to institutionalize projects. They wanted them to go through the planning and wrapping up and do it, and then quit and go on to something else. We monopolized not only the use of NARL, we also monopolized the budget of the Office of Polar Programs in NSF and there were other people waiting in the wings. I have to say in retrospect, I'm sure that this whole being the director of all of this did not make me only friends - I am sure of that and I'm sure there were a lot of people around at the time that were a little miffed that we got all that money and all that attention and all that support for a significant number of years. This is 6, 7 years almost.

(550)

BS: *The Navy - ONR was happy because for the first time we had something significant going on. We really never had anything before that.*

NU: Nothing of that kind since IGY.

BS: *No.*

NU: It was essentially a 20 year time span between really major coordinate efforts with not only different institutions, but also different countries participating because at AIDJEX especially in the '73 pilot study, we had Japanese people on there, we had a whole bunch of Canadians. But anyway, the other thing that Tom Owen did to us - he handed me this thick book and said, "This is the Navy Operations Manual. Probably 97-1/2% of what's written in there will not apply to you. But I want you to go through this anyway, because if you pay attention to all the things that are in there, then you will not forget anything."

And he also forced us to make a perk chart of the whole project. At the time I didn't even know what a perk chart was so we went to the business school and since we had, we were kind of _____? - we had funny money lying around, so we were able to just hire somebody - a very clever guy from the business school actually, who had no idea about science, but we sat with him for a few hours and he came back for maybe a couple of weeks - every day for a couple of hours and he asked around and he got a flavor of this project and he made us this enormous perk chart that - well, frankly I never used it because I couldn't read the damn thing - but Tom Owen was very happy when we sent that to Washington.

BS: *He'd understand that. What was the name of the Business School?*

NU: It was UW Business School.

(600)

BS: *Oh, I see. OK.*

NU: Our local Business School. But, you know, these people who are in management. They are very clever. They learn about fundamental things of systems management that have a much broader application than just business. They can apply this to manufacturing and probably to military operations and to research operations. I have to say I was very impressed with how fast this person

(End of Tape 2 - Side B)

(Begin Tape 3 - Side A)

(000)

BS: It's now the 16th of November 2000, I have Dr. Untersteiner on the telephone. I'm Brian Shoemaker, the interviewer, and Dr. Untersteiner is the interviewee. I'm in North Bend, Oregon, and Dr. Untersteiner is in Seattle, Washington, at his home and we're continuing on. This is Tape 3 of a series of tapes of an oral interview with Dr. Untersteiner.

NU: So, anyway, the field part of AIDJEX was deployed, it went ahead, we broke - the main camp broke up. It is always the main camp that breaks up because of any crack traveling through the ice - the largest camp is the most likely site, so it was the main camp that broke up in October, but with the help of everybody - NARL and everyone in our contract helicopter and the contract between _____, we were able to relocate the main camp to one of the three satellite camps and turn that one into the main camp and complete the project. I think we were extremely lucky in every respect. We had no logistical mishaps of any major nature. We had no serious accidents. A couple of people got hurt a little bit. But no loss of life. No serious, really serious injuries. No serious illnesses. No gun accidents or anything of that kind. So I think we have to consider ourselves extremely fortunate that things went that way. And then, by the time we were ready to shut down, I think Brian Shoemaker had been relieved by Dick Schauss, and I don't know where Brian . . . Brian where did you go after that?

BS: I was a squadron commander in San Diego.

NU: Brian went back to his Navy duty and Commander Schauss, Richard Schauss came up to be the CO of NARL and it was with him, then, that we took down the flag and called it complete. We did that in May of 1976. Subsequently, of course, there was _____ of three papers written and a symposium was held and a hardcover book was published - edited by Robert Pritchard. We had set up a data bank for all the data. There was, I believe for the first time in an Arctic project, there was a very structured policy written about who gets the data and what are the obligations of the PI to share the data. We had categories that . . . of data that enjoyed no protection at all, for instance - basic weather data and water depth and navigation data, where are the camps and where are the data buoys - that was considered in the public domain, a priori, and then there was a second category of data that enjoyed a one to two year protection. These were data that were based on specific developments and ideas by the PIs and this protection would give the PIs a chance to take the cream off the data themselves before they have to share them with other people. And then there was a third category of data - it was so highly specialized that they wouldn't be of any use to any other person anyway, so they reverted to the total control of the person who took the data. This is essentially a policy that worked very well. What did not work very well is that we did not have enough money to really see through the data archiving phase of this project and there are various stories that could be told about how it took several years, in some instances, for the data to get analyzed and to get placed in the National Data Archives simply because people didn't have enough money and NSF, and the other funding agencies were so eager to bring this thing to a close.

(50)

I think that was a lesson that was remembered in the SHEBA project that we'll discuss later. They have an explicitly defined data evaluation phase, a period, and there is money for people to do that. I think this is where AIDJEX fell short.

But I'm back to my personal history. So AIDJEX was completed and the next thing was that, of course, we had . . .

BS: While we're right here, I want to just back this up.

NU: We had several scientists working at the AIDJEX office. This AIDJEX office was a place that we had rented off campus. It was clear from the beginning that the project could not be housed inside an academic department because it was simply too large and so we went off campus and we rented some space and we had the AIDJEX administrative office and the data manager and the series of PIs of people who had done part of the projects - the modeling group Max Coon, and Bob Thorndike and _____, and Bob Pritchard and Bob Brown for boundary layers and Mark Allbright and Frank Carsea - many people and so with AIDJEX completed, and everybody kind of geared up and excited about projects, they all started writing proposals for continuing their work. This really was a shock to many people, because these proposals - most of these proposals, in fact, failed probably - oh I'm not trying to fault the review process, but it could have been that _____ that most of these proposals would fail because of the very decision or the very policy that we were aware of at the beginning was that they don't want to institutionalize this project. They wanted to say, "OK, you got your turn and now it's somebody else's turn." So most of these proposals were turned down not so much because of their deficient scientific merit, but simply because NSF didn't want to continue this on.

BS: Was Joe Fletcher gone from NSF then? '76?

NU: Yes. I'm sure he was probably gone. Yeah. He was gone by then.

BS: *Rutford was running things then.*

NU: Yeah. You're right. It was Bob Rutford and of course, Bob Rutford liked AIDJEX very much, but I mean he was also under some obligation from the overall policies established in NSF - he just couldn't do it. So what happened was that several people went back to the academic departments. Others like Max Coon and Bob Pritchard, in fact, started a company. This was a time when Northern oil development was still very much a possibility, including off-shore drilling platforms or navigation in ice covered waters with special tankers or even not special tankers. Anyway, they saw a lot of opportunities for engineering studies and Max Coon and Bob Pritchard started a company, and it was a period of, not to call it disintegration, but a severe scaling down of AIDJEX.

(100)

Institutionally at that time, there was also something going on at the University of Washington on our campus. AIDJEX first had been housed in an entity called Division of Marine Resources and the Division of Marine Resources had mainly the Sea Grant program the Applied Physics Laboratory and a few smaller entities and the university decided to disestablish this division of Marine Resources and do something else instead which was to make the Sea Grant office an independent separate entity and . . .

The majority of people left the AIDJEX office - there was no more AIDJEX - but a few, for instance, Allen Thorndike and Drew Rothrock, continued to get NSF funding. Now my own moves at that time were that the Admiral Geiger who was at the time Chief of Naval Research, came to Seattle to be briefed about how AIDJEX went. He was in his

first - but I think at that time it was after his first year as CNR in Washington, and after we had briefed him he took me aside and he said, "You know what. You are probably a little burned out of this high adrenaline kind of activity and we need some advice about Arctic research back in , at ONR - why don't you come to Washington and help us out designing a five-year plan?" So that sounded just right at the time. I was, in fact, ready to do something else for a change, so I got the approval from the university of a leave of absence and went to Washington as, under the Intergovernmental Personnel Act which provides for the exchange of personnel between the federal government and academic institutions and also business and the deal was that they the ONR would simply continue to pay my salary. I would be working at ONR initially for one year. Well, by the time this was all done and could be put into affect, Admiral Geiger was gone. It turned out that Admiral Geiger expected to get a second tour of duty as Chief of Naval Research which would have been quite uncommon at the time. As far as I know, all CNRs served for only two years before they retired. He, in fact, did retire, but by the time I actually went to Washington, the CNR was Admiral Bachokle who was a completely different type person. Geiger was, I believe, an expert in satellite techniques and highly classified matters of targeting and missile warfare and Admiral Bachokle was a former submarine commander. And Bachokle had absolutely no use for me in Washington.

BS: *He liked Waldo Ryan.*

(150)

NU: Yeah. So I made an attempt, as it turned out, about 20 years too early, I made an attempt to convince Bachokle that maybe now and then they could run one of these submarines in an unclassified scientific mode or not even unclassified, but just in a research mode and I remember how flabbergasted Bachokle was and he said to me

something like, "Are you totally out of your mind? Do you have any idea what it costs to operate one of those submarines? And don't make an idiot of yourself, in other words."

BS: *We had the Genard up there during AIDJEX. Did they not contribute?*

NU: That's right, but I remember what incredible machinations it required to do that and also how, in fact, Waldo Ryan managed to, how should I say?, extract \$300,000 out of NSF under some pretext of having to make some modifications, which of course - I think it was just a fictitious backschish that the Arctic submarine lab wanted out of NSF for doing this.

BS: *Did they provide any data for the final report?*

NU: Absolutely. They did, we had and they lived up to their promises. They came in April 1976 and they took a track that we had agreed upon earlier. I can't remember the name of that Captain who was in charge.

BS: *Hank Childs.*

NU: The CO of the ship.

BS: *Yes.*

NU: The captain in the Pentagon who did all the negotiating. Big, enormously big guy. I didn't like . . . it doesn't matter. So, yeah, they did - they took 1400 kilometers of profile and 6 months later we had the data. But that was perfect. All they did was the draft of the ice with a narrow beam upward looking sonar. And that's no repeat. It was. . . it was nice

in those days. Even then one profile was useful, but of course, it would have been much nicer to have a repeat track or to have one in May and one in September to see the effect of the summer. But in any case. . . so it turned out that I was called, in ONR I was called Advisor for Arctic and Polar . . . Special Advisor for Arctic and Polar Affairs and nobody wanted my advice. I met once a week with Elliott Weinberg - very smart guy who was in charge of the physical sciences program and we had a very nice chat every week. But other than that, I did whatever interested me because nobody cared. And in a way, this worked out very nicely because I was able to write a few papers and work on some committee documents. I worked with the Polar Research Board and the National Research Council doing a lot of these survey documents. So anyway, it was by no means wasted.

BS: *What year was this?*

NU: This was 1978 and '79.

BS: *OK.*

(200)

NU: And meanwhile, back at the ranch here in Seattle at the office, the remaining people from AIDJEX were doing their thing. Then I think it was in early '79, Ned Ostenso and _____ Webster came and said, "Look, you are now here in Washington and it's so interesting and you want to stay here anyway, why don't you join NOAA and become Director of the Office of Ocean Programs? That's the top job in NOAA oceanography." Well, I was too stupid to ask the right kinds of questions. I was hesitant. I wasn't really sure I wanted to stay in Washington, but I thought, "Well, you know, maybe this is not

such a bad deal." And they pinged on me and I finally said, "OK, I will become a Washington bureaucrat. I'll join you." I went through the whole hiring procedure. It was a senior executive service job. I had an office that had as many square feet as my house where I'm living in here. And so I said to my - I had three secretaries and I said, "Please somebody. Point me to some filing cabinets. Now that I'm the Director of the Office of Ocean Programs, I have to find out something about ocean programs and what is going on in oceanography in NOAA." "Well, uh. .Sir, we . . .uh. . actually we don't have that kind of information." So I said, "What do you mean, you don't have that kind of information? How can you be an Office of Ocean Programs when you don't have information about ocean programs?" "Well this is all handled by the labs outside the AONL in Miami and PNEL in Seattle and we suggest that what you do is go out there and find out about what ocean programs are." So this was the first blow below the belt. I thought this was insane that there is an office that doesn't know what it's directing.

BS: So there was no central function for ocean programs. It was all parceled out to different organizations.

NU: It turned out that this was, in fact, not anything like a line responsibility job. This was just for hob-nobbing with headquarters. At the time there was a NOAA administrator who was a lawyer and had no idea about either the oceans or the atmosphere. It was all downtown ritualistic budget negotiations and what can we do and what can we not do. It was the time of zero-base budgeting. One of those insane bureaucratic exercises where they tell you, "Suppose beginning tomorrow you have no budget at all, and you can ask for your budget to be restored in order of priority. So what is the first thing you will ask to get from us and what is the second thing and the third thing and the fourth thing?" And all of this. And of course, it would have been obvious to the most light-weight kind of intelligence that you can't really do it that way. This is some schematic device by a

person who has no idea about what goes on in the real world. Anyway, there was an enormous amount of time wasted.

(250)

As I said before, we had - my office was in Rockville, my apartment was in Alexandria, I had to drive to Rockville every day at 7:00 o'clock in the morning in the city of traffic. And then I had to sit in these - everyday at 8:00 o'clock in these totally discouraging staff meetings where everything was just ritual. Never any substance. And when I raised any question about how good is this laboratory and why are we sending - why do we continue to send money to this project that is completely dead? I was told that you can never say that a project has lost it's vitality and it's time to shut it down and do something else instead because they will not allow you to do something else instead. They hear only this project is no longer. . .

So then after only two months of this, I was convinced that I'm not only incapable of doing this job and even if I were capable of doing it, I would hate it and I would almost certainly come into serious conflict with the Establishment and if I didn't go voluntarily, they would probably have to fire me after less than a year. So I agreed with Ferris Webster who was the Deputy Administrator for - I think it was called Research and Development, i agreed that I would serve out one year and then gracefully take leave and say that that's all the leave that the Washington would give me. And unfortunately (fortunately?) I did not quit my faculty - I did not resign from my faculty position here, but rather applied for an unusual third year of leave of absence and they were generous enough to give it to me. So after a year, I just had really left Washington and came back to Seattle. I have to say that even to this day, I think Washington is an exciting, stimulating place, and I would have, I would have easily stayed there and become a Washington bureaucrat if I had had an interesting and doable good job. But as it

happened, I didn't, so I just left. And I came back to what was left from AIDJEX and we were off campus here. We were renting this place and we decided that we need an administrative if not more encompassing affiliation with the campus, so they Applied Physics Laboratory, which previously had been an almost completely Navy dominated laboratory was opening up to the rest of the campus considerably, and so after some negotiations with George Beckman who was the Provost of the University at that time, we decided to make the remains of AIDJEX - we call it Polar Research Center - and make it a department of the Applied Physics Laboratory.

(300)

But it turned out that this was a very good move. It was a good environment. We hired . . . we had our own budget. We were free to operate our business in much the way we wanted.

BS: *Who's the "we" there? Who are the people that were. . . ?*

NU: OK. There is - I was the Director of this Polar Science Center. Then the lead PIs - Principle Investigators - were Drew Rothrock and Jamie Morrison, Andy Highberg was kept as being the world's greatest logistics expert for fielding Arctic projects. We had Dick Trobridge who was my Deputy and Administrative Manager in the earlier part. He made the move to AIDJEX and, of course, we had in training a few younger people and we had intense collaborations with the campus. With people in Civil Engineering - Reid Parmutter and Jim Evans. With people in atmospheric science - Gary Macutt and Tom Grenfell and Bob Brown, so it was. . . it was also good for the Applied Physics Lab and we really were an intense bridge to the campus during the time - beginning the time of the campus unrest and anti-military, industrial military Establishment and all of that -

1969 and '70. APL was criticized very heavily for doing too much classified research and not being part of campus and the response was to establish a policy that would bring APL more, in a more cooperative and integrated status with the rest of the campus. And we didn't do ANY classified work. So we were a very good vehicle for APL. So you see we have people who had many collaborators on campus and we also, I think we made a big start in training graduate students who were either in civil engineering, or in oceanography, or in atmospheric science, or in geophysics - getting their respective degrees, but working with Polar Science Center on Arctic projects.

(350)

I should say here that the last thing that I did when I was still Project Director for AIDJEX in 1978, I twisted the arm of Ben Vogel in Office of Polar Programs so hard that despite all their reluctance to fund any more of AIDJEX, I got us, I believe, a \$700,000 grant to start the Arctic Data Buoy Program. And I believe that this was probably the best \$700,000 ever spent in the Arctic because that program flourished. The quantity and reliability of the buoys improved. Bo Buck, Polar Research Laboratories, had developed an air-droppable version of those things. We had significant support from the Canadians, from the Navy, from other agencies. Anyway, 1979, was the beginning of a number of things. Not only the continuous monitoring of the Arctic sea ice by _____ radiometers, it was also the beginning of the buoy program and it turned out that after I left, Roger Colony took over the management, the attempt to continue selling the buoy program. He was very successful and in doing that, the program exists to this day with a much broadened support base. It's now called International Arctic Ocean Data Buoy Program and we have, looking back now on a beautiful 20-year data set and a lot of years were when there were about 20 buoys floating around. We have a really fabulously interesting set of data that shows the kinematics of the Arctic sea ice.

BS: *Are you partial to the data that Bo Buck took during AIDJEX?*

NU: No.

BS: *Well, he took data out at those 10 remote stations.*

NU: I know that there was a lot of interest in ambient noise and not only ambient noise, but also noise generated by submarines, but we . . .

BS: *That's why the Genard was there.*

NU: Right. It was in our interest not to become involved in classified . . .

BS: *Oh absolutely! I . . .*

NU: Because it would have complicated our life and it was best for us not to know any of the side purposes of whatever the uh. . .

(400)

BS: *Well one of my jobs was to make sure you didn't.*

NU: Yeah. Exactly.

BS: *But it was very fruitful. And I can't talk about the fallout of it, but it was significant. Big bucks were involved later on, and of course you knew about the programs. Jumper*

and others moved over to operating out of Thule. That was the fallout of AIDJEX as well. So anyway . . .

NU: So maybe in future years this will gradually be declassified and come to the surface. So, but I'm glad to know that there was some other benefits reaped from this work. That there were just not about science at the time.

So we're now back from Washington and it's 1981 and we moved the "Son of AIDJEX" so to speak, over to the Applied Physics Lab and we called it the Polar Science Center. And it was a very, very good several years that followed. The Buoy Program was succeeding. All kinds of interesting work was going on. Allen Thorndike had left to become Professor of Physics at the University of Puget Sound in Tacoma, but we still collaborated with him a lot. He was only an hour by - still is - only an hour by car away. So this was quiet and nice. The Provost - because I had been kind of yanked around a little bit, the Provost was generous and simply assigned my faculty billet to the Applied Physics Laboratory.. I was, except for the Director of APL, I was the only person with a university salary, that is with a hard money salary. Everybody else was on soft money.

(450)

Then in 1983, all of a sudden, there was another call from the Provost, "Yes would I please serve as Interim Dean of a newly established college?" It turned out that Jim Baker who is now administrator of NOAA, Jim was before that chairman of the Oceanography Department, and he was very keen on becoming Dean of this newly established college. That was a move that the university had been contemplating for several years. The University of Washington, of course, has extensive activities in marine related issues. We have one of the three large Oceanography Departments besides Scripps and Wood's Hole in the United States. We have a Sea Grant program. We have

the Applied Physics Laboratory and various other - Center for Qualitative Studies that was mostly about fisheries and other marine matters. The university thought that it would serve a good purpose to have a college that is specifically dedicated to marine - to teaching and research and outreach and public service in the marine areas. So they established a College of Ocean and Fishery Sciences. The School of Fisheries was a separate college before that, but they wanted to meld all this together.

BS: *And you became the interim Administrator? Is that correct?*

NU: Sorry?

BS: *You became the interim Administrator?*

NU: No. Wait, wait a second. No, it's complicated. Jim Baker wanted - he was Chairman of Oceanography at the time and he very much wanted to be Dean. Once the College was established, he was made Acting Dean and he was Acting Dean for quite a long time and finally did become Dean in 1983. So he, and oh he was looking at at least a period in office of five years. Our Dean appointments at the UW campus are typically made in five year increments. Some deans serve longer, but not many. Most people have enough of a deanship after five years. So he was Dean of the College of Ocean and Fishery Sciences in 1983, and not a few - oh not even a half a year went by when he got a call, "Would he like to be President of JOI?" Joint Oceanographic Institutions.

(500)

That is a non-government, what should I call this? association of all oceanography departments in Washington. And it is to represent oceanography to the government and to

the funding agencies. I think it's also a little bit toward lobbying. Anyway, Jim Baker could not resist that. He was barely made Dean when he already quit. And he wanted to go and he had to go to make up his mind very quickly. So they immediately, they needed a dean like by next week. So they came back to me again and said, "Look, you've been acting so many things. Why don't you become Dean of this College?" So I said, "Well you know this is probably just about the last thing I want to do. But OK." So in loyalty because the university was so generous letting me go for three years when I wanted to. So anyway, I served for two years as Interim Dean for this College of Ocean and Fishery Sciences and then they ended up in 1985, they ended up - I was there for two years - they ended up hiring Ross Heath from the University of Oregon, I believe. And I was back as Director of the Polar Science Center until 1988 and in 1988 they asked me if I would take the chairmanship of the Department of Atmospheric Sciences which was something that I really liked. An offer that I liked because the Atmospheric Science Department I consider my academic and spiritual home and they have been very generous and very good to me and I was always treated fairly and it was an extremely friendly and congenial and high-quality department. So it was an honor to be asked to come back.

(550)

So again, George Beckman, the Provost, took my billet and said, "OK, from now on your billet is in Atmospheric Sciences," and that's what I did for 9 years. I served the first 5 year period and then they said, "Well now you're 65. How about another increment?" So I said, "OK, there is nothing else I want to do anymore anyway." So I stayed until I retired in 1997.

BS: *How old were you when you retired?*

NU: I was 71.

BS: *I ask this of everybody. It's interesting how late. . .*

NU: 71. You know, by the time I retired, this non-discrimination of old people had already gone into effect. If I had wanted, if I had not wanted to retire, I could have stayed another, well essentially until a committee of peers declares me mentally incompetent. But I didn't want to push it that far.

BS: *Ha-ha-ha. No you don't, do you?*

NU: No, no I don't because you know.

BS: *You want to go out with your head up.*

NU: It was only me who knew that I was becoming incompetent. I didn't want other people to know it.

BS: *You don't sound incompetent Norbert.*

NU: So anyway, I retired at 71, which was just right. Also, as it turned out, our retirement system has been so incredibly successful - this TIAA/CREF that everybody who retired after I would say 1985 - well maybe '85-'90, after that got a raise when they retired. And the people who are working now and who are retiring five years from now. They will get a huge raise.

(End of Tape 3 - Side A)

(Begin Tape 3 - Side B)

(000)

NU: I should mention another thing that I got involved in and I'm still involved in. That out of the blue in 1992, I got a call from Washington, in fact, from Gordon McDonald who - I don't even know what his official title - oh, I think at that time he was at the University of California at San Diego. Anyway, he called me in a completely different capacity. He called me to ask if I would serve on an entity called Environmental Task Force (ETF) that Vice President Gore wanted established. It was well known his interest. I had, in fact, when he was still a senator, I had testified a couple of times in the Senate and now Mr. Gore wanted this Environmental Task Force. And would I be willing to serve on it? And I said, you know, "Yeah, sure." I had no idea what this was and well, it turned out that this was to advise the CIA on the potential unclassified scientific use of some of these surveillance satellite data specifically and also other data. So they asked me in order to have anybody to look at this, we first had to be given access to these classified data. And this, this was something really that made their skin crawl to get a bunch of egg-head scientists and give them some very high level clearances that allowed them to look at all satellite data. But anyway, it took a while, but we did get these clearances and then we were briefed and briefed and briefed. It was called Death by _____? We sat there in some dungeon in Washington for hours and hours and hours and day to day and were briefed about all this and then we wrote a fairly extensive report and it had an unclassified and a classified section. Anyway, the White House thought and Mr. Gore's advisers thought it was really worthwhile so - this was 1992-'94 - in 1994,

they established a group called MEDEA. MEDEA is not an acronym. It's in parallel with the Jasons. The Jasons are a group that has been advising the Navy for many years on, well mainly on issues of the Cold War and especially submarine.

BS: *I'm familiar with it. Bill Namburg used to be head of it.*

NU: Who used to be the head of it?

BS: *Bill Namburg. He was from Scripps.*

NU: Walter Monk was on it and . . .

BS: *Walter Monk's still on it, I'm sure.*

NU: Walter Monk is still on it. And I don't know. They have a place down at Scripps. Anyway, the people that we dealt with at the time belonged to an agency whose name even was classified. Well now it's been declassified. But the NRO, the National Reconnaissance Office - we couldn't even say NRO at the time, it was so secret.

(50)

And, well, as a result of that, all kinds of activities started and we wrote a bunch of reports. The one, for instance, a broadly based report about satellite data in general. Then a report about ocean dumping of chemical weapons. Ocean dumping had occurred in the wake of World War II and the dangers, the potential dangers from that was an unclassified report. But as I said, in 1994, this former Environmental Task Force was converted to a more steady state entity with a small MEDEA. There is an article in

Scientific American, I believe it's in February of 1999 that's called, "Scientists in Black." That was written based on information that the CIA Public Relations Office gave to the author of this group, and it's a fair, non-hostile account of what we do. In 1997, I was asked to be on the Steering Committee of this MEDEA. Of course, many things had happened in the meantime. For instance, the Kyoto Conference and the meeting that is going on as we speak in Holland having to do with the emission of greenhouse gases with possible plantological effect and maybe commitments by countries to curtail their greenhouse gas emissions and all things that are very touchy, very sensitive. They are not only fraught with scientific uncertainties, they are also fraught with political shenanigans where people claim what they are doing and in fact not doing it and . . .

BS: *You mean scientists?*

NU: Well, there are in some cases, ways of actually monitoring whether they are doing or not doing what they say they are doing. And so this is very much at the intersection of public research and public policy and clandestine operations. And this work is continuing. Of course, now the Steering Committee had a meeting last Friday to decide how we will interact with the transition team in the White House and by the time we went to Washington, there was still no transition team and there still is no transition team, so we had to decide to write a white paper to some generic transition team.

For instance, one of the things we were able to do had to do with looking at Arctic sea ice in the summertime. This has been a real problem for many, many years and a widely recognized problem. We know that when June comes around in the Arctic, the snow melts, the ice is covered with melt ponds, the positive albedo feedback swings into action and it's very, very important just how much water is standing on top of that ice because that effects greatly the amount of absorbed radiation and the amount of ice that

melts and this is potentially the kind of process that could, in a warmer climate scenario, wipe out the sea ice.

(100)

And we have practically no systematic data about the surface conditions in the summertime because in June, July and August, there is this very persistent stratus cloud cover over the Arctic and it's really very tough to get observations. Passive microwave images don't show what we need to know and any reachable(?) frequencies don't penetrate the cloud cover. So there is a huge data gap. But some of the, what they call national technical means (NTM) - some of the NTM resources allow the acquisition of data showing the surface conditions in wave lengths that allow us to distinguish between melt ponds and water covered ice and drain ice - not non-ice, bare ice, and also the ocean surfaces. And we had asked the authorities to take such data whenever possible. And they did. And a co-author of mine and I published a paper showing the potential of this technique and it was even possible to get something in the unclassified literature that was called a derived product where we're not saying anything about how the data were taken, and where we only show the resulting analysis. And neither the Journal of Geophysical Research (Centers?) nor anybody else objected to this. So this showed the potential. And when the SHEBA project was fielded in 1998, we had made all the preparations that this particular satellite instrument was turned on and was taking data in the summer of '99. This was the _____ for SHEBA when the ice breaker was there stuck in the ice and all the people were working. And then with the help of the National Science Foundation,

BS: *Was it the Office of Polar Programs?*

NU: No, the Director of NSF was involved in that because - Rita Coburn - because she is also a member of the _____committee. So with her help, we got a set of pictures - it so happened that there are 59 scenes of a size much larger than any aircraft can take from like a helicopter based on an ice breaker. This is a 20 x 30 kilometer scenes - very high resolution - one meter resolution were declassified and they can be obtained from the National Snow and Ice Data Center in Boulder, Colorado. So this was a direct benefit of MEDEA. Sort of using it's location with one foot in the scientific arena and the other foot in classified _____to do something to help out the scientific community in interpreting their data and hopefully expand the scale of what they are looking at while they are on the ground, while they are limited to whatever photographic work the ice breaker based helicopters can do. This work is continuing. There are now various requests for assessments by other agencies in the government and I fully expect that this will not only continue, probably my guess would be regardless of what or who will be our next administration because I think it proved to be a really interesting and valuable activity.

(150)

One other thing that I could mention is that there was an unclassified Chinese agriculture study where satellite data were used to map crop lands and it turned out that the results were surprising even to the Chinese and were much more accurate than any of the reporting that was - any surveys that were collected on the ground about land use in agricultural activity. So there's a whole host of things of that kind that are potential fields of activity for this MEDEA and we expect actually that the membership and the breadth of expertise will be enlarged in future years.

BS: *Well, neat. OK. So that's MEDEA. Polar Research Board. Tell me how you interfaced with them.*

NU: MEDEA. Let me just check this . . . OK. Uh. . . . finally maybe another word about the National Academy of Sciences and it's operating arm the National Research Council. The National Research Council is a very large organization. I believe they are in Georgetown, if I'm not mistaken, at least they were in Georgetown. I don't know how many employees they have, but it's many, many hundreds and it's many hundreds of boards and committees and panels. Between 1965 and 1994, I have served on 13 different panels and boards and committees of the National Research Council and the one that is still around that has changed from the status of a committee and elevated to a board is the Polar Research Board. The Polar Research Board is actually a very interesting entity because back in the '60s at the first - I don't know when it was formed. It probably was formed and may be coming right out of IGY. My guess would be it was Larry Gould and - can we turn the tape off?

BS: *OK. You just mentioned the Polar Research Board before we turned the tape off.*

(200)

NU: Yeah, the Polar - I'm not positive, but my guess would be that the Polar Research Board was established in the wake of the International Geophysical Year in 1957-'58. My first contact was when I first came to this country in the early '60s and the people there were Larry Gould and Hugh Audesshaw and Paul Palmeroy and Nick Washburn and Bill Keel and Bert Crary and I believe those - that was the first crew of the Polar Research Board. Over the years, I think the relationship of the Polar Research Board with the agencies that they were supposed to advise could be considered rocky, I would say. Early

on, when Tom Jones was in charge of Polar programs at NSF, I believe that the relationship between them, NSF and the PRB, was positive - was cordial. It's the purpose of the National Research Board to advise the US Government. That's what the NRC was established for and the NRC can do this on their own volition with their own motivation and without anybody asking them to or they can be asked by a funding agency to undertake a study. I think lately the modus operandi has become more the latter - that agencies ask for a specific study or that the staffer from the PRB goes around and says wouldn't you like a study of this for a particular subject or say, "We believe you need to look into this," because the operation of the NRC is quite expensive a proposition and it costs the agency quite a bit of money each time they ask for one of those assessments. In any case, as I said, it was probably the relationship between Polar Programs and the PRB was, I'm sure, very good initially and then it went up and down. I believe that when - before Joe Fletcher - who was before Joe Fletcher?

BS: Um, Jim Zemberg.

NU: No.

BS: No? He was in there somewhere. He was there during AIDJEX, I guess.

NU: He was on the National Science Board. He was not in Polar Programs. Who was Polar Pro . . .? Was he?

BS: He was Polar Research Board because through him I . . .

NU: No, no. I mean who was at NSF before Fletcher?

BS: *Oh. Before Fletcher, it was Tom Jones. Yeah. Joe relieved Tom.*

NU: Oh it was Tom Jones.

BS: *Yeah. Then Rutford after Fletcher, then Ed Todd.*

NU: So Tom Jones was followed by Joe Fletcher, and of course, Joe Fletcher, having just come from making a start as the Project Director for AIDJEX, it was quite, quite good. After Joe left, came Bob Rutford and Bob Rutford was not so much interested in the Arctic, but he was very interested in the Antarctic and the PRB, being the former member organization to SCAR, of course, was very important.

(250)

And so I believe under Bob Rutford's administration in Polar Programs, the PRB also did well and was energized and had the impression that they were - well, I was on it at the time too - I don't know how many. I served two terms. I was on it for eight years. But I was on many other panels at other times. And so we felt energized. We felt that we were doing something useful and somebody was paying attention. After Rutford left, things became dramatically different because then came Ed Todd and Todd was a Washington bureaucrat of the surest denomination and he didn't want anybody to give him any advice. My impression over the years was that Todd pretty much was against everything that was an activity, so it was not even clear what you were up against, because you were up against everything. And he was there for several years. And then, if anything, things got worse with Peter Wilkness. And I was close - I could observe this at close quarters because Sherry Abbott was the staff person at the PRB and that was almost a personal vendetta. It sometimes, it looked like Polar Programs wants to drive the Polar Research

Board out of business completely. They were starved financially and it was so . . . and Sherry Abbott was an extremely competent staff person for the Polar Research Board, but she got so frustrated that she moved inside the NRC to another job because she couldn't take it any more. And not much happened during Wilkness's time and certainly she didn't pay any attention to what recommendation the PRB made. On the whole, I would say that the NRC and all the Polar Research Board and all these other joint panels and special panels and coordinating committees and whatever they turned out, whatever products they produced and then presented to the agencies probably did not have all that much effect, but still I do not consider that time wasted. And I'm sure most of my colleagues feel similarly. It is to go there once or twice a year and to grapple with a difficult project or with a difficult question is something that is simply illuminating. It's an educational experience.

(300)

I think the members of such groups had to educate each other and you always come away better informed and if nothing else - I mean even if nobody pays any attention to what recommendations the group issues as a group, I think everybody goes home knowing something that they didn't know before and they're being able to do their job better. So I think it was not a waste of time.

While we're on this, I should mention the Nansen Drift Station. It was a thing that happened after AIDJEX - after the completion of AIDJEX. I don't know how this, how this was brought up and it was entirely possible that this was again this very modest, nice Walt Whitman who kicked off AIDJEX. Somebody came up with the idea of using a decommissioned ice breaker to repeat Nansen's drift from the New Siberian Islands out to Franz(?) Strait - what we now call Franz Strait between Greenland and Spitsbergen. The Coast Guard ice breaker *Burton Island* was about to be decommissioned and the idea was

to keep it in operation, modify it, turn it into a drifting laboratory and repeat the Nansen drift. Somehow this idea got a lot of steam behind it, especially from the Navy, I believe.

BS: *Yes.*

NU: And a special committee was set up - the International Research Council - to develop a research plan for the Nansen Drift Station. They put me in charge of that committee because - one of the really gratifying experiences - because AIDJEX was just over. I had all of these contacts with all the participants. We had kind of a high level of visibility and we let out, we got out the word that there is a possibility of doing this project. There was some money. At that time, Brian, Jim ___berg was chairman of the Polar Research Board. It was not - it was a committee of the Polar Research Board but with other boards participating. It was a totally ad hoc committee. It was all the people who could possibly make a contribution to such an enterprise.

(350)

This was not only oceanographers and ice people. This was also upper atmosphere and sediment people and earth crust people, and tectonics people and acousticians. It was, it was a wonderful experience. We had a workshop. We laid out a plan. We made people write their essays. It turned out that everybody was so excited and so enthusiastic about this that the report was finished before the NRC had gone through the ponderous procedure of setting up the committee. So when we got notified that this committee is in existence - like two weeks later, we submitted the polished report. This is a document of the National Research Council. It's called a report by the - I don't have a copy here - a report by the Committee of the Nansen Drift Station. It was a really inspired piece of work. No jealousy. No position jockeying. It was just an exciting thing. The Navy was so,

I remember many meetings with people coming up from the shipyard in San Diego - Navy shipyard in San Diego. The Navy was so keen on this that they spent \$400,000 or \$500,000, unscheduled money of designing, making design changes on the *Burton Island*. Among other things, of course, that was a World War II ice breaker - very, very Spartan quarters. We had to make more space below deck. I believe it had 6 engines. They suggested that we take 4 engines out and make that space available for labs and other things. And then they were going to weld on brackets to the outside of the ship for and then make outriggers and put little, you know, pre-fabbed panel buildings and labs that are practically hanging overboard to accommodate enough people on that ship. And all of that design work was already going on. Well, we submitted our report. ONR, and I think that was Admiral Geiger still, it was 1977 - ONR pledged the logistics, the entire logistics budget to the tune of something like \$25 million if NSF would support all the science programs.

(400)

Well, there were so many meetings, they all blur in the past. This is, after all, this is 25, 24, 23 years ago. Anyway, the whole thing came to naught. And through the best of what we were ever told or what was ever divulged, it was the State Department - high level decision. We can't do this because it will aggravate our already bad relations with the Russians. Maybe they were particularly bad in 1976 or '77, and NSF found itself unable to sort of go uphill with this whole thing. I was even, in 1978, they sent me to Russia to sound out the Russians. I came back with the clear impression that the Russians thought that this was an extremely good idea and that many individual Russians would have been happy to participate in this. ONR was not going to do any classified work or were not going to do anything that anybody could recognize as classified work. So we were just getting ready to issue an open invitation to Russians or anybody else to participate in this

and at the time, it was really only the Russians that were . . . the Germans were not in it and the Norwegians weren't much in it either because they realized that everybody was so preoccupied with the Cold War. But in any case, somehow at the top level of government, this thing was stopped and all of this high powered planning work and all this thinking work that had gone into the scientific planning was for nothing. On the other hand, maybe it wasn't. Back in the same vein that I was talking about before, maybe it wasn't for nothing. Maybe it was a worthwhile mutual education exercise.

BS: *That's Trishnakov who was at Mirny during IGY?*

NU: Trishnakov was where?

BS: *At Mirny during IGY? Is this the same Trishnakov?*

NU: He was where?

BS: *He was at Mirny Station at Antarctica during IGY.*

NU: Well yeah. Could, that could work.

BS: *Yeah. Well, OK.*

NU: Trishnakov was a hero of the Union. He was on at least one - he was a leader of one of the early drift stations, maybe North Pole not II, but maybe III or IV or V. And he was in the Antarctic and he was a very smart guy. Very pragmatic. He was not a dyed-in-the-wool Communist Bolshevik. And the KGB guy was also a very civilized guy. This was in 1978. And they took me out there and they nearly killed me with booze and champagne

and food and stuff and the bottom line was that they couldn't believe that this was not a military, a Navy inspired, activity. And I don't know if I was able to convince them that, in fact, it was not.

BS: *You see that's what I mean. They're red-neckers.*

NU: It was kind of irrelevant, because our side nixed it. And I never learned - nobody ever said to me or to us what exactly it was. And who told NSF to stop it or maybe not NSF - maybe told ONR to stop it. I never learned that. But I had no clearances of any kind at that time. So I don't know. Maybe sometime. You know, in the vein of your historical study, it would be nice to find somebody who would know that.

BS: *Hmmm. Boy, it's too bad. I think Bob Geiger's died, but he would know.*

NU: Bob Geiger would know.

BS: *He would know how to move around in classified circles and find out.*

NU: It's possible that Ron McGregor knows.

BS: *Yeah. That's a good thought.*

NU: Max Britten knows.

BS: *I'll ask them.*

NU: Is NSF? I don't know. Who was at NSF in '78?

BS: *Todd.*

NU: Todd. OK.

BS: *He had no clearance other than secret clearance. He wasn't partial to some of the things that we did in the Antarctic.*

NU: I mean, he didn't know what was happening anyway.

BS: *That's right. I'll ask McGregor.*

NU: Ask McGregor.

(500)

BS: *Yeah. He's a very accurate source of information.*

NU: He's very alert and he's fine, you know.

BS: *Oh, he's wonderful.*

NU: You saw him.

BS: *Looked pretty good, didn't he?*

NU: Yeah.

BS: *Oh yeah.*

NU: Yeah. He looked like that for the last 30 years.

BS: *Yes. He looks great. . .*

NU: Any of these other old guys, you know?

BS: *Hang on a second here.*

NU: I know there was a Founding Father's Workshop in, what's that town in Florida? Orlando. Orlando in 1993. And right there at this was, uh. . . we wrote a nice paper after that workshop and it all showed how the models, how the climate models produce the wrong cloudiness and, of course if you produce the wrong cloudiness then you produce the wrong radiation regime and a lot of wrong things. And clearly this is at least in part because the surface energy balance is not treated right in these models and that's why we had to have these observations. And then, it turned out that this was a kind of a - it turned into something of a buzz word approach that, in fact, was carried way too far. But if you ask me how did this come about the first time, I don't know.

(550).

BS: *Oh no, no. I just wondered what the links were to AIDJEX.*

NU: You know, what is interesting, though, Brian, is that it was almost 20 years from IGY to AIDJEX that anybody spent the summer on the ice. It was another 20 years plus

that somebody spent a summer on the ice because our last summer was the summer of 1975. It was 23 years, the summer of 1998 - nobody spent the summer on the ice. And of course, a lot of corporate memory gets washed away.

BS: *How about on T3 though. We had that going all through the '60s.*

NU: I mean, that was a different thing, though. It was not sea ice.

BS: *Yeah.*

NU: Yeah. There were all these people. I mean, not always, but . . .

BS: *Yeah.*

NU: But see, the morphology, this whole water thing because it's a big difference whether you're on three meters or on 40 meters of ice.

BS: *I'm going to change the tape here. I've just got a little bit left.*

(End Tape 3 - Side B)

(Begin Tape 4 - Side A)

(000)

BS: *This is Tape 4 in a series of tapes with Dr. Norbert Untersteiner. It's the 16th of November 2000 on a telephone interview between myself, Brian Shoemaker, in North Bend, Oregon. and Dr. Untersteiner at his home in Seattle.*

So what else are you involved in now polar -wise besides MEDEA?

NU: Well, I'm - oh, I didn't say that - in 1998, I was invited, sort of, by an informal telephone call - no, I was not invited, I was TOLD that the University of Alaska has an endowed professorship that was endowed several years ago by a grant from the State of Alaska. They put a million bucks or something like that in an account and from the proceeds, from the yield of that investment, they fund a chair that they called Chapman Chair after the great Sydney Chapman, who had an intimate relationship - Chapman was the Mr. Geomagnetism and Mr. Aurora for many years.

BS: *Head of the IGY.*

NU: And he was one of the founding fathers - one of the four founding fathers of the IGY and he spent time in Fairbanks and he spent time in _____, and so they have a Chapman building up there and they decided to honor Sydney Chapman and they had hired Keith Roncort - famous English solid earth geophysicist, fellow of the Royal Society, and Roncort got killed tragically, in San Diego. Got murdered in San Diego. So the Chapman Chair was vacant and so they said, "Well, this is vacant. Wouldn't you be interested in it?" And I said, "No, no, really not." This is two years ago, three years ago. You know, I don't want to do this anymore. So they said, "Well, but maybe you should apply anyway so that we can have some applicants." So I sent in my CV and I said "Yes, I would be interested." Well, and before you know, I went up there and gave a couple of talks and blah, blah, blah and before you know it, I'm now Chapman Professor of

Physical Science at the University of Alaska as of 1999. I established a visiting lecturer program up there and I spend February, March and April in Fairbanks and I cycle through about ten to twelve important people from the field to lecture, to give lectures, typically two a week - Tuesday and Thursday - about any topic that has to do with Polar climatology and then we take reprints and bind them together and those are the Chapman Lecture Series and the students and everybody who attends gets a copy. The attendance is sort of between 10 and 30 people. A handful of 3, 4, or 5 graduate students, but mostly faculty and researchers who come to these lectures and it's been very, very interesting. I like Fairbanks. I like the University of Alaska. And in __parallel?_____now I've been involved in various, sort of more informal ways, with the International Arctic Research Center. This is the US-Japanese new building that is attached to the old Geophysical Institute Building. Dr. Sofoo resigned from his directorship of the Geophysical Institute and he's now Director of IARC, and I've been functioning as sort of an informal advisor a little bit on matters of IARC.

(50)

This is all still in flux and I don't know if it's going to lead to anything, but it might. So anyway, I consider this as a side activity in my role as Chapman Chair. I promised the University of Alaska-Fairbanks no more than three years as the Chapman Chair. The third year is coming up and I have not talked to anybody about the future because, you know, at my age, I don't want to make any long commitment. Maybe I'll tack on another year or two, but it remains to be seen. So that's my assignment here. That's my other current Arctic activity. I still have some data, believe it or not, unused data back from IGY and I'm going to salvage those and write a couple of more papers, but that's just sort of what I do in my own time.OK?

BS: *OK. Looking back on it, how do you view your life in science?*

NU: Well, I - for me, I did, I would say, almost the right thing. Maybe I would have preferred to be an engineer and build hydroelectric dams in the mountains or something like that, but given the possibilities, I think I was really lucky. It was a very good time to be a scientist. It was a very good time to be on the right side of the Cold War. You could feel good about everything you did. I'm very happy I left Europe. I feel far more at home now. I like to go back to Austria because it's very pretty, but it's contribution to the world is mainly Arnold Schwarzenegger and the crystal from _____. And so, it's wonderful to be there and go skiing and go hiking and go to all my _____ where I was when I was a boy, but I'm not at home any more there.

BS: *Are you a US citizen?*

NU: I've been a US citizen since '73.

BS: *Yeah. My wife feels the same way.*

NU: And I would never go back and I'm really glad I left. So I think it's a, you know. . . . the amount of things that I had to do in my life that I really hated are really quite small. Yeah, well, of course I hated to be a soldier in the German Army in 1944. It nearly killed me, literally. Of course I hated that. But that wasn't very long. But compared to a person who has to go every day to some boring job to do something that they don't really enjoy simply because it brings home the bacon has my full sympathy. My life wasn't like that at all. Most of what I did was very, very interesting and if I had been born independently wealthy, I might have done the same thing just for my own amusement. I think I was lucky and I did the right thing. I should say my father thought it would be the right thing

- I was clearly brain-washed. I executed everything that he wanted me to do, pretty much. And if he had lived to see that, he would have been very happy. I did most of the things that he would have wanted to do if he had chosen another path.

BS: *OK. Well, let's end it there, unless you can think of something else that's pertinent.*

NU: Well I think that that's - maybe we can revisit this a little bit when I see the transcript.

BS: *That's a good thought. No one else has suggested that. Of course none have got their transcripts yet except Gordon Cartwright. Do you know Gordon?*

NU: No.

BS: *Well, he's the guy that first wintered with the Russians at Mirny.*

NU: I know the name, but I don't think . . .

BS: *Yeah. He's 91 and he's been pressing me to get it and even offered to fund the transcription, so he's got it and we're kind of using him as a guinea pig as to how we're going to do transcriptions. And what I'll do is get you a tape. Well I'm going to do that after the first of the year. I'm got to get some paper work done. I get on the plane next Sunday - week from .Sunday for Argentina and I'll be gone until New Year's. Yeah lecture and tours. We're going to South Georgia and it's going to be fun. We're looking at Shackleton's - where he went over the mountain and doing all this stuff and they pay me to talk, with slides. I can do it endlessly.*

(100)

*So anyway, I'll be doing copies of the tapes. The main copy that we've got here goes to Ohio State. It'll be archived. They'll make copies. We'll send you copies and send one to the transcriptionist and she'll send a hard copy - well computer for both of us - computer copy to me and I'll look at it and what I did with Gordon Cartwright - I inserted a couple of questions for what I had asked him on an aside and we turned the tape back on and he just plunged right in, so I inserted those as questions. But he's going to - he told me on the phone that he wants to add some big sections because he left them out. And we've decided to do that in **bold** so that we can keep - I don't particularly care if we save the old tape or not. However, Ohio State Archivists want to keep that, so we'll have a different style of type for insertions. But as far as I'm concerned, between the interviewees and myself, we can reword what's said because sometimes there's a lot of uh, well maybes, and it gets - this gal that's doing the transcription work did it for 25 years at the University of Illinois. She happens to be my daughter's mother-in-law and she actually can do these things. She has a foot pedal to turn the tape off, But she doesn't use it. She types along as fast as the tape rolls and she gets all those little things. And I had to clean them up for Gordon's to make sense of what I said, but more so with him because he was searching his brain. What we've done here has really got me thinking. God damn, we can sure do this a lot faster if I can just pick up the phone and spend an afternoon with somebody on the telephone. You and I knew one another, but most of these guys, I've never met. And to call them up and suggest something like that, I don't know how it's going to work, but I'm going to give it a try in the Spring. As you know, Karen Brewster up at the University of Alaska at the Department of Oral History there, she's going to do seven interviews for us or for me - I'm kind of running it. And we've closed that out since you and I were in Boulder together. And I haven't heard back from her. She was up in Barrow at the time. We've given her the list. We've got her the funding and if this works*

out, I'm going to try and hire other interviewers who, say, live in the Northeast that can get all those people up there. And there's actually a lot of people down in Florida who are retired. So I think this has worked out pretty good.

NU: Yeah, I think so. I think so too. And when I see it, I might wish I had emphasized things differently, but I think we covered a lot of ground.

BS: *It's a very good interview. Yours and Willy Weeks have been the two longest and the most thorough. And I think that probably is because both of you have published a lot and are used to organizing your thoughts whereas many of the others, particularly the non-academics like the pilots and the ship drivers - they bounce around all over the place.*

NU: Well, if you are used to lecturing, it forces you to just sort of say things in a certain . . . try to organize them in a certain way so that the audience can follow easily

BS: *I'm going to pick a couple of these guys who I've had experience in the field with and I'm going to say let's try this by telephone. The other advantage on it - some of these guys get pretty old and after an hour or two of taping, they're worn out. For instance, Lloyd Bebee over at Squim - have you been over to the Olympics Game Park there?*

(150)

NU: Oh yeah.

BS: *Well, that's - he was the chief photographer for Walt Disney in IGY. He spent a year at Little America. He went ashore on the first boat when there was nothing there and filmed it from start to finish and also the building of the Byrd Station.*

NU: Where is he now?

BS: *He owns the Olympic Game Park. That's him! He IS the Olympic Game Park at Squim, Lloyd is.*

NU: I see. I asked you, hey Brian, I asked you this before. Is he related to this guy with the steel ball in deep sea?

BS: *I didn't get back . . . I got back to him, but I didn't ask the question because there was a little crisis when I called and that was on my list of a couple of things to discuss with him.*

NU: Does he live there?

BS: *Oh, he lives there up on the hill and these animals roam around and he had some wolverines that were tame. They were looking in the windows at us and he had a puma he put over his shoulders to show me. And he had 27 grizzly bears there. Did you know of or see?*

NU: Oh yeah. I've driven through there many times.

BS: *Well, Grizzley Adams on TV was filmed with his animals. All of the Disney Wildlife Series - or let me give you a little history. I've got it all on tape for Lloyd. He started trapping in the 20s or 30s - he's 88 now - as a bounty hunter and he started keeping some of the young. And then he started actually trapping the adults - bears, elk, pumas, and you name it - wolverines. And then he got interested in movies. He's a very interesting*

person. Didn't finish high school. Decided that he wanted to make movies of his animals and basically he's got a way with them. And even the adults he's trained not to be - to be non-aggressive, at least with him and many of his handlers. But he can command them to do things that they would do in the wild like chase a rabbit down - release a rabbit and have the puma go get it, and coyotes and bears and wolves and he even had three polar bears there for a while after he filmed Wild Arctic for Walt Disney. All of those movies were filmed with - anyway, he sent his first movie of his animals to Walt Disney and they called him right away. And they had been thinking of doing this series on vanishing prairie and the wild arctic and you remember, all those Disney movies. He's the guy. And they're all his animals.

NU: I knew that some of these bears were in movies. That they were trained. That they were sort of put out to pasture there.

BS: *That's right. He had three polar bears there. One of them lived for 46 years and he's got pictures of him riding it. He's got pictures of him and several of his workers riding - then Disney sent him for a year to Brazil and he did the same thing down there. He left that Game Park behind down there and he visits it even today once in a while. He brought some of the workers home from there who still live on his ranch there. But anyway, I started to tell you. He gets real tired. And I'm pressing him and he couldn't even talk straight. So I said, well let's take a break and go to lunch. I had to do that with Gordon Cartwright too. But Gordon rejuvenated and came back and Lloyd did for a little bit, and we finished. We were about finished up anyway. Anyway, I called him back after I came back from Boulder and his wife answers the phone and said, "Did you know that he had a stroke when he was talking to you?" And I said, "No! I didn't know" She says, "Yeah. That's when he couldn't talk straight." So I said, "Well how is he?" And she said,*

"Well I'll put him on." Well, he's fine. But he was in the hospital for a week. He went in that night and I'm kind of sensitive to that. I'm really watching guys real . . .

NU: You deal a lot with us old farts? I have to be careful.

(200)

BS: *Yeah. Well it turns out, you know Martin Pomeranz?*

NU: Yep.

BS: *Mart and I did - Mart's a very old friend of mine from the '60s in Antarctica.*

NU: And also an early member of the PRB.

BS: *Oh yeah. Well he was IGY. He had studies in Thule and at Swarthmore and. . .*

NU: What is he? What does he do?

BS: *Well, originally started with cosmic ray studies.*

NU: Upper atmosphere.

BS: *Upper atmosphere, cosmic ray studies, balloons at 135,000 feet, and he has cosmic ray facilities - well they're still operating today at Thule, Churchill, Swarthmore, I think Haleakala, McMurdo and South Pole. But he got into bigger than that because the balloons he was putting up he realized could be captured by the polar gyre and stay aloft*

for 16 or 17 days looking out into space to carry all kinds of things. So he got into astronomy and all the astronomy that's done at South Pole today and all the construction that's going on down there today is the Pomeranz Center and that's across the board from neutrino studies to cosmic rays, of course, and he's a collaborator with - now I'm trying to think - started the IGY, he's a cosmic ray guy too - Van Allen.

NU: Van Allen?

BS: Yeah. Sidney Chapman was at Van Allen's house and Lloyd Berkter leaned across the table to him and said, "I think it's time we had a third polar year." And he said, "I got all of that on tape from Van Allen."

NU: Yep.

BS: But anyway, some of these guys get so tired, we have to take big breaks. And it's hard for a tape guy to say, "Well, OK, I'll come back next week." Because you're talking about a plane ticket to and from and rental cars and all that. You try and get it all in one shot. I've got a feeling if I see a guy in the future and I'm doing that, at least, I'm going to say, "Look, we can finish this by tape," (telephone?) because I think this went good regardless of the phones breaking down. And possibly, if I work out my techniques right, maybe I can sit at home and do most of these and it would be much more effective.

NU: With the speaker phone, it is so handy.

BS: Yeah, I'll get one too. I think . . .

NU: If you have a good speaker phone. I'm speaking at a very modest voice level and I'm four feet away from my phone and it's - you sound perfectly good and boy, it beats traveling and traffic and all that.

BS: *And we can say, "Look, I've got to go to lunch. I've got to go shopping with Mama." That's the other thing. I've had guys rushing it because they had something else to do. And if you get to a point like that, you can say, "Look, if this is too tiring or if you need to do something else, why don't we continue on next week," like we did. We picked another .*
..

NU: Brian?

BS: *Yeah.*

NU: When you talk to the transcriptionist, make sure that they write at least double spaced.

BS: *Oh yeah. She does that.*

NU: With a wide margin.

BS: *Yes.*

NU: So that you don't have to struggle to fit the scribble stuff in there.

BS: *Yes. Actually it's almost quadruple space because we're going to publish these in double space or sorta double space. A good space between them.*

NU: OK. Good.

BS: *And so, I had plenty of room when I was working on Cartwright's. Anyway, I'll be back for two or three weeks in January and I'm going to get your tapes to you then. And I'll get the copies to her, but she's already started on a batch of the first ones.*

(250)

NU: Should I give you my Fairbanks address? I'm going there on the first of February.

BS: *Yeah. Let me put it here. I'll put it right in my notes. OK.*

NU: OK. It is Chapman Chair, College of Science, Engineering and Mathematics, 354 Natural Science Facility, University of Alaska, Fairbanks, 99775.

BS: *99775. Have you met William Ransom Wood up there in your goings and coming?*

NU: Uh, not this time. But I met him years ago when he was - he was Chancellor - President.

BS: *Yes, yes. He writes and calls me all the time with all kinds of ideas for different things. I think he's lonely. Yeah. I met him when I was at Barrow. But he's very interested in the Polar Society and so if you run across him, just say hello for me.*

NU: OK, I will.

BS: *And S_____ Akasofu too.*

NU: OK. Yeah. You know. . .

BS: *Not very well. I knew him when I was at Barrow and then I knew him - we had a conference up at the North Slope a few years ago and he was there just finishing off the building of the International Center and he gave a briefing on it and we talked.*

NU: OK. OK, Brian. Good.

BS: *OK.*

NU: And thank you very much.

BS: *Well thank you. What's your phone number up there, by the way?*

NU: In Fairbanks? 907/474-7758.

BS: 7758.

NU: And the secretary, you know I am not there a lot - just a sec. The secretary's name is Ali Cook and her phone number is 474-7608.

BS: *OK. I assume your e-mail's the same?*

NU: It will be switched over. Yeah. E-mail stays the same. It's automatically forwarded and Brian, I will inquire about Link Washburn and I'll send you an e-mail if I find out anything.

(300)

BS: *That's good. Karen Brewster. I've told her about you and that you're interested in the program. That you've been interviewed. And I don't think she'll need any help. She knows what she's doing, but if somebody up there inquires as to why we're doing all this, I'm going to suggest that she direct them to you.*

NU: OK. I'll make contact with her.

BS: *Good. OK. Well, my regards to your family.*

NU: Thank you very much and I hope you have a wonderful trip.

BS: *Well thank you. I'm looking forward to this one.*

NU: OK. Take care.

BS: *We'll see you. Bye. Thanks.*