Dr. Carl Benson  
June 22, 2001  

Karen Brewster  
Interviewer  

(Begin Tape 1 - Side A)  

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KB: This is Karen Brewster. Today is June 22nd, 2001, and I'm here at the Rasmuson Library in Fairbanks, Alaska, at the UAF Campus with Dr. Carl Benson for an oral history interview for the Byrd Research Center's Project on Polar Scientists. Carl, thank you very much for participating.  

CB: Well, thank you. Glad to help if I can.  

KB: OK. Well, why don't we just get started right at the beginning which is when and where you were born.  

CB: And why?  

KB: Well, I don't know, but maybe "and why?"  

CB: I was born in Minnesota in June, 1927.  

KB: June what, 1927?
CB: June 23rd.

KB: *Oh, Happy Birthday!*

CB: Tomorrow.

KB: *Tomorrow. Will you tell us a little bit about growing up . . . your childhood, your parents and family life?*

CB: Well, my parents were born of immigrant families from Sweden. Both my mother and father were born of immigrants who had recently come from Sweden, and my father was the youngest of nine. He was born in 1900, on a street in Minneapolis which has now been restored. It was one of these places that had been built for the workers on the Milwaukee Railroad. Interesting old houses. And they moved in there . . . it must have been in 1883, roughly. So, my father, was the youngest of nine, but four of these children died shortly after they were two years old. So, it was an amount of grief that is hard for us to even imagine today. And of the five that reached adulthood, his sister was just 21 when she died during the 1918 influenza epidemic. And my father was being drafted into the Army in World War I just as it came to an end. And I remember my grandparents on both sides, except my father's father who died when he was only 12 years old. And so, of course, I never met him. But, I was raised with two younger brothers - one born in '29 and the other one in 1935. And then my father died in 1942. He had pneumonia and I think there was tuberculosis in the family. We children all test positively for it, but it was bronchial pneumonia and then tuberculosis that killed him in 1942.

    In 1944, I don't know if you remember, but that was when the War was underway and rather than being drafted, I decided to go into the Navy. I was interested in Naval aviation, so in 1944, I was able to finish high school in '45 before going off to boot camp. But, I got out of the Navy in September, 1946, just in time to start at the University of Minnesota. There, I majored in geology and had two minors - one in mathematics and one in physics. And I met a lot of very inspirational people that had a lot to do with modifying my own views and helping me, one especially in mathematics, several in physics and, of course, several in geology. Bob Sharp, whom I met, was at
Minnesota then until 1947, after the War. He then went to Cal Tech and in 1952, I met with him again and started talking about possibly using my Greenland work as a Ph.D. subject.

KB: *He was a geologist?*

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CB: Sharp? Yes. R. P. Sharp. Robert P. Sharp. He was the Director of the Division of Geological and Planetary Sciences at Cal Tech from, well, I don't know . . . He wasn't Director right off the bat, but he was one of the longest running Directors and really shaped the whole division at Cal Tech in terms of laying the foundation for it's present approach to life. Well, having met him early on and then having some communication with him, I finished with my university bachelor's degree. I really finished in 1949, but I graduated in March of 1950 and went with the US Geological Survey to Northern Alaska.

George Gryc was very influential at that time for me. He visited the university repeatedly on his way to working on the Arctic Slope and he had originally been a member of the University of Minnesota, or graduated there. So, many of the young men that went up as field assistants, as cooks, as mechanics or what have you, came out of the University of Minnesota's Geology Department. And I had a marvelous field season in 1950, working with Irv Tollier and Bud Kent. We went to the Utukok River, starting over by the Etivluk River, very close to the site which is now called the Mesa Site - this archeological site that's been worked on extensively by Mike Kunz here from BLM. I think it's a fairly well known site. It's very close to where we started by the Smith Mountain Lakes. And we were there from May and we traveled on foot and used Weasels to cross between streams. Many of the streams had been mapped by riverboat going down from up into the Brooks Range, down to the Colville, so the geologic map at that time, in 1950, was a series of little places along the streams that had been mapped. So, you had the geologic contacts and rock types and the basic mapping done only along the streams. And the thing that we were doing in the '50s and since then was to do the mapping in regions between these streams, and so we moved from the east and headed west in the northern foothills of the Brooks Range.
Like I say, it was one of the really outstanding field seasons that I had. I had been wanting to get into Arctic and Antarctic work. Ever since I could remember, I was interested in Alaska. And when I was in high school, one of my friends wrote in the yearbook, good luck to me under the Ross Sea in the Antarctic. And the reason for that was that we had been involved with commercial diving on a small scale. We built our own equipment. This was before Scuba units were known. So, we had a pump, a hose, a lifeline and then we made helmets and we had weights around us and we would go down and mainly were repairing water intakes and looking for boats and motors and things in the lakes in Minnesota. So, diving was a real interest. And when I was in the Navy, I was an aviation machinist's mate, but I did a lot of reading on Navy diving which was the world leader in diving technology at the time. I think it still is.

So, that diving idea, although we spent a lot of time at it, I pretty well lost track of that when I went with the USGS in 1950.

KB: So that diving was when you were still in high school?

CB: We started it in high school and I did it at the university while I was an undergraduate at the University of Minnesota. And I also spent a lot of time traveling through the western states, working in logging camps in Washington and on the railroad in Oregon and Washington, picking up tracks and laying new track lines and just the sort of thing you do for labor on a railroad. I also worked in a pear cannery at nights sometimes. This was work that I did for a while to build up cash for the university, although the GI Bill was a great help in getting through. It's one of the fine pieces of legislation the country's ever come up with.

Well, after the experience with the USGS on the Arctic Slope, I went back to the University of Minnesota, started working on a Master's degree, and took an assistantship with SIPRE - the Snow, Ice and Permafrost Research Establishment - which since turned into CRREL - Cold Regions Research and Engineering Lab. I worked with that organization while I was in graduate school at the University of Minnesota from 1950 through the end of
1951. And then I went full time with SIPRE or CRREL, as it is today, from the end of 1951 through to the end of 1956. And at that time, I worked in the Central Sierra Snow Laboratory, learned a lot about micrometeorology, and worked with Robert Gerdel at SIPRE. He was the Chief of their Climate Research group. And we started looking into measuring the snowpack, deep snowpack in the Sierras and I happened to be there in that winter of 1951-52, when the heaviest snow on record came and a train called The City of San Francisco, was stuck and couldn't move. The highway, US 40, going over the mountain, was closed down for more than a month. It was quite an exciting time to be up there.

Bader, who was the Chief Scientist - Henri Bader - at SIPRE/CRREL, wanted me to go to Greenland with him and I left the lab in the Sierras, met with him, and we went in 1952, in the spring, to Thule Air Base which was just under construction at the time. One of the things the Air Force was concerned about was that Thule had no protection at all. This was during the Cold War. You remember Joe Stalin was still alive until 1953. And the base at Thule was truly a remarkable thing. It was, at that time, being built at a rate that I had never imagined. They were putting up, basically, a building every day. And the base had no protection. There was no radar protection around. No anti-aircraft. The runways weren't big enough to take the bombers they wanted to base there. So, Bader and I went out to several sites. The idea was to look at the ice sheet to see if we could figure out ways to measure the accumulation rate at different places and the temperature to come up with basic physical characteristics of the Ice Cap that were needed in order to put up a radar station. We had two sites and wherever the site was to be located, it had a lifetime dictated by the rate of accumulation because every year it's going to be buried by the amount of annual snowfall and there's no melting, so the next year you get in another layer and so on.

The first step was to know what is the rate of snowfall at the site so you know how fast it's going to be buried. And, of course, you needed to know the temperature to know if you'd be in any threat of melting, which is a real damaging feature for any kind of a structure you might put up. So, we started working on this and we had several very interesting experiences. In 1952, the Air Force was trying out a new aircraft, the SA-16.
It was a modified bolt hull with skis on the far ends of the wings under the pontoons so they would act as skis and it could land on land. Retract the wheels and it could land on water. The idea was, it could land on snow anywhere. So, it was a triphibian. It was a marvelous idea. But, the C-47 on skis was the work horse and it just had been working fine. And they thought this was going to replace it. So, they landed it out at what became Site 2 - a radar site, eventually. They couldn't take off. It was too heavy and the bolt hull had too much friction, so people suggested, "Well let's go in and land with a C-47 and bring all the people out." Well, that would have been embarrassing to some, apparently, because they said, "No, if this thing can't get in and out, then certainly a C-47 can't." Later, of course, C-47s landed routinely in that area with no trouble and had before.

But, during the time the aircraft was stuck, we hooked up with a search and rescue group to drop supplies to them. Let them build a house and built a little hut out there for shelter. And I had a great experience with that because it was a B-17 equipped for search and rescue. The bomb bays were set up to drop supplies and I flew with that thing all around in North Greenland on different functions, once I got to know their crew. And poor Bader was stuck for about 2 weeks out there.

KB: *So he was on the . . .*

CB: He was on the plane. He had gone out there . . . it didn't crash. It just landed and couldn't take off. But, for some reason, that morning, he was going to go out on that flight. I had something else to do. Otherwise, I would have been there with him. But, eventually, they brought out a great helicopter, brought the people back, stripped the plane and were able to fly it out. And then, to rescue the stuff that had been taken off the aircraft, we formed a small group - the Air Force did - under Don Shaw of the Arctic Desert Tropic Information Center. It was going to go out and pick up the things that had been abandoned, bring them back to Thule and I wanted to go along because I was interested in getting a traverse on the Ice Cap. And Shaw agreed that I could come but, we couldn't spend any extra time digging holes and doing measurements. I thought, that's OK, I'll just do that at night. It's light all the time. So, we were driving and working during the day. Every time this thing would stop, I'd get out and dig a hole and do the pit studies I wanted to do. And I remember being so exhausted after roughly a week of that that I was having a dream. It seemed like a dream, but I heard a police whistle going over and over and over, and we had stopped with
the Weasels for some reason and Shaw pulled up along side me and he was trying to wake me up. I had fallen asleep sitting there as we were stopped. And he was right within a foot from my head, blowing this police whistle to wake me up and I thought I was in a dream. Well, that was a pretty good indication that there's a fatigue factor that sets in, even when you're young and full of energy.

Well, I went back to Greenland in ’53 and ’54, but in the winters from Greenland, went back to the Central Sierra Snow Lab and then we were doing some testing of snow compacted runways. And this was going on in Northern Canada, near Kapuskasing. It's up in the James Bay area in northern Ontario. And the Canadian Army and Air Force were working together with the US Corps of Engineers which CRREL was part of. So, I worked with them for 2 years up in Kapuskasing, parts of the winter and part over in Goose Bay Labrador, where we were building snow compacted runways. And also, we were testing them on the Greenland Ice Sheet.

Well, that was sort of a diversion from this main stratigraphic work that I wanted to do and that the Corps of Engineers and the Air Force agreed needed to be done because they had to understand the properties of the Ice Cap if they were going to build on it, travel on it and they just needed to know more about it.

The program was overwhelmingly big. I just can't get over that. When we look at the way we budget things today, and think back at how things were going then, there was never a nickel exchanged. There was never talk of money. If we needed Weasels, if we needed an aircraft, we just put in the request and we got these things.

KB: And this was at CRREL, or SIPRE.

CB: SIPRE was the organization at the time. But, for the work in Greenland, we had Weasels, we had aircraft and I was just impressed that we were able to use this equipment when we wanted it. Thule was being built, at the time, and by 1954, it was the biggest US Air Force Base in the world. The only one that exceeded it was the SAC Headquarters at Offutt Air Force Base in Nebraska. Well, this essentially year-round field work that I was doing with CRREL - I may say SIPRE or CRREL, it's all the same - working in the winter on these various projects and
then as soon as the buds would appear anywhere on a tree, it was time to go back to Greenland. So, it was about four years of perpetual winter for me, which I enjoyed every minute, I must say. I guess not every minute. There were lots of frustrations.

KB: What were some of those frustrations?

CB: Well, in 1953, we had a project with the French to go in on the Ice Cap and the Air Force forgot to ask the Danes for clearance to go. So, we had to curtail what we did. We thought this was crazy. We'll have to make sure that doesn't happen again. In 1954, we got to Greenland, and the Island Commander based at Thule, the Danish commander, said they hadn't heard of our group and we had no clearance to go where we wanted to go, so again, we had to modify where we went and what we did. 1955, I pleaded with people that we can't let this happen again. It happened two years in a row. 1955, we got to Thule and found there was no Danish clearance to do what we wanted to do. So, we managed to find that they had garbled up something. I think what we were planning to do was considered an Army tractor train. We were using Weasels which I guess you could consider tractors. But, they didn't count that. We ended up modifying the plan that we had there and actually made it work. Partly, our navigator on the trip, decided that if they had made errors, we would make navigation errors too. So, we called in positions that were always in the cleared area.

We managed to carry it off pretty well exactly as planned. But, going to Thule. You asked about frustrations. We left from Westover Air Force Base in Massachusetts, and we flew in the big C-124 cargo planes which perhaps people today don't even remember what they look like. They were enormous. A Globemaster, it was called. You drive things in and shut the door and fly it anywhere in the world.

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So, we were going to fly these four Weasels that we had specially rebuilt for Ice Cap work, based on our past experience, and fly them to Thule. Dick Reagle, who was the assistant leader on this 1955 expedition, stayed in Westover. We had agreed that I would go with the first Weasel, get to Thule, and then he would wait until the other
two had been shipped and he would go after the total of four had gone. And then he came up with the last one. We got to Thule and found there were only three. Where was the fourth one?

It turned out that it was off at Goose Bay, Labrador. The C-124 had landed and had some trouble getting it's doors shut. They unloaded the cargo. The motor pool officer saw this fantastic Weasel coming off all decked out in ways he hadn't seen before - a special cab, a roof rack to hold things on, all the pontoons adapted to hold fuel cans - we did a lot of work on these things. So, he put it in his motor pool. And we actually had to send someone. We couldn't get through without sending someone down to Goose Bay, Labrador, to get this thing, put it back on a plane, and bring it on to Thule. Now, you talk about frustrations. It sounds funny now, but when you're going through this and you're waiting for weeks. And then we had to build wanigans.

KB: And you also had a limited field season. Not having that Weasel delayed your trip, right?

CB: That's why we went early. Well, it didn't delay it. People would ask, "Why are you going so early to Greenland?" We went as early as March. This particular year we went in April. Early . . . well, at the end of March. "But, you're not going to go out on the Ice Cap until May." "But, we need this time." The mentality in the States was that you get there, the Weasels are sitting there idling at the curb. All you have to do is get in and go. Well, of course, nothing could be further from the truth. We had to build our sleds. We built the fuel hauling sleds, built the wanigans. These were assembled there in Thule. Some of the sled design had been put together in Wilmette, Illinois, where the SIPRE headquarters was. But, the basic wanigans we needed for a lab and for living space, we built right there out of parts at Thule.

Well, you talk about frustration. The Corps of Engineers had heard there was an Arctic Transportation Group, Transportation Corps, and the one person that we were supposed to see within the Corps of Engineer group wasn't to be found. And we found from someone that they had seen him and that he was probably in his barracks and didn't think he could get up. He was so sodden with alcohol. He was in such bad shape that they had to fly him out to the States. He was really, really in bad shape. And this was our contact. We went to the Transportation Corps and when we asked for equipment, they said, "No, if you're with this engineering group, they owe us a backhoe, a bulldozer, dragline." There were all kinds of problems they had with it. So, they wouldn't give us anything. So, I
went to the Air Force and they didn't recognize, really, what we were doing. We weren't part of their operation. So, I saw a friend of mine, Bill Perkins, who I had met two years earlier. He was head of the construction of the radar site at Site 2.

I told him my sad tale, that we had no place to work, we needed access to parts and equipment, and he just held up the keys to his car. He said, "That yellow pick-up out there, it's yours as long as you need it and I'll get you some space in Hangar One," where the contractors were working, "and you can use the dunnage heap." He gave us permission, so we built our sleds out of the dunnage heap using space from a contractor that had nothing to do with us. And it's interesting when you think this was the longest trek that the US ever did in Greenland and still, I think, in terms of time and number of stations, I don't know of anything that matches it. And yet, we were essentially dependent on personal friendship, a borrowed truck, borrowed space in a hangar, and putting things together from the dunnage heap. It was fascinating, the little incidents that would come up. When you asked about frustrations, that's just sort of a thumbnail sketch of it.

KB: Why don't you tell me a little bit about the trek and the traverse and what you were doing there and what you discovered?

CB: Well, we found a way to basically determine the annual layers and we would verify this by doing repeated pit studies in the same area year after year. We would dig into a place that was undisturbed by the pit from the previous year which, of course, fills in very fast because of the wind-blown snow. And so, a year or so later, you can't find these pits except by the stakes that might mark them. Well, we found by repeated studies that we had a usable stratigraphy and then in 1954, and again in 1955 - especially in 1955 - we used oxygen isotope ratios, working together with Sam Epstein from Cal Tech because I had been in contact with Bob Sharp and Epstein at Cal Tech and the oxygen isotope determinations, the O-18/O-16 ratio had proved very useful in some glacier studies in Alaska just in terms of getting an altitude gradient for its variability. And it looked like it might be a good way to look for
annual gradations in the snow, in the oxygen isotope ratios. Because in the ratio changes, it's the normal oxygen 16 and the heavy oxygen 18, is depleted in low temperatures - it tends to precipitate out - the heavier one precipitates out faster. So, the ratio of 18/16 varies and it varies quite significantly from say -50 to -10 in a year. And you can measure it to a precision of .1. This is instead of per cent, parts per hundred, it's parts per mil or parts per thousand. That's the way we measure it.

But, after we had several good places to calibrate this, we found the oxygen isotope ratios were giving a beautiful representation of the annual units. So, we used that in cooperation with or in connection with the other studies we were doing, just stratigraphically measuring layers and carrying them from one pit site to the next, at 10 mile intervals from the coast in for 100 miles.

Well, in for basically 200 miles, to the first station of the first radar site. From there on, we moved at 25 mile intervals to the center of the Ice Cap and then headed south, which was at 77 degrees north. We headed south, went all the way to 70 degrees and then out to the coast through the French Central Station. And in addition to working out an annual stratigraphic layering and determining the water equivalent of the layers, we measured temperature and the temperature variability across the whole ice sheet then, first by seeing that the temperature at 10 meters deep, where the mean annual temperatures of the air is well represented by that 10 meter deep temperature. At that depth, the temperature doesn't vary by more than about 3/10ths of a degree, summer and winter. So, we found that we had a very good gradient of altitude where the dry adiabatic lapse rate was controlling the altitude gradient of temperature along the surface and we used that as a guide, combined with a latitude gradient that we measured above the altitudes where no melting took place. And that correlated pretty well with the gradient along the coast from Upernavik, north to say 82 degrees by Alert, on the Ellesmere Island.

Using those two gradients, we drew up a temperature isotherm map of Greenland that is not bad even today. And then, from the accumulation, we made an accumulation map of Greenland. We did some corrections on the annual balance that had been first done by Bauer and Lovee the several efforts at mass balance. Our measurements showed that the mass balance was more positive than any of the previous estimates, but still very
close to equilibrium. The conclusion we had was the ice sheet was so close to equilibrium that your present methods couldn't detect any deviation from it.

We also measured and came up with a model for the densification of snow - how you turn dry snow into glacier ice just by the overburden of added layers. And we came up with a fairly good picture of that. Also, we found that different regions on the Ice Cap, where you have a rock formation that varies from limestone, mudstone, silt to sandstone, to coarser gravel in a single formation, these are different facies in a formation. We found that the ice sheet could be looked at as a geological formation and it had facies in it that were governed by the extent of the melting that takes place or doesn't take place in an annual unit as it goes through a year's cycle.

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So, we came out with a region above which no melting takes place and that was called the dry snow facies. Below that, you get partial melting, but not completely melting the whole year. Yet there's percolation in small ice masses. We call this the percolation facies. And then the bottom end of that, the snow wets all the way through the whole layer. That was the wet snow facies, down to the snow line where the individual annual unit is melted away completely and you end up with bare ice. And with that pattern we managed to draw together, by 1960, a facies map of Greenland and that checked out very well with the synthetic aperture radar studies done, well let's see, it would be 35 years later, really, by Mark Quanastock and others at NASA. The dry snow facies gives no radar return, so you get a dark area. The percolation facies with all the irregular ice masses in it are excellent radar reflectors and so you get a brilliant radar reflection and that forms a band all around the ice sheet, which turns out to be the brightest radar reflector on the planet - that percolation facies in Greenland.

And then you get the wet snow where you don't get the same brilliant reflection and then the bare ice. The bands, the glacier facies that we set out as a result of this pit work were beautifully verified and, well it was satisfying that it just agreed so well with the satellite determinations with the synthetic aperture radar.

KB: *Tell me about the traverse. You said it took 120 days?*
CB: The longest one in 1955, was that long. Earlier, they were probably just a couple of months at a time. But, this long one, we had to do a lot of very careful planning to bring equipment out to build a tower at the French Central Station after we found it. We had planned our air drops well ahead. They were all fixed up by us at Thule on pallets, in barrels, numbered for the air drop and the number of the individual barrel. The drops were made by the Air Force using C-54s, sometimes C-119s, and also C-47s for some of the drops.

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But, the main dropping was by the C-54, which is equivalent to the DC-6 in the commercial line. And then the cargo plane, the C-119, where the whole back end opens and the barrels just tumble out the back. All these drops were made without parachutes. We had a system worked out that elaborated on what the French had come up with for free drop. You'd set up a drop zone and the aircraft would fly at an altitude of about, oh . . . not much more than 5 meters above the surface. And they'd fly like this for perhaps 5 or 6 miles and then be over the drop zone, drop, and then make another flight. And being in radio contact with them on the ground, we could see and judge their altitude better than they could often judge it while flying. So, they would depend on us to say, as we did have to tell them several times, just abort the drop and climb as fast as you can and get out of here. And then they'd made another run and do it at the proper altitude. It couldn't be too high and it couldn't be too low. In four years, we dropped over 100,000 lbs. of fuel and food and various bits of equipment without any loss, basically. We did lose a few things, and curiously, those were special items that were dropped by parachute from higher up. But, the free drop with these very heavy-duty barrels and things packed the way we had them, it was remarkably successful.

KB: And how did you live out there? Were you in wanigans or were you camping?

CB: We were camping. We had no heat overnight because we were afraid of fire problems. We had sleeping bags and several people slept on cots that we set up inside of the Weasels. There were six people - two were sleeping in Weasels and two each in two of the wanigans. We had set up four bunks and in the daytime, the bunks would go up and we'd use them for - one was a mess wanigan where we'd eat and the cooking generally was done in a big tent
outside because, when you're cooking, the water and condensation from heat and especially the water evaporation would condense into sleeping bags or into the inside of the building - I shouldn't say building, it was just a canvas covered metal hoops. We had insulation in there. The insulation - a lot of it - was made just by cardboard. Old scrap cardboard boxes and we stuffed fiberglass down in the inside just to insulate the canvas-covered wanigan. But, the cooking, like I say, to do it inside the wanigan usually ended up with grief by all the water that was there. You had to do a certain amount of that. But, to avoid any kind of fire danger or also the danger of CO by having carbon monoxide with an enclosed burner, we would just rely on body heat in a sleeping bag. And another trick we would use, all of our food, of course, was frozen. If you wanted something thawed for the next day, what we would do was take it in the sleeping bag with us at night.

KB: *And you said there were 6 people on this.*

CB: The longest one, there were 6 people. That was about the general size - anywhere from 4 to 6 people.

KB: *And what did the different people do?*

CB: Well, we had radio contact with Thule every day. We wanted someone along who was primarily a radio man. In 1955, we had a man, Jim Holston, who was the radio man and had a radio of his own exactly like the two that we had with us on the Ice Cap. We had one radio in the lead Weasel, one in the radio Weasel, so that if we separated the group, we'd be in radio contact. The radio contact was all done by CW instead of voice and we just found that you couldn't rely on voice communication all the time. But, the CW, you know the continuous tone Morse code system, that would work beautifully. In the past, when I had been working there, when the Army was setting up, they'd usually like to have a mechanic along. Someone with some kind of medical background just for emergencies, and then a navigator to keep figuring out where you were and set the course.
And I found that working within the military didn't work very well because the people didn't have enough to do, or
motivation on the trip. And if it would get to be a long trip with a small group, it would seem better if we could have
every person selected knowing what they were getting into and each person have a research project to work on. That
was part of my recommendation when I was ready to leave SIPRE in 1954. And, as I said, Bader said, "Well, these
ideas look good. You can modify the Weasels, you can select the people," and, well, he just offered me to do it. So, I
called Bob Sharp and said this is something I can't turn down. I'm going to do this year and then I delayed my
entrance then for a year. So, we ended up with an MD who wanted to go out on this expedition and do physiological
studies on us. And we had several ideas that being isolated for this length of time, we wanted to get some idea of
how the bacteria count on your skin and back at the base of your nose - how these changed with the time and
whether, with a group, if we measured this on everybody before we started and half-way through and then at the end,
we were going to get some idea of how much mixing of the "bugs" took place by living this close together for
that long. And the navigator was George Wallerstein, who was an astrophysicist at Cal Tech at the time. He was
finishing his Ph.D. there. He came along and did a research project on refraction of the sun below the horizon under
different situations on the Ice Cap where we'd have a long hour with low sun. It was an ideal set-up for that.

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Also did a study on magnetic declination as we moved. Then he would determine the position with a theodolite
getting the sun shots. And we built a gyro. Dick Reagle, who was the assistant leader on this, and another
glaciologist who was also a Navy pilot - had a lot of experience with, well obviously with flying and with glaciology

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(Begin Tape 1 - Side B)
CB: . . . to run the vacuum pump, we put the small engine up on top of the navigation Weasel and had the vacuum pump down inside and the gyro and we were able to hold a good vacuum and to run the gyro with no problem that way. This meant that when we stopped, we determined an azimuth. We'd have a day and a half at each stop. We knew the azimuth either when we came in if the storms kept us from seeing the sun and we'd get the gyro set with the proper azimuth and start the engine before we'd start the Weasel to go and so we'd have the gyro running and indicating our direction. We used the odometer on the Weasel to determine the distance from point to point. When we got to a station, we'd start digging the pit and do this in the evening when we arrived. Then we'd roll the glaciology sled over the tarp, over the big canvas tarp - one of the runners - and we'd use that then as a wind shade and a cover for the pit. We'd lay 2 x 4s over the top, put down the canvas tarp and then the next day we'd finish the pit. But, we'd go as deep as we could the first night and measure temperature. The next day we'd spend basically the whole day working on the pit and into the next morning until about noon and at that time, we'd have the navigation Weasel start and the radio Weasel would go ahead and then the mechanic Weasel and the glaciology Weasel would come on 6 to 10 hours later, following in the tracks, depending on if it was terribly stormy, we would not wait that long. But, we'd put in markers every 10 miles. We had stakes that we put so it would help us in the latter group to keep track of where the first bunch had gone.

We used this separation of two groups in travel for another reason. The first group would take readings on an altimeter every mile and in some cases on these long treks, we'd do it every 5 miles - take an altimeter reading and do a (ramson,ramsohn?) study. And then the next group coming along would have been stationary all the time the first group was moving and that stationary altimeter would be read on an hourly base to give a track of what the air pressure was doing so that it could correct pressure changes from actual altitude changes in the moving vehicle. And then as the second group moved, the first one would read the altimeter every hour while that one was traveling and the second group then would be getting altitude records as it traveled. So, the two put together gave us a good picture of the altitude across this area. When we were doing this, the maps were not all that accurate and so that was another thing we were doing was carrying an altitude line from Thule into the center, down to the side, then back out to about several 100 miles from Sondre Stromfjord where we ended up.
KB: _So everybody on the crew helped to do something._

CB: Well, yes. The medical man had something. The navigator did and the radio man was interested in the radio and he was also putting together something of a story that he made good use of for several years as a radio announcer in Indiana. The mechanic did his Master's thesis - he was a mechanical engineer who had also worked a lot on stock car racing and he knew the Studebaker engine and the whole thing was easy for him. These are easy things to work on. So, he kept us in good shape mechanically, but we did drawbar pole tests for him to see the effect of traffic pulling in the snow with a Weasel and a sled. There were a lot of interesting things, what happens both to the snow and how the vehicle performs in it.

(50)

And he did a Master's thesis at Northwestern on the research that he did on the trip. So, everybody kept pretty busy and that was a much better operation than trying to do it with military people assigned just to do a specific job that didn't keep them busy all the time.

KB: _Did you ever have problems with the two groups being so far apart. I mean 6 hours difference is a long time._

CB: No, the maximum difference was 25 miles. And then when we'd arrive, we'd start this pit. We'd finish working a whole day at the site and into the night and into the next morning as far as the glaciological work went. The glaciological work took at least 2 days at a site, so we'd break it up into a piece the first evening. The full day and then the next morning and then travel. So, we had that pattern down and basically followed it with the exception of air drops, which were located on this thing where three air drops and then the stop at the French Central Station. Otherwise, we held to that schedule without a break for basically 120 days. With the air drop, we'd add a day. It took a full day just to go pick things up and get them loaded. It was a big deal to get an air drop. And that's when you get your mail. Come in with a bundle of letters and of course, you couldn't send any mail out. My wife was, at that time,
we were engaged and she was very good at writing letters and sending cookies and doing things like this. So I'd get a big wad of letters and I would line them up according to postmark and then read usually one a day or a couple a day. It depended on how impatient you'd get. But, I liked to piece them out because I knew we weren't getting any other mail for a long time.

KB: *And with those pits you were talking about before... those are 4 meter deep pits that you had to dig by hand.*

So, did all 6 people help dig or were you doing that by yourself?

CB: Oh no. We'd shift around a little on the digging. We had help and we worked out a system where these pits had good clean vertical walls. They were easy to work on and that was a fair amount of exercise.

KB: *What were your experiences with a small number of people in close quarters for an extended period of time?*

CB: Well, it depends to a large extent on what they take with them in the beginning and how dedicated they are to what they're doing. Some people had more problems than others and I'd say that there'd be some tensions built up here and there, but in my mind, it wasn't that big a deal. I think some people... if they got really to thinking about, "Well, this is really longer than I thought it was going to be," even though we were following the plan we had set up initially, I found... I've been on quite a few treks like this and there's an interesting thing that halfway through the time that you're there, there seems to be a little change where people start, after halfway, there's an effort to say, well, they want to see things come to an end. Whereas building up to that, it seems easier. It's a strange phenomenon, but I've heard that other people have said the same thing if they've got some long trek. There's something about after you're halfway there, there's an inclination to want to speed it up.

KB: *And so that 1955 was the 120 day...*

CB: That was the longest. Each one of those years, '53 and '54 were long sessions. The same sort of thing. We built the pattern and what we did in '55 was based on the experience of these, especially '53 and '54. 1952 was just a crash
effort of going in and finding out . . . getting some rough idea of what was going on. And it was rough, but it laid a
foundation that was essential to build on.

KB: So now, after this, you were doing a nice chronology of what you had done and we stopped here. And now, what comes next?

(100)

CB: Well, when I left CRREL and went to Cal Tech and worked on this material for my dissertation and was involved in courses and just basically was there from '56 to the beginning of 1960, and then I came here. And this paper, then, was published by SIPRE in '62, which was based on the dissertation which was done in 1960. When I came here to the Geophysical Institute, I had been interested in coming back to Alaska and I knew Neil Davis. He and I were roommates for a while at Cal Tech in graduate school. And talked a lot about coming back here and the Geophysical Institute sounded like a good base. He was here and we exchanged letters and I met Chris Elvey who was the Director at the time. And he visited me there in Pasadena. And then we had one child in Pasadena while I was in graduate school and another one on the way who was 6 months old when we headed up the highway. So, I went to pick the family up from Pasadena - they were in Southern Minnesota - and then we drove with a trailer behind a Chevy station wagon and pulled in here in about, I can't remember the exact date whether it was March or April, of 1960.

KB: And you arrived here to do what?

CB: To be a Professor or Assistant Professor at that time in geophysics and I taught also in the Geology Department. Troy Payway was head of the department. I was interested in teaching some and then also my main function was research within the Geophysical Institute. The first projects that I really started on here were - one was low temperature air pollution, ice fog problem. And that has gone on intermittently and was a very interesting
project to get into. And SIPRE/CRREL was interested in it and Gerdel set up a contract with us to start doing some research on it.

Then I did work on stream freezing. One thing I'd been interested in for a long time was the freezing of turbulent water in small streams where they'll flow for a month after you form a good ice cover on Ballaine Lake or something out here. You've got little streams flowing and there's a fascinating problem of supercooling in the water that's responsible for what happens and the Goldstream Creek out here is a place that I picked for a study and we actually finished, two students finished Master's degrees on that project, and then we started working on the seasonal snow. The seasonal snow cover here is a perfect setting to study the formation of these coarse crystals - this depthhoar which forms at the base of a snowpack and that is one of the critical things in determining the annual layering in the Greenland or the Antarctic Ice Sheets. These alternating layers of depthhoar and wind slab. And no place in the world is it better set or certainly places like interior Canada, Siberia and interior Alaska where you have very low winds, very strong temperature gradient across the snowpack, and here it lasts for well over 100 days. Whereas in most cases, on forming this sort of depthhoar in an ice sheet or any glacier, the conditions of formation will come and go, but they won't last for say more than a couple of days before they cycle through something else. Here you can have a strong temperature gradient with temperatures at the base of the snowpack running at about -3 degrees. It's a very close melting point. Whereas on top, it will run as low as -40 or -50. And that temperature gradient across a snowpack of 40 or 50 centimeters is a driving force for setting up vapor transfer and the growth of these large crystals at the base of the pack and the transfer of mass through it has formed a marvelous experimental setting.

And I started building tables up in a wind sheltered area down by the university farm. The table was high enough so that it didn't interact with the snow or the ground below, so you had air under the table. And we put a white surface on it - either painted white or just white paper and plastic so that it would reflect any solar radiation hitting it. And that was basically at the beginning of the season. And once we built a snowpack on that, we had the same snowpack forming on the ground as we did on the table. But, the physical properties of the two were
completely different because on the ground, you have a high temperature source at the base and you have a strong
temperature gradient. On the table, you have no temperature gradient at all, so you don't form any depthor at all on
the table. And you have a very fine grained snow that just is like it fell, basically. You can see the layering persist all
through the winter. The layering on the ground, the snowpack on the ground, tends to be destroyed by the formation
of the depthor and the mass is transferred from the base up into the upper part of the snow and here where the
temperature at the bottom of the snowpack remains below freezing, you have a transfer of mass from the soil below
up into the snow. Now in most places where people live where there is a snowcover, the base of the snow, that is the
ground snow interface will be at the melting point and there can be some transfer of mass from the snowpack down
into the soil below to keep it moist or to keep it from drying out.

Here it's the opposite. The soil actually dries out under the snow. We've measured water content of the soil
under the snow in the top 2 centimeters going from more than 30% water by dry weight of the soil, to less than 6%
as the winter goes on. So, it's a drying from a hard frozen mass to a dry powder that you can crumble in your fingers.
And this is a very interesting piece of the environment here. But, in studying the seasonal snowcover in some detail,
it's been a marvelous site for research and we've done papers on it. And Matthew Sturm did his Ph.D. on this
measuring convection in the snow. And that's been a very interesting project on it's own.

Then a lot of the work that we did was on the Arctic Slope. The snowcover there is totally different from
what it is here in the interior. And breaking down the snow into three major types in Alaska has been one of the
things we've worked on for a number of years. There's the tundra snow on the Arctic Slope and in higher areas here.
Then there's the tiaga snow in the interior which is the sort of thing I just described. And then there's the maritime
snow in the southern mountains which is very much like the maritime snow in the central Sierras all the way down
in California.

Another thing we've done a lot of work on here was the glacier volcano interactions and the Wrangell
Mountains have been the main source of action there since the early '60s. And that took off or followed on from
research that was done in the '50s when Terrence Moore was president here at the University of Alaska. He and
Serge Korff from the University of New York, worked on a cosmic ray program and Moore actually did the flying.
He served as the bush pilot for the operation and did landings up at the top of Mt. Wrangell at 14,000 feet in his
Supercub on skis.
And people like Fred Mylon, Bob Elsner, Bucky Wilson, they were all involved in these cosmic ray expeditions from '53 and '54. Bob Elsner I wanted to come to Greenland with me in '55 for this long expedition, but he was tied up on this work here on Mt. Wrangell. So, then in '61, the first season I did on the summit of Mt. Wrangell, we found we had . . . the main reason we went there was to look for a place in Alaska where you had all the glacier facies from the dry snow through the percolation wet snow and down into the ablation. And Mt. Wrangell looked like the ideal site for you to have the whole suite of facies that you have scattered over a huge distance in Greenland . . . you'd have it all in one mountain here. And it turned out that's exactly what we do have and we've made studies on it and done research on this now since 1961.

In 1964, with the Prince William Sound earthquake, the mountain was shaken enough to trigger some major changes in the heat flux at the summit. And we spent a lot of our activity following up the kind of changes and using the melting of the ice as a calorimeter to get at the heat flow from the volcano. And that's a study that we're still active in. Now there have been several theses and Master's theses - Roman Motika did his Ph.D. work on that problem - and it's a fascinating subject. Still underway. We're linking that in with studies on Mt. Logan and Mt. Bonachurchill. We look forward in the future to doing cord rolling across this whole basin.

KB: What did you discover about the snow accumulation on the North Slope?

CB: Well, the main thing about the Arctic Slope was few people knew how much there was. The Weather Bureau gauges are notoriously bad in missing most of the precipitation. This had been first picked up by Bob Black of the USGS and he published on it in 1954. All the gauges at Barrow record much less than the actual precipitation that comes in. We followed up with this measuring on the tundra, comparing it with the gauge and by the end of November, we had more snow on the tundra than the gauges were picking up through the whole winter. Then, we designed other types of gauges. The Wyoming Snow Shield that was developed by NOAA money in Wyoming is a gauge similar to the Russian gauge which essentially is two concentric snow fences with the gauge in the middle.
But, it's a shielding on the gauge. People have experimented with shielding precipitation gauges for a long time and always there's this problem of the gauge undercatching the true amount of precipitation. So, we installed, with Ron Tabler and R.A. Schmidt from the Forest Service, I worked together with them in the mid-'70s, and we installed some Wyoming Shielded Snow gauges on the Arctic Slope and it was very interesting to compare those values with the Weather Bureau unshielded gauge and what we were measuring on the tundra.

(250)

And then we also measured snow in big drifts that don't fill up. These were drifts that essentially catch all the snow that comes from a certain direction. And we found there are two primary directions of wind transferred on the Arctic Slope. It's basically the storm winds out of the west and the prevailing winds out of the east. There are some variations, but very close to being 180 degrees apart in direction. And we found the drifts form year after year in the same places. They only vary in size and by measuring these drifts and the water equivalent within them, we've got some idea of what the flux of windblown snow is across the Arctic Slope and it's variable. Extremely variable from year to year in quantity, but not in direction or basic shape. The average snow from the east, prevailing winds transport about twice as much snow as the western winds. And they run about 70 tons of snow per meter perpendicular to the wind through the winter. And the western winds run about 35-40 tons of snow per meter. So, what we're trying to do is get a better feel for the amount of precipitation on the Arctic Slope which looks like it's been underestimated at least by half and from a hydrologic point of view, that's a fairly serious error. So, we're still working on that sort of thing.

The structure of the snow varies a lot. The Arctic Slope and the region just say from the Brooks Range north, the base of the snowpack temperature can drop very low, -10, -20 degrees whereas here in the interior it might drop down to -10 early, but once the snowcover forms, it comes up and it's about -3 to -5 degrees all winter. In the south, once you get south of the area of let's say Copper Valley and beyond the mountains in the maritime area, it's at zero degrees at the base. So, there's a very clear difference. Matthew Sturm has worked on this state across Alaska in much greater detail following up what we started on.
I guess the other thing I've been involved in here is with students. I don't have a list with me, but both Ph.D. and Master's students - there are probably 30 MS I've been involved with and I can't remember exactly. If you want, I could get the numbers on it.

KB: *Your best guess.*

CB: Well, I think there's been 15 Master's that I've actually been chairman of and 3 Ph.D.s, but the total involvement with both Master and Ph.D. is probably close to 40 - between 35 and 40.

KB: *I think I just lost count.*

CB: . . . a summery or list, you know. If we wanted to take a break, I could get some lists like that or at least have numbers better than just off the top of my head.

KB: *Well, maybe we could just talk more about what it was like working with students and what that's meant to you.*

(300)

CB: Well, I'd say I've learned an awful lot from students. And one of the exciting things is to see in teaching at either the undergraduate or graduate level, is to see somebody really pick up on a subject and understand something - the spark. There's something that you can tell when people really see something, especially if there's a problem they struggle with and all of a sudden they really understand it and that's a great experience. You share that experience to some extent. I'd say this ice fog problem that I mentioned, Sue Ann Boling here did her Master's thesis and Ph.D. on subjects related to that and a very good study on the radiant effects of ice crystals radiating in the air in addition to the radiation that's taking place from the air itself. In other words, adding the crystal base. I worked on this with Ingvar Gotholson, a Norwegian who was here and we published a paper on this some years ago in the *Journal of Applied Meteorology.* And Sue Ann picked up on that and just took it further. What we did was to treat
the ice crystal as a radiating black body within a little element of air. And she looked at the sort of a gray scale across the whole spectrum of sizes and this is a subject that's receiving more attention now with the atmospheric radiation measurement program, which I've been on their Advisory Board but not doing any research with them. But, I'd say it's been exciting to see the development. I'd say I've learned something, just like the students do, from each one of these projects when you carry through to a Ph.D. It's very much like going in and doing it over again yourself.

And working with students, the ideas that you have and that they have that they can flush out, you can do so much more when somebody is really keenly interested in something and working on it. It's a completely different problem than as if you were to take on some work and just hire helpers. So, there's a marvelous interaction between a professor and a graduate student that you have to pretty much go through to understand it, I guess. But, I've enjoyed that very much.

KB: And what about your experiences in the Antarctic?

CB: Well, I went to the Antarctic in 1961-62, and I went down there in cooperation with both what was then the Center for Polar Studies at Ohio State which is the Byrd Polar Center now. And I worked with a group at Byrd Station. Well, actually, I was alone at Byrd Station and at the Pole, but I worked together with Mario Giovanetto and others from the University of Washington on Roosevelt Island and we tried to determine the accumulation rates at these places. In Byrd Station, I did a comparative study of northern Greenland at just about the point where our turning point was in '55 because the rate of accumulation and the mean annual temperature are almost identical to what they are at the Byrd Station.

So, what I was interested in was how does the stratigraphy and the physical properties of the snow in general vary at Byrd Station compared to this point in Greenland where the basic parameters are the same. Well, it turns out there were differences and the explanation I could give was that there were parameters that varied between these two
places that we didn't have to consider at either one. One of these was that the range of temperature variation from the average temperature - the deviation from the average temperature was greater in Greenland than it was in the Antarctic and it looked like Byrd Station was in a much more maritime environment in that the temperature fluctuations from the mean were smaller. And also, the wind at Byrd Station was significantly higher than it was in these points in that part of Greenland.

Across most of Greenland, the wind pattern is probably similar enough so that it wasn't a variable from point to point. But, when you start considering that position with the Antarctic, then the difference in wind came up. And I found that the density of the snow was higher - was clearly measurably higher at Byrd than it is in these points in north Greenland. So, the two factors that I mentioned, temperature and wind, would both contribute in that direction. If the temperature deviates from the mean to a greater degree in the summer, this means you've got higher temperatures in the system, in the snow layering, than you do at Byrd. In both cases you have the same gradients, but you shift them to include some higher temperatures in Greenland and the vapor pressure of water over ice varies strongly with temperature. So, if you get higher temperatures involved in the system, you're going to have more vapor transfer, more deposition formation, which means lower density. Also, in the Antarctic, because you're going to have lower density with less deposition formation - I mean higher density because of the less deposition - you also have higher density because of more wind packing. So, the wind packing and the range of temperature both work in the same direction to make the density values higher at Byrd Station then they were in the north Greenland Stations, even though mean annual temperature and rate of accumulation are basically the same. So, that was an interesting result and my experience in the Antarctic came to an end because my wife was pregnant at the time I was down there and had difficulties and ended up losing the baby while I was there. And I didn't even hear about this until a couple of weeks after the fact because the communication wasn't very good.

(400)

And then I headed back. This was back to Alaska and that was the winter that we had a record-breaking cold that still stands. The temperatures stayed below -40 for over 10 days. The maximum didn't get up to -40 for that long in December of '61. And I decided as long as I was going to be working in Alaska and living here and doing
field work in the summers at my wintertime here and there was plenty of field work to do, but to go down all the way to the Antarctic and leave my family at risk which is what I found I had done, bothered me to the point that I decided not to go back to the Antarctic myself. Now many others have been going to the Antarctic and now it's quite easy to go down there for a short piece of time and come back, which it wasn't at that time.

KB: That's one of the things I was going to ask you was what it was like working in the Antarctic in those early days?

CB: Well, they weren't that early. Well, the first wintering down there was '57. My youngest brother was at the wintering site at the South Pole Station with Paul Siple and so I had had ties with these people and I was really hoping to go down to the Antarctic and I know people - Bert Crary and Larry Gould and others had wanted me to go down there while I waited, but I was finishing my Ph.D. and when I came here, I made that one trek down there basically as soon as I got here. But, the timing just wasn't right for me to be involved in the follow-up after four consecutive years of snow-traversing in Greenland and then finishing this up. And the work that I did in Greenland was no good unless you spent the time thinking about it and writing about it and analyzing it. And it took quite a bit of time to do that.

KB: So did you ever go back to the Antarctic?

CB: I haven't been back since 1962. But, quite a few people from here have. Now Will Harrison and Keith Echelmeyer and others from here who have been going down on some drilling projects and the Cal Tech drilling project, we've been involved with that, working on the ice streams. I say "we," but I mean my colleagues, Will and Keith here. They have been active as have people in the biologic areas here in the Antarctic. But the travel, going to and from, has been made a lot more convenient than it was when it seemed like you'd go down there for a fair chunk of time and didn't come back until you were finished.

(450)
KB: *How did working in the Antarctic compare with working in Greenland?*

CB: Oh, very similar. In both cases, you have a flat horizon ice cap surface. I think the one interesting thing if you're used to navigating with the sun was to see the sun essentially move in the opposite direction. When you're looking at the sun here, you're expecting to line it up and it moves across from east to the west and there, you're moving from left to right. There you're moving from right to left when you look at it and that's a little disconcerting at first.

KB: *Maybe that's why they drive on the other side of the road, too.*

CB: Well, they don't in the Antarctic.

KB: *No, but I mean in New Zealand and Australia.*

CB: I don't know. I wouldn't try to put any rational explanation on that.

KB: *It's interesting. Having never been to the Antarctic, I didn't realize, you're right, that the sun would be in the opposite direction.*

CB: Well, you're looking at it from down below. It was interesting if you're looking at the sun from the point of view of navigating.

KB: *What about the logistics and facilities and equipment and all that? Were there differences between Greenland and Antarctica?*
CB: Well, it depended on these individual stations that are set up in the Antarctic. They're reasonably comfortable to be in. When you're outside, it's similar in either case. But, in Greenland, I was always living out either in tents or wanigans or sleds that we pulled around and in the Antarctic, on Roosevelt Island, I was doing the same thing. It felt the same except that the vehicles were bigger and better.

KB: Where, at Roosevelt Island?

CB: In the Antarctic. Well, they were using Tucker Sno-Cats, which is a good vehicle. The Weasel's a very good vehicle, but people get impatient and don't know how to handle them. The Army used to ruin them very fast and they would tell me, well a Weasel, you can't go anywhere with them. So, we got over 30,000 miles that we pulled sleds with them with no trouble at all. It was just a question of . . . when you look at the people who said they had trouble and you watch the way they drive them, you wonder why they don't have more trouble.

KB: Is that an indication of how they drive on the road as well?

CB: I would imagine. But, it's a very interesting vehicle. A Weasel was designed for landing. It's a landing craft with carrying cargo, so it's amphibious.

(500)

The USGS used them a lot that way and since we didn't need them to go in water, on the ice cap at least, or hope we didn't, we made some modifications of the pontoons and the whole system and the thing that impressed me with looking back on it, it was like having an infinite budget compared with the way we operate here. I wanted to modify some Weasels for the Ice Cap and figured that we had to do that before we went back in '55. The head scientist, Bader at CRREL, told me, "Well, if you're going to take - you need four. Modify eight, because we can use them other places." So, the Weasels were all surplus. They were standing in storage at Fort Belvoir at the Corps of Engineers in the east. Our base was in Chicago. And when I wanted to go and check on what they were doing, we
drew up the designs on how we wanted them to be done. So, I went to Fort Belvoir and looked at what they had
done and they had made some mistakes and I wanted to correct them. I just laid out what the corrections were. They
grumbled a little bit, but they did it. The point is there was no exchange. Today you'd be budgeting these things.
You'd be talking about how much does it cost? One thing they did wrong - they put the exhaust pipe up right in the
inside in the back part of the Weasel. And I said, "Well, this is hopeless. You can't have that thing there. You'll burn
yourself." Well, they thought it was cold there. You'd want some heat. Our problem is to get rid of the heat when
you've got an engine sitting in here. So, we had to re-do the exhaust to get it totally outside. But, this is interesting
when you get people who haven't had experience and they start thinking what it might be like and they can do things
that are very detrimental and you have to be on top of it and just keep straightening it. But, I come back to the point
that I was just impressed that when I wanted to go back and check on what they were doing, I got tickets to go there
and spend the time to inspect these things and make changes and go back and today there'd be some question for
how much do you pay for this or for that? And I guess I got used to this idea that the budget that I thought of for our
whole expedition in '55 was just hiring 4 people. The clothes were provided, our food was provided.

(550)

Everything on that trip was basically a research operation, but one of the things we wanted was lighter weight food.
The Army C-ration runs 6.4 lbs per man day. This is a number I'll never forget. And the 5 in 1 rations run just about
6 lbs per man day. And I wanted to get our weight down to 2 lbs per man day. And the Army's reaction was, "Well,
C-rations were good enough for us. You guys ought to be able to use them." And I said that the problem is not that
they're not good enough. They're too good. It's a banquet in a can. It's too expensive. We can't afford to carry it. And
there's a lot of water in it. "So what do you want to do? Go to dehydrated eggs?" "Exactly," I said. "That's what we
want. Everything dehydrated." So, they were experimenting at the time with dehydrated products which has now
come into rations. But we worked with a man at the food and container institute of the quartermaster corps and he
developed. . . they had fruit juice crystals which I'm surprised never made it onto the market. They could replace
frozen food. But, it's a can about smaller than a frozen juice can by almost half, but it will make the same quantity of
juice and it's just powdered. The juice is dehydrated into little crystals and it tastes very much like the frozen juice.
So, we had a mix of grapefruit juice, and orange juice crystals. And they were in little cardboard cans, the little container. And I could pick up a package that wasn't much bigger than this tape recorder that would hold a dozen cans. And we had enough of the juice for 6 people for 120 days, or that's what we calculated - 150 days. I could pick that up just in my two hand and pick up that whole pile of these juice crystals. That meant we had to melt water. The Army on the sled trains in Greenland was operating at 10 to 12 lbs per man day of cargo for food.

(600)

But, they were carrying cans of juice - 46 ounce big juice can. They were carrying those in metal cans. All that water, basically. And it was interesting. We wrote a paper on the logistics of that operation, just on the food. We made dehydrated . . . they had meat, eggs . . .

(End of Tape 1 - Side B)

(Begin Tape 2 - Side A)

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CB: The idea was that they would provide the stuff and we worked with them as part of their development with the idea we would write a report on our activity with it. And that was a rewarding thing in itself. We wrote a paper on the air drop systems. We wrote one on the French accumulation markers as we found. . . there were a lot of little pieces that came out of this. Unfortunately, in the SIPRE/CRREL series and some of those could just as well have been put out also in journals and I regret in some cases that some of that work for the French wasn't done that way, because I found later that things that I thought they had received, these reports, some of the people that should have received them didn't. And that was unfortunate.
Coming back to this business of the logistics, the food was very successful and our ability to haul stuff meant that the main cargo that we had to really worry about was the fuel. The gasoline turned out to be the biggest single item, of course. When we finished the 1955 expedition, on the way out from the French Central Station, we had been in contact with the French and I had worked with them earlier in north Greenland. So, we got poles or aluminum tubing exactly the same as they had put out and we made sleeves so that we could attach a new extension onto the tube just with a sleeve like a great big hose clamp, you might say. It was about a meter long. But, we'd stick the new one on top, put the sleeve and basically extended their markers so that they'd be good for the future. They were planning to come back the following year. So, when we went to the end of our traverse, we were picked up by a C-47 on skis out of Sondre Stromfjord - we're jumping back now here all the way to Greenland. But, the pick up of our group involved leaving the Weasels out there on the Ice Cap. And we had tarps, great canvas tarps set up that we'd wrap these Weasels in just like a big Christmas package. And they were all wrapped up and packed so that the snow would just bury them, but the tarp would keep them in reasonably good shape. The French were to come and dig these out and use them, then, to go back to the EGE line - it's a European Greenland Expedition that followed. They were two years delayed in doing this and the commander of Sondre Stromfjord Base was sort of a crazy man. And he saw these Weasels that we were leaving out there and he thought well he could use them. He didn't think the French would ever come and get them or our plans would ever work out. So, he went out to get one of these and they were going to take it back in an airplane. Well, they couldn't get it in the plane, so he was going to cut it up into some pieces. And they basically ended up destroying one of the Weasels and not getting anything out of it. I think he was removed from duty and sent off to a home for the bewildered finally, but this is really an interesting thing. A crazy person can do an awful lot of damage in a hurry. So, when the French did come, they dug the Weasels out and I've got some slides given to me by one of the people on the expedition later of our Weasel pulled right up onto the surface and with a little bit of work, they had them going and grinding right back in on the Ice Cap. That was an interesting follow-up on that whole thing.

KB: One thing I was wondering about that we sort of touched on a little bit was the military-civilian relations in all of this various polar work that you've done, both Antarctic and Arctic.
CB: Well, basically it's worked pretty well in the sense that it's got ups and downs. It can be exasperating. You have the problem of different motivations, but science hasn't had very good luck in picking where to go and get a lot of support.

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Usually basic research is done at a place where there is some kind of a facility. Now often times, if the military is involved, they're there for some reason and they have an enormous amount of money and equipment and everything else and so you can go there and work using that equipment. And the motivation, though, if you have an Army group crossing the Ice Cap in Greenland, and I've had some experience with this, their goal is to cross the Ice Cap - get across to the other side. That's the goal. Turn around, then come back. If you're goal is to move 10 miles and spend 2 days digging holes and making measurements and weighing tubes and measuring temperature and plotting up results and then move 10 miles and doing it all over again, this is great. That's what you want to do as a research project and you understand exactly why you're doing it. They think you're completely crazy. And so there's a real mix because their goal there is just to go over to the other side, come back or whatever it is. Just to move. And the idea of actually looking at the snow, measuring it, thinking about it, is just foreign. It's a concept that you just can't communicate why you're doing that. I mean to many, that's a real problem.

And yet the money that goes into a military operation such as Thule is so mind-boggling and I've said this before, but I keep coming back to it. The work that we did, as extensive as it was, if you were to budget this through the National Science Foundation, just think of what you'd be budgeting. Aircraft. Now when they were supporting the drilling sites up in central Greenland, they were charging $1000 an hour for flying these C-130s and things like that. That's just the rate, so you figure well this is what it's costing. You're trying to account for everything. We had airplanes coming from a troop carrier wing in Texas, flying C-54 up to Westover, Massachusetts, then to Sondre Strom, on up to Thule. Thule, you know, is almost 7 degrees north of Point Barrow. You're looking at something 77 degrees north. And that airplane and the crew would load up our stuff at our request - I mean we had set the time when we wanted this air drop. That's another reason we had to stay on schedule. They fly this stuff up, drop the materials and all the material was provided without any exchange of money or ideas of what the costs were. Then
that aircraft would fly back to it's home base all the way back in Texas and we'd have other airplanes doing things. All of this done was like the sweat on the back of your hand to that operation at Thule. It was so huge that what we wanted to do didn't matter at all in terms of money, in terms of equipment, in terms of anything at all except that we tended to be a nuisance because we didn't know what we were doing. That's the way the military looked at it. But, as far as the scale of the operation . . . that's just mind-boggling when you see it go.

One other example came to mind here with the military branch of the US Geological Survey. The USGS people and those of us in CRREL or the other civilian research groups were basically people who had spent a lot of time living outside, working outdoors, camping, hiking, doing things on their own. And it was funny when sometimes you'd hear some people in the Army group talk about, "Well, we have to be here to take care of these civilians."

(100)

So, it was interesting when the USGS group wanted to be dropped off someplace north of Thule, they were doing some work and these guys said, "Well, what are you going to do when you get there? There's no camp there, there's nothing there." And he said, "Well, don't worry about it." So, they were going to be packing and carrying and they had tents and they had their sleeping bags all set up for just mapping and working and so I said, "When do you want us to come back to pick you up?" "Well, we don't want you to pick us up here. We want you to pick us up over here at the end of Inglefield Land." "But how are you going to get there?" he says. There's no road, there's no way. He said, "We're going to hike and we're camping and we're mapping and we'll end up down there and you can pick us up in two weeks." Well, these guys in one breath were saying, "We're here to take care of these guys," like they had to be taken care of and washed and fed and they were totally unprepared for the style of operation that people were using. And one of the Army officers that I got to know fairly well said, "Well, you've got to remember we've got a lot of kids in here from all over and you people are past masters of the trail and there's just a difference in the way you think of doing things." That was one of the difference.

I think the main thing, though, must have come later in the military when they started to account and budget for things the way they do now. Because now anytime you talk about doing anything, they said they can't
afford it. That, of course, is a marvelous way to turn somebody down if there's something you don't want to do. You just say we can't afford it. We don't have the money. Everybody will sort of back down on that. And since that argument wasn't pressing when we were operating there, it was kind of interesting, nobody had that particular argument. But, it was always that you were breaking some kind of rule. I remember on May 13th, Friday the 13th of May in 1955, was when we actually started out on the Ice Cap. I wanted to leave that day and we wanted to get underway because we were starting to pick up things that we needed that people thought we shouldn't be taking and so had a Weasel that had a heat gauge that didn't work. I went to see about replacing it with one of the Weasels at Sondre Strom. "No, no, no, no way we can do that." Just didn't even push that any more and Allen Skinrud, our mechanic, and I went down one night. I sat in one Weasel looking around and he took the heat gauge out of one on the line and replaced it with the one that we had that was bad and we took the good one and put it in and drove off and no more was said. Well, it started to get around that we were operating and things were moving in our direction and we needed a ladder and we saw in the frozen food section at one of the contractor warehouses they had big cans of frozen strawberries. You know that'd be a good thing to have along, and so just before we left, we went down and signed off, I would sign and the contractors didn't know who I was. I was a civilian scientist with a group and military didn't know, so they'd accept my signature on things and we got off on the Ice Cap and people started saying, "Hey, you took this and you weren't supposed to do this." Then they started noticing all the stuff we had and pointed out if we had taken everything they said we had, we wouldn't have been able to crawl up the hill with these thing. But, that was interesting. You just had to do things in a different way because there was always somebody that was willing to say, "You can't do this, or it's against the rules," or some problem. And now that they've got money, they can just say they can't afford it. I think that's compressed a lot of these witless arguments into one.

KB: I'm wondering how you persuaded the military to let you do the things you did. I mean, you were a civilian, but you were working for a military research group.

(150)
CB: That's right. It was a Corps of Engineers Group and basically the argument was very simple. You need to understand this Ice Cap. You have to know immediately if you're going to build a radar station, how long this is going to last and we can tell them if we get some information. And we were getting that kind of information. They understood that. They understood the idea that we should learn about the physical properties of the ice sheet and that's basically what drove this. As far as they were concerned, it was such a small effort and sounded like it was a reasonable thing to do. And so, once you had that authority, off you go.

The problems were always at the small level. Once we needed some screws. I remember this incident where a sergeant in a supply area wrote a requisition out for a box of screws of a certain size. We were putting a sled together and we needed these. And when I came back, he said, "Nope. We don't have any. And there are no screws like this in Greenland." And so, I challenged him in front of some others. "Well, look at your watch. Let's see how long it's going to take me to get back with these screws that I need." So, I took off in a little jeep to a warehouse where I knew they had this sort of parts, went in and talked to an Air Force man back there and I said, "I need some screws like this one." "How many do you need?" I said, "Well, I'd like to have a package." And he said, "Well, why don't you take two and you won't have to come back." So, I got two gross of these things and came back and said, I think in about 15 or 20 minutes I was back and I said, "Here I got the screws. You said there were none in Greenland." "You can't go at it that way," he said. "You've got to go through a requisition." I said, "We tried that, I gave you the requisition and it didn't work, so we have to go the other way." Ever since then, when I'd see him, he would refer to me as "Yes SIR. SIR." He was surly. But, it was interesting. Sometimes you'd get in these positions of actually challenging people and you know when to push and when not to. It's sort of an education in itself.

But, the one thing that I think is in summary of a lot of what I've seen, my career doesn't go back all that far. But, the rate of change of technology that we're experiencing, it's amazing. Things that we did, not only have we changed our way of doing things, but they've gotten so much slicker. Let's take the navigation. We had a gyroscope from an airplane run with a little Briggs-Stratton engine running a vacuum pump to make it go. We had to determine our position and we figured it was pinpointing if we could get it within a 10th of a nautical mile with sun shots. This meant the radio recording WWV, putting it onto a stopwatch, taking the sun shots and then calculating a position. Now, with a GPS that you hold in your hand, you're getting your position. You can determine azimuth and you can steer . . . That's a change in technology that is so completely different and this has happened just within a few years,
let's say from 1955 when we were at the cutting edge of what we did. When we worked on Mt. Wrangell, we were taking measurements of glacier movement from benchmark control points. We had surveyed in the set of 11 points up at the summit around the whole summit caldera and to measure flow movement in the caldera or any changes that we were after in the craters, we would take a set of measurements and then radio them back to Fairbanks where they had 8-place log tables to calculate this out.

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They did the calculations, then, on a computer. We had a mainframe computer. And then radioed the results back to us. That was in 1961. By 1975, we had a Hewlitt-Packard calculator with us where we could have 10-place figures on what we calculated. We could do all of the calculations right there on the spot. And that's a fantastic change and yet, in '61 when we worked out this thing and '65-'75, really, was the big change. 1965, we were using the computer on the campus and radioing data back and forth. By '75, we were doing all the calculations right in the field. The surveying that we did on the McCall Glacier - the surveying that I learned and there is still a lot of merit in learning how to do basic optical surveying - but, on the McCall Glacier, we surveyed a line across the glacier just to measure the surface relief of the glacier on a couple of cross-sections with the idea of re-doing them in the future. The first one we put in with a theodolite and for distance and for altitude we were using a stadiorod. The second time we were doing measurements, we used the subtense bar with the theodolite. Now that's where you can measure distances optically better than you can tape them. The next thing we were doing was using an electronic distance measuring device attached right on to the theodolite. Now that's three steps of technology changes just in a few years and now, you can measure some of these positions that we were measuring with a GPS system. You can actually do some of these profilings with GPS. The difference it makes with the surveying that we did on the Mt. Redoubt - another volcano-glacier interaction problem we were working on, we'd set up a set of measurements to measure the glacier movement and get lifted into one of our field triangulation points and do a whole round of measurements from one point taking an azimuth on another point, then you pack up the equipment and backpack it which took an hour and a half, maybe two, three hours to get set up on the other point, and then you have to measure everything from that again. Now you can triangulate the two together.
Sometimes, while you're hiking over and getting set up on the next point, the weather would close in to the point that you couldn't make that second round of measurements, so next time you'd have to start all over with both sets. Now, with the distance ranger, you go in and set up a mirror on each point and from one single set up, you measure the distance and the angle. You measure everything you need from one point. And again, we go from that to measuring with GPS. You can measure precisely the positions accurately enough to measure the motion. So, we've seen changes in technology in my career that have been astonishing. The surveying that I started out using was very similar to what it's been for the last couple of hundred years. All of a sudden, you see changes just coming one after another so fast that it's hard to keep up.

The other thing, of course, the biggest change has been with computers. The handling of large amounts of data and the ability to analyze data quickly and with fewer mistakes.

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Mistakes are often times the little bitty things like adding a number wrong or getting something twisted. That sort of thing is way reduced and also the speed of handling the numbers has increased. I'd say the number of actual calculations that I've done by hand is in the hundreds of thousands, really, and a lot of that could be done on spread sheets faster. But, in addition to that, I'm impressed with how our concepts have changed. When I started working in Alaska in 1950, let's say 50 years ago, the concepts of plate tectonics weren't in our minds at all. They weren't there as tools of thinking to interpret the geologic complexities in Alaska that now are starting to make sense because of these new ideas. And the same in biology. You've got molecular biology, DNA, RNE, nobody heard of these ideas. These are concepts in addition to tools, techniques have come, but now our whole methods of approach and our whole mental approach has changed. And that is really striking and I think people of my generation who had their biology classes before this started have to go and do a refresher course to catch up. And you asked about the relationship to students. I learned from my students how to use computers. Many people that I deal with have used computers now in high school or they've played with them at home when they used them in their undergraduate work. When they come to graduate school, these things are just second nature. I've been writing by hand on the back of old pieces of scratch paper and turned it over to a typist, they put it into shape and then I read through it and
we look for errors, and retype it a couple of times. And now we do this stuff on a word processor and the changes
are just really so fast that I think people, in general, in the professions have a hard time keeping up. And I constantly
think of how much this means to people who are less well educated and tend to look at all this technology and these
new ideas with a sense of fear because there is so much coming on that they don't understand. And I think that is sort
of working in society in general to give us a little backlash against what we're doing.

KB: What about changes in glaciology. In particular, you mentioned plate tectonics and molecular biology - the
changes in concepts. Anything in glaciology that's changed?

CB: Oh, yes. Greatly. The concept of serging glaciers was just struggling into first being recognized that it happens
and now there's been a great deal of research that's come up with concepts that we have a pretty good understanding
of the mechanism. There's still more to learn, but again technology, the synthetic aperture radar with interferometry,
we're able to measure glacier motion from repeated satellite images in the areas. We never imagined we could do
that. And looking for the snow line on glaciers where you ideally want to see how high the melt goes in September,
just before the winter snows start to come. That's a very difficult thing to do if you're not right at the spot at the right
time, you're guessing at where it was.

(300)

Once the synthetic aperture radar data came, we could see that you could find the snow line position even in the
winter under the snow cover. You could detect it with radar with satellite interpretations that we had never imagined
would come out of that. And the determination of these glacier facies by, again, synthetic aperture radar, which
when it first came out, I thought it was an interesting thing. It was just like low angle air photos, but it's gone far
beyond that and what we can get out of it.

Well, those are some examples. The pace in science in general has gotten to the point that we have so many
disciplines and so much going on and everyone that I figure is capable of reading some of these papers is too busy
writing their own papers to read. So we're piling up these papers. We've got a silent tower of Babel - the Biblical
story of all the languages. Well, we've got all these papers in essentially different technical jargon and just the quantity of them, if you stop and think about it, bothers you. So, you don't do that. You just keep plodding ahead.

But, I think the one thing that I haven't mentioned much about that has been very important in my life is my family. My wife, I met back in 1952. We met on a train. We both enjoy trains and happened to meet on a train when that was the way to travel. I met her in '52 and we got married in '55, so for three years and two years of that we were engaged and for the time that we were engaged to be married, I was on the Ice Cap for 24 months of that. Or total time, I guess was 24 months, whether it was in the mountains or in Greenland. But, after we married, I'd say the stability that I've gotten there and getting to know her field which is nursing - she's been doing nursing here and in California and in England. We spent a year on sabbatical leave at the Scott Polar Research Institute which was very interesting to me professionally and also for her. She spent some of the time working in the British Health System at (Adinbrooks) Hospital in Cambridge. We had our children. We've got two girls and one boy. And when you stop and think of what you do, the family is all sort of intertwined there. It's pretty hard to separate out. It's been valuable and I understand the value even more as I get older and I find that one of the regrets I have in this business is how much time you spend away in the field during times when you could be with the family, say in summertime in Alaska. And yet, you find yourself on the Arctic Slope, on the McCall Glacier or up in the Wrangell Mountains or down in some other place. Bob Forbes, one of my old friends here, used to say the name of the game is "You Can't Win." You're just tied up all the time with something.

(350)

So, you try to break away and do some things with the family, but I'd say this game has some barriers in there. Some people overcome it better than others.

KB: One thing . . . we sort of left off with your career . . . you came to the Geophysical Institute in 1960, and some of the projects you worked on. I think you were Director of the Geophysical Institute?
CB: No, no. I was Chairman of the Geology and Geophysics Department for a while. The department chairmanship rotates and I had it from '69 to '72 or '73 - three years anyway. And that is a question of just . . . I mean everybody whose ever done this knows what it means. You take on that and it means to a large extent you're just taking care of all kinds of little problems in the department. You're still writing proposals to get support for your research and carrying on the research in the field, and analysis or whatever you do in the lab or in your office, plus teaching, plus administration. It gets to be a very complex, heavy duty. And I've asked Bob Sharp how he did it. He's still known today as the . . . as a matter of fact, he'll be 90 years old here in two days. But, he's recognized at Cal Tech as being one of the finest division chairmen the place has ever had. Very accomplished in his teaching, his research. He's outstanding in his research and work with graduate students and his administration. That well-roundedness and his ability to run that division for more than a decade like he did and lay the keel for it the way he did . . . there are very few people that do that. Sverdrop was one who was outstandingly good and he really founded Scripps Oceanographic Institute, but his research in the Arctic with Allman and others . . . it's a combination of talent in the field, the ability to teach, the ability to do administration that only comes around . . . I don't know how many orbits it takes. They aren't common.

KB: Now, when did you retire from the Geophysical Institute?

CB: I retired formally in '87. I've been here and still working with graduate students and as a Professor Emeritus since then and supporting myself pretty much the same as I always have. But, the beauty is it takes a lot less funding and a lot less effort at raising money to support students and field programs. I get by for a tenth of what I used to.

KB: That's interesting. I didn't realize that as an emeritus, do you not pay the university overhead or something?

CB: No, you still pay the overhead. If I do any research projects, it still goes through the Institute. The game plan is the same, but it's just that because of a retirement base, I just don't have to put in as much salary time onto a proposal.
That makes a big difference in keeping the cost down. Plus not maintaining a whole program. I was running with several graduate students. During my career here, I'd have as many as six research projects underway at one time, plus teaching courses, plus doing whatever else we were doing. And it gets to be a very heavy pressure and the amount of distraction you get when you're involved in too many projects is a very serious thing because if you have six projects and you have a breakdown in one of them every year, that means you've got lots of problems. And then new proposals. I think the pressure is high and also, I saw this with the military, but I think it's true in our society in general, if somebody comes in your office and you're sitting there thinking or if you're reading or writing, they'll come in and say, "Oh, I see you're not busy," and they'll want to talk to you. On the other hand, if I was stripped down to my scivy shorts doing push-ups, they'd come in, they'd see me doing that, and they'd say, "Oh, I'm sorry, I don't mean to bother you," and they'd leave. So, there's something about physical activity in our system that immediately people recognize as busy, but I don't think the same is true with what you might call more intellectual activity. Even, well say reading or writing. I found when I was on sabbatical, we had a year in England - 1968-69 at the Scott Polar Research Institute - and I saw a different flavor, a different attitude toward this sort of thing. The idea of actually doing things that were less physical seemed to have a great deal of respect in the system where I think here with the pressures that we have, there's a dynamism about the system that maybe be missing, too, in England. But right now it seems to me that there's a sense of business that seems important here. People say, "Are you keeping busy?" My response now is, "Yeah, I'm so busy, I can't get anything done." And you know what I mean? You can get terribly busy on things, but the real progress on something sometimes comes when you're spending an awful lot more time thinking.

**KB:** You mentioned the McCall Glacier and we didn't talk about that project and what you were doing there. And when that was.
CB: Well, it started in 1969 and in some ways, it's still going on. We went on into about '75. It was a study on the heat and mass balance of the glacier and it followed up . . . McCall Glacier was an IGY glacier and it was selected by Dick Hubley. It was named for John McCall. John McCall was the Head of the Geology Department here at the University of Alaska and he died in the polio epidemic in either '53 or '54. And he had been involved in a climb, a rescue that left him excessively tired and apparently when this polio hit him, it carried him off very fast. Then, Dick Hubley who was active in selecting several of the glaciers for the IGY, named the glacier and selected it as an IGY glacier. So, the research on it started in 1957, and Dick Hubley died on the glacier. The program pretty well came to an end after that.

In '69 was when we picked it up. Gerd Windler and Gunter Weller and I. Gerd Windler was the principal investigator on that and started it while I was still over in England and then I came back and we just did basic studies. We drilled and measured temperature in the glacier. We determined its mass balance and the program we wanted very much for the USGS to take that on as one of the index glaciers never happened. But, we've been following up on it and Keith Echelmeyer, now, is measuring it as part of his program of doing airborne soundings up the long profile of a glacier. He's got a system in the aircraft of a laser profiler that measures the distance from the aircraft to the glacier and this idea had been in the mill for some time, but nobody could make it work because you didn't know where the airplane was. The altimeter isn't good enough to give you the proper altitude and your navigation isn't good enough to know just where you are, but the thing that Keith did was to combine it with a precision GPS unit in the plane and have a secondary unit on the ground to cancel out errors that are built into the GPS by the military. And then fly the aircraft right along the glacier from the bottom right to the top, right along that center line called a longitudinal profile. And comparing that with the measurements we had in the past, he's essentially continuing this research on the McCall Glacier and we're still doing these surveys across lines, but this is one of the best ways to show long term changes in the total mass of a glacier.

(500)

So, Keith has been doing this on glaciers in about 70 of them now, all around different parts of Alaska. And the McCall Glacier is one of the key pieces of that. And the nice thing about McCall Glacier is that we know enough
about it so it's an excellent calibration of the technique, too. But, in addition to McCall Glacier, we've worked on
problems of glacier dammed lakes and the rapid draining of them. We did some work on that over in the west side of
Cook Inlet. Mt. Spur and Mt. Redoubt are the two that have eruptive action. But, this particular lake is just dammed
by the triumvirate glacier and occasionally drains out under the glacier and we first were alerted to this by the high
water picked up on a satellite and we went down and found the water level in the lake had come up quite fast and
there were flowering plants buried that you could see well underwater. So, we expected this thing to go out and I
managed to get the state geologic survey to fly vertical air photos over it and we put in control points and flew over
it just before it drained and then after it drained we got another set of photos. From these, we could calculate exactly
how much water had drained out and how fast it went out. And we've done several studies on that particular project.
There are a lot of very interesting exotic things that have happened in Alaskan glaciers and it's been a real joy to
work on.

KB: And what are you working on now?

CB: Right now, I'm talking to you. I'd say I'm working on a couple of projects. One is working on understanding the
evaporation rates from some of these cooling ponds as sources for the ice fog. We're measuring with stable isotopes.

Irving Freedman of the USGS and I have been working on a project of trying to determine the amount of actual
man-made water, that is water made up of new water molecules with hydrogen from the fuel of combustion and
oxygen out of the air and those molecules are very different from the normal meteoric molecules and so we can get
some estimate of how much of this ice fog is actually man created. And we're looking at the sources. It's a follow-up
of something we did a long time ago, but we're taking a fresh look at it. So, right now, I'm devoting some time to
that and also, to the projects on seasonal snowcover. I'm trying to pin down better the errors in gauges and apply this
to a reassessment of precipitation accumulation on Greenland. I'd like to get back to that. A lot of people are looking
at that problem now and I see some problems in the way they're doing it and we'd like to make another attempt at a
good accumulation distribution for the whole of Greenland Ice Sheet. But, there are a lot of little things in the
drawer that need to be worked on.

(End of Tape 2 - Side A)

(Begin Tape 2 - Side B)

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CB: We're basically in the position of writing a paper to summarize the whole 20th century on the remote, hard to
get to region that we've covered so well by air photogrametry now. Since 1972, we've done it almost every year -
calculated the volume losses in the north crater and measuring the changes in the terminus of the glaciers on the
sides, some of which are affected by the outflow of volcanically heated water and some of which are not and there's
quite an involved picture there. We're still well underway on that.

KB: One question I have is how did you become interested in science when you were young?

CB: I don't know. There's always been some interest in this, but I can remember some good teachers in high school
that triggered the event. I remember one experiment that impressed me a lot done by a man named Norton who did
his little experiment with a glass tube where he'd drop a feather and a coin and you'd see the feather flutter down and
the coin just drop right to the bottom. Then he hooked up a vacuum pump, pulled all the air out of it and we watched
the feather and the coin drop simultaneously and they both hit the bottom at the same time. That impressed me. I
thought, well that's a real good illustration of the effect of the air resistance.

Then I had a physics teacher who was actually a Ph.D. in physics who got his degree during the Depression
and ended up teaching in high school and I had a lot of encouragement from and enjoyed him. And then when I was
in the Navy, I decided that I needed to bolster the background that I had in mathematics and science, so I started
right at the base. And the course I had in geology, just seemed like this was a great area to go in and I built that up with people both in the physics and math departments. From there on, it was just a question of constant reinforcement, you might say.

KB: *What about glaciology? Why that?*

CB: Well, I first was interested in mineralogy and just basic field geology. Then, when I started working on problems of snowcover in the Sierras and reading about a lot of this work done in the Arctic, this seasonal snowcover and perennial snowcover are the dominant things in the environment and the interaction with solar radiation and meteorology. It just became a natural thing to work on if you're interested in these regions. And the polar regions have always held an attraction for me, even when I was very little. I remember reading about it and being impressed with Byrd's Antarctic work and Larry Gould and others coming back talking about what they had done. I think you do stimulate young people with something that for one reason or another makes them turn on. A lot of people look at this and say, "Well, that's a terrible thing to do."

KB: *What were your first impressions when you went to the polar regions?*

CB: Well, the first impression I had, I guess it wasn't a polar region, but it was up in Arctic Alaska, and I just enjoyed it and enjoyed also having known a lot of the people that started the Arctic Institute of North America like Link Washburn, Sharp and other people that were involved. The Arctic and the Antarctic had regions of remote areas that were exotic, there was an interesting history to them and interesting problems to work on and it just seems you just get hooked - there's some of that. I think perhaps, I don't know if that's as common now as it was. People tend to focus on tools, techniques, problems, getting in and out of areas fast.
I think we've lost something. If you look at the USGS, the people who were active early on in the early days of Alaskan research and later, right up until the present, you've got people that have had intimate experience living in the country and hiking over it for long periods of time. Bob Detterman is a man who typifies this perhaps better than anyone. He passed away last year, but I remember being at a meeting with him where we were talking about heritage sites here in Alaska. The Park Service was interested in identifying certain key sites. And he and I were on this committee together and we'd talk about a region and it was almost always possible for him to say, "Yeah, I know that site. We camped over there. We walked up on this site." We talked about the hot springs on Sadlerochit Mountains. He said, "Well, they're not that hot. We did bathe in them and we camped around there a while." And in terms of the structure of the mountains and an intimacy of living with them that you don't get if you just target an area, go in with a helicopter, spend a couple of days and then come right out, or it can even be a shorter time. Maybe only a day. And there's a feel for the country that these people had who were that intimate in it that I don't think we'll ever get again in the sciences because the pressure of speed is too strong.

I think there's an historical perspective that is also hard to get now. There's a tremendous interesting history of science in general and of this work in polar regions and keeping that somehow in the minds of people and in the structure of our . . . I don't know if you'd build it specifically into courses or how you do it, but there's something missing that we lose track of the history. A lot of it, even the recent history, is quite exciting. I don't know if this is true worldwide, but I think in the US, it's gotten to be extreme. I had a talk with an admiral recently who was Head of the Navy League. He visited Fairbanks. And he told a shocking statistic that the Navy recruits coming into boot camp in one of the boot camps where they were testing people - only 9 % of them had heard about the attack on Pearl Harbor, which . . . Some say, well, that's just for history buffs. But this is important. This is what triggered the US from being a 20th military power in the world and just a can't-do-anything nation to the world super-power that we know it to be today. That was the event that triggered it and for people to not know anything about it at all, running around driving trucks and operating in this society, you wonder. . . . it's almost like we're a race of fruit flies that live for 24 hours with no memory of anything. And I think it's somewhat of an alarming prospect that we lose track, not only of old history, but more especially of recent history that tells us how we got to where we are today. What are the decisions that people made that brought us to the point we are, internationally and nationally. And I think that perspective has really been diminished. But, in the sciences, there's so much exciting information that
we're losing track of by just trying to move as fast as we are. And there's no way to turn it back. It just means you've got to add something to it. And I don't think you sacrifice much if you sacrifice the Laverne and Shirley Show for two episodes a week or something like that.

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KB: *Speaking of history, one of the things you mentioned is the IGY. We haven't touched on what you were doing during the IGY.*

CB: Most of the time I was working on this paper here.

KB: *Your Ph.D.*

CB: Right. When we finished up in 1956, the IGY was just kicking off and my brother went to the Antarctic as a seismologist with the Pole Station and then he followed up from one thing to another. He finished his Ph.D. here in 1963 and then went on. He's now working with NASA in Goddard Space Flight Center, the youngest brother. The other one, the middle brother is more normal. He's a mechanical engineer with 3M.

KB: *It's interesting that you all three went into scientific fields.*

CB: Pretty much except Bud, the middle one, the engineer is the guy that can, he's very sharp. He can fix anything and he's developed a lot of patents in silicate polymers and things of that nature.

KB: *Did you ever winter over in any of this field work that you did?*

CB: No. In Greenland, I wintered over, of course, up on the Central Sierra Snow Lab, but in Greenland, I'd get there, say, as early as March and as late as November. But the mid-winter, there wasn't any operation that I took part
in. Most of this was mobile tractors and it's easier to do that when it's light. And I think the wintering - there's been a lot of winter activity here in field work on the Arctic Slope and we've done some things in the Wrangell Mountains. We were measuring temperatures up at the summit in February, so I mean we've been operating - we do in our field research within the Geophysical Institute, it's a year-round operation. Things are going on whether its auroral studies which I'm not involved in directly, but the work on seasonal snowcover and a lot of the meteorological work that we've done here just runs right through the year. But, as far as wintering over in Greenland, no, I didn't.

KB: *And in the Antarctica.*

CB: No. Well, that was winter here, but it was basically for the Antarctic summer period.

KB: *But, it's potentially a different experience spending the year at South Pole Station versus doing your research here in Fairbanks and getting to go home and see your family.*

CB: Yeah, that's right. Now, my brother who wintered over there for the first year - they were down there for over a year - but, he seemed to enjoy it. Anytime you get a little station like that, in the Antarctic especially, you're closed off because at the South Pole Station they can't land in the winter and the station is pretty well isolated from everybody for a certain period of time.

KB: *When you first went to Greenland, do you remember your first impression when you got off the plane?*

CB: Well, I remember thinking how the air . . . there's a certain cleanness about it. I was struck by the beauty of it. When I first went there, the Northstar Bay off Thule was still frozen over, but the simple environment was great. We spent time out on the Ice Cap and then in the summer, when the water was open, the striking contrast of that blue water, the white icebergs and the blue sky - the blues and whites really are so outstanding. I'd say Greenland is one of the most beautiful places. And of course, there are many spots here in Alaska the same way.
But, getting away from populated areas was really the impressive thing to me. And I think the people that were in the Air Force who were at Thule and grumbled and griped and carried on about how awful it was, they saw a different thing. They saw barracks, they saw the mess hall, they saw some workplace. They traveled back and forth in trucks. To them, they could have been anywhere in the world. They didn't plug into the place. And I think that was the biggest difference between people who would be assigned right to a base and didn't fly around or somehow get out into the country at all.

KB: *We've done a pretty good review of your career here. But, what would you consider your greatest accomplishment?*

CB: I don't think there are any great accomplishments. Well, I'd say probably the thing that . . .

KB: *What are you most proud of? How about that?*

CB: Well, I'd say probably some of the students I've worked with and certainly this work in Greenland was a major piece of my life and one that I'm proud of. I'd say the work that we did on the seasonal snowcover and the mountain glacier-volcano interactions has been a very fascinating thing and is still underway. I think the seasonal snowcover is interesting as a piece here. It's just like you take an ice sheet, run it down far enough until not only the snow melts and you've got bare ice, but you carry it out onto the place where no snow lasts. In northern Alaska, you've got the snowcover there for 9 months a year. So, that's a long piece of time. And then to look at the interaction of that snowcover with the rest of the whole ecology, the first thing you notice is that when the snow melts, it's late enough so that what vegetation starts growing, all the time the vegetation is growing from the very first time it starts, the sun angle is going down, solar radiation is decreasing all the time the vegetation is growing on the Arctic Slope. It just gets a real bloom and then the snow is back. So, it's a real interesting, short period.
The small animals that live there like lemmings are a good example. They live their whole life, basically, under the snow. They're just out briefly in the summer, but they breed, they bear their young, they eat, they carry on all winter long all under the snow, tunneling around in this loose, low density depthor layer. They come out from under the snow at great peril because the snow gives them protection from the cold winds, from the low temperature and from predators that can see them like owls and foxes and anything else as soon as they're out on the top. So, they stay underneath. And when you start looking at the winter environment, there is just a tremendous amount of activity in our society of scientists that live in the south and come to investigate the Arctic regions in the summer and then leave because when the snow comes, they assume that everything's going to be in cold storage until they get back and nothing's happened. But, there's a tremendous amount happening and going on there. Understanding that winter environment and it's interrelation with the snow has been an exciting thing. And one of the things I like about the University of Alaska is the tremendous concentration of research and researchers in different fields. Brian Barnes, working on ground squirrels and their hibernating behavior - we get close to him and what he's doing and he interacts with us and there's a mix across disciplines here that is probably easier than it would be at a big university because we have a lot of variety.

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We have excellent people and it's small enough where you can have communication. I'd say if there's anything we've fallen down on at the University of Alaska is not making more use of that fact that we have this intimacy and potential exchange of ideas. It's pretty good, but it could be better. We're trying to do some work on that with the Arctic Institute of North America which we have the US Corporation based here now in Fairbanks and the Canadian Corporation based in Calgary, Alberta, and that organization started during World War II. Basically, the ignorance experienced by both Canadian and US people working in the north convinced them that they just had to understand this environment better and that organization started simultaneously in Canada and the US and has persisted through to the present. And I think we're hoping to make more use of that here than we have. We've been building on this, but that's a possibility in helping this intercommunication, too.
KB: *In opposition to the accomplishments of your career, what about any particular regrets or disappointments?*

CB: I guess all the undone things. There are projects that get pushed aside when you're moving fast from one thing to another and there are several little bits that I'd like to finish up and I'm working on some of them now. But, in terms of major disappointments, I don't know. Maybe it's just my looking at life with a filter that kind of blocks them out, but I don't see too many of them.

KB: *And you did mention earlier about the difficulty of life as a field researcher and having a family.*

CB: Yeah. That's a problem and I think some people are better than others at handling that, but it does take . . . you have to spend time doing things with the family. And the thing that I found is that kids grow up faster than you ever think they're going to. You turn around twice and they're from diapers to asking for the keys to the car.

KB: *You've also mentioned a few names along the way, but who would you consider your mentors?*

CB: Well, I'd say Sharp would certainly be the one that has been important. Bader, at CRREL and Gerdel. And teachers, I'd say, in physics, Near. I had a math teacher named Bucklehyde who taught some elementary math, but in terms of basic things like trigonometry, analytic geometry, calculus, but he was one of these outstanding teachers and another one was Weinberg, Joe Weinberg at the University of Minnesota - a teacher. That was one of the big disappointments for me, or a traumatic thing, when McCarthy was swinging and this was in the late '40s and my beginning physics course at basic physics, sophomore physics, I had three different professors in that course teaching it because two of them were fired.

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The first one was Joe Weinberg, who McCarthy got after and then came Oppenheimer - Frank Oppenheimer, the brother of J. Robert Oppenheimer. He was actually the first one to be fired. The McCarthy group got after him and I
don't remember what it was, but he left the University of Minnesota and the president of the university was challenged then - Morrow was his name. He was challenged about Weinberg. They said, "Under any circumstances you're not going to fire Weinberg," and he said, "No." And they pinned him down and they said, "Well, if he's indicted by the Congress," and they had in these hearings, they tripped him up and got a perjury charge on him and so he was let go. Ernie Loughgrin, the final of the three, he finished the course with us, but that was awful hard on young people taking the course and to have people you respect greatly treated that way. Weinberg was not only an excellent physicist, but he was a music scholar and one of my girlfriends at the time was there majoring in music and she was impressed with him, just in his handling of lectures and things he did in the music department. And he disappeared. Last I heard of him, he was working with some kind of a plastics outfit doing things that just were way below his level.

Frank Oppenheimer turned up after Sputnik. There was a big interest in science and science teaching and Frank Oppenheimer had gone off to do ranching in New Mexico and he ended up in Colorado in Pagosa Springs. And it turned out that the science fairs were being won by kids from Pagosa Springs. They were building their own equipment, doing very exciting things and it was Frank Oppenheimer teaching. So, he went on from there. They took him back at the University of Colorado in the physics department. Then he founded the Exploratorium in San Francisco. And then he died since. I wanted to see him when he was there just to remind him of these events and that I was one the kids in his class. But, that was a real blow. That infuriated me and made me realize how dangerous something like this McCarthy era could be to the nation because they were just causing damage. I think they probably did more damage to the US than anything that could be done from outside the US. So, in terms of major disappointments or things, that figures in too.

KB: Now what about, can you think of people who might consider you one of their mentors?

CB: Oh, you'd have to ask them. I don't know.

KB: You don't have anybody that you . . .
CB: Oh, I have several that I'm still close with. My goodness, we still work together. Dennis Trabant here with the USGS and Roman Motika who is in the Juneau area. And Matthew Sturm who is with CRREL, I work with him more closely right now than anyone and he's become not only a former student, but a very close friend and a colleague. So, I guess it just keeps rolling. I hate to mention names because I know I'll omit someone.

KB: So that leads me to the question about people you've worked with and can you talk about the people as individuals or the types of people and what people have been like?

CB: I don't know. I think it's hard. So many of these colleagues you work with become good friends. I'd say I've just been very lucky in having a lot of colleagues to work with who are good friends like my office mate, Will Harrison. I consider him a good friend. I have great respect for his abilities. He's an outstanding physicist, glaciologist. He went into nuclear physics first at Cal Tech and he went into glaciology. Keith Echelmeyer who is also working with us. Gunter Weller. Gunter and I have been close on several projects in the field. There are more, but those come right off the top.

KB: Maybe you can talk a little bit about what it was like to work at the Geophysical Institute.

CB: Well, it's an organization with a lot of camaraderie. It's a good family base, really. It's grown. It's very much bigger and different from when I first came there. But, there's a feeling of kinship within the Institute that's always been very good. It also is perhaps one of the best research institutes of that type in the sense that it is extremely broad. Originally, it was focused pretty much on the aurora and I think Bill Wilson, who recruited both Elvey and Sidney Chapman to come here, had the right idea. He figured they should really focus on things that dealt with the winter environment and the aurora was the first step they wanted to get at. One of the main reasons was radio communication was the main way of communicating and it was subject to terrible interruptions by magnetic storms, auroral activity and things like this they wanted to know more about. So, it was a great thing to focus on.
Wilson - I don't know if that name means anything to you. He was in the chemistry department. He was made Acting Director by Moore of the Geophysical Institute and his first step was to try to get some real leadership from outside that dealt with the auroral studies. His first thought was . . . he checked with people who knew something about this and they said, "Well, the obvious thing to do would be to go after Sidney Chapman." And people laughed here. "Well, Sidney Chapman, he's certainly not going to come to Fairbanks." So, Wilson thought, 'Why not? We don't know until we ask him.' So, he got Sidney Chapman to come here and look at the system and Wilson told me a lot of this when we'd ride back and forth to Eielson when they had a swimming pool there and none here. And Chapman was impressed that this would be a good base because they had just built a new geophysical building which is now the Chapman Building. And he said he didn't want, in Wilson words, Chapman said, he didn't want to be involved in the machinery of running the place. They'd have to find a Director. He only wanted to be an advisory scientific director. So, again, the question was who could do it? And Elvey, C. T. Elvey - he never used his real name - Christian Thomas Elvey - C. T. He went by the initials. He was at China Lake. He was an astronomer, but he was running the China Lake Naval Weapons Station and we were very active there. And he agreed. Again, Wilson went out and recruited him to come and take this on and he liked the idea, so he came as the Director. And he was Director until Keith Mather took over, but he was Director for more than a decade and really laid the keel for a lot of this. But then, it was Neil Davis, I think, that influenced him, too, to broaden out a bit. And so the idea of getting into glacier work and snow and ice work, Elvey came and saw me when I was at Cal Tech and offered me the position here. So, I was the first one here that didn't use the Earth as a place just to bolt down instruments. In other words, I was going to look at the Earth itself and then I got involved with Payway in the Geology Department, too.

The next one that came on to broaden the base was Ed Berg, in 1963, and he started the seismic program here. And then, we had some visitors in meteorology. Got some activity in atmospheric sciences. So, the Institute has gradually broadened out from what was a fairly narrow focus until now, I think it has a broader scope than almost any other institute this way that's active in geophysics.
KB: OK. Maybe you could talk a little bit more about field work and what that was like, doing that.

CB: You mean living in tents?

KB: Well, you were out in the field a lot, it sounds like.

CB: Yeah. Maybe too much. A lot of what we did on the Arctic Slope was based in little tent camps. We'd be dropped off with ski-equipped planes and work at a site for a while then move somewhere else. The work on Mt. Wrangell started with tents. That was quite an interesting start in 1961. I was still finishing up editing for this report that CRREL was going to publish and had it down there in Copper Center, Glennallen. We were waiting with very bad weather in 1961. We had put a cache up on the summit with a Supercub, just a little bit of food and some things, near where the old huts had been in 1953. The Jamesway huts were in ruins, but I thought it would be good to get near them in case we wanted to use any of the material out of them. So, we waited for nearly two weeks for the weather to break enough to get up and this particular night, I had been working on the paper almost through the night and Jack Wilson came over. He saw me right out at the hangar where we were flying from and he said, "The weather looks good. We ought to go up this morning." And so, we took off at about 5, 6 AM in the morning. It was very early. We got up on the summit and the weather started to close in and he said he had to get out. So, I was going to push him because the only way you can take off was to get a downhill run up there. The Supercub didn't have the energy to fly on a level, but I was pushing him over the edge and I saw these clouds coming and he said, "You want to come with me or are you going to stay?" "No, it's been so hard to get up here, I'll stay." So, I pushed and I was thinking to myself, 'This is really kind of dumb. Here I am with this airplane and I'm pushing it over the edge,' and then the storm started in. And I started picking up this equipment. Well, I had also worked at high altitude in Greenland - 10,000, 11,000 feet at high altitude for a long time. But, I had never had to do like we did here - just fly from basically 1,000 feet up to 14,000 feet in an hour and land and get out. So, I started feeling that. Plus, I
hadn't slept and I hadn't eaten that morning and I decided to pole this sled that I had with a tent and get it over close
to this area where that hut had been.

(450)

And as I was starting to put up the tent, the storm really broke and it was hard to make the tent stay up, so I used my
pack and my ice ax and other things to hold the tent down. It was an open bottom tent. No floor in it. Army stuff that
we had gotten because it was easy to get the equipment from them, but it wasn't the best equipment. So, finally by
night I was sick. I was sick all over the side of the tent and on the ground and I got in, dug a hole by the side of my
head - I think it took most of the day. It was probably 9 or 10 PM that night before I finally had the tent up. And I
got in and dug a hole by the side of my head and went to sleep. And it was a day later. They had flown over and they
had seen the tent up, but couldn't land. And a day later, a fellow named Sam Scott, a fellow who was working with
us from the Army, was there cooking some tea and helped me get sorted out. But, that was about as close as I've
ever come to doing something very stupid on the mountain because everything was combined to be the wrong way.
Storm, no tent, and no sleep, no food, no brains.

KB: *And were you by yourself?*

CB: By myself. All by myself. That's why it was such a problem. He was going to bring someone else up, but he
couldn't get back up for another day or so, so it was a problem. And then we lived in tents there. We had lots of
storms that blew the tents apart. We had a little two-men mountain tent that survived. We broke two of the Army
tents that just were completely destroyed. And we dug a hole where the Jamesway hut had been. It had filled up with
snow and the volcanic heat had turned a lot of it into ice, so I got Jack Wilson who was flying for us, to bring up
some axes and we just cut out a lot of ice and we camped right inside that thing. And we did that for about a month.
All the pit studies we did were based out of that cave. And generally we'd live in tents and when we go up now, we
always work around the edge. We had things to do on the sides of the mountain. Spend as much as a week or two
weeks and then go up to the top more gradually. But, that fast trip up, sometimes you have to do it and it's not a good thing. It knocks me out every time for a couple of days. I end up being sick.

KB: *So, are you still going out in the field?*

(500)

CB: I am, but not as much. I was hoping to get into the Wrangells again last year, but I've been working on the Arctic Slope field problems and the last two springs, I haven't been up there at all.

KB: *So, that story you just told on Mt. Wrangell, is that the worst field experience you've had or has there been worse?*

CB: Oh, I don't know. I remember in Greenland, we crossed heavily crevassed areas and pulling a Weasel with one sled or in some cases, two sleds and you feel the back end of the Weasel sink down into where a snow bridge is breaking under you and you know you've got two sleds to pull across it, you feel one crash into the side and then the next one . . . So far, those are pretty heart-in-the-throat experiences. But, I'd say probably the most precarious thing I ever did was that first night up on Mt. Wrangell - that first day and night combined.

KB: *What about your best field experience?*

CB: Well, I'd put them all in that category. They've just been marvelous experiences and when you have a feeling of accomplishment, if you're getting something done, that carries you a long way. And if you have a beautiful setting like we've had sometimes. I remember being on the summit of Mt. Wrangell looking up at the Alaska Range and seeing the sun setting and shining and reflecting like copper across all those lakes out there. In the mountains, you're seeing them from the reverse of what you do here. It's a pretty stunning sight. Greenland was a fascinating place. My experiences have been . . . again, you look at it with a filter, but I've seen a lot of very exciting and beautiful places.
KB: Can you comment on the benefits of field work? Now it seems like science is not so focused on field work.

CB: Oh, it is. It's focused. It's just that I think what I was getting at when I mentioned the intimacy that people like the USGS had when they could spend a lot of time in the field, there's a different feel for the country than you get when you're rushing in and out real fast. But, people in this game, if you're working on glaciers and then the mountains, you're going to get stuck at times when you have to spend a lot of time whether you want to or not.

And the people who enjoy the work and enjoy being in the mountains usually take that in stride. It's a question of just seeing all the aspects of it. You don't want to be there with the sun shining all the time and nothing else. All in all, I'd say that I've been a very lucky person. I've done things that I have wanted to do and I've been in places I've wanted to go in. Places that aren't all that easy to get to. I don't know, but I'd say it's been a very privileged life in that sense.

KB: How do you think the Arctic has influenced your career?

CB: Well, it sorta is my career.

KB: Well, could you have done the type of research you did other than the polar region?

CB: Well, not digging snow in that sense. But, in terms of the interest in research, I imagine you could find a career in ________. People I know and knew very well have been involved in coral reef research and that's a fascinating thing and my time when I was in the Navy, I was in the tropics for a bit and then the Caribbean and there's beauty all over the place.
KB:  *Right.*

CB:  But, the polar regions have held a strong pull on a lot of people and I just happen to be one of them, I think. And when you read stories about lives of people like Nansen or Knud Rasmussen and the Thule Expedition and the work they did. One of the things he was interested in there was languages. A fascinating piece of work. And I think the Arctic just has had a special place. Your question is a little hard to answer because it would be different in other places. But, I think you can get turned on to a lot of different things.

(600)

KB:  *You could have done snow studies in the Sierras, for instance, or in the Alps.*

CB:  It's very different though. The polar regions with the perennial snow, that's different. The Sierra snow is gone every summer, no matter how deep it gets, there are a few glaciers. And in the Alps . . . you find people in glaciology . . . there is a society you know, the International Glaciological Society. One of the hallmarks of it is how international it is. Many meetings that I go to, international meetings, you'll find people sitting in . . .

(End of Tape 2 - Side B)

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(Begin Tape 3 - Side A)

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CB:  Americans here, Japanese over here. . . . the Glaciological Society is almost unique in the way it mixes people. I have friends in Japan, friends in Scandinavia and England and all over the US and Canada and when you go to these meetings, it's just one big gang. It's an international group with people working on Alpine problems, Antarctic
problems, or Greenland or wherever . . . the Alaskan glaciers. You find an interest that spans any kind of national boundary. And I think overcoming these national boundaries, just ignoring them to recognize you have friends all across these areas based on your interest in what you're doing, that's pretty exciting and it also makes a mockery of these national boundaries and it makes people find it a little harder to get all fired up and want to go to war with some other country when you've got close friends there. And that's one thing that's come across to me, especially after my time in the Navy. One of my close friends in Japan was in the Japanese Navy. We were both in training at the time when the Hiroshima bomb went off and he was close enough at a station to actually see the effect. And we have camped together, we have been in each other's homes, we've worked, we've written papers together. If I listed the top 20 friends I have, he and several other Japanese would be in there. But, here not too long ago, there was a war on and it makes you think. This is a silly way to run the world. And that's one of the nicest things about this particular society, though, is the complete disregard for nationalities when you have any of these international meetings. It's strikingly so.

KB: That's good. Another question is about any kind of documentation you might have from your career like photos or if you kept field notes, those kinds of things. If you have that kind of stuff and if so, what?

CB: Well, I'm working on Field Book No. 55 - small field books. And I have books that I keep track of not just field work, but just for every day work. I'm up to book, I guess about 40 of them, plus other notes, so it's not a question of a lack of this stuff. It's just lots of it. And then, of course, there are papers that are bibliography of papers written all over the place that tend to get lost. If you'd just make a list of them . . .

KB: What about photos?

CB: Photos probably measured certainly in the 50,000, 60,000 range. I mean a huge number. There are slides, there are photos. A lot of the photos are taken for documenting. Some are well catalogued. Others are in shoeboxes. The ones that are cataloged the best are the ones that are used in papers and you keep track of. Photography has always
been a key thing in the field notes and the documentation, so it's more than just pictures of people doing this and
that. My wife complains that most of the photos don't have people in them.

KB: *Pictures of snow pits.*

CB: Snow pits, mountains and different odds and ends.

KB: *Now in your field books, you just keep entries of what you're observing or what you've written about what is
going on today or more like a diary?*

CB: They're mostly field notes about the project. You keep anything about summarizing what went on in a day in a
separate sort of log book. But, the field notes, it's a problem how you keep track of these things and for work that's
been done on different projects, the field books are pretty well organized. But, there's a lot of material, too, that
hasn't been written up adequately.

(50)

KB: *Another thing I was thinking about was letters and correspondence, but you were out in the field where you say
you got mail in, but you didn't send mail out.*

CB: Yeah. Letters . . . I have correspondence files, but I think Claus Naske says you should save all this stuff, but a
lot of it, people just throw it away. It's hard to keep track of all the stuff, but I guess someone like Claus is saying all
of this stuff should be kept so somebody could go through and reconstruct what happens in a place, but with the lack
of interest in history, I don't think there's too much interest in that.

KB: *Well, I think we've pretty much covered all the questions I've thought of ahead of time and that we should have
gone through fairly detailed. Is there anything else that you wanted to talk about that we haven't talked about?*
CB: I don't know. I guess, perhaps, we didn't go into family things like I said with the three children. My oldest daughter has a degree in Earth Science and also a background in Russian. She has a Master's and has worked on glaciers of the Russian Far East and worked over in the Chukots Peninsula [on map it is labeled Chukchi Peninsula] and served as translator to the Americans that were there, too. And then she was looking at the glaciers moving from the Chukots Peninsula over to St. Lawrence Island, so she spent some time out there. And on the Russian coast. Mainly on the Russian East Coast there. And my second daughter, her big interest for all her life has been horses. But now she's working at the university in the Benefits Planning Operation in the Statewide system. And my son, the youngest one is a hydrologist or he's a chemist. He got a chemistry degree here and then a Master's in hydrology from New Mexico Tech, so they're all doing their thing and the second daughter, Erika, has given us two grandchildren which is a main attraction. If my wife had any ideas of moving out of here, they all went away when the grandchildren showed up.

KB: So all the grandchildren are here.

CB: All three of our children are here and the two grandchildren are here, so that's a pretty good anchor for Fairbanks.

KB: You know how you had mentioned early days where you did four years of all winter, how do you do that?

CB: Well, being away in the summer from the time the spring shows up when you head off to Greenland and you're on the Ice Cap where it's always well below freeezing, and you come back to the States at the time when the leaves are falling and the snow is coming down, that's the way you do it.

KB: I know, but how do you do it mentally and emotionally?

CB: I don't know. I've never craved summer. Not like that. I mean, I found that more amusing than anything else.
KB: Maybe it's because you're from Minnesota.

CB: Well, they get some hot summers there. My mother's family, my grandparents there came into Canada - my grandfather did. I don't know where my grandmother entered, but he went across from Montreal and one of his sisters - they were in the Seattle area. One of his sisters settled right there near the Olympic Peninsula or on the Olympic Peninsula, right down from Olympic National Park. And he and another brother ended up in Minnesota doing logging and then worked with the newspaper and different things. It's interesting how close I got to know especially my mother's father because he lived the longest. I had known him and worked with him on things. But, this is what impresses me how fast thing are changing. He told me once that the tools he used in Sweden, in farming, were the same as his father had used and his grandfather, great-grandfather . . . back through the generations, what they did.

The tools and the approaches were the same. But, he said, if you want to see those tools now, just one generation later, you've got to go to a museum to see them. And, he told me he saw his first electric lightbulb in Montreal and it was 1888, and then he watched the development of electricity in general, the telephone, things we take for granted. The automobile from doing everything with horses. Automobile, then airplanes, radio, television, and satellites. He was over 90 when he died and satellites had been up and the potential of this and aviation . . . it's just incredible to think of this all in one life. And then from his time up through mine like I mentioned earlier, the concepts of plate tectonics and molecular biology and the computers and satellite position determination . . . we've on a run here that's frightening because you can't possibly say that where we are today things aren't going to change. I think we're going to see changes in this next century that make even the past century look slow because it's going exponentially and the question is, how do you keep up with it and how do you keep up with what we're doing with the Earth? We're pumping gases into the atmosphere that are screwing it up. We're straining all the fish out of the ocean, cutting down all the forests, removing habitat. We're watching animals go extinct. In just another generation or so, it may be that
our kids will have to see elephants and gorillas only in pictures. This is shocking and there's a possibility that we could do something about it, but the forces that are moving in this fast pace look so overwhelming that it looks pretty gloomy from the point of view of biologic diversity or just the environment that we know in general.

KB: *It looks pretty gloomy for life in general in the future. Is there going to be a planet to live on?*

CB: Well, there will be a planet, but . . .

KB: *We're not going to have water, we're not going to have electricity, we're not going to have all the things that we're so used to.*

CB: Well, we'll have electricity. That's just a question of making it. But, the biggest problem is changing life style. We have places in the world where people don't have clean water to drink or to bathe in and yet, Americans insist on flushing their toilets with drinking water. There's something wrong here.

KB: *Or filling up swimming pools.*

CB: Filling swimming pools. But, I think the question of redesigning houses with gray water systems so that you're not flushing your toilet with drinking water. A good example was Thule Base when the buildings were set up there. The individual barracks were up on gravel pads and then they were set on blocks so they didn't interfere and they didn't interact terminally with the ground at all. Of course, there's all frozen ground there. So, they had three tanks in the building and a water truck would come and put fresh water into one tank. That water was used for wash basins and showers and then it went into another tank, the holding tank, the gray water tank. That was used to flush toilets and then that sewage went into a third tank and then a sewage truck would pick that up and haul it away. Well, they cut down the total use of water enormously this way. Right now, we're in a position where all over southern California and the US in general, we could have houses built with this sort of gray water system so that you didn't use the absolute premium water for sewage disposal. And the ideas of how to do this are in the system, but the
ability to carry this off and make it count . . . I think this is where a lot of the application in science is going to have to go in the future. Just to make it possible to carry on our life with using a lot less of the resources and not in such a way that you're freezing in the dark, but just not wasting not as much as we do.

(150)

KB: They have the ability to make cars much more fuel efficient than they actually do.

CB: Cars. All through our system, the biggest problem especially in the US, is that we've moved very fast and the big question is "Why are we so wasteful?" Not to ask are we wasteful. We're terribly wasteful. But, why are we as wasteful as we are and what can we do to cut it back? Art Rosenfeld at the University of California in Berkeley building research group has been exploring this problem for quite a while and he was recently made an advisor to the governor of California. And this is the sort of thinking they're doing - how do you reduce the need for as much energy as we're using? And it's a redesign, to some extent, of the society. But, a lot of it's making use of old technology. We're flying big airplanes right now from points that could be very efficiently and just as rapidly served by efficient rail systems.

KB: If we had those fast rail systems.

CB: But, why don't we have them? We don't have them because we're spending all of our transportation money on building roads. We could build these. It's just a question of the political will to do it right. It's not that it can't be done.

KB: It's the political and social pressures. And the economic pressures.
CB: Well, the economic pressures are all in the line of building these things, because that's the best way to go. We use less fuel, we create jobs in building these systems and in maintaining them. We do less pollution of the air and by far use less fuel. The only reason we don't do it is because we didn't do it last Wednesday.

KB: Well, it's the economic pressures that it's the big car corporations, the big oil company corporations, who would lose a nickel. So, they pressure the politicians to do it so they can get it in their pockets.

CB: That's part of the problem. That's a very short term view, but we're crashing into the future so fast that I think we're not going to be able to afford that. But, when I stop and think of what we're doing, there are many interesting problems that need to be solved in science, but the application of some of this intelligence to the way we're carrying out our daily life is now becoming not just something we ought to do because we ought to do it, but it's becoming something we have to do in order to survive, is the way I see it.

KB: Well, anything else?

CB: No, I think if you think of anything. You've sure got a lot of stuff there. I hope it makes sense to someone.

KB: I think it will. Thank you.

CB: Thanks.

(End of Tape 3 - Side A)

(End of Interview)