Col. Joseph O. Fletcher
23 January 1997

Brian Shoemaker
Interviewer

(Begin Tape 1 - Side A)

(000)

BS: *This is an oral history interview with Colonel Joseph O. Fletcher, taken at his home in Boulder, Colorado, on 23 January 1997, as part of the American Polar Society Oral History Program to preserve North America's heritage of polar science and exploration. Interviewer is Brian Shoemaker.*

JF: Well, my involvement in the Arctic was pretty inadvertent, you might say, but I'll start back . . . . I went to college at the University of Oklahoma and there I became involved in ROTC because Oklahoma is a land grant college. I remember when I came there as a freshman and they told me I had to take ROTC and I told them, no, I didn't want to do it because that isn't what I came to the university for. And after some negotiation, they told me that I could enroll, but I would never graduate until I met the two years of compulsory ROTC. And so the upshot was that I didn't take ROTC the first year that I was there.

BS: *Which year was this?*

JF: I entered in 1936. But, by the time I got into the second year, two things happened. One is that I found out that the ROTC was riding horses and I grew up with horses. So, that seemed pretty attractive to me and as a matter of fact, I started taking the basic ROTC. I even was on the polo team for one year and enjoyed it, really, very much. But, that was a horse artillery ROTC unit. So anyway, because I was one year behind in ROTC after going four years, I went to MIT and, well, I did the first two years, which was the mandatory part. By that time, I had figured out it was better to be an officer than an enlisted man. And we were obviously going to have a war, and so I enrolled in the Advanced ROTC. But, I only had one year, because I was a year behind. So I took the second year of ROTC at MIT during my first year of graduate school. And so, then I was called to service immediately, when I finished that year, which was June '41.

BS: *What did you get your graduate degree in?*

JF: Well, meteorology. So, I had a full year of graduate work in meteorology there because that was . . . I had applied for something I saw on the bulletin board which was a year financed by the Department of Commerce leading to the degree in meteorology and, I forgot, they paid $100 a month of something like that, plus all your fees and stuff. So, I was lucky. I managed to get into the Graduate House. I spent that year . . . I don't know if you know the layout there.

BS: *I do.*

JF: Well, the Graduate House was right across the street, Mass Avenue, so it was right on top of the school. And it made it really very convenient. So, I went there in 1940 and finished in 1941 and immediately went on active duty, first in the field artillery. And I was at Fort Sill, and then I was transferred to the ordinance department because MIT was ordinance. Oklahoma was field artillery. So, they needed ordinance officers.

BS: *Doc Edgerton at MIT then?*

JF: What?

BS: *Doc Edgerton, did you know him then at MIT?*

JF: Yes. I knew him and his lab assistant was my roommate. I had three roommates. There were four of us who occupied one of the big apartments in the Grad House and one of them was Edgerton's assistant.

(50)

So, I used to go over and shoot bullets through lightbulbs and things like that while he took pictures. So, I remember that well, yes. Well, anyway . . .

BS: *Back to Fort Sill.*

JF: Well, yeah, I didn't stay long there. I got called to duty in field artillery because that's what my original affiliation had been and then I got transferred to Aberdeen because they needed ordinance officers, but that isn't what I wanted to be in. By that time, I had decided I wanted to be in the Air Force, and so I had tried to volunteer for the Air Force. Anyway, after some difficulty, I was transferred to the Air Force and was first assigned as meteorology - a weather officer down to Alabama. And so, for a couple of months, in 1941, I was in that duty. That was my first Air Force assignment. But I, meanwhile, applied for pilot training. So, I was accepted into that and I graduated from pilot training in the spring of '42.

BS: *Did you go overseas?*

JF: Well, I had overseas orders. My first set of orders, I was assigned to . . . I took fighter pilot training, and I was assigned to a P-38 squadron in Australia. See, this was after Pearl Harbor, by that time. And I got as far as San Francisco, where you had to wait for a week or two to get space on the airplane. And meanwhile, played poker. And I was waiting for my turn when I was called back and reassigned to a unit at Langley Field, which was the first Sea Search Attack Squadron. And that turned out to have a lot of consequences, because our job - this was the first aircraft outfit to be equipped with microwave radar in the world. And, in fact, there were 12 airplanes to start with and two were lost and so most of 1942, there were only 10 airplanes with microwave radar. In the meanwhile, they had decided to put it into production and it turned out, the production set started coming off the line in about February of '43. And they put them on a large number of airplanes, and better airplanes, like the B-24s that had more range.

Between February, '43 and June, '43, the German Navy lost almost half of their Atlantic submarine fleet and there have been a couple of books written about that, one of which I've read. Fascinating. Because that's when the production sets came along and that really made the difference. But, during '42, there were only 10 airplanes and our job was, when they had a confirmed position along the East coast, our job was to go out in the middle of the night and fly an expanding square around that position and if you got a sighting, you made an attack run on it because they could crash dive as quick as you could turn around. We were flying B-18s. I don't know if you know what that is, but . . .

(100)

BS: *I do.*

JF: You do? That's the only airplane I ever saw that took off, cruised, landed, and dived and spun at 110 miles an hour.

BS: *Everything.*

JF: But, it was the work horse and we would go up and down the coast, chasing the submarines because the German submarines had their own way at that time. They were sinking ships right in Chesapeake Bay. The ships were trying to hug the coast, you know, so you could fly from Miami to Boston and be in sight of half sunken ships. I've forgotten the tonnage, but they had an enormous field day at that time. So, we would fly out of Langley, and they would move farther away, and we'd go up and fly out of Cape Cod at Otis Air Force Base, and then they'd go down south and we'd go down and fly out of Puerto Rico and Miami. And they would move back up and that's the way, in 1942, really, it went.

So, I spent most of '42 chasing submarines with this squadron. And then at the end of '42, you may remember that the North African invasion came right at the end - October, November, something like that. But, they had to have cover against the submarines that had to go through the Straits of Gibraltar. And so, my squadron was redeployed to cover the approaches to Gibraltar. And they were wiped out in a month. But fortunately, just before . . .

BS: *Who was wiped out? The submarines?*

JF: No, the squadron. And that's kind of a footnote to history. Spain was neutral, officially. But, the fact is that Franco was lending Spanish air bases to the Luftwaffe and they were flying 109s out of Spanish bases. And, of course, the B-18s were a set-up and they were using B-24s, too, but they got completely squashed. But, just by sheer luck, I was pulled out of the squadron just before that. And that happened because I had a little training in meteorology and I very quickly had found out that you could see things with microwave radar that you couldn't see with other kinds of longer wave radar. And I was a physics major and I was interested enough to want to investigate it. And, of course, I was aware that the index of refraction depended mainly on

humidity as well as temperature. And so I was trying to design a humidity sensor for the B-18 that we were flying. And in those days, you had to have a permit. You couldn't go down to the Radio Shack and buy a meter without a piece of paper, you know, part of the war control. And so, Langley was nearby. I jumped in an airplane and flew up to Washington and I had a classmate from MIT in the Pentagon and I thought he could get me a permit so I could at least buy some instruments. And, anyway, I went in and he said the same thing, he said, "My God, there was a delegation down here yesterday looking for someone just like you and we didn't know about you," because they wanted a pilot who is a meteorologist and who had an electronics background.

(150)

And I had all of those things. And he said, "How'd you like to go back to MIT and be assigned to the Research Laboratory - the MIT Radiation Lab?" And so, of course, I said, "Do I have time to pack my bag or shall I just go from here?" So . . .

BS: *You weren't married then?*

JF: No, I was single.

BS: *You could just go.*

JF: Yeah. So, they let me pack my bag, but you know, the next day, I was on my way to Boston. So, that was January, I guess, about the 1st of January, '43. So, I spent the next 6 months in the research group at their Radiation Lab both as a researcher and as a test pilot..

BS: *Flew out of where?*

JF: East Boston. That was before they had anything at Bedford. I had been up to East Boston before with the B-18 because we had a good deal of support from the Rad Lab. But, I moved into my old apartment at the Grad House and the only difference was, I was working on the roof of the building next door. And that was great. So, that lasted until about September, I guess, of '43. And, of course, I got very interested in what you could do with microwave radar. Things that weren't being done at that time. And especially things which had to do with meteorology. But, I was a lieutenant and so the question is, who's going to listen to you? Put it that way. I tried to talk to somebody in the Air Force about taking advantage of what could be done, and I couldn't get anybody to listen, for a few months, anyway. It took about 6 months and finally, during the summer, I got somebody who would listen to me and the Air Force picked it up and I was reassigned to the Signal Corps as an Air Corps person. But, the Signal Corps Development Laboratories are in New Jersey, their whole network of them. So, in the latter part of '43, the Air Force, more or less, adopted my recommendations and they got together with the Signal Corps about plans for implementing them and I was reassigned, then, to work with the Signal Corps in doing it.

The gist of what we did was: One - there were two things that you could do that had not been done before. One, of course, is use microwave radar for a direct observation of meteorological processes, especially thunderstorms and hydrometeors of various kinds. And you know, the two airplanes we had lost from Langley always intrigued me because they just vanished. We never knew what happened to them. They just . . .

BS: *What planes were these now?*

JF: They were B-18s.

BS: *On reconnaissance missions or . . . ?*

(200)

JF: Anti-submarine.

BS: *Oh, this is when you were in the squadron.*

JF: Yeah. As I say, there were two airplanes that were lost and nobody knew whatever happened to them. But, one of the possibilities that really intrigued me was that we did run across water spouts in that area. And you know, in the middle of the night, you can't see a damn thing. You're on instruments. We were at 300 feet and you know, if the target for a water spout would look just about like a submarine. And you would naturally make an attack run on it, and . . .

BS: *You'd get turned upside down.*

JF: Well, if you flew into the water spout, you'd never recover at that altitude. So, one of my theories was that that might have been what happened to them. And so, I was trying to investigate the meteorological side. And, of course, at MIT, I was assigned to the Propagation Research Group, so it was right exactly along those lines. So, anyway, this immediately led to the conclusion that you could do a whole bunch of things that were not being done because there were microwave radar sets that had been deployed around the world for surveillance and certain there are only a few, and there are a whole bunch of microwaves that were being used for fire control - anti-aircraft fire control. Well, the point is, anything that you could use for fire control could easily be used for measuring winds. Instead of using theodolites, which you could only use in (caboo) weather when you really didn't need it, you could get the winds through clouds. And there were hundreds of microwave sets already in place in all the war theaters that could be used to do this by developing a procedure for doing it and then, organizing and getting it done.

So what we did was to - I went down to New Jersey and what we did was to develop the procedure for using all of the operational sets that had already been deployed around the world for fire control, and we'd write a manual on the procedure and how to do it and we designed a set of radar targets and had them manufactured. And for each war theater, we'd send, well I asked the Air Force and they detailed first, five guys who were graduating from meteorological school to assist me and then 10 more that we would send to various theaters around the world with a set of instructions and authorization for putting these equipments into use. And then, for 100 officers that would follow them, who would first get 9 months of training at Harvard and MIT and would become the real experts, you might say. And so, we did. We carried out that project and that was a lot of fun. So, that was my introduction to the technical side of the Air Force.

(200)

BS: *And radar target.*

JF: Well, we set up a system where all over the world, we enlisted the anti-aircraft fire control equipments for measuring wind slough and the surveillance radars for meteorological observations involving convective storms and so on. And, this meant that really, in each theater, you had one or two lieutenants - they were very junior officers, but they were the only ones that had the right kind of training in those days. But, they had the right kind of papers so they could call into service the deployed equipments.

BS: *Did they do sea surface observations too? You certainly did in the B-18 squadron on the radar when you were looking for submarines.*

JF: What do you mean, sea surface?

BS: *Different phenomena on the sea surface like wave height, currents . . .*

JF: No, we did not do sea state.

BS: *Yeah. Sea state.*

JF: We did do anomalous propagation. We had techniques and while I was at MIT at the Rad Lab, I set up a system where we were forecasting anomalous propagation and that turned out pretty well. But, when I was at MIT, I spent an awful lot of time flying, mainly measuring the complex reflection coefficient which is a function of a whole bunch of things - the meteorological conditions, of course, but also the sea state, the angle of incidence and so on. Because not much was known about it in those days. And so, we had an elaborate program of measurement of this sort of thing. And the group I was with, the Propagation Group, we set up, for example, a range between Marblehead - you know where Marblehead is, it's just north of Gloucester?

BS: *Yes.*

JF: Between Marblehead and Portland, Maine, which was over the sea, because you know the coastline makes a big curve in there and well, probably the most graphic incident is one that is described in that book you had. Because there were certain conditions . . . we were measuring the reflection coefficient was the main thing, but we did encounter anomalous propagation to trapping effects. And on one occasion, we were measuring reflection coefficient by flying in on a constant heading at a low altitude and various altitudes on the beam that the radar set that was set up at Marblehead and that way, you cover a whole range, of course, of reflection angles. And one day, I went out to do such an experiment and I couldn't get a signal over Portland, and I

climbed as high as the damn airplane would go which was 12 or 13,000 feet and still couldn't get anything.

(300)

And so I decided I would let down as low as I could go and let down and there were about 50 feet - you know the signal came bursting in at about 150 decibels, because we had a trapping layer which was very low. Clear conditions. And so, that was, I believe, the first set of measurements on trapping conditions. We flew in as low as, I remember one run at 20 feet off the deck all the way from near Portland to Marblehead. And it was a fully documented case. Our aircraft, of course, was all instrumented so we were able to measure signal strength and everything that you could measure and recorded on the aircraft.

BS: *Fascinating. Now this was when? '43? '44?*

JF: This was '43. This would have been about April or May of '43. That, by the way, the MIT Radiation Lab put out a research of several volumes after the war and this is described in Vol. 13, I guess it is, on propagation. But, that particular day . . .

BS: *It was a surprise. Quite a discovery.*

JF: Yeah, it was at that time, because, as I say, we were just learning what it takes to have an anomalous propagation condition and so on. And to be able to measure the meteorological elements and the radar elements was really quite an advance at that time. Well, anyway . . . I got whipped up in that and spent the rest of '43 and into '44 with the Signal Corps laboratories and then I got involved in reconnaissance - aircraft reconnaissance because, by that time, I had become sort of the Air Force microwave radar exponent.

BS: *Guru.*

JF: And I got involved in developing the aircraft for meteorological reconnaissance. And the first one was a B-25 squadron. You know what a B-25 is.

BS: *Yes.*

JF: Metroline. And so the first job was to design a configuration of the aircraft. You know what a B-25 looks like.

(350)

And, of course, the microwave radar that was the obvious one, was the one that was designed for the B-29, which was the APQ-13 and that used a retractable dome at the belly and so on. But, at any rate, the B-25 has the fuselage - it kind of tilts up - and you could make a configuration in a fixed dome. So, we put a APQ-13 dome under the belly of a B-25 and developed a configuration which was Wright Field was in charge of implementing the designs and they, of course, did it at their - they had a bunch of depots around the country. So, I did two squadrons of B-25s with the APQ-13 fixed dome, but also with meteorological instrumentation and then, after the B-25s, I guess, was the B-24. We did two squadrons of B-24s and sent them - these were all to the Pacific. And then, two squadrons of P-61s - this was the Black Widow. I don't know if you ever . . .

BS: *I don't know that one.*

JF: This was a Northrop design. It was called a Night Fighter. It was like a P-38. It has . . .

BS: *I know what it is now. Yes. I've seen pictures.*

JF: That had it's own radar. It was designed, what they called a Night Fighter, so you didn't have to start from scratch in order to get a microwave installation. So, they simply adapted it to the reconnaissance role.

BS: *When you say reconnaissance, is this reconnaissance for ships and aircraft?*

JF: This is meteorological. Strictly meteorological.

BS: *OK. I got you.*

JF: And then, I was detailed out to the Mariannis B-29, 20th Air Force flying out of the Mariannis from Guam. I was based at Guam which was the headquarters.

BS: *Anderson?*

JF: No, not at Anderson. It was called . . . Anderson's in the north part of the island. It was called Depot Field in those days and later it was called Harmon Field. And I took one of the B-24 squadrons in there. But, that was not my job. The guy who had been designated as the squadron commander was still back in Oklahoma City training the rest of the squadron and my job was, really, to set up a reconnaissance system using the B-29s, because they were the only ones that could reach Japan.

(400)

You know, in those days, the jetstream was not known. We didn't know it. In our radar winds experiments in New Jersey, we had run into what we realized later was the jetstream, but we didn't know what it was at the time. And Lotsby was at Chicago, and he sort of worked out the theory and published the first explanation. But, the point is that meanwhile, over Japan, the B-29 campaign had run into the jetstream. And they didn't know what it was about either. All they knew was that they were encountering 200 mile an hour winds, and if they were going upwind, they spent too long over the target and if they were going downwind, they went over it so damn fast, they didn't hit it. So, it created a sort of emergency. So, by that time, having been involved in all of this reconnaissance design, I was a natural one to get sent out to see what could be done.

So, I spent a few months doing that. And we set up a system using the conventional aircraft. They already had the APQ-13 which is an excellent radar set, so really all you had to do was to focus on procedure and techniques and try to use the normal instrumentation of the aircraft. And the system that we set up was that every 7 hours, there would be a single plane reconnaissance flight and they would be assigned normally three or four individual cities. It wasn't always the same ones.

BS: *You go in ahead of the bombers?*

JF: Ahead of the bombers, but in order not to telegraph the punch, it was done on a regular schedule, every 7 hours, we'd have a reconnaissance flight. And that was really target reconnaissance, though you did as much meteorological and other kind of reconnaissance as you could. And we cranked it into the reporting and analysis system for the meteorological service, but basically, it was just target reconnaissance.

BS: *Have any trouble with Japanese fighters?*

JF: No, because we normally flew at 31,000, which was pretty high. A B-29 could do it. And we didn't carry bombs.

(450)

We carried extra gas, but no bombs. And the fighters, they just couldn't get up there quick enough, at least. I remember, there was a big scare at one point, because the rumor was going around that they had one of the German jets, whatever it was called, they had it made an appearance out there and, of course, the rumor got blown up and what not. I was never attacked by a jet and the (Ak-Ak) was generally pretty ineffective at that altitude.

So, anyway, I was out there until about the end of '44. Then, I got called back again because by that time, I had become identified with anything involving aircraft radar or reconnaissance, so they had other tasks that I was assigned to do. So, I got brought back to the New Jersey-Washington area and. . . try to remember, I guess instead of being assigned in New Jersey, I think I was detailed to the air staff at that time, because the interest was still in reconnaissance. But, certainly I knew nothing about the Arctic in those days. But, I had come to a lot of notice involving radar and reconnaissance. So, after the war, well . . . because I had spent so much time in the US instead of overseas, they had a system of points, you know. You had two points a month overseas or three, and one point in the US. Anyway, I didn't have enough points to get out at the end of the war in '45. And so, the war ended in the summer of '45, and the Air Force came up with another special assignment - the All Weather Fying Division which was based at Ohio State. It was at Lockburn.

BS: *At Lockburn.*

JF: Just south of town, you know. So, anyway, I went out there and started organizing. One thing that came out of that was the Thunderstorm Project.

(500)

And for the Thunderstorm Project, the two squadrons of P-61s that I had been the project officer for getting modified had never gotten deployed. They were scheduled to go to the Philippines and the war ended before they ever got sent out there. So, they were put in storage down in Texas. And so, they were perfect for the Thunderstorm Project. So, I got two of them sent up to Lockburn and then we got the rest of them assigned to the Thunderstorm Project which was later conducted down in Florida at Pinecastle. And they worked out perfect for that.

BS: *I guess they would. They had a lot of assets available I imagine then.*

JF: Oh, yeah. It was perfect. And I still remember the first two that came up. As I said, they were in storage someplace in Texas, and they were flown up to Lockburn by ferry pilots and left. And I started using them, and about the third take-off from Lockburn, one of the engines went out. And fortunately, that's a nice airplane, by the way, but fortunately, it didn't result in an accident. But the point is, we came around and landed and you know what the problem was? In the storage, they had put silica gel bags in the carburetors.

BS: *Oh really?*

JF: And they never took them out. And one of the bags finally burst and fouled up the carburetor. Well, anyway, I was trying to get out of the service, but I didn't have enough points to qualify and quite by accident, I happened to, I forgot for what reason, but I had to go to Washington for something or other, and I happened to be on the elevator with the head of my service and he said, you know, "What are you going to do?" and I said, "Well, I'm going to go

back to graduate school. I'm going to UCLA and get up on the roof of the physics building and get a suntan and enjoy life for a while and finish my graduate school."

(550)

And he said, "Well, is that really what you want to do?" And I said, "Yeah." And he said, "Well, do it. What do you need? Go, and we'll think of something to call you. We'll call you a project officer of some kind," and that's what I did. I said, "Well, I'd like to take some of my lieutenants." And he said, "All right," so I picked up three of my lieutenants and all four of us went out to UCLA and enrolled.

BS: *You were a Colonel at the time, or . . . ?*

JF: Oh no. Hell, I was a Major. And we said, here we are, and we enrolled. I should have gone back to MIT because I had completed a lot of work there, but I was thinking mostly of the suntan. So, anyway, I went to school for three semesters, I guess. I got a Master's degree in physics there at UCLA and then I interrupted that. I should have stayed there and finished the Ph.D., but I had a guy that worked with me during the war that came through and talked me into going to Wright Field.

So, I went to Wright Field in '47, and there again, I was involved in radar and instrumentation and so on, because that's what I was identified with by that time. And I was head of what was called the Research Plants Branch, but that was a headquarters job and I guess I spent about a year there, and then I was reassigned to Boston to the . . .by that time, part of the Radiation Laboratory had been handed over to the Air Force, or to the Air Corps it was in those days. And part of it was retained by MIT. And the part that was handed over to the Air Force was called the Cambridge Research Laboratories and there were two divisions. One was the

electronics division and it had about five laboratories - there was one laboratory for antennas and for various other facets. But, that was almost a spin-off from the Rad Lab.
(End of Tape 1 - Side A)

_____

(Begin Tape 1 - Side B)

(000)

JF: In the meanwhile, down in New Jersey, partly as a spin-off from the work that we had been involved in there, they had set up a couple of laboratories on meteorological instrumentation. And what I wanted to do was to transfer those to Cambridge and to have a parallel division to the electronics laboratories for geophysics. And fortunately, my boss was influential enough in the Wright Field hierarchy to set that up. And so that's what they did. And so, I became the Director of what is now called the Air Force Geophysical Laboratories. In those days, it became what was called the Geophysical Directorate. But, it consisted of about 5 laboratories, a couple of which had come up from New Jersey and the others were formed from what was already more or less available in the Boston scene. And they're still operating. It's now called the Phillips Laboratories. And I haven't been there for decades now, but Ned was on a trip - a committee that did a review there recently. And Ned sent me an account of that along with a picture that they took from the front lobby, and so on.

BS: *Everybody was there.*

JF: It's very interesting because it all has it roots back in what I've told you about really stemming from the anti-submarine days. So anyway, that was the third time. I lived in the Graduate House three different times. I lived there a year going to meteorology training and I

was there about 6 months with the Radiation Lab, and when I came back after the war, I had moved into Grad House again.

BS: *That was '47.*

JF: Yeah. The end of '47. So, '48-'49, I was there, working with geophysical labs.

BS: *You flew too?*

JF: What?

BS: *You had planes up there too? Aircraft?*

JF: Yeah, by that time they had built a strip at Bedford. They didn't have the installations that they have there now. Hanscom Air Force Base. Yes, I failed to mention that, but I should say this was really better than you realized because when I got back in '43, when I got reassigned up to the Rad Lab, I went to the guy I was told to report to in the Air Force - he was in the Pentagon - and so he used to fly up to Boston on week-ends just to get his flying time, and usually I'd go out and have a big talk. And he was complaining because I was not doing what I was supposed to be doing there. And I was trying to promote what I thought was the projects that I mentioned which eventually succeeded. But, in the meanwhile, in order to do the kind of measurements that we wanted to do, or that I would like to have, I was measuring reflection coefficients and so on, I needed an airplane. And so, by talking with him, he got me an airplane. And so it was assigned to me and me only. Nobody else flew it. And I was able to chop it up and put on instrumentation and so on, and so really, from that day on, I had my own airplane. And when I came back again years later, after the war, again, I had an airplane at Bedford. So, it was very convenient.

BS: *That's right. The researcher running the Air Force.*

JF: Well, they were good. I must say, it took me about 6 months to get them to notice me. But, when they did, they did it right. After that, they never refused anything. And gave me just about everything I wanted.

BS: *And this took you up through what, '48?*

(50)

JF: Yeah. Well, then, in '49, the first thing that happened was the Air Force was going through sort of a transition. The Berlin Air Lift was in full swing. And the Air Force adopted a policy that everybody that had not had a full tour overseas, they wanted to get out and do something useful doing a flying assignment. So, the first thing they did was to come up with a set of orders sending me to fly C-54s on the Berlin Air Lift. And that came as quite a surprise. I mean, at the time, I was Director of Geophysics Labs, but it got my attention, because the message was, you damn well better get an operational assignment or else you'll be sent on something that you're not interested in. So, I was offered an opportunity to take a squadron in Alaska and I said, "Hell, yes. I'd be delighted to do it." Because that was more or less in my line. It was a long-range reconnaissance squadron and it was a technical squadron and so . . .

BS: *This was at which base?*

JF: At Eilson. So, I went up to the CO of the 58th Strategic Reconnaissance Squadron, then called VLR (Very Long Range) because . . .

BS: *What kind of aircraft?*

JF: B-29s. We were flying B-29s, but we had very long treks. We were flying way over weight and our job was to, and this was just before the Korean War started. So, the upshot was that almost my whole tour in Alaska was during the Korean War. And our job was to fly off the Russian coast. On alternate days, we'd go between Eilson and Tokyo, either one direction or the other. And on the alternate days, we'd go along the north coast - Siberia - and do a big dog leg up to the North Pole, which was called the Ptarmigan trek.

BS: *Those were the Ptarmigan flights.*

JF: The north ones were. The others were called the Loon Treks. We had two or three.

BS: *Loon?*

JF: Loon. L-o-o-n. We had two or three alternate treks for Loon and two or three alternate treks for Ptarmigan.

BS: *Ptarmigan was flights up over the Arctic then?*

JF: That was on the north side of Siberia. And up to the Pole.

BS: *I see. How far would you go along the Siberian coast?*

JF: Oh, about over to Tixie. Just short of Wrangle Island, actually. The turning point was over near Wrangle Island and then up to the Pole and back. That was the westward Ptarmigan Trek. And then there were a couple of other Ptarmigan Treks that were farther to the east.

BS: *What was the purpose of going to the Pole?*

JF: There was no coverage. I guess you'd say it was simply to provide a fuller picture of the map analysis. We had, by that time, we had developed a drop sohn which we had done in New Jersey, by the way. That was part of . . .

BS: *Drop sohn, for a submarine?*

JF: No, no, for meteorological, like radio sohn, only dropping it out of the airplane.

BS: *OK.*

JF: So, we had a pretty good suite of meteorological instruments, but also, of course, the main mission was radiological. We were equipped with filters. I guess I should mention that while at the Geophysics Lab in Cambridge, came the Bikini tests and the bomb tests, and we sent a team out to develop a system of filter recovery and analysis. And so, AFTAC was the Washington based organization, you know, that ran this system.

(100)

But, they equipped 6 squadrons of B-29s with filters and the Alaska Squadron was one of them.

BS: *Now, you're position in the squadron was? Were you the CO of the squadron?*

JF: Yeah.

BS: *OK. That was your first command then.*

JF: That was my first command. That's right.

BS: *And that was '58?*

JF: Yeah, that's right.

BS: *And you flew the first Ptarmigan Flights.*

JF: I went up there as a Major. The guy who was a squadron commander had brought the squadron up from California. He was a friend of mine. I had worked with him in the reconnaissance program during the war. And he was a full Colonel. And anyway, there were no promotions in the Air Force. Between the end of the war, the froze promotions and they didn't start them again until the Korean War. So, anyway, I made Lieutenant Colonel while at that assignment in Alaska and then full Colonel shortly after coming back again.

BS: *The Ptarmigan Flights. I've heard of them, but the purpose of Ptarmigan Flights, again, was weather reconnaissance?*

JF: Yeah.

BS: *That when you discovered ice islands?*

JF: Right. That was when I got introduced to the Arctic, you might say. And I had never, up to that time, hell, I thought the coldest place in the world was Harvard Bridge, you know, that goes across the Charles.

BS: *I think it still is.*

JF: From the Grad House to Boston, because I used to have to walk across that. But, anyway, that was a complete introduction to the Arctic for me. But, I'll tell you, it was an education.

BS: *So the colonel introduced the Ptarmigan Flights and then you became CO after him and continued to ___the flights.*

JF: Correct. His name was Carl Rock. He was a great guy. He had been a reconnaissance flight commander during World War II and I had equipped his airplanes, and so on. So, we were well acquainted.

BS: *OK. Well, Ptarmigan's on my list of things here to ask you about. So I've crossed that off. I didn't know what Ptarmigan was, but I've heard about it from old Arctic hands for years.*

JF: Yeah, Carl Rock started the system and because he brought squadron up from California, they all did their two years and came up for rotation at the same time. And so, I got there with a completely green crew - air crew and ground crew as well. And to complicate things, the Alaskan Command had decided that they wanted to make Eilson an operational base. It was just a big runway at that time and the squadron was based at Ladd which was over at Fairbanks. And they had a short runway which was up against a river at both ends and they really, a 3 mile runway was very welcome with an overloaded B-29. So, I got there in time to move the squadron over to Eilson from Ladd and I got there late in the year and that was a nightmare. I'm trying to think of how to describe it. But, to make the story short, I got there about Christmas time. We were just starting. They had 200 people at Eilson who were just, you know, caretakers. I moved in on top of them with a squadron of 12 B-29s, about 110 officers, about 600 enlisted and a high priority mission and they didn't have a goddamn thing for support. Maintenance, supply, you

know - it was a caretaker operation. And, to complicate things, we had a very high priority in the Air Force priority system. And because we had a high priority, when they improved the B-29 engines, which was really a very poor engine, I don't know if you know them.

(150)

BS: *No, I don't.*

JF: 4360. It's the same engine they had later on the P2V.

BS: *Oh, I know what that is.*

JF: There was a time in Guam when the average engine life was 20 hours.

BS: *Wow.*

JF: Thirteen hours up to Japan and back, which meant 4 times thirteen - that's 52, so you could figure your odds of getting home on two or three or something. It was terrible. After the war, by the time I got up to Alaska, we had the average engine time greater, but it was still not very good. So, the Air Force was trying to improve the engine. And so they had a big remapping in which they did a whole bunch of things. They changed the valve overlap to get a tighter cylinder. They had floating valve seats. There were a dozen things that they did all at once. So, anyway, because we had a high priority, we had the top priority for getting the new engines. So, in February, goddamn it,

BS: *Which year was this now?*

JF: This was by that time, '50, we got orders to change all the engines. Well, shit, we had no . . . we had one little birchwood hangar that you could drag an airplane in and just have that much room at the wing tips. And we had to use that for pre-hitting mission aircraft. For maintenance, we had 4 canvas shelters to use as a nose dock. And otherwise, you were out in the open.

BS: *Wow.*

JF: So, we had 48 engines to change in February. And you can imagine what an introduction that was. And with a green crew. The other guys were like me. They had just come in the previous summer or fall and so they had no experience in the Arctic.

BS: *Cold weather maintenance.*

JF: Well, you know, part of the time it was 60 below and it was below 40 a lot of the time. Well, anyway, we changed all the engines and then we started to fly and we had 12 engines that year with less than 100 hours. They were popping like popcorn. And I complained and tried to get somebody to pay attention and nobody would pay much attention. So, I grounded the squadron which is a pretty big step to do at that level. And that got their attention. So all of a sudden, I had delegations from Wright Field and from the Tech Reps and everywhere else. And it turned out, they had made a mistake on the production line and in this modifications they had made on the 4360 engine. The big mistake was that they had used an improper tolerance on the risk pin where the piston connects to the . . . and they had left too much tolerance so that the risk pin was beating the bushing, the bronze bushing, was beating it to pieces. And, of course, then, that cylinder would fail and then the pieces start flying around, and then all the cylinders would fail. So, the engines that failed that we had opened up, you know, all you found was fouled junk, because. . .

(200)

BS: *Couldn't tell what had caused it.*

JF: That's right. The whole row as gone and they did run down the trouble. But then, by that time, it was about March and we had another 48 engine changes in the open. So, we had like100 engine changes during the coldest part of the winter with green crews. And that, I will never forget that first winter.

BS: *Wow. Were you married then? Family with you?*

JF: I was married on the way to Alaska. When I got my orders for Alaska, I got married.

BS: *Take your wife?*

JF: Yeah. That was her first introduction to the Air Force. To begin with, we had no living quarters at Eilson. We lived in the barracks with the rest of the squadron. And then later, we had three Quonsets for officers and three Quonsets for enlisted for that whole outfit and anybody else who had their family had to commute 26 miles to Fairbanks and living quarters were very tight there too. It was really a hardship post, which I thought of in later years, because later I spent three delightful years in Norway which was designated as a hardship post. And you got extra pay. And I'd think back to the days of Fairbanks when it really was hardship and you didn't get any extra pay. And you couldn't get any . . . I had a bunch of guys who had been through the Bataan Death March, for example. And I had tried like hell to get them transferred to more comfortable billets in the States. They were married. They had kids. And I got slapped down. I couldn't succeed in getting them transferred because the Korean War was on and the headquarters took a hard line.

BS: *And that was all a part of Ptarmigan.*

JF: What?

BS: *That was all part of Ptarmigan.*

JF: Yeah. So anyway, that was my introduction to the Arctic.

BS: *When did you spot the first ice station? Not ice stations, but T targets?*

JF: Well, the first T target which was T-1 had been stumbled across before I ever got there. That was, in fact, early on, at the time Carl Rock had brought the squadron up, you know there was so little known about the Arctic that they were still looking for undiscovered land and things like that. And T-3 had been sighted and was classified. It was too big to get it into a three combination sight, but it was classified information about it. So, naturally, this interested me. And I started a system of keeping a look out for any similar (targets) because it stands to reason, and it turned out to be the case, that the radar image is quite distinctive. I mean, as you know, you can't miss it, really. So, we started a system of keeping a watch, put it that way. And I guess T-2 showed up, which was the big one. It was the biggest of the three. And then T-3 was the third one and I'm trying to remember exactly the date. It kind of escapes me. I think that must have been about November, of 1950. It was in the dark time of the year. In the meanwhile, T-2 . . . I guess that was, T-2 was . . . well, both of them were within a few months. But an interesting story this just reminded me of this. Meanwhile, unbeknownst to us, the Russians, after their 1937 expedition, had set up another drifting station, which was their Number 2 - North Pole 2. Well, anyway, and I guess this was in November of 1950,

(250)

BS: *So you knew that they had people down on ice stations.*

JF: No, we didn't. I'll tell you what happened was that . . .

BS: *But, you knew about the '37 expedition then, didn't you?*

JF: Yes. Well, I read about it in part of my general background reading. But, one of my crews came back. Well, I guess this was about November of 1950. I was waked up in the middle of the night by a message from the Ptarmigan crew and they reported seeing runway lights hundreds of miles out on the pack ice. And so they sent what they call a service message. A service message gets everybody out of bed all the way up the chain of command. And so I was the first one that got waked up and it went on through the Alaskan Command and Air Command and got clear back to Washington. So, anyway, when the mission came back, I interviewed each of the crew members individually and talked to them and the aircraft commander was really a very reliable guy. I mean, I knew the people and my conclusion was that they saw runway lights, for Christsake. And they were sufficiently excited by this that they departed from 500 millibars and made a letdown which was against the rules anyway, but they did and when they were going through about 10,000 feet, the lights went off. And as near as I could conclude, that's what happened. That's what they said happened and there was no other explanation and so that's what I reported, too. And nobody believed it.

The intelligence officer for the Alaskan Command down in Anchorage, you know, don't remember his name right now, but at any rate. They called me down. I had to jump in a Gooneybird and went down to Elmendorff. I went in. Talked to the general and he had his intelligence officer there and they said they didn't believe it. And I tried to tell them, I knew these people. And I know how to weigh what they say. And they said, no, they saw a reflection

of the moon or something in the open lead and what not. Well, years later . . . and the point is, well, at the time, the report was discredited, but they didn't get me to back down. I stuck to my story. But, it was rejected by the chain of command. Years later I got acquainted with Treshnikov and Somov - Somov was a Station Leader for North Pole 2. Treshnikov was at the Arctic and Antarctic Institute and he happened to visit them at that time and they told me about becoming aware, they heard an airplane, and they went outside and looked and they saw the lights of the B-29 and they persisted for some time.

(300)

They couldn't see them, but they presumed that he was sighting down, which he was, and so they turned off the lights.

BS: *They knew it wasn't one of their planes.*

JF: That's right. And my point is that they got a big laugh out of this. But, I discussed this with Treshnikov and Somov when they were together and all three of us, you know, drank a bottle of vodka and had a big laugh about it. I told them about the . . .

BS: *Was it in Leningrad that you did this?*

JF: Yeah. In Leningrad. This was in 1969, this was my first visit there.

BS: *That's when I saw Arliss V. I was a grad student.*

JF: Interesting sidelight. It tells you something about the intelligence system.

BS: *Well, that's very important, you know. That leads up to, where are we now, 1950-51?*

JF: Well, yeah, but I must say, the information never did connect really with me. In order words, I never realized that there was a drifting station. Put it that way. I believed the report that the crew made, and I stuck by it, but I couldn't explain it. And we had no confirming evidence. It was only years later that we found out that that was North Pole 2.

BS: *Interesting.*

JF: At any rate, I got interested in the idea of putting a station on an ice island. You had T-2 and T-3.

BS: *You were still Skipper of Ptarmigan.*

JF: Yeah. And my boss, who was a Colonel down in Anchorage and his boss was the Commander of Alaskan Air Command who was General Armstrong at that time from the, what's the movie, "Command Decision?" Frank Armstrong? So anyway, I persuaded my boss that this would be a good idea. And because of my wartime service, I was well enough known among the people back in Washington in my service, so they were, you know, they would listen. And the upshot was that, anyway, they were finally persuaded that after my tour was over, I could get an extension for, initially, it was for 3 months. Turned out to be 6 months. And we would set up our project and try to establish a permanent station on an ice island.

(350)

We . . . I never considered doing it on the pack ice. I can see how the Russians could do this. But, my idea was a bit more ambitious - to get a place that you were sure was permanent and that

could be developed and things of that sort. And, of course, at that point, you could bring in the argument about using it as an emergency landing field and maybe as a staging base and things of that sort, which you couldn't do talking about a pack ice station. So, anyway, this was - they agreed to set up such a project so that when my tour was over and I was up for _____ back to the States, I could stay on to do this. And so,

BS: *This is . . .so, you were relieved of command first and given a special project.*

JF: You're right. Exactly. And so that started about the end of December, I guess, of '51. So, anyway, there were a lot of uncertainties, of course. For one thing, the guys who were on the general staff down in Anchorage thought this was a crazy scheme and were very negative towards it. And by that time, I had become acquainted with General Olds who had replaced General Armstrong as Head of the Air Command, and I could tell that he was sympathetic towards it, but he was being advised by his staff that it was a nonsensical project and was doomed to some kind of emergency rescue operation, and so on. So, this created some complications.

But, the head of the Air Force Weather Service back in Washington was a guy that I did a lot of work for during the war in what I described. So, he knew me and had confidence in what I was doing and had visited my squadron in Alaska and so on, so he got behind it in the Pentagon and so they approved it. The Alaskan Air Command said, "We propose to do this," and they got approval from the Pentagon to go ahead and do it. But, because of the uncertainties and because of the many forecasts of bad outcome, I wanted to make sure it succeeded. And we had, in our reconnaissance activities, we had developed a pretty good picture of the weather patterns.

(400)

You know, 85% of the time in July and August, for example, you've got low overcast. And you know you can't do anything of that sort. So, I wanted clear weather and I wanted to optimize the weather factor. And we knew, by that time, we knew how to operate in the Pole. So, I picked, of course, the earliest Spring that we could operate in. As soon as we had enough light to operate in the Spring, to do the operation and we went in on the 15th of March.

BS: *1950?*

JF: '52. And before going in, it was just a handful of people and we were operating on a shoestring, I got a $10,000 authorization from Alaskan Air Command to buy a generator and some warm sleeping bags and some things like this, you know . . . as I say, they were very nice about it, but we were really a shoestring operation.

BS: *And what type aircraft?*

JF: Well, that was a good question because I wanted to . . . well, to back up a little. In preparation for doing this, I had done a reconnaissance flight. I diverted one of our Ptarmigan flights back in the summer of '51, and as you know, you have 85% of the time, you have overcast, but there is a period of maybe one or two days when you can have clear weather, and when we had that kind of an opportunity, I grabbed one of the mission aircrafts and did a reconnaissance of T-3 and T-1. T-2 had gone over, but T-1 was over towards Ellesmere.

BS: *What did you say, you'd gone over the Denmark Strait?*

JF: Yeah. T-3 was up headed for the Pole and T-2 was over near Ellesmere and not in as interesting a location because it appeared to be pretty much stuck and I have a nice reconnaissance film. There was a problem because from Barrow, the Gooneybird doesn't have

the legs - at the time. Well, the T-3 was at about 89 degrees and so, really, the only feasible way to get there is by way of Thule.

(450)

And the trouble with that was we were at Fairbanks. And there, I lucked out because the cold weather test division at Eglund, part of the Proving Ground Command - they had sent the C-124 up for cold weather test - you know each of the new airplanes are given a cold weather test period, and so up came a C-124 with a nice guy - I forget his name now, but as the project officer, to run a bunch of flights which would be the cold weather tests for the C-124. Well, I got acquainted with the guy and I talked him in to giving us a ride to Thule. So, he loaded up a whole C-124 which carries a lot of stuff, as you know, carried the whole damn thing over to Thule just as a favor. And otherwise, we never would have been able to haul our stuff to use Thule. But he did. And we went over and we did that.

BS: *He flew the Gooneybird over.*

JF: Well, not then. We just flew the C-124. And he loaded up to the gunwales and that, essentially, was all our stuff. Everything I wanted to take out to T-3. And I remember, I guess it was January of '52 and we came in without notice. About all they had was a flight land notice. And I remember, they had a guy who was the Air Force Commander at Thule and you know, Thule was only about half built at that point and this was sort of a holding group during the winter. And you know, he was surprised to see us and, you know, what are you going to do now? Well, when I told him we were going to go North from here, he couldn't believe it. But, anyway, he was very helpful and we unloaded all our junk and went back to Ladd field with the 124, but I'd say that single 124 flight was really the key to the whole thing, because without it, I don't know how the hell we would have gone anywhere. And when it came March then, I enlisted the

10th Rescue Squadron. Berndt Balchen had been the first CO of the 10th Rescue Squadron and he had already rotated back to the States. He was in the Pentagon and there was another guy named Arnold who was new squadron commander, and they were very unenthusiastic about this project, too.

(500)

BS: *Berndt was?*

JF: No, no, Berndt was fine, but he had left, oh, 4 or 5 months before. So, he was not around any more and Arnold and his guys were not keen about it at all. But, by that time, the general was. By that time, he was on board, the general. So we enlisted the 10th Rescue Squadron and they had a couple of C-54s and a couple of Gooneybirds on skis.

BS: *And they were based at Thule or . . . ?*

JF: No. Elmendorf.

BS: *Elmendorf.*

JF: Well, one of the Gooneybirds was at Ladd Field, at Fairbanks. So, anyway, we did the usual thing, you know. Prepared the operations plan and enlisted the things that we needed and in the meanwhile, I had set up a little Quonset on the ice of the Cheena River there at Fairbanks and had picked up my initial cadre which was three other guys and the four of us moved in and lived there for a couple weeks to make sure we had all the things you needed in the boondocks. The only way to tell is to do it. And then, you make a list. It's like going camping. Put on your pack and walk 100 yards, put it down and then go back and pick up all the things you've forgotten.

BS: *I know.*

JF: Well, that's basically what we did. But, at any rate, we had our stuff together by then. And with our stuff already cached over at Thule, all we had to do was to use the Gooneybird and C-54s and we could go across. We stopped at the Canadian - what's the?

BS: *Alert?*

JF: No, not Alert then.

BS: *Frobisher?*

JF: No, Resolute Bay. We stopped at Resolute and then went from there to Thule. That was just to get over.

(500)

BS: *Get some gas.*

JF: And then came a problem. You know, during the time that I had the B-29 squadron, we used to make practice landings at some of the Canadian air strips just in case you needed it in an emergency. And so it was a routine thing to send a message to the Canadian Authorities and request permission to use the strip. And we'd been in to Resolute and a couple of the other places.

(End of Tape 1 - Side B)

(Begin Tape 2 - Side A)

(000)

BS: This is an oral history interview with Colonel Joseph O. Fletcher taken at his home in Boulder, Colorado, on 23 January 1997 as part of the American Polar Society Oral History Program to preserve North America's heritage of polar science and exploration. Interviewer is Brian Shoemaker.

JF: Because of our B-29 experience of making practice landing at some of the Canadian strips, we had come to take for granted approval. They had never refused us. It was a routine kind of thing. So, at any rate, when I got to Thule, well even before I got to Thule, I was still in Alaska, I sent a request, of course, notification that we wanted to use Alert, because even operating from Thule, you needed to stage at Alert. And I took for granted that they'd say, "Fine," and this was in the Spring, and I had already checked and found that the strip at Alert had been cleared and was operational and ready for use. Well, I didn't get the answer until I was already at Thule and the answer was, "No, you are not allowed to use Alert." And they suggested, they sent the message back to the Pentagon to the Alaskan Air Command and to me at Thule and said, "No, no authorization to utilize Canadian strips, and we suggest that for this project, that it be postponed for at least a year and you should coordinate with Canadian authorities and do it in conjunction with (I've forgotten the organization in Ottawa that was the R & D organization)."

But, you can imagine how I received that because shit, I had finished my tour in Alaska, I had orders for my next assignment, I had gotten a three months delay and postponing it meant scuttling it. And so, the question was what to do? Well, it turned out that the Danish representative was at Thule at the time, so there was a Canadian representative and there was a

Danish representative and I didn't have authority from either one of them to use their facilities or territory or anything and yet, you know, to stop at that point would have meant jettisoning the whole thing. So, we had a little huddle and we decided to stage without the Canadian help - without using Alert. And so, the idea was to pick a spot, pick a refrozen lead that you could land with wheels but we could use the C-47 with skis, stash a bunch of oil drums, fuel drums, and then use that as a staging point. So we went up Frobisher or whatever - what is that channel? Eureka?

BS: *I know what you're talking about - it's between Ellesmere and Greenland.*

JF: That's right. And at that time of the year, you've got clear weather almost all of the time. It's colder than hell, but it's clear. You can see what you're doing. And we could triangulate on the northern tip of Greenland and on the northern tip of Ellesmere and so you could find the thing again. And you really needed to . . . you know how hard it is to find anything. So anyway, that's what we did. We put out about a dozen drums of av gas the day before and staged there on the day we went up. And, of course, once we got up there and got established and the Canadians heard about it, they were falling all over themselves saying you're welcome to use Canadian facilities, you're welcome to use Alert, you're welcome to anything you want.

(50)

But, that's why we didn't use Alert. I went into Alert flying from T-3 later on, but not flying from Thule.

BS: *Where was this cache, fuel depot cache? How far north?*

JF: It was about, as I say, it was just barely within sight of the mountains you could see.

BS: *Oh, visual.*

JF: Oh, visual, absolutely visual. You had no other way of doing it. But, this was my point. We had carried a theodolite just in case we needed it. So we could triangulate on the peaks, on the northern tip of Ellesmere and the northern tip of Greenland just to make sure that we could find the place again. But no, this was all visual.

BS: *Well, neat. And what was this, this was March?*

JF: This was the 15th of March. That was the date.

BS: *Sun's just getting up.*

JF: Just getting up, that's right. And we had a C-54, and so I sent out the C-54 a couple of hours ahead of us. We scheduled the time it would take to do this and I wanted the C-54 to find the ice island. We had located it flying out of Thule because we only knew in a very, very general way the area to look in before. But, we had located it a couple of days before, but I wanted to locate it on the day we were going to go in. So, the C-54 was sent out a couple of hours ahead of time and they had a kind of a rudimentary radar that was helpful. Radar didn't work much, but it is better than nothing and so they found T-3 and we scheduled ourselves so that we'd be getting into the vicinity with the C-47 about at the right time and then, they gave us a signal - VHF signal - so we could home in on them. And, as a matter of fact, well you know the routine with the 180 degree ambiguity and the truth is, we went the wrong direction first.

BS: *I've done it since.*

JF: Well, when we found the signal fading, we turned around and did home in on the C-54, who was circling T-3. And so, we found it and made a landing and the C-54 was carrying some drop stuff that they could give us for the initial supplies and they did and so that was the start.

BS: *Now the flight up there took how long with the Gooneybird?*

JF: You probably remember distances better than I, but it's what, maybe about 700-800 miles from Thule to the Pole?

BS: *No, it's less than that. Less than that, but I can't remember.*

JF: Must be around 700, something like that. So, it took us a little time to refuel the drums that we picked up. So anyway, the C-54 had enough endurance so that it didn't stretch them any. We had to refuel the T-3 to get the C-47 back from the air drop.

BS: *Now you first went in to the lead on the way north and refueled. Was this a little hand pump on a 55 gallon drum?*

JF: Right.

BS: *And uneventful landing on the sea ice, on the lead?*

JF: Uneventful landing on the sea ice lead. That's right. But, it was a rough landing on T-3 because it didn't break a ski on that trip. On the next landing, it broke a ski. So, it was a little rough, but it was uneventful.

(100)

BS: *Where did you land there? Was it where we built the camp later near the bay? There was a bay on . . .*

JF: Coley Bay?

BS: *Coley Bay, yeah.*

JF: Well, I can remember . . . let's see T-3, kind of like that. This would be Coley Bay. The initial camp was about here, and we landed . . . the initial landing was about like that.

BS: *89 degrees north.*

JF: Right.

BS: *Well, later on a piece broke off of T-3. Which piece was that? Remember it broke in two over by Barrow.*

JF: I don't know. I remember reading about it, but I don't know where it broke.

BS: *It was 12 miles long, originally.*

JF: Seven. Seven by four.

BS: *Well, interesting. And how thick was T-3.*

JF: That was the first question we wanted to find out. Bert Crary came in on the second landing and that was one of Bert's projects. Anyway, the answer is about, as I recall the first measurement we had was 162 feet. It was about 160 feet.

BS: *Who was on that first flight with you. Can you remember all their names?*

JF: Oh, yeah. There were two who were going to stay with me. And this was a colleague that I had had, the head of the Arctic Arrow Medical Laboratory was a guy named Rhodahl. And he was a good friend of mine. The Arrow Medical Laboratory was at Ladd Field and he volunteered and that served the purpose of having a doctor handy. And Rhodahl, also, was very knowledgeable about cold weather and we had gone out and camped together at 60 below around Fairbanks, so we had gotten used to each other. And the third guy was a Captain from Anchorage. His name was Briniger and Briniger was sort of a survival specialist or cold weather specialist and he had been involved in a couple of exercises that the Alaskan Air Command had run. So, there were three of us who stayed at T-3, myself and Rhodahl and Briniger.

BS: *How do you spell Rhodahl?*

JF: R-h-o-d-a-h-l.

BS: *OK. So, the Gooneybird stayed around and the C-54 orbited, waiting for the Gooneybird to take off. They wait till they'd take off to go back?*

JF: Not quite. By that time, they were getting low. So, they had already departed, but not by much. It had only recently gone, so anyway, they were available if needed.

BS: *Who was the flight crew? Do you remember that?*

JF: Yeah. I can't give you all of them, but I can dig them up.

BS: *I think they're on the Ice Station book. I can look them up.*

JF: Erhardt, Lou Erhardt was the Gooneybird commander. The C-54 was the head of the 10th Rescue Detachment at Ladd Field.

(150)

His name was Gordon Bradburton. He was a Major. Real nice guy. As I say, the Rescue Squadron was very unenthusiastic about this in the beginning, but once we got into it, they were very good.

BS: *So they left you there.*

JF: Yeah. After some negotiation.

BS: *What did you live in?*

JF: Well, I forgot. I left out a detail. We were at Thule, ready to go and I had circumvented this scheme about not having authorization from the Canadians or from the Danes and along came, the day before, damned if the Commanding General of the Alaskan Air Command didn't show up flying from Alaska, because by that time, he had become interested enough that he couldn't resist. He wanted to be involved. He was an interesting guy. This was . . . this guy's name was William D. Old. He had been the Hump Commander. You remember the Hump into China? He

had been the operator of that operation. So, he had a feel for logistics and for meeting unconventional situations and so forth. So, Old was with us in the Gooneybird landing.

BS: *Oh. General Old was.*

JF: Damn right. He insisted on going with us. That was not the plan. I didn't expect him to be over there at all, but he showed up the day before and he damn well wanted to be engaged. So anyway, that complicated things because when we landed at T-3, he took one look and said, "The hell with this. Let's get out of here." And so we had a bit of negotiation, trying to get that sorted out. But, he was a pretty good guy and we did reach an agreement. So, he climbed on the C-47 and went back with Lou Erhardt. Lou Erhardt, by the way, still lives in Anchorage, and I have his address here somewhere. He was a very good ski pilot and he retired, of course, early on and he flew for Air America in Laos and went through that operation. I've talked to him a couple of times, but not in the last few years. But, I do know he's still going strong, living in Anchorage.

(200)

BS: *I think he's a member of the Polar Society.*

JF: Could be.

BS: *I'm pretty sure he is.*

JF: Well, anyway, that's about it.

BS: *Well, they left you there, though. There you are. You're camped out. How was that?*

JF: Yeah, well, the best account of that, by the way, is a book written by Rhodahl. Let me look. It was very popular. It was published in about 30 languages. Here's the French version, here's the Japanese version, this is the English version, here's the Russian version, here's the Spanish, and here's the Norwegian. So there are a few more I have upstairs, but I picked it up when I've been in other countries, I've picked up copies whenever I came across them.

BS: *Well, interesting. Very interesting. So, you were left there that night. Did you have the tent up when they left?*

JF: No.

BS: *You lived in a tent though, I take it.*

JF: As I say, Rhodahl and I had practiced all of this back at Fairbanks. And we decided to use the standard Army mountain tent. It's a double wall, pyramid. You know what it looks like.

BS: *Yeah.*

JF: Well, we had two of those. We only used one, and I had ordered with the funds that I got from Alaskan Air Command, I had ordered four Wood's top quality sleeping bags - four-star sleeping bags and they were really good, you know. They are a hell of a lot better than issue, that stuff, and they were big. They were 90 by 90 and you folded it over and made it 45 by 90. So anyway, they were the key. You could be comfortable at 60 below in that.

BS: *Then, the second flight came in.*

JF: I should say, my great fear was having an accident of any kind. You just can't afford to do that, so one of the things that I had set up pretty firmly was that they were absolutely not to come back until we asked for them. Because I didn't want any airplanes tooling in there and having an accident or some other complication. So, the instructions were that they were absolutely not to come back until asked for and the Ptarmigan Flights, you know, my squadron, we had a walkie-talkie - a 400 megacycle walkie-talkie, would deviate from their Ptarmigan Track just enough to be overhead so you could use the walkie-talkie. So that would be an emergency. So that's what we did and they stayed away. And it was about 2-1/2 weeks before the second flight came in. Or no, it was about 2 weeks. And during that time, it was just Rhodahl and myself and Brineger. And so that was time to do a little reconnaissance on foot and to make a number of decisions about, can you get in there with wheels and could you get off again and where to make the camp and to do things like that.

(250)

And that worked out fine. After a couple of days, the Ptarmigan flight showed up overhead. They had rigged up a little make-shift air drop and they dropped, you know, a bottle of scotch and a box of cigars and a cake because Rhodahl's daughter had just arrived and they were notifying him of his first child and before I left T-3, my first child. Or second, no it was my second by that time. But, anyways, and so I sent the instructions to the B-29 crew and they, of course, relayed it when they got back to Eilson.

BS: *Said it was time to come, for the second flight.*

JF: Well, they showed up every 3-4 days, so when it was time, yeah, for them to come in again, I asked them to come. On the second flight, that's when Bert Crary came in and took Brineger out, and Rhodahl.

BS: *That's the one they broke a ski on?*

JF: Right.

BS: *Did they have a spare one with them or . . .*

JF: No.

BS: *How'd they get off?*

JF: Well, it was just lashed up. That's about all you could do.

BS: *So you took off with the wheels.*

JF: No. No, it was Lou Erhardt. It wasn't me.

BS: *Oh, I see.*

JF: Lou, same guy.

BS: *You tied the nose of the ski up. Is that correct?*

JF: It was one of the main skis that broke. And it was just lashed in place and he was able to use it and get off. And then they repaired it when they got back to Thule.

BS: *And you still stayed.*

JF: Yeah.

BS: *How long were you out there?*

JF: Oh, I stayed as long as I could. I stayed for three months. It was already, it turned out to be about 6 months in all, because my tour at Eilson was up in December, and we didn't get in to T-3 until the middle of March, and I didn't go out until the end of June.

BS: *Pretty soft then? The snow? The ice?*

JF: Yeah, it was starting to get sloppy. That's right.

BS: *When did you go to North Pole?*

JF: Well, I wanted to do a bunch of things and I . . . once we got started, I wanted to keep the ski C-47 at T-3 and get a little fuel carried in and several things I wanted to do. One, I wanted to visit and possibly recover the R4D that Ed Ward had left on the ice. Do you remember that?

BS: *No.*

JF: In Ski Jump II, it didn't go . . . Ski Jump I went very smoothly.

(300)

On Ski Jump II, they had the P2V and they were having trouble with the engines and various things, and the upshot was that they hit a bump in the ice and busted up a ski and they abandoned the R4D out at one of the stations.

BS: *Which station?*

JF: I don't know. Well, one of the hydrographic stations.

BS: *I see, on the sea ice.*

JF: On the sea ice. That's right. And, of course, that had not been seen or that was written off. But from talking with Ed Ward, it appeared the airplane was in pretty good shape and if you could get it off, it would be probably otherwise OK. So, I had hopes of visiting the site and if it were feasible to do, maybe even recovering the aircraft. That was one thing. Another thing I wanted to do was to go over and land on T-2 and be able to explore it and look at the conditions and so on. I wanted to land on the coast of Ellesmere because that's where we thought the ice would come from, originally. And so you really have to make a visit to get anywhere with that. So, those were the things that I wanted to do. And so, I asked the Air Command for the use of the C-47 for 30 days. Well, as I say, the staff was all against it because this was an invitation to disaster in their minds. But General Old, by that time, was rather sympathetic to my wishes and he wanted to say yes, but he was being told by his staff to say no. So, the upshot was that after we were set up there and this was, by this time, we'd gone in the middle of March and by this time it was about the middle of April. The C-47 was scheduled in again and I wanted to be able to keep it. General Old had his own problems which I knew nothing about at that time, but anyway, we had been in communication by radio and I had insisted that I wanted the airplane and he had said no, and I had reiterated the request. Anyway, when it came in he came in with his instructions, and he said, "OK, you can keep the airplane for a month on one condition and

you can do anything you want to, or in your judgment you want to do during that time. But there's one condition. The first landing that you make anywhere else except T-3 has got to be at the North Pole." And he was *very* explicit. And he meant it. So, that's what we did. And I didn't understand until later why he did it.

(350)

And the reason, it turned out - not the whole reason, but one of the reasons, was that he was also sympathetic to another fellow who I'd never met. What the hell's his name? I'll think of it in a moment. But this was a World War II fighter pilot who had a very good war record. He had, I forgot how many airplanes he'd shot down, but he was an Ace. But, he was a crazy guy. And he had a whole bunch of accidents of various kinds. He'd been court martialed a couple of times and gotten in trouble, but the general liked him because he was an Ace with a good record. And so he had bailed him out of a couple of embarrassing things. Well, he had gotten into another one. He had slugged his co-pilot. They were on some kind of a mission over around Nome somewhere and his co-pilot had objected to a couple of his shenanigans and so he had slugged him. And it had been quite an episode. So, there were charges pending. Bill Benedict. That was his name. So, General Old was trying to think how to get Bill Benedict out of trouble and I didn't know any of this at the time. But anyway, he had picked Bill to take the C-47 on the mission up to T-3 and the idea was to get Bill involved in something which would give General Old an excuse for bailing him out of this latest mess. And so, one fine day, Bill Benedict showed up. This was, as I say, it was some time later. About the middle of April by then, and the C-47 there and instead of Lou Erhardt, it was Bill Benedict that was the pilot and I had a note from General Old giving me these instructions, you know. This is the airplane you can keep.

BS: *Here's your pilot.*

JF: And, yeah. And the first landing has got to be at the North Pole and after that, you can do your own thing. Well, I followed the instructions. That's all. We waited about 10 days or so to make sure that the weather was just right. And then we made the trip up to the Pole so we took Crary and his assistant and took a bunch of measurements and so on. Then I stayed at T-3, but Bill went back with the airplane and when he got back, this, of course, gave him quite a bit of notoriety and it gave General Old a chance to intervene and get him off the hook.

(400)

BS: *Tell me about the sweater incident. You're wearing a sweater.*

JF: What:

BS: *The sweater you're wearing here.*

JF: Oh, well, that was. . . that sweater was knitted by a friend of mine who was one of my radar cohorts at the Radiation Lab and at Signal Corps labs. He'd worked for me during the war. His name was George Austin and he had been one of the four that I had taken with me to UCLA. While at UCLA, we naturally got acquainted with girls and George married one of them. I had introduced them and later on, when I got married, George was my best man, too. But, anyway, it turned out that Jeanie, the girl that he married, had knitted the sweater for me. And so this was a gesture to say that, yes, I appreciated the gift and wanted to show it at the time. And by the way, George Z. there is the parents of Tracy Austin who plays tennis. She was Number One for a while. Not too long. Navratalova beat her out. But there was a period of a few months when Tracy was Number One.

BS: *So, tell me about the landing. The first landing at Pole. I understand you were back in the back and going to throw a smoke or . . .*

JF: Oh, yes. Well, neither Bill nor I really were very good at judging ice thickness. And my idea that I favored was to land on refrozen lead because you're, you know, you're not so apt to hit a hump and break a ski and so on. Bill had even less experience than I, so he deferred to my judgment on that. So, anyway, we went up and we looked around in the immediate area. We had one of the navigators from the 58th B-29 squadron as navigator and we picked out a refrozen lead and were going to land on that. And we made a couple of low passes over it and we also had picked out on old floe which was right next to it, but which was really very . . . looked like it was much more chancey because it looked rough and we didn't know how rough.

(450)

So, the upshot was that we made a couple of low passes over both of them, and then we decided that we would make the landing, but I wanted to put out a smoke flare. And so, I went . . . I was on the co-pilot's side. Bill was on the pilot's side and I wanted to toss a smoke flare out and I wanted to toss it at just the right time, so I'd be able to see it. And so I went to the rear of the airplane to toss out the smoke pot, or the bomb or whatever it's called, and Bill, then, was left in his seat. And as I said, he was kind of a wild guy, and damned if he, instead of making the low pass which we had agreed we were doing, he decided to go ahead and set down. So, it was a surprise to me whenever . . . he just cut the power and set it in on the floe, not the lead. And so I was at the back door with the door open at the time and I was mad as hell, at the moment. But, things went all right, so . . . .

BS: *You were the first one out?*

JF: I was the first one out because I was at the door.

BS: *Oh, you fell out.*

JF: I didn't fall out, but I jumped out. As soon as we came to a halt, I thought, hell, I might as go ahead and jump out. But, that's how I happened to be there.

BS: *Any problems with getting off of the floe?*

JF: We were . . . well we were both worried about it because it wasn't very . . . that's why we had been looking at the refrozen lead. And we had made a couple of low passes and on the second pass, we saw what I took to be a big pile of bear doo on the lead and it turned out that that isn't what it was. We had failed to reel in the training antenna and it was our trailing antenna that had pulled off and was laying there in the middle of the lead.

(500)

But, it's a good thing we didn't land on it because, on the lead. We did core through the ice and it was only about a foot and a half thick. Only about 15-16" and that might have been enough, but it might not, too. So that was a good thing that Bill landed on the floe.

BS: *How did you nail the Pole, navigationally? How did you do that?*

JF: We were only 100 miles away and Bill had brought up with him one of our best navigators from the B-29 squadron and so he was using, he was, of course, in daylight, and so all he had was the sun. But being only about, a matter of fact, it was only about 90 miles from T-3.

BS: *The DR was pretty good.*

JF: The DR was pretty damn good and that was backed up by his sun measurements and we had about 4 hours on the surface while we were there.

BS: *And he confirmed it.*

JF: He was taking sun measurements all during that time, so that's all we had.

BS: *Kept that record?*

JF: I think so.

BS: *Didn't register it with National Geographic or American Geographical Society?*

JF: I don't think so.

BS: *What was the science that Bert Crary and the others did? What did they do out there during that first period of weeks?*

JF: Oh, Bert was there for 2-1/2 years.

BS: *Oh, he stayed for 2-1/2 years, on T-3?*

JF: On the Ellesmere Shelf, on T-3, most of the time. On the Ellesmere Shelf part of the time. But, he was there almost continuously for the next 2-1/2 years and until he was appointed Chief Scientist for Antarctica. Or he was Deputy Chief Scientist. Harry Wexler was appointed Chief

Scientist and he appointed Bert as Deputy Chief Scientist for the IGY in Antarctica and I was madder than hell because, not that he isn't good for it, he was perfect. But, I was madder than hell to have him taken out of the Arctic. And Hugh Audeshaw, you remember him?

(550)

BS: *Um-hum.*

JF: Couple of guys, they flew up to Thule to interview Bert and to talk him into taking this appointment which they did without my knowledge. By that time, I was down in Maxwell and other duties, of course. But, anyway, they talked him into going to Antarctica. So he spent 2-1/2 years around T-3 and Ellesmere and then he was 2 years in Antarctica without ever going out.

BS: *But what was the focus on science that he did? He and others - the science program.*

JF: Well, really, that was wrapped around Bert.

BS: *Weather?*

JF: No. No. Bert is an old seismographic. . . He was a geophysicist and his specialty was seismicity. He had worked for several years in Saudi Arabia and in the Persian Gulf just early at that time. He had worked in South America, and he had worked for the geophysics lab in Cambridge, and that's how he happened to be there because I had come from Cambridge. And of course, as soon as we started planning T-3, I called on the Cambridge bunch to provide the scientific component and I had met Bert before, but I was not well acquainted with him. So anyway, and Bert had had some experience the year before. Bert had been up to Alaska doing some seismic work around Barrow - he and his assistant.

(600)

And his assistant, which was carrying dynamite for him, what the hell was his name? He was later the President's Science Adviser, or no, the President of the Academy of Science, what the hell is his name? You know him.

BS: *Phil Smith. Was Phil Smith his assistant?*

JF: Phil. No. He was never head of the . . .

(End of Tape 2 - Side A)

_____

(Begin Tape 2 - Side B)

(000)

BS: *He was carrying dynamite for Bert.*

JF: Yeah, well, that's just an anecdote. And so that had been, you might say, Bert's introduction to the Arctic. But Bert's specialty was in . . . we didn't have a well planned out scientific program. His specialty was acoustics and so, of course, we took such . . . we were making regular acoustic measurements of ocean depth. We, a little bit later on, we didn't really start it before June, we were doing refraction acoustic measurements. You know, you can lay out the geophones on the ice island which takes quite a bit of space and if you can get somebody to drop a bomb the right distance away, you can do a refraction shot which gives you a very deep profile

and Bert was doing some of that work. He had used gravity meters in his geophysics work and so he had a couple of highly accurate gravity meters. We, of course, set up the available meteorological instrumentation for taking upper air soundings as well as a full array of surface measurements. And the oceanographic measurements that Bert could conveniently take.

BS: *Depth.*

JF: Oh yeah.

BS: *Had a seismometer the whole time, I imagine.*

JF: And, of course, we were interested in the thickness of the ice island and various details of that kind. So, initially, it was sort of a rudimentary make-shift arrangement doing these kind of measurements.

BS: *But after you left, they funded it for . . .*

JF: Oh yeah. It became a very elaborate set up. The Navy, as you know, was doing various kinds of acoustic measurements about propagation.

BS: *There's a Gooneybird out there, or there was years later when I was there, up on a pedestal.*

JF: That one. That's it.

BS: *Yeah. What happened there?*

JF: Well, I'll tell you. When Bill Benedict came up and we made the trip to the Pole and so on, this was a Gooneybird that was owned by the Alaskan Air Command Depot and the word had gone out that it would be used for T-3, but they guys at the Alaskan Air Command were very unenthusiastic about this and they picked out the worst airplane that could be found because they never expected to see it again. And there are a whole set of anecdotes that have to do with things that went wrong with that airplane while Bill and I were using it. To give you an example, I reached down to change the gas tanks on one of our flights and the goddamn shaft came apart, way down into the pedestal. There's no way in the world you can get to that in flight.

BS: *So you couldn't get the gas.*

JF: Couldn't get the gas. And fortunately, we were close enough to T-3 so we were able to get back to it. Another case . . .

BS: *Did you do maintenance at T-3?*

JF: No.

BS: *Just serviced the aircraft.*

JF: Yeah. That's all that you could do, really. Make-shift things, but there were no facilities of any kind.

BS: *You said another case. I interrupted you.*

JF: I'm trying to think, Bill had a case on his way back to Thule. What the hell happened? Oh, yeah. He lost an engine and had to spend the last 3 hours getting back into Thule on one engine.

But he did get back and then they tore into it and what they found was a whole big wad of Form Ones - you know, the clearance forms you make out, that were two years old. They could find the date on them. And apparently, somebody had stuck them in the opening and they had somehow got pushed into the gas tank and two years later, they had ended up disintegrating and stopping up the fuel line and so that was that trouble.

(50)

And you know, there were a whole bunch of things like that happened with that airplane. It was a wreck. But, at any rate, we had good luck with it. But, I left in June and that airplane . . . actually Bill got it back to Elmendorff and it was used again later in the year, in the fall, and in the fall, when they brought it up for use in a resupply visitation, they wrecked it. And so, knowing that I had used it a lot in the Spring, the guys at T-3 took a picture of it. That's that bottom picture. And they said, you know, "Here's your old airplane. We brushed off the tail so you could read the numbers and it looks like it's going to stay there for a while." And so that's the winter of '52. And then, 10 years later, ARL took this picture. And somebody, I don't know, maybe it was you, sent me that picture.

BS: *No, it wasn't me. That was before my time.*

JF: So anyway, and then, years later, when I was stationed in Norway, I had to go down to Paris to the NATO headquarters two or three times a year and as I was signing in at the NATO headquarters, I looked and over the desk was hanging this picture. And I looked at it a few times and it looked familiar and I finally got up and read up and read the caption and said, "For Christsake, that's me!" And this was obviously a painting and I've never found the original. It could be hanging somewhere in the Pentagon or it could be in the (Douglas) headquarters somewhere. But I recognized the picture because this picture, this part of it is the same as -

where is it? Well, anyway, it's in here. Where's the Xerox I gave you? Yeah, it's a take off on that. That's this part of it. And this part of it is a take off on that.

BS: *It's a collage sort of.*

JF: It was a collage.

BS: *Yeah.*

JF: And I have never come across the original, wherever it is.

BS: *But, they've made a lot of prints.*

JF: But, when I found this at the NATO Headquarters, I said, "I want that." And there was a Master Sergeant, I think, running the desk, or something. Anyway, it was quite a rout getting it and they finally agreed that they could give me the picture, but they couldn't give me the frame, because that was government property. So, we took the picture out of the frame and you can still see where it was rolled up. I rolled it up and carried it back to Oslo and had it mounted and this is where it is now.

BS: *Wow. That's neat. Did you ever meet, you told me about meeting Somov and Treshnikov, how about Avin (Petanyan)?*

JF: No.

BS: *He recently died just a few years ago. He was 98.*

JF: I never met him. No. Somov, I knew quite well.

BS: *So you knew about the Russians' ice stations and all the ones they had out there for years. What about Otto Schmidt?*

(100)

JF: Never met him. The guy I really wanted to meet and I never met either but who was alive for many years - Chernivitsky, who was the pilot on their 1941 expedition, which was forerunner of, what'd they call it? Ski Jump? He did a similar thing. And he did a great job. But, they got back to Moscow in June '41, and that was just a few days different from Hitler's attack on Russia or the Soviet Union, so of course, they had their mind on other things.

BS: *Well, of course, Petanyan and Otto Schmidt were kind of the fathers of all of that for them.*

JF: Well yes, that's true. But, no, I never knew (Kopanin) or Otto Schmidt.

BS: This is the second day of taping, the 24th of January, 1997, again with Colonel Joseph O. Fletcher, and we'll continue on with his Polar experience starting out with a wrap-up of the establishment of T-3.

JF: Well, I tell you, we found that we had to find a closer place to stage from than Point Barrow because of the position of T-3 and limited range of the Gooneybird, so we figured we could do it from Thule. I was planning on using Alert as a staging point, but because we had not made prior arrangements with the Canadian authorities in Ottawa - at the last minute, I had requested permission to use Alert which was routine for us before, when we'd been considering them as

emergency landing spots, but by now, they had a sense of what we were doing and so they said, "No." And that created a bit of a problem. And the Danish representative for Greenland just happened to be at Thule at the time and I don't know, they didn't say we couldn't use their territory, but I was afraid to try because they could very well say no.

BS: *Didn't want to hear the answer.*

JF: Well, I don't know. So, the point is that the Danish representative was present, but the point was that I didn't want to try to go through all the rigmarole of getting authority from the Danish government, and so on, which, by that time, would have been inevitable. So, I figured the best thing to do was just stage off the sea ice which we could do by going out and picking out a spot and if we didn't tell anybody, nobody could object because they couldn't say it was on their territory if they didn't know where it was. So that's what we did.

BS: *How far north of Alert was that?*

JF: I'd have to guess, but it was about as far as you can see, because as I said, we had a theodolite that we could triangulate. It was clear weather, and you could see the peak of Ellesmere and you could see the peaks of Northwest Greenland, so I took a theodolite along and took a good triangulation and there was no trouble in finding the place.

BS: *Did you establish that with the Gooneybird or the C-54?*

JF: With the Gooneybird.

BS: *OK. Landing on, was it a refrozen lead?*

JF: Yeah. It was a refrozen lead. And I have that all on film, by the way.

BS: *I saw your film at Ohio State.*

(150)

JF: I used that at Ohio State at that meeting.

BS: *Uh-huh. Very impressive.*

JF: As I mentioned to you yesterday, T-3 had been discovered a couple of years before. It had not been seen for quite a while during the last year or two. And at the time, it was classified because you know, they didn't know at the time whether it was land or ice. So, anyway, I've forgotten the details, but we had it declassified, the records to it, and one of my professors at MIT happened to visit in an inspection team in Alaska and he urged me to write an article about T-3. And so, I wrote an article which we published in *Tellis*. You know *Tellis* magazine? Well, that's the Swedish magazine, the geophysical magazine that is old and famous. It's sort of like *Nature* in Britain. And so, I published an article in *Tellis* and I gave an article to the meeting of the AAAS and that's what is referred to. Mildred Crary now is asking for reference because she says, you know, this was in Bert's manuscript, but they don't have a reference. And that was in 1950, so that was two years before we got around to . . .

BS: *Putting the station out.*

JF: Yeah.

BS: *So where'd you go after T-3, after you established that? You were on temporary duty.*

JF: Well, as I say, I was brought back and assigned to a weather group. We had the 4th Weather Group was part of the R & D Command and their job was to operate the weather stations that were in support of all the research installations around the country. So, we had 30 stations and that included the missile range, which was just being set up at the time. The Nevada Test Range, Los Alamos, those kind of installations. So, the Air Force consolidated management of all of those R & D support operations into one group which was part of the R & D Headquarters in Baltimore.

But, during the time I was there in Baltimore, at the Air Force Headquarters, the argument was being made mainly by a group of MIT scientists, that the US was vulnerable over the Arctic and that they ought to have a DEW line warning system.

(200)

And the Air Force was not very enthusiastic about it at all. But, there was a big push by the civilian scientist community to build a DEW line and President Trumann was President and he was being pressured. Well, Trumann decided he didn't want to do it because the Air Force was against it. But, there was so much pressure, they decided they would do something. So, the decision was to build the section around Alaska, and reserve the decision to build the rest of it until later. So, the decision was made in November of 1951, to build the Alaska section of the DEW line. Well, you know what the weather conditions are like in November. They wanted the goddamn thing operational the following September. So, what do you do? Well, the Air Force cranked up to do this with no idea how to go about it and they let a contract with the Western Electric Company which was a division of AT&T. And the AT&T assigned a company project director and they set up the head office in lower Manhattan at 120 Broad Street which was almost the intersection of Wall Street and Broadway. And so, that was my office for a while. But, most of the time we spent in the Arctic because then, I was assigned as the Air Force Project

Officer and their primary contact in carrying this out, which meant that anything they wanted from the Air Force, they came to me about. And you know, we got together jointly on the planning and whatnot. And so I had an office down at 120 Broadway, but was most of the time in Alaska.

And so that was in, I guess I got my marching orders - the decision had been made in, I remember very well, I think it was the 20th of November and about the end of December, I got my assignment. So, from then on, I was conferring with the Western Electric Company and trying to arrange the things that were needed. And the idea . . . well, how do you go about it? There were, as I recall, 16 sites carrying the intermediates. I've forgotten, I think it was about 8 or 9 main stations, and a similar number of intermediates. One of the main stations is near Point Barrow and you're familiar with it. So, the question is, how do you go about it in order to get the job done? And the immediate problem, of course, well . . . the first chance you'll have for bringing a ship in is along about August. So, you plan on that being the main effort.

(250)

But you've got to do a lot of things to expedite things before then. So, I got together . . . I went to the Navy, and got an arrangement which the Air Force consummated to . . . you know (Pet IV) had accumulated a bunch of tractors and sleds and wanogans that they had used for the exploration of Pet IV and they were just sitting somewhere. A lot of them were not operational and needed maintenance and so on, but they were there. That's the main thing. So, the first thing to do was to buy from Pet IV, to take all of their equipment off their hands - wanogans, sleds, tractors and what not. And that was a lifesaver because that meant that you could get started right away to do some things that could be done during winter and you could get ready for the sea lift. That was a key factor. Another factor was that the transport squadron - there was one transport squadron in Alaska which had been flying C-54s that I had used at T-3, they had gotten 124s and that was an enormous increase in airlift capability. And so, that made it possible to airlift a lot of

stuff. I've forgotten. I think it was about 3000 tons that we airlifted with the C-124s during the Spring period getting ready. So, a combination of the 124 capability and the Pet IV machinery and sleds and so forth that were on land, allowed us to start right away in February. And to start getting ready for the operation. That really worked well, and I must say, the C-124 squadron did a great job. The Navy was cooperative. Pet IV was cooperative. Everybody, of course . . the Navy Research Lab was involved in various support activities.

BS: *Was Larry Irving there then?*

JF: What?

BS: *Was Larry Irving there then? He's the one that started it in '47, I think.*

JF: No. It wasn't Larry Irving. In fact, I'm trying to remember.

BS: *It was before Max Brewer's time.*

JF: I don't remember just when. . .Max came on board either shortly after or about that time. I knew Max in various ways over the years and I can't remember now when Max's tour started. The Lab was very helpful, but as I say, the main agreement that I had to work out with the Navy was witih Pet IV administration because they owned all the equipment that we needed. And then, the Western Electric people took over with their own subcontractors from the Pet IV and repaired the machines that needed repairing and got everything ready.

(300)

But needless to say, it was a very hasty operation. But, a lot got done. I was amazed at how well it worked, really. And the upshot was, you know, we had the damn thing operational by the end of September in '53.

BS: *And that was just the Alaskan section.*

JF: Only the Alaskan section.

BS: *And that was from the Canadian border around to what? Cape Whales?*

JF: Yeah. I was deeply involved, of course, through most of that time, but by the time that sea lift got mounted, the wheels were turning. But everybody had their own instructions and I went up in, I forget, it must have been about June or the first of July and the sea lift, by that time, was in full operation. And they did what you could predict they would do. They got off, they came about a month too early and they played cards for a month waiting for the ice to go out enough for them to dive in. So, they dove in too early. They should have waited even longer and, of course, things closed up and they had a big emergency and what not before the ice really cleared out enough. You can predict in advance, as we did, that that would be the routine. But, there's no way anybody can prevent it because they want to take advantage of all the time that's available and it's a short window and there's no way you can keep them from getting up there a month too early. But, my point is that by that time, I found out, I was completely superfluous. I mean there were a bunch of admirals from the sea lift command that were running the operation and they were dealing directly with the Western Electric folks and so, I got up to Point Barrow and stayed at the Lab and, as I say, all the wheels were turning. Right or wrong, the thing was in full operation. So, I decided that there was really nothing I could do that would affect things particularly. And the big question at that stage was the rest of the DEW line. And the big issue was, should you try to go straight across Canada from the Alaska boundary or should you try to

jump up to Banks Island and go across the islands to Lancaster Strait because that had some advantages in terms of ship access and so on. And so not much was known about the geography, you know, in the area. So anyway, I made a new plan for myself right on the spot because at the time I got to Point Barrow - I was staying at the Lab - the *Burton Island* showed up. At that time, it was being operated by the Navy.

(350)

I'm trying to remember the name of the Skipper. That'll come to me, I think, later. Anyway, I got together with the Skipper of the *Burton Island* and asked if they would take me over. They were going over to the eastern Beaufort Sea anyway. They had some scientific projects of several kinds. I don't remember now the details of what they were. But what I wanted to do was to pick up a Weasel and carry it over on the *Burton Island* and have a chance to explore the shore, you know the off loading facilities, the sites that were proposed which would be the southwest corner of Burton Island up to the northeast corner of Burton Island.

BS: *Burton Island or Banks Island?*

JF: Oh, excuse me, Banks Island. And to use the *Burton Island* to carry me over. So, I inquired around Point Barrow and they found a Weasel and I wanted to make sure it would work because we were going to have to float it ashore somewhere along the way. So, I drove it out to the little pond that's near the lab at Point Barrow.

BS: *Just north.*

JF: Well, a little bit to the east.

BS: *Yeah. There were a bunch of them.*

JF: Well, anyway, I drove it out in the middle of that pond and the goddamn thing sank like a rock because, as you know, they take an ax and chop a hole in the bottom so that the melted snow will leak out during the winter. And you can't see where they've made the hole, and so I didn't know there was a damn hole in there, but I found out that it sank. And that was the only Weasel that could be found and so to make a long story short, I didn't want to give up. So we got a truck and they got a long cable and attached the cable to the Weasel. It had sunk under. . . I don't know, it isn't very deep. Maybe 10 feet of water or something like that. And dragged it out and then there was a landing craft there available and I'm trying to remember who the hell that belonged to. You know the thing that has a . . .

BS: *Ramp.*

(400)

JF: Has a ramp, well, anyway. . . the Weasel, of course, didn't work, but they bulldozed it up the ramp and onto the landing craft and managed to take it out along side of the *Burton Island* and the *Burton Island* had a winch that they could lift it and they lifted it up and placed it on deck. And between Point Barrow and Banks Island, the ships crew completely tore down the engine and drained the water out of the cylinders and got the engine running. So, by the time we got to Banks Island we'd mended the hole that had been cut in the hull so that it would float and had overhauled the engine.

So, in the meanwhile, of course, the *Burton Island* had a couple of helicopters and we did a little helicopter reconnaissance and that was very helpful. But, what I was setting out on was to reconnoiter what appeared to be the crucial points in the alternate routing of the DEW line. So, anyway, we went over and worked both sides - the south side of the strait and the Banks Island

side using the helicopter. But then, we needed to have more time and do a much more careful reconnaissance and I got two guys to volunteer who were on the *Burton Island* - not Navy, but scientists. They were there for scientific projects and they, more or less, bypassed their project and joined my team. One was a professor from the University of Washington and one was a Canadian from Ottawa from the Headquarters that oversees their R & D system and his name was Trevor Harwood. Did you ever get acquainted with him?

BS: *Who was the one from Washington?*

JF: Baker was his name.

BS: *Baker?*

JF: He's dead now. I forgot his first name. But anyway, they both volunteered to join my crew and we got the *Burton Island* to put us over the side down near Banks Island. Managed to tow it in. They had to use the whale boat and towed it in to where there was a little bay, just to the east of the . . . you know the southwest corner or Banks Island is kind of sharp and just to the east is a bay called (DeSalis) Bay. So anyway, we got towed in to DeSalis Bay and then for the next month or so, we were on our own.

(450)

I made an agreement with the *Burton Island* to . . . . they had their own projects that they still wanted to do, of course, but their projects involved going up what's the name of that strait between Banks and Victoria?

BS: *McClure? No, that was to the north.*

JF: Yeah, that's to the north. There's a narrow strait that goes up between Banks Island and Victoria.

BS: *I can't recall.*

JF: Anyway, at the north end, we agreed that we would meet at the north end of that strait and in the meantime, I would have, my two guys that were with me, would have time to tour around a little bit on Banks Island and examine the beach conditions and so on, the questions that involved the siting of the possible DEW line station. So, anyway, we spent a month then on our own.

Oh, one other thing. In order to do this, we needed some supplies. I wanted some tents, you know those little two man tents. And some camping equipment in effect. So, I sent a message back to Alaska. By that time, of course, I was well known in Alaska. I sent a message back and asked that they have one of the B-29s air drop the supplies and I gave them a list of supplies. And so they came over and air dropped it at Banks Island while we were at sea with the *Burton Island*, by the way. And so we had to retrieve the parachutes from the sea which was quite an exercise.

BS: *Oh, they made the drop in the open water*.

JF: Yes, they did.

BS: *Floated? Was it all protected?*

JF: Well, no. I told them - you know I had that 400 megacycle walkie-talkie and they were a little worried about how to do it and I told them to use the whale boat as the target and get as close as they could and they damn near sank it. But, they didn't hit the boat, but as I recall, we

retrieved everything without much trouble. So anyway, the *Burton Island* waved good-bye and left us.

BS: *What time of year was this? August?*

JF: August, by then.

BS: *August, '53.*

(500)

JF: '53, that's right. And so, we backtracked over to Nelson Head, that's the name I was trying to remember. It's on the southwest corner of Banks Island. And that would have been a great site, really because you had a little elevation. It had a whole bunch of other geographical advantages. And we did, by the way, write a fairly comprehensive report on all this siting, but I don't know where it was ever retained because in the end, that is not the route that was chosen. As you know, they decided to go straight across instead.

But, that was quite an adventure that summer, the three of us in the Weasel crossing Banks Island. I had a spare set of tracks, and at one point, we broke a track and had to replace it on the Weasel and we got up near the northeast corner of Banks Island and sure enough the *Burton Island* was offshore in that strait. But, the ice conditions were pretty heavy, so there was a problem. The skipper, McCarty was his name, he . . . well, I wanted to get the Weasel back on board, of course. And by that time, the Weasel was running and I remember the other two guys, he sent in a chopper and picked them up and took them back to the ship and the last two nights I spent by myself in the Weasel before, because we were . . . I was anxious to retrieve the Weasel and his point of view, which was correct, was that the conditions were not good enough for that. So on the third day, he sent the whale boat in and the Weasel was running, so we could drive it

in. Well, he sent the whale boat with a pump which could be carried in the Weasel so that if we did take on water, we could pump it out.

(550)

So, we took off from the shore with me in the Weasel and with the bailing pump going, bailing out the water because there were enough waves so that it was taking on water over the edge of the Weasel. Well, anyway, the damn bailing pump stopped working and then we started getting heavier and heavier and pretty soon we went down. I guess it was maybe two or three hundred yards from shore the damn Weasel went down and the whale boat picked me up.

BS: *Fished you out.*

JF: Fished me out, yeah. I got a lot of kidding from the guys on the *Burton Island*. There was a Canadian Commodore, Robertson, who was later the Canadian representative for the combined sea lift command that they had, and I remember Robbie's tale was that the water was full of ice, and his tale was that I didn't get wet above the waist because I was swimming so hard. So anyway, I can show you the spot where the Weasel was sunk. And of course, there was no question of trying to recover it from there. So anyway, I went back on board. But, the *Burton Island* then went back down the channel from there and my two volunteers and I reconnoitered two or three more sites - two of them on Victoria Island and one of them a bit to the south, which turned out to be later used in the southern route of the DEW line, the one which was chosen.

BS: *Let me change the tape here.*

(End of Tape 2 - Side B)

(Begin Tape 3 - Side A)

(000)

BS: *This is an oral history interview with Colonel Joseph Fletcher, taken at his home in Boulder, Colorado, on 24 January 1997.*

JF: So that was what, '53, and then I then was reassigned. I went down to Maxwell. I went first to the Air Command and Staff School and then the Air War College and they kept me on on the faculty there for three years. So, I went there in late, soon as we got back from Alaska. So, I left the DEW line project and spent the next three years down at Maxwell.

BS: *Did you teach?*

JF: I don't know whether you'd call it teaching. I was on the faculty at the Air War College. I lectured, yeah. They had me lecturing. I went up to the Navy War college two years, also, and lectured on air operations. This is the experience of the B-29 operations out of Alaska. So, my lectures were air operations in the Arctic. Mainly the part of the Air War College that I was assigned to was called the evaluation staff and more than anything, it 's like the Rand Corporation. You were free to do special studies and I undertook to do a big study on strategic air operations in the Arctic and it was classified top secret and SAC took strong objection to it because one of the things I emphasized was the vulnerability of places like Thule and the SAC staging bases. And they objected strongly to that part of it. But, at any rate, it was a big . . . it was two volumes, each about an inch thick. But, I've never seen the light of day since then because, as I say, it was classified top secret and it was strongly objected to by SAC and I think . . . I've had some good input from the air staff, but I've never seen it used for anything.

BS: *I understand. So, when you were in the Arctic, there was Ski Jump going on for the Navy. Did you know much about that?*

JF: Well, yeah, as I said in '51, I thought that was Ski Jump I. You're telling me that there was an earlier Ski Jump.

BS: *Well, there was the High Jump in the Antarctic in '47.*

JF: No, I'm talking about Ski Jump.

BS: *I don't know. I really don't know.*

JF: In the Arctic.

BS: *Whatever you were involved with.*

JF: Well, in '51, as I said, the Pautuxent, I got a request, they wanted to send up some navigators to fly with us for two or three months to get experience in Arctic navigation. And I've forgotten their names, but there were, I think, three of them who came up and spent two or three months with my squadron at Eilson. And so that's initially getting acquainted. That was in the Spring. And then, what's his name? There were two of them. The oceanographers. Well, anyway, the main group came up and Ed Ward was in charge. And they used a C-54. And that worked fine because C-54 is pretty easy to use in cold weather. And I was terribly impressed with that because, well you know what they did.

(50)

So, they asked me then, although the Ski Jump airplanes were supported partly by Eilson, but partly by Ladd, they asked me to be the host because they needed a bunch of equipment - heaters, for example. Miscellaneous stuff. Power units and I don't remember. I think there were a couple of power units, and several heaters and some miscellaneous stuff. So, that's when I met Ed Ward because he was in charge of the operation. And, as I said, I thought they did a beautiful operation. Picking out a place . . . they were landing on refrozen leads. And they could land the C-54 and transfer supplies and fuel if needed for the Gooneybird. And so anyway, I followed their operation. I didn't go out with them or anything, but I followed their operation with great interest and was much impressed with what a nice job it was.

BS: *This was doing oceanography, right?*

JF: Yeah. And I'm trying to remember, what's the name? The oceanographer who was in charge from Wood's Hole spent a lot of time on T-3 over the next couple of years and I'm trying to remember. It's just on the tip of my tongue. His family owned a big manufacturing company which was familiar to all of us. And . . . let me see if I remember it. At any rate, he came back and he spent, as I say, Bert Crary spent over two years at T-3 and on the Ellesmere Shelf and about half of that time, he had this oceanographer from Wood's Hole.

BS: *I'm trying to think of who it was. It wasn't Norbert.*

JF: No. He was never with Wood's Hole.

BS: *OK. Let's see. You're just finishing up the War College, and what'd you do after that?*

JF: I was assigned as a student to the Navy War College. I had been lecturing there for the last two years as a guest. There were five Air Force officers sent to the Navy War College. I was never quite sure whether they were hostages or what. But, it was great. I enjoyed the . . . I went there and that was why . . . I was asked to be the Air Force rep for Antarctica and I couldn't do it because I had to report to Newport in June for the beginning of the War College. So, that was in '57, and we finished in the summer of '58, and from there, I went to Norway.

BS: *But, you were involved with IGY for the Arctic bases. When did that come in?*

JF: Well, that was just before. I had to get back. The point is that I put the program together and the basic program was to organize two drifting stations and to get the necessary support and so on.

BS: *Where was your headquarters for that?*

JF: The Academy.

BS: *In Washington. And what year was that?*

JF: That was the early months of '57. But, as I say, I had to be in Newport by June, and I remember, I wanted to take a little time with my family for a holiday which we never were able to do because I spent the early months . . . and Bert Crary and I were together. He, also, was assigned to the Academy.

(100)

There was no NSF then, and the Academy was the operational organization for putting the IGY together. And they set up this headquarters out in, I think it was on 11th - 1130 22nd Street or something like that. It was only a couple of blocks from the Cosmos Club. And I forget the east, the crossways street that goes over into Georgetown.

BS: *It must have been M Street.*

JF: M Street. Yeah. About a half a block off of M Street on Eleventh Street. That was it.

BS: *And that was the headquarters for IGY.*

JF: That's right.

BS: *How many people were there?*

JF: Oh . . . Hugh Audeshaw was selected to run that office and I don't know . . . they must have had 25 or 30.

BS: *This is for both Arctic and Antarctic?*

JF: Yeah. And Bert, at that time, was putting together the Antarctic part. And I was putting together the Arctic part. And, of course, Bert and I were old friends.

BS: *Tent mates.*

JF: Yes. So, we had lunch together every day and saw a lot of each other.

BS: *What were the two stations that you had during IGY?*

JF: Alpha and Beta.

BS: *Alpha and Beta? And Alpha was T-3?*

JF: No, that was Beta. Alpha was on sea ice. That's a different story. I went . . . I knew what T-3 was and it was secure. We had to get the Air Force to do a search to find T-3, but once it was located, it was a simple straightforward task to get a flight in there and set it up. So, I didn't . . . I never personally engaged in setting up what turned out to be Beta at T-3 because that was given to the Northeast Air Command to do. But, the other one had to be on pack ice because we had no suitable location on an ice island, so I decided to do that one myself. But, to do it more carefully, because by that time, we had inroads from the Air Force that could be translated into the support. So, I asked for 10 flights. My squadron at Eilson had converted from B-29s to B-50s by that time. So, I asked that they set aside 10 flights to do a good thorough reconnaissance and pick out the best available location as well as we could. And so I personally went up and I asked the guy who had been in the squadron when I was there to join with me and also Tom Cunningham. Did you ever know Tom Cunningham.

BS: *Ummm. Not sure.*

JF: Tom Cunningham was a Catholic priest who was the parish priest for the North Slope. But, he was a Reservist somehow, and he had been. . . well, he was very friendly with the Alaskan Air Command. He was their Arctic expert. And I didn't know him until then. So anyway, I teamed up mainly with him and took him on several of the B-50 flights and we covered all of the Arctic Basin between Alaska and the Pole, reconnoitering to try to pick out an optimum location.

(150)

And I'll tell you, it was very discouraging because, well, we never did find a really optimum location. But, I picked out a spot that I thought was the best that we had been able to cover. And so, after we had picked out the location, we got the Rescue Squadron and they had a Gooneybird and a SA-16. You know what the SA-16 is?

BS: *No.*

JF: Albatross.

BS: *Oh yeah. OK.*

JF: Yeah. And there had been a number of Albatrosses modified for skis. And so, we used SA-16 and the Gooneybird and using this time, Point Barrow as the jump-off point, went out to the spot that I had picked out, which we had been able to mark well enough so that we could find it again. And surveyed the site more or less. And that's where I made a big mistake, because we tried to mark it. But, to make a long story short, even after all this search, we went back to Barrow and I left from there. But, when they went back again, they were not able to find the spot and so they picked out a whole new spot. Same general area, of course, but it was not the ice floe that Tom Cunningham and I spent, oh, half a day drilling holes and checking the ice thickness and so on. But, anyway, after all that reconnaissance and after picking the initial spot, it wasn't used because they couldn't find it.

BS: *So, something else became Alpha.*

JF: Nearby. But, it was a different piece of ice.

BS: *Thick floe?*

JF: Became Alpha, yeah. And Alpha finally . . . it lasted about a year and it was crushed, as you remember.

BS: *Um-hum. Was it on a refrozen lead? You landed on a refrozen lead and then went on to the . . . ?*

JF: Yes. I'm trying to remember.

BS: *The modus operandi when I got there was to land on a refrozen lead, which had moved on to a thick chunk of ice.*

JF: Yeah. I guess that's what we did in the reconnaissance phase. As I said, we had only one day. We did the reconnaissance with the B-50s and picked the spot and then the two airplanes came up from Elmendorf and staged out of Point Barrow and we went out and Tom Cunningham and a couple of helpers spent half a day drilling holes and checking thickness and so on. We had about 10 -11 feet thickness, which is pretty good. And at that point, I left, because I was already overdue. I had a due date to get back to Maxwell and head for Newport. And Norbert, I met. Bert Crary and I had talked a lot about how to man the science section and Bert was in touch with a glaciologist in Innsbruck - I forget his name now.

(200)

And at any rate, through that connection, between Bert and Innsbruck, they recommended Norbert Untersteiner. And neither Bert nor I had ever heard or seen Norbert Untersteiner. He was

a younger guy then. But, we agreed to it. Norbert was on his way, and I met him in Seattle. He was on his way up when I was on my way back then, so we spent a day together in Seattle and I told him all I could about background and told him who to report to in Alaska and then he headed north and I headed back east. And that was the end of my involvement in IGY.

BS: *How many people did they have on Alpha during IGY?*

JF: You would have to ask Norbert to have an accurate answer to that, but I think it was about a dozen.

BS: *Um-hum. Was he Chief Scientist on it?*

JF: He was Chief Scientist, and he was in charge of the . . . there was an Air Force contingent. I think a lieutenant. Yeah, there was. And I've forgotten the name now. But, Norbert was the Station Chief.

BS: *And how about Beta? T-3?*

JF: I've forgotten who was the Station Chief now. It was somebody under the Air Weather Service and it was supported by the Northeast Air Command. But, as I say, aside from the very first arrangement, I never participated in the reoccupation of T-3 because I figured that was simple and straight forward. And I don't know who the Station Chief was then.

BS: *What were the goals on Alpha and Beta?*

JF: We had a scientific plan, to be sure. But, I don't remember now any of the details. It was mainly oceanographic measurements and meteorological measurements. And there was a limited

amount of, I think they had all-sky cameras because there was a lot of emphasis on the ionosphere. And we talked about an ionospheric sounding station, but we never put it in place. The Upper Air Station, the Meteorological Sounding Stations I talked the Army, as I say . . . the IGY didn't provide much, I mean, the Academy and the IGY fund didn't provide much fiscal support and I remember, I had talked the Army into providing the Upper Air Stations. What was her name? There was a lady over in the Pentagon who worked for the Signal Corps - Weeden. Francis Weeden. That's a significant name. And I remember talking her into providing all of the logistics and equipment for the two Upper Air Stations.

BS: *That's on both Alpha and Beta.*

JF: Yes.

BS: *Francis Weeden.*

JF: W-e-e-d-e-n.

(250)

BS: *Was she an Army officer?*

JF: No. She was a civilian Army employee, but she was in charge of the meteorological component of the Signal Corps headquarters. She was under the Signal Corps. And I had gotten acquainted with her, originally, during World War II. She was in charge of that activity even at that time. But, she was pretty influential and she was pretty cooperative and easy to work with and so I remember that was a real coup to have somebody come up with the resources to provide the two Upper Air Stations because they're pretty expensive.

BS: *So, you went from that to Newport.*

JF: Right.

BS: *Your family live with you in Newport?*

JF: Oh yeah. I had been up there as a lecturer even a couple of times before, and the second time, I knew I was going to come there as a student, so I did a little reconnoitering. And I had a chance for renting a house that would have been an ideal location. But, instead, I rented a house on a point out into the bay, whatever they call that - Long Island Sound. And that was a dumb move, because being exposed out on that point. Christ, it was cold and windy and wet all winter long. And I kept thinking that I'd had an opportunity to rent a much nicer house that was a little bit inland, but we enjoyed the year there. I enjoyed the Navy War College.

BS: *You do the standard curriculum?*

JF: Well, there was a senior course which I was in. And there's a junior course and I forget what that's called. And they're both standard. And my roommate - one of my roommates, in one of my cellmate in, you know in the library, you have study cubicles, was Dennis Wilkinson.

BS: *Admiral Wilkinson?*

JF: Later, he was an Admiral, yeah. And Dennis, I had met him before because while on the faculty down at the Air War College, there had been a group of about half a dozen of us had visited the sea trials of the Nautilus when they were operating out of Key West and Dennis was the Skipper. So, that was a very vivid experience, really. And I had gotten, I wouldn't say

acquainted, but I had met Dennis during that time. Then we ended up as fellow students. He was still a Commander at that time. By that time, I was a Colonel, but we were fellow students. And we used to play poker every Friday night.

BS: *Well, he did well with the Nautilus. Were you involved with the transit of the Nautilus or the Skate?*

(300)

JF: No, but I can tell you something about that. Dennis, not every week-end, but every month or something like that, he was making a trip down to Washington to talk with Rickover and it wasn't until near the end of the course. In the meanwhile, Dennis and I did a lot of talking about the Arctic. All right. But it wasn't until near the end of the course that Dennis told me about the plan to take the *Nautilus* and the *Skate* to the Arctic in the summer of '58. By that time, I had my orders for Norway. But anyway, they invited me to go on that cruise. But, by that time I had my wife and three small children. They were all under 5. And we were going to a foreign country and the first time in Europe and what not and so, I declined at the time because I didn't see how we could manage it, really. And so, I flew across with my family when my time was up and didn't get to go on the cruise.

But, the *Skate* . . . I was seeing a good deal of Jim Calpert because, by that time, I was acquainted with him too, and they chose Norway as their first port-of-call. Not the *Nautilus*, but the *Skate*. Anderson had the *Nautilus* and Anderson went straight back to wherever it was he went. But, the *Skate*, their first port-of-call was Bergen and they hopped on the train and came over to Oslo and they were my guests in Oslo for about a week and I had a couple of parties and I had a beautiful place to live in Oslo and we had a couple of very nice social gatherings there.

BS: *Interesting.*

JF: Jim had not all of his officers, but he had several of his officers from the *Skate*.

BS: *You mentioned that you were involved with the development of the ski-equipped C-130.*

JF: By the way, Jim, in his book that he wrote later, tells about his visit there in Oslo.

BS: *Oh, OK. I've got the book at home.*

JF: Yeah, well, you can look it up. I remember that he has a page or two telling about the time they spent in Oslo.

BS: *You mentioned that you and General Barkas were involved with the ski-equipped C-130 development during IGY or before.*

JF: Well, as I say, when I came back from Alaska, I was very sensitive to the kind of airplane you'd like to have if you're in the boondocks.

BS: *Now this was '52?*

(350)

JF: This was '52, yeah. And at that time, I became aware of the C-130 which was not yet in service. And so I started trying to promote getting a ski configuration. And, as I said, I was about a year in Baltimore with the R & D command and I didn't have any success. This was something that had to be ruled on by the Air Force in the Pentagon. And I remember going down there several times. They just weren't interested, but later on, it must have been about . . . it was while

I was down at Maxwell that they made the decision to build the rest of the DEW line and that involved a couple of stations on the ice cap and for doing that, hell's bells, there's nothing comparable. I don't know if you could do it at all without something like the ski-130.

And at that point, I had occasion to go up to the Northeast Command, and I met General Barkas who was the Commander of the Northeast Command - an old fighter pilot and he was a real ramrod. And at any rate, I talked Barkas into pushing the 130 and he went down to the Pentagon and rattled enough desks so that he got it approved. And then both Bert Balchen and I were on the modification board. And they spent some time developing the ski configuration. And I don't remember how many, but I remember the total number. There were 17 fully equipped ski-birds produced in the first batch. One thing I do remember is the price of the C-130 which in those days . . . I remember the first contract with Lockheed, I think, was I've forgotten whether it was for $100 or $200 or something like that and they were around $300K a piece . . . $350K. And you know, now - or later on they were 20 million a piece.

BS: *$26,000,000 now.*

JF: Oh really. But, I still remember that first contract because it was something like $350,000 a piece.

BS: *And you say you had 17 and they were sitting in Oklahoma?*

JF: Ardmore, Oklahoma.

BS: *During IGY.*

JF: That's right.

BS: *You don't know when they were manufactured. It was before IGY though.*

JF: Well, it was not long before because they were having a little bit of trouble with the props and I remember one of the excuses that later came up about flying from Christchurch to McMurdo was that, this was the A model, of course, and for the A model, you could do it and it was feasible, but you didn't have enough fuel for a lot of margin. You couldn't, for example, if you were close to McMurdo, you couldn't get back to New Zealand. Beyond a certain point you would have been committed to going in.

BS: *About half way.*

JF: A little more than half way, yeah. So, there were two complications that were raised whenever the Air Force and the Navy got to arguing. And one was that it was stretching, it was pushing the safety factor because of the limited range. The other was that they were having a little bit of trouble with the props and they wanted to work the bug out of that. Neither one of them were serious. I mean you could have gotten around either one of them. The real problem was political. By that time, I was up at Newport, or committed to it. And the Air Force was saying, "Sure, we'll help you. Tell us what you want us to do." The Navy was saying, "Give us the airplanes. We don't want you down there."

BS: *But the first fliers were Air Force. First C-130s were Air Force.*

JF: That's right.

BS: *Or Air Guard. Which?*

JF: No, it was Air Force. It was a squadron from Tennessee, as I recall. So, that was the way they finally settled the argument.

BS: *But they were there for about a year, right?*

JF: No, they were never deployed down there. They were . . . this was C-124s.

BS: *Oh yeah. I know that.*

JF: And they were just doing air lift from Christchurch to McMurdo. And I don't know how long they spent. As I said, by that time, I was occupied at Newport.

BS: *Oh yeah. I knew that, but I meant the C-130s, when they finally came . . . the first ones that came were Air Force for about a year. One season. And then Navy got there.*

JF: Well, that was later. That was about 1960.

BS: *'60. Yeah.*

JF: I heard about that, but I don't know the details.

BS: *The Navy wasn't first and you know the Air Force, the Air Guard is taking that mission from the Navy, so they've gone full circle.*

JF: No, I didn't know that.

BS: *In 1999 or 2000, I think, the shift will occur.*

JF: The New York bunch?

BS: *Out of Scotia, New York, right. And the Navy will be done.*

JF: No, I didn't know that.

BS: *So, that's a shift. The helicopter flying is being done by a contractor.*

JF: Yeah, you're right. I'm remembering now that along about 1960, there was some kind of an emergency. I think somebody had to be evacuated and they called on the Air Force to use the C-130 and the VXE-6 was so enthusiastic about it that together with NSF, they set in motion the procurement to get, I forget how many, several C-130s. But, that was about 1960.

BS: *Three at first. Well, let's see. You went to Norway. What did you do there?*

JF: I was the Chief of the Air Mission to the Norwegian Air Force. People keep confusing that with being Attache. I was not the Air Attache. That was a part of the regular Embassy set up. But, you see, at that time, Norway was a member of NATO, but with certain conditions. One was that there be no nuclear weapons in Norway and there would be no foreign troops of any kind. So all the time in Norway, we wore civilian clothes and were as inconspicuous as possible. But, the US paid for their whole Air Force - the equipment, the airplanes, the support system. We did the training in the States and so on. And to administer this, I had about 125 tech reps and about, oh I forget - it was about 35 officers. And this was called the Air Mission - MAAG - Military Assistance Advisory Group.

BS: *And you weren't involved with the Arctic then? Polar stuff? Straight Air Force.*

JF: Straight Air Force, yeah. That was only to the extent that Norway . . . Norway's a third - if you put a pin in the southern tip of Norway and rotate the geography 180 degrees, you're south of Naples. So, the Norwegian frontier is about a third of the total NATO frontier and of course, Norway, at the that time, the whole population of the country was less than 3 million - about 2-1/2 -3 million. But, it was very heavy on radar, of course, being such a big frontier. So, we had a huge warning system to build, and the Norwegian Air Force had about a dozen squadrons - operational squadrons. Two of those I managed to make ASW and there was a big fight in Norway, as a matter of fact, about that because when I came to Norway, the Norwegian Air Force was not at all sympathetic either to the ASW Mission or to the Navy, you might say. They had all been trained in Britain and unless you flew Spitfires, you didn't amount to anything because to them, that was the role of air power.

BS: *Fighters.*

JF: Yeah, fighters. And all of them, all of the senior officers had flown Spitfires in Britain. And that's all they really were interested in. But, they did have two Catalina squadrons that, I don't know where they had come from, but the structure was there. And I wanted to convert those into a real functional ASW outfit and I couldn't get P2Vs. Of course, I'd been introduced to the P2Vs through High Jump, and I knew the 4360 engine a hell of a lot better than the Navy people did, I think. That's the same engine as the B-29. And they got into deep trouble, most of which was unnecessary by not having had the cold weather experience with that engine.

BS: *And they had these on the Catalinas?*

JF: Oh no. On no, hell. The Catalina. You know what that is.

BS: *Yeah.*

JF: But, what I'm saying is that I wanted to convert . . . use the squadron structure and get the P2Vs, but they weren't available. And so I worked out an alternate plan which was to convert, first to the Grumman - what was the designation? We called it the SA-16.

BS: *Ummmm . . . there was the Catalina, then we went to the . . . I've forgotten.*

JF: Well, anyway, that's the . . . I managed to get funds for doing this, so I got together with the Grumman Aircraft Company and made an ASW configuration of the SA-16.

BS: *S2F.*

JF: S2F, you're right. Yeah, that's it. And we did that. And then, in the next phase which was some years later after I had left Norway, they converted to P2Vs. But, they did all their training with the SA-16s. And I remember, there was a similar squadron down in Sicily. Catania. And I used to take the Air Force crew down to Catania and spend a few days with that squadron at Catania when they were trying to learn how to really be functional.

BS: *So, after Norway, you went back to the Air Force?*

JF: After Norway, believe it or not, my next assignment was back to school to McNair.

BS: *Fort McNair.*

JF: Yeah.

BS: *The National War College or . . .?*

JF: Well, no, at the companion. The Industrial War College. There's two parts, as you know, so I went to ICAP and graduated there, then, I guess that was . . . I started in '61 and finished in '62, and then from there, over to the Pentagon on the Air Staff, or I was Chief of the Long Range . . .

(End of Tape 3 - Side A)

(Begin Tape 3 - Side B)

(000)

JF: I retired in '63, and went to work for Rand. I remember because of the experience of getting the SA-16s and ASW configuration for Norway and so on, I had gotten acquainted with the project officer of the Grumman Company and when they heard about it, Grumman offered me a job as what they called Vice President in the International Division, which really meant being a salesman for their product in Europe. And I didn't find that to my liking, so I took the job at Rand at less than half of the salary because they offered freedom to more or less do what you pleased and choose a subject that you were really interested in. I remember this because I remember Grumman couldn't believe it when I turned them down . . . they kept wanting to resume the subject and they couldn't see why. But, I enjoyed Rand. I spent 7 years there.

BS: *What did you work on?*

JF: At that point, I knew a good deal about NATO and in particular, about Norway and Scandinavia. But, also from being deeply involved in long range planning through the Pentagon,

and so forth, and I was interested in policy and long range planning. And had some particular knowledge about northern Europe, so the time I spent at Rand - I spent about half the time - I used to alternate. You'd spend about 6 months or 9 months doing a monograph on something or other and so I would do a military study and then the next 6 or 9 months, I would do an environmental study. And on the military studies, I did a monograph on NATO's Northern Plank was the title, and I still think it was pretty good. It's still classified, unfortunately. It came out "secret" and about a year ago or two years ago, had a big downgrading exercise and it came down to "confidential" and so it still exists and is distributed still as "confidential." But it's not unclassified yet.

BS: *I think I read it at the War College.*

JF: Really? I thought it was pretty good because I put a lot of effort into it.

BS: *Well, that's stuff we did a lot of reading on.*

JF: And I ran head-on, of course, with some of the Rand experts who were, you might say, from a different school. And because of the controversy, there were several people at Rand. Well, Daniel Ellsberg was on the sideline sort of, but I was trying to remember - Mack Hoag, I think, was one. I don't know if you know any of those people, but at any rate.

BS: *I know Ellsberg. Schlessinger was there?*

JF: Jim Schlessinger was selected by the Rand management to be referee because it was me against them. They wanted various things changed and I was refusing to do it. And Jim Schlessinger was appointed as referee and at any rate, I didn't change it. It was published. They decided to publish it the way I wrote it, but which was very uncomplimentary to the US and to

some of the people in Norway. And I recommended certain things that were anathema to that administration at the time - the McNamara administration.

(50)

I was trying to think of a simple way of explaining. The US policy, as you know, was to threaten massive retaliation. Any attack on a NATO ally would be treated like an attack on the US with a massive retaliation. My argument was that this is just not credible in certain circumstances. For example, take north Norway, which was very attractive to the Soviet Union at that time because their whole Atlantic fleet had to be based at Mermansk and at the North because they couldn't get bottled up in the Baltic or the Med, for that matter. And so, Mermansk was the main base, but the whole Kola peninsula was one big military camp. My argument was that at least the massive response ought to be modified so that it could be seen in two stages. Namely a local retaliation against targets that they really couldn't afford to risk and there are so many key military targets in the Kola peninsula that that was easy. I mean with the range of only a couple of hundred miles, you could target specific things that they absolutely could not afford to risk prior to a general war, in other words, in a limited conflict.

So, my strategy was to threaten those key things, which was enough - it was quite enough for regional deterence and not make the massive threat which was the standard response. And with that in mind, I equipped - we had F-86s in the fighter units in Norway - and I equipped them - they didn't get them until after I was gone, but with F-104s which were fast enough and had the means to deliver weapons against the defensive forces that were available on the other side and while I was there, I had the Norwegian Air Force . . . they never had nuclear weapons in Norway, but I did have them practicing with dummy shapes which I figured was enough to worry the other side, and it was, it turned out. Well, these sort of things were anathema to the Rand thinking at that time, or to some people. But, as I say, I published that monograph the way it was written and later when Jim Schlessinger became Defense Secretary, he adopted it 100%.

BS: *He came to Point Barrow, and that's how I knew he knew you.*

JF: Yeah.

BS: *You'd just been up there. The Polar Research Board, they were with them. We met at Barrow. I was the host and I met you there. Of course, I knew about you because I had just pulled everything_____ in the Spring. I wasn't very happy about that. But, anyway . . .*

JF: I haven't seen Jim for years now, but I occasionally have because that brings up the other subject. You said what did I do at Rand? Well, the first study I did, as I say, was mainly on Norway and defense tactics, but mainly strategy. But, the other thing I wanted to do was to learn something about the problem of global climate and at that point, I didn't know much, even though I had been Head of the Air Force Geophysics Lab, I hadn't really learned much about the subject.

(100)

I had gotten interested enough when I was flying in Alaska, so that I wanted to study the problem. So, what I would do was to spend half a year or more doing a military subject and a monograph, and then a half a year of more on a climate subject. And first, I studied the role of the Arctic and there's a fat monograph on that - a couple of them. In fact, two or three. And then, the Antarctic and there's a monograph on that. And this was mainly a learning exercise for me. And, of course, as I learned and understood more about the subject to earn my keep, you might say, I published it as a monograph. But, it was mainly a learning exercise. And that led to a number of things up to the present, you might say. I think we finally got the problem corralled. I studied the Arctic and, as I say, I published results. And the Antarctic. And the bottom line is that

my conclusion is that although these are things that could be studied and had been, at that time, little studied, but they don't, how do you say? They're secondary, really, to whatever it is that drives the global climate and my attention then was shifted to the tropical - to the Equator - and especially the tropical Pacific. And I, you might say, sort of settled on the strategy which goes something like this. If you look at the energetics of the global system, the strongest heat sink, of course, is Antarctica. And the strongest dynamics in the global ocean and atmosphere is in the southern high latitudes and so on. And you can say some similar things about the Arctic, but it's not nearly as strong. And when you look at these things, though, you are driven to the conclusion that, when it comes to climate change on the scale of either year to year, decade to decade, or century to century, the most dynamically active region is the Tropics.

BS: *Short term.*

JF: And that is associated with short term. And so, my attention got, after having studied the Arctic for a while and then the Antarctic and written it up, I was drawn to the tropical regions and that led to a whole series of things which led to my going to NOAA. At the time I did my Antarctic study, the data ended, you might say, at the Antarctic perimeter.

BS: *Converges?*

JF: Well, my point is, there was no ocean data. And there was very little atmospheric data. And you really couldn't delineate the behavior of the system without more data. It was like running up against a brick wall. And I sort of, not abandoned, but temporarily stopped at that point and took up other duties and at that point, my family and I were starting to get a little tired of southern California and I wanted to take a break, so I went on sabbatical and went to the University of Washington, initially for a year, and then a second year, to set up AJAX, because I'd gotten

acquainted, or further acquainted with Norbert and had given a couple of seminars up there and so on.

(150)

And this had led to the general idea of the AJAX experiment and so I agreed to go there for a year to see if we could set something up. And that's what I did.

BS: *So, you and Norbert put it together in the concept stage at the University of Washington.*

JF: Yeah.

BS: *Who else worked on it?*

JF: And Rand . . . I invited Norbert down to be a consultant at Rand and he used to come down every now and then, spend a few days with me in Santa Monica, and I would go up and spend a few days in Seattle.

BS: *Were there others who worked with you on it at the time?*

JF: Norbert had a couple of students who were helpful, but in putting together the plan, I remember very clearly putting together the project plan, for example, that you included in the justification and what you were trying to do and all that. And besides Norbert, I mean at that time, there was no AJAX staff, of course. I'm trying to remember. What's his name? The guy that went down to the University of Puget Sound and was Norbert's graduate student who worked out the annual heat budget of thermal transmission through the pack ice? And they developed a model.

BS: *Willy Weeks?*

JF: No.

BS: *It wasn't Willy Weeks. Martin. . .?*

JF: I know Weeks well. Weeks had been in the lab in GRD, but Willy wasn't involved at that time in any significant way. But, the fellow I'm talking about died between 5 and 10 years ago. He went down to the University of Puget Sound. He worked for NASA. He was NASA's point man on sea ice. What the hell was his name? I know you know him.

BS: *He was on AJAX when we put it out.*

JF: No, he was involved in developing the plan, the program plan. Norbert and I were the main authors, but I remember him as the main one who was involved. He was one of Norbert's students and there was another person whose name I don't remember now that helped do that.

BS: *You went from there to what, NSF?*

JF: Yeah. Well, first of all, my family liked Seattle so well, they didn't want me to go back to Santa Monica, and so I applied for a second year for a sabbatical from Rand and they agreed. And I never did finish out the second year because in the meantime, about a few months into that, I got the NSF job.

BS: *You relieved Tom Jones?*

JF: No, Tom was my boss.

BS: *Tom was your boss.*

JF: Yeah.

BS: *But, you were Head of the Division of Polar Programs?*

JF: Right.

BS: *But, he moved upstairs.*

JF: You're right. He had moved up.

BS: *And you removed him as DPP and he moved up.*

JF: Right. Exactly. That's it. And Bert Crary had moved up with him, so both he and Bert were out of DPP at the time.

(200)

BS: *What year was that?*

JF: That was '61.

BS: *'71.*

JF: Excuse me, '71, yes. And I kept that job for three years, but while in that job, I was interested in the Arctic and the Antarctic, but while at NSF. . . well, I learned a lot when I was at Rand. Mainly, just from studying, and I had a good friend, Professor Jakob Bierknes. Do you remember him?

BS: *Yes. A meteorologist?*

JF: Yeah.

BS: *From Norway?*

JF: Yeah. Well, he was a UCLA professor, but during the year and a half that I spent at UCLA, I was studying physics. My major was in physics, but I took a number of meteorology courses and I got to be quite friendly with Jakob Bierknes. And when I went to work for Rand, I bought a house in Santa Monica canyon and Bierknes was a neighbor. He was about a block away. And because of the interest, I intervened at Rand, and we hired him as a consultant. So I used to, you might say that during the several years I was at Rand, I had Jakob Bierknes to consult with and I learned a lot from him. I had a great admiration for him. He was a great guy and I think he had some insights that were very peculiar to him, you might say. He was an observationalist, put it that way. And his approach was to study the system the way you would study an engine. If you came out and found your car wouldn't start, you know, you'd start diagnosing how the goddamn thing works, diagnosing how to go about finding your troubles and what not. Well, it was his general approach to the Earth and the atmosphere and the ocean. And so anyway, I more or less had my thinking molded by his philosophy, you might say. And, in the process, you might say, my general approach was, if you look at the energetics of the global system, you find, of course, that the Antarctic is the great heat sink, of course, but the year to year or longer term variability is not so great as to make it dominant. Put it that way. The short term dynamic element occurs in

the Tropics and that's where you need to look for the signals. And starting with that philosophy, I came to the conclusion that, well . . . that that's the general approach to take. Look for the signals that you can associate with real changes of the global dynamics and then start trying to understand those signals and what they mean.

(250)

And what that leads to and again, that was through Jakob Bierknes, the signal that stands out is the sea surface temperature, the El Nino phenomenon, the sea surface temperature in the eastern equatorial Pacific. Now that's not the driving force. The driving force is in the atmosphere, but the signal was the sea surface temperature and that was known enough, even at that time, and Bierknes had done most of the work. Bierknes' approach had started in a rather similar way to mine, generally, you liked to understand how the machine works. And his interests led him to study, first, the north Atlantic, because that was close to Norway and it had the most data and what not. And he studied the North Atlantic and the Icelandic low, and so on, first. Then his attention was shifted to the South Pacific, to the equatorial Atlantic. And he did a couple of treatises on that. And then his attention was shifted to the eastern equatorial Pacific because he realized that that's where the biggest signals were. And it was about at that stage that I started learning from him, you might say. And so, at NSF, there was one stage in which the atmospheric division was invited to have a director's review on the GARP program.

BS: *GARP is Global Atmospheric Research Program.*

JF: And the guy who was asked to put this together was Gene Bierly. Do you know Bierly?

BS: *No.*

JF: He was not the Head of the Atmospheric Division. Who, at that time. . . the name escapes me. White was his name. Well, anyway, the subject was unfamiliar enough to him so that he came to me and asked if I would do that part of it - the GARP second objective, which was to understand climate. And he'd put together the rest of it which had to do with the GARP first objective, which was shorter term things. And anyway, I still have that. I have it around here somewhere - the director's presentation for that GARP program. But, the strategy, you might say, was identify the most energetic features of the machine. Then, look at the record and see how they vary with time and if you can explain why they vary with time. And, if you can't, try to understand not only how they vary, but why they vary. And when you do that, you're led in a certain way. You can start with the biggest signal that can be identified which is the sea surface temperature in the eastern equatorial Pacific.

(300)

But ,when you think about the problem, it becomes obvious that that has to be a result of something else. It's not the forcing factor. Well, it's not the dynamic factor. Put it that way.

And it eventually sunk in on me, you might say, that the dynamic factor is deep convection in the Tropics because the deep convection in the Tropics drives the primary thing that drives the Hadley circulation, drives the tropical oceans. It controls the sea surface temperature and various other things. And then you become aware that there are some very unique and interesting features, namely, if you pay much attention to the atmosphere in the Tropics, and I spent the last part of the war in Mariannis and that's where I first started hearing about the easterly waves, for example, and you become aware of the lid in the Tropics on convection. And gradually, you become more aware of the discontinuities. For example, the triggering of deep convection and that was the first time I became aware of the fact that most of the time in most of the area in the tropics, things are pretty calm because you don't have deep tropical convection. But, at a certain temperature which is close to 28-1/2 degrees Centigrade,

you break the stabilizing forces and it's like the Dry Line in the Midwest that triggers tornadoes and super-convective cells, that you start getting deep convection and that was really the dynamic factor and from year to year, this shifts in the east-west direction and gradually this is the E Nino phenomenon that we've become familiar with. Well, anyway, we proposed that we set up a large scale experiment to try to understand and unravel the El Nino.

BS: *This is when you were at NSF?*

JF: That's right. I had two jobs. I was the Head of Polar Programs, but Tom Owen was a former Navy-type. He was a classmate of mine at the Industrial College - was Head of that part of NSF. He had been Head of ONR. Anyway, Tom was very supportive, and I became aware of the fact that NSF is not set up to do a multi-year coordinated and purposeful experiment. Put it that way. They are in the process of giving out grants for short term specific things, but they're just not organized for continuity in strategy or implementation.

(350)

And that's why I went to NOAA, because first I got involved in planning for a climate program and Guy Stever adopted that all right. The first climate initiative budget line item came out of the program review that I'm talking about, because Guy Stever was very receptive and we had some other policy interests from outside NSF. And so he proposed, it was the only new line item proposal in the NSF budget for that year - It was, I guess 1974 or '75, which led to the climate program at NSF.

BS: *But, you had run the program for three years - DPP.*

JF: Well yes, but the last year of those three years, I had a second title which was Director of the Climate Program. We had no climate program. It was just being put together, but they had the title and that gave the license to work on it in terms of drumming up support and so on.

BS: *You pushed AJAX through.*

JF: Well, yes, but AJAX, by that time, was over.

BS: *Well, AJAX - we put it in the field in '75, and pulled it back in '76.*

JF: Well, OK. That was the following year. I left in '74.

BS: *But, you got NSF to back AJAX.*

JF: Yes. Not just NSF, but NSF would have never done it by itself. ONR and the Canadians and several other quarters all chipped in - NASA. I forget now. There were half a dozen sources of support.

BS: *How was the mechanics of running DPP? How did you find those?*

JF: Well, first of all this had developed out of the IGY, as you know. And that's why . . . I'm trying to remember if I ever knew where they picked up Tom Jones.

BS: *That was in 1959. He was at the National Academy.*

JF: Yeah.

BS: *He came over.*

JF: I'm not sure. It was through his chemistry professor at Columbia and they had trained Bert Crary, of course, from the Antarctic program. And so Tom Jones and Bert really ran DPP.

(400)

But, both of them had moved up to different jobs by the time I got there. Bert was very influential, I think, in getting me nominated for the job. And, of course, he knew me well from previous experience. And I enjoyed the job. I enjoyed that very much, really. But ,when I came there, it was all biology. There was no program manager for atmosphere. There was no program manager for oceans. There was no program manager for glaciology. Can you imagine that?

BS: *I know.*

JF: What I would consider the most important subjects for Antarctica, there weren't even program managers. And the most influential program manager, there were several program managers, there was what ? Ray ____ was Upper Atmosphere, and George Lano, which was the main program, was running the biology program. George was all right. He was good. Some of the others were not so good.

BS: *Mort Turner was there?*

JF: Who?

BS: *Mort Turner?*

JF: Mort Turner was there. Mort's a nice guy, but he had no managerial capabilities. So anyway, it was a very weak staff. Put it that way. And the most active guy on the operational side was Phil Smith. And Phil was a pretty effective guy and he was very active, but of course, he was no scientist. So anyway, that's what I came into at NSF, yeah. And I tried to build it up. I recruited program managers for each of the subjects that I mentioned. Gunter Weller, for example, I brought in as the meteorology program manager. And I think we made some progress along those lines, but the big operation every year, of course, is the annual resupply and the McMurdo activities and what not. One of the things, I made a list when I came there, I made a lot of I won't say enemies, but I ran afoul of a number of places. When I came there, there were over at the Navy Yard, the Navy was maintaining an Admiral. What was his name now? Mack something.

(450)

BS: *Kelly Welsh, or after him? I know who it is. It was after Kelly Welsh. Right?*

JF: No. I think it was before.

BS: *And Kelly came along after.*

JF: Might be. But he, in fact, had been at the Navy War College when I was there. He was Commander in those days along with Dennis Ruthinson and others. Anyway, he showed up again as head of that. But, my point is that he had a staff of about 40 people or something like and they went down to Christchurch for the social season. But, the rest of the year, they really didn't have much to do, it seemed to me. And that first year I was there, which was '71, what did we call that? Black December? Nixon had a huge budget cut. He cut - the DPP program was cut in half, roughly, and so we had to cut back on a lot of things. And I wanted to cut back on overhead and I took advantage of that budget cut to get the leverage for doing it. And we did

away with the whole installation over at the Navy Yard - the Admiral and the 35 or 40 people he had for staff. Retained the support force, of course, and in essence, the few functions that needed to be continued were transferred to Quonset instead of the Navy Yard and the point of contact was Assistant to the Secretary of the Navy, who sort of did the staff work that was absolutely necessary. I don't remember . . . I remember his face, but he was . . .

BS: *Well, the position now is . . . or was, when I was there, was under the Assistant Secretary of the Navy for Research.*

JF: That's right. And the other problem in NSF was, of course, that none of the other parts of NFS had any sympathy when it came to budget competition with DPP. That's something they could do without.

(500)

And so, the other political objective was to get the DPP budget handled as a separate line item that could not be tampered with or raided by the other parts of NSF. And we got that done. But, we almost had DPP abolished because the National Science Board - you know what that is - they were very much aware that they would like to take the DPP budget and distribute it in other parts of NSF and break it up into 50K grants and part of the normal business. And so the National Science Board, as a body, had a hearing on this and I remember, I gave them a bunch of briefings and the upshot was that they appointed a committee of 5 to render a separate examination report and the 5 people who were on this . . . the two people who were most alien to DPP, who wanted to abolish it and distribute the money elsewhere, I remember them very well because this was Neurenberg was one, and the other one was later the President of the National Academy of Science. What the hell . . . ?

BS: *Phil Neurenberg was at NSF then?*

JF: No. He was on the board. On the National Science Board. Anyway, I took the group of 5 to Antarctica and briefed them probably with 10 - I had, I forget whether it was 8 or 10 - lectures or briefings you might say on different aspects. You know, what were we doing about the science? What were the big problems? What were the things to do and what not? And they came back to converted. They were strong supporters. They came back and rendered their working group report to the Board strongly supporting DPP and that, more or less, ended the push to abolish DPP.

(550)

So, anyway, those were the main activities that I remember being absorbed in there. At that time, though, I should say, during 1971, we started out with a budget of about $38 million, and it was reduced to $23 million, right in the middle of the season. We had to cancel about half of our research contracts and that's when I laid up Eltana. There are only a few ways you can save money in the middle of the season. And we laid up Eltana and did a bunch of other cutbacks. We gradually came back and at the time I left DPP, three years later, the budget was back up to about $40 million. But, now it's about - last I heard, it's about $150 million.

BS: *$196 million.*

JF: I was going to say, it's somewhere between $150 and $200 million.

BS: *$196 million next year. I get that from my son-in-law. He's in charge of logistics for ASA.*

JF: But, I had worked out an agenda for myself of things that I felt needed to be done and I've named a couple of them - the things that we did do at the time. But, very high on the list was to try to streamline the air operation. And I left NSF before that became . . . I mean, at the time, we were facing some things that needed the air operation very bad. The rebuilding South Pole, for example, and rebuilding the station over at the base of the peninsula. Not on the coast, but the . . .

BS: *Siple Station?*

JF: The ionospheric station.

(600)

BS: *Yeah. That was at Eight and then it moved over to Siple.*

JF: Well, yeah. Both of those desperately needed to be rebuilt. And my timetable was that as soon as we could get past that, we could afford to try to streamline the air operation and make it more economical or more efficient. And I left NSF before we got to that point. But, the reason I left is what I was getting at, was that. . .

((End of Tape 3 - Side B)

(Begin Tape 4 - Side A)

(000)

JF: Anyway, the system of laboratories that NOAA maintains offers the possibility of having the continuity of purpose to put together a coherent plan that would last over a number of years for solving a problem. In other words, a climate investigation. And that was the thing which really attracted me to NOAA. And so, Bill Hess came along and offered me the job of coming in here as his deputy and I said yes. That was the main reason. The other reason was that my wife didn't want to come to Washington, but she was willing to come to Boulder. So, I had maintained the household in Seattle when I left the university for those three years. So, anyway, it turned out that way and the first coherent plan in investigation was to investigate the tropical Pacific.

BS: *Now, your office was where?*

JF: Here.

BS: *I mean, where here? Up at NCAR or . . . ?*

JF: Oh no. We have a set of buildings on the south campus rented from the university. We're building a building to consolidate into right now and have money in the bank to do it. But, they're still occupying, in fact, the rented buildings from the University of Colorado. But, that was the Headquarters for the NOAA laboratory system. There's 11 laboratories now. There were 9, I guess, at that time. They've revamped it a little bit. But, the headquarters was all here, so you could put together, you might say, a broad, long-range plan. By it's nature, it has to be interdisciplinary. It's not something you can go out and do in one year. It has to be done over a span of years and so the initial plan for doing this we called EPOCS - Equatorial Pacific Ocean Climate Studies.

And, see, I came to NOAA in September of '74. I presented the plan and the budget initiative first in '76, but it didn't get approved in '76. That is, I didn't get out of NOAA. Bob White was the head of NOAA. But, the next year, in '77, Bob got behind it, and that was the

budget for '78. So, that was the first year of actual funding. So, we started the investigation then and it developed into a whole series of things. I mean, EPOCS became a broader program - an inter-agency. And for a brief time, this had the acronym OASIS and that was adopted by the WMO and became TOGA - Tropical Ocean Global Atmosphere and it became a program of the World Climate Program thrust. And in relatively short time, EPOCS became expanded until it had about 20 countries making contributions and here we are, 15 years later, and I think this has pretty well been worked out. I mean, I think we've solved the TOGA problem, the interactions between the oceans and the atmosphere. It can now be modeled with credible models in the atmosphere and ocean side - made a lot of progress and more or less achieved those objectives.

(50)

But, the culmination of this that I think is really the most important, nobody's heard about yet except me and a couple of other people. And that is, as EPOCS developed into TOGA, we came to understand the dynamics of the ocean and atmosphere in the tropical Pacific all right and that's where the action is. But, what we've come to realize later is that. . . well, let me hit two or three points. Number One - what changes about global climate? I mean, which parameter reflects the important changes? The answer to that is not global surface temperature that you read so much about. That's probably one of the worst things that you could guess.

BS: *It's not a good indicator.*

JF: The most influential factor is the strength of the global circulation of the atmosphere and so you ask the question, is that really a variable? I mean, will that vary from decade to decade and century to century and so on. So, you have to look at the record and there I left out something I meant to mention. And that is, while at Rand, I mentioned after doing the Antarctic study, I ran up against a stone wall, because there was not data from the global ocean. I went up to Monterey

and Paul Wolf was Head of the Fleet Numerical Center at Monterey. Paul had been the aide to the Senior Navy Officer at Anchorage when I was in Alaska and I had taken the two of them on a flight to the North Pole at one time. And he was a lieutenant in those days, but we had met. And that's probably the reason that I had heard that he was running the Navy Center up there. Anyway, that's one of the things I need to give the Navy credit for is that Paul gave a very sympathetic ear to the problem of not having usable data from the world ocean. And I think it was the Navy and mainly the Fleet Numerical Center who initiated the initiative at that time to get this archived. And, you know, it was then picked up by various others who pitched in. But, I think it was Monterey that really started that off, and mainly Paul. And he had a guy working for him by the name . . . it was a Latvian name . . .

BS: *Tivo* . . .

JF: Yes, it was Tivo something.

BS: Short guy. Full of energy. I was an oceanographer/meteorologist there. I went through the course. ____ me to the Arctic.

JF: I remember it was Paul and Tivo who really carried the water on this and it was later picked up by the WMO and various other . . . they ended up splitting it up among different countries. The Germans took responsibility for the Atlantic, for example. And they did a good job. The Dutch took responsibility for the Indian Ocean and they did a good job. The US took responsibility for the Pacific and they did a mediocre job. It wasn't near as good as the Dutch and Germans. But, the upshot was that they dug out all this back data since 1854, digitized it and put it on tape. So, when I came to Boulder, this was sort of the situation. There was a lot of tape. It wasn't all done yet at that time. It amounted to about 800 tapes.

BS: *Now, what was your position at Boulder? You were in charge of all the labs, or the NOAA lab?*

JF: The Deputy. Bill Hess was in charge of the labs and I was his deputy.

BS: *Was he in charge of NOAA or just the labs?*

(100)

JF: No, just the labs. And I came in as his deputy. But, anyway, I took a look at the situation. As I say, after a couple of years, the Germans had done a nice job and the Dutch and there were about 800 tapes all together that had all this digitized stuff on them and it amounted to about 150 million observations from 1854 up to 1979, at that time. But, it was unusable because it was in different formats, some of it . There were errors that had to be traced down and whatnot and it needed to be brought together, sorted out in time and space, and compressed, and put in a usable form.

And so, I submitted initiatives in NOAA for three years running to do this without any success really. And finally, I made an agreement with a couple of other people in NOAA - the head of the data service, for example, that even without any budget initiative and without any support, let's just start doing it and it has to be at a very low level because, hell, you can't afford to do very much of it. But, let's lay out the tape and start doing it, which we did. And the upshot was what is now known as the COADS data set and that is now all the data. It's now about 500 million observations. More than that now, I guess, because they were adding to it at the rate of about 100 million a year in recent years. But, originally, from 1854 to 1979, it was about 150 million. We brought it all together, it was examined for errors and programming and so on. It was sorted out in time and space. It was formatted in a two degree grid by month. And to read through . . . we finally, it took a year or so to bring together from the Dutch and the Germans and

other sources, all of the basic data sets. And to just read through it without doing anything to reformat, or anything else, at that time was about $100,000 worth of computer time, just to read through the goddamn thing once. And that was a problem. And when Bill Hess left NOAA, he became Head of HEEDCAR and they were buying a new Cray. And I got Bill - they had a run-in period. A test period that lasted about three months, I think it was. Well anyway, I got Bill to dedicate about a third of that time of the new Cray to sorting out this bunch of tapes and that was the initial sorting out, which was a lot of the work of most of the others. From then on, it was mostly refinement. Of course, that is the basic source of our knowledge about how the climate has behaved. The dynamics of the system over the last 150 years.

BS: *A valuable tool for predicting global change.*

JF: Well, it's the only way you can find out how the system behaves. Now what do you find out? The thing that you find out is the most important variable is the strength of the circulation and the strength of the circulation has changed radically over time and most of the change takes place suddenly.

(150)

Gradually also, but the biggest change takes place suddenly. And over the last century, most of the change took place on about three or four different dates. Since 1854, the strength of the global circulation was stronger in the 1850s and 60s than it is today or ever has been between. After peaking in the 1860s, in about 1874, about the mid-'70s, there was a sharp decrease and that's one of the few discontinuities in time, I'm talking about. There was another . . . then there continued to be a decrease and some variability until about 1900 and there was another very sharp decrease occurred about 1900. And then with variability, it continued to decrease so that the minimum was in the 1920s and '30s and since then, it's been increasing with sudden increases

about, well, maybe you could say around 1940, but mostly about 1962 and 1976 are the two dates that stand out.

BS: *What happened in '62 and '76 that . . . ?*

JF: There was sudden jostling of the global system. Put it that way. But it's on the increase and especially since 1976, there's been a rapid increase. Now to put this in perspective, you'd like to know what changes, where does it change, how does it spread, and a whole bunch of things and I can give you an outline and say, in 1874, the sudden decrease in the strength of the wind and, mind you, all we have is the surface obs. The sudden decrease was strongest in the northern hemisphere higher latitudes - northern hemisphere westerlies and the mean speed of the surface wind, for example, over the north Atlantic decreased about 2 meters per second against a background of say 7 and a half. That's a big change. That's like a third, almost.

On the other hand, the big decrease around 1900 was not there. You can see that 1875 change all over the planet and it's reflected in the southern hemisphere all the way down to the southern hemisphere westerlies. But, it's weaker as you go south. And as I say, it was strongest in the northern hemisphere westerlies. The 1900 signal was a sudden decrease that was strongest in the southern hemisphere westerlies and again, it was a sudden drop of about two or three meters a second against a slightly higher average of say, 7-1/2 meters per second. And that was really sharp. It occurred in about a space of a year or two years. At the same time, the monsoon rainfall in India dropped by about a third. The discharge of the Nile River dropped by a third. The monsoon rainfall in China increased by 25% and you can see the signal all over the world. The Colorado River changed by about 15-20%. And so these are the kinds of things.

(200)

And so the problem can be summarized by saying, how do you understand the changes in the strength of the circulation? And when the circulation changes, of course, a lot of other things change. For example, evaporation is proportional to the - not the square, but the first power, of the surface wind. And so, you change the surface wind by 20-30%, you change evaporation by 20-30% and if you change evaporation by 30%, what goes up eventually must come down. It means a similar change in precipitation, and in heat. The heat, when it evaporates, is taken from the ocean. When it condenses, it's given to the atmosphere, but in a different place and in a different elevation and so on. But, these are huge changes. What I'm saying is this is what characterizes global climate. And all the other parameters can be derived from this.

So, the question is, how do you explain these sudden changes? Well, only about four or five years ago, did I feel we were getting on top of this, which I think we are now. And that was when I realized that everybody has talked about the maximum sea surface temperature in the Tropics and what not. That's not the problem. The problem is the amount of deep convection, 60,000 foot type, because that's what drives the Hadley circulation. That's what creates most of the high clouds and so on. And it's not the temperature of the surface or the maximum temperature that is significant. The thing that is significant is the area of ocean that is above the critical temperature that triggers the convection. That's 28-1/2 degrees. You can use 29 or 28.

BS: *28-1/2. You mentioned it earlier.*

JF: But, 28-1/2 is the most accurate.

BS: *And what's that figure called. What's the acronym for it?*

JF: I don't know of any name for it. I don't know if it has a name.

BS: *The critical temperature or the _____ temperature or . . .*

JF: The first person I know of who pointed this out was Herb Reel back in the World War II days and Herb Reel and JoAnn Simpson did a classic paper on this in which they pointed out that the whole global atmosphere could be driven from the Tropics by something like 1100 deep convection columns and that this was a very non-linear process. I mean, if you start warming the surface of the ocean, nothing much happens except low cumulus and so on until you get to this critical temperature. Now there are a bunch of things that are surpressing convection, of coure. There are thermodynamic properties. The amount of buoyancy that is created by the column itself. You're a pilot. As you very well know, the sheer, the vertical sheer is a very important limiting factor. And the point of it is that there is a certain temperature at which deep convection starts to occur because it overcomes all these inhibiting factors. And that's about 28-1/2 degrees.

(250)

And the first person, I think, who really seized on this was Herb Reel, probably. But anyway, the thing that's critical is the amount of the tropical ocean that this deep convection goes on in. And that is the area that is warmer than this critical temperature. And if you stop to think for a minute, that makes lots of sense because if you see . . . I recently have gotten ahold of some photographs taken from space of the convective area in the western Pacific and you might say all of the area takes part in the process. In the deep columns themselves, the upward vertical speed is probably ten times bigger, maybe 15, I don't know the right number, than areas of substance. But, most of what goes up in the fast moving columns is substance that is local, according to Yock Koopner who's been the program manager for the Koger Core. Yock says this is like 75%.

BS: *Is local.*

JF: Is local in the tropics. I mean at (messel scale.) The other 25% or 20% is what drives the mid-ocean highs and that's large scale substance that we have read about in the textbooks. So, the point is that the ____ there is taking place either as a region of substance or of updraft. And so the total amount of area is proportional to the total amount of activity and that's what drives, that's the basic engine that drives the short term changes year to year.

Now, one manifestation of that is the El Nino and the ocean reacts in various ways and now TOGA has become the primary instrument for year to year climate prediction. But, the same feature that namely the region of deep tropical convection that is responsible for the TOGA type variability is also what reflects the century scale variability. But, it's not apparently related to the TOGA process itself. And this is one of the things that I've been bemused about in recent years is that what is driving the longer term variability, because the first thing that I came across is that if you look at it on the decadal scale and longer, again, you see a beautiful correlation between the size of the warm pool and the strength of the circulation. In particular, what I did is I looked at the . . . with the COA data set, you can do. Until we had the COA data set, there was nothing.

BS: *COA data set is?*

JF: Consolidated Ocean Atmosphere - the (Mauri) data. Anyway, compare the size of the warm pool, warmer than say 28-1/2 and compare it with the strength of the circulation and of course, the index that we can get our hands on easily is to look at the surface wind and average from say, 30 north to 30 south - that includes half the surface of the sphere and the whole sub-tropics and so on, and you find that they correlate exactly on a year to year basis, on a decade to decade basis.

(300)

BS: *What drives the growth and shrinking of the warm pool?*

JF: You put your finger on the key question. And if you had asked me that three months ago, I would have said no more than that.That's the key question. But about two months ago, I happened to come across a guy who was in Boulder. He had been invited to Boulder by NCAR and by the State's Environment Lab from the University of Berlin. And it turns out that this group at Berlin had been contracted by NASA to recalibrate their measurements of the solar constant. They had a cavity instrument they'd been flying now for, by now, it's about 20 years for measuring the solar constant outside the Earth's atmosphere. And they had never been satisfied with the translation of their measurement into the solar constant that they can believe. So, the University of Berlin had expertise here, so they have a contract for doing this. Number One - they conclude that the variability is actually 4 to 5 times greater than NASA had thought or anybody had thought. That's a key.

BS: *The variability of the size of the solar pool.*

JF: No. The variability of the sun's irradiance measured outside the Earth's atmosphere. The second thing is that they find that there are certain reactions which occur between high energy protons and certain elements of the Earth's atmosphere which create new isotopes. Chlorin 37 is one of them. I think it's Cecium 10 and Chlorin 37 are two key ones. But, the point is that these isotopes are later entrained in the atmosphere and show up in the ice cores. So that by analyzing the ice cores, they've enlisted the Swiss group and the French group at Grenoble and they are able to come up with what the solar constant has been at various times in the past.

BS: *Back down through the Vostok cores?*

JF: They've run it back as far as 1650 already, which is the (maunder) minimum of solar activity. It's been known for a long time. And the results are beautiful, really, to look at.

BS: *Is there a cycle? Can you identify a cycle?*

JF: Yes. And that's the next . . . but first I want to emphasize that if you can identify the cycles that are responsible, as you say, you can not only trace and validate the past, but project in the future, which we've never had a way of doing before, because up to then, the question has been the one you ask. - what controls the size of the warm pool? And we had no explanation that really hangs together on that.

(350)

But, now it appears that the main thing that controls the size of the warm pool is the strength of the solar radiation. And the 11-year cycle is minor. That's almost noise. The two cycles that dominate are the 88-year cycle which is the Glassberg cycle and a 207-year cycle and I don't know what the classical name for that is.

BS: *88-year cycle is the which cycle?*

JF: Glassberg.

BS: *Glassberg. And 207 years.*

JF: And 207, and they dominate the record. So, by projecting these two and they're not exactly in phase, you know. You get an irregular . . .

BS: *They go differently.*

JF: They go differently.

BS: *You've got one that doing like this and one that . . .*

JF: But, if you take that combined record and compare it with the COADS record back as far as . . . it reflects beautifully the things I've mentioned - 1875, 1900, 1976, even 1962, and so on. And so the validation appears very good indeed. And as I say, you can project it into the future as you please.

BS: *Interesting.*

JF: Well, it's fascinating because . . .

BS: *Absolutely fascinating.*

JF: Because, in my mind, what is clear is that this is what dominates your question, what controls the size of the warm pool? It's reflected almost immediately in the size of the warm pool because the Pacific Ocean is the great gatherer of solar energy and if you look at the ocean circulation or the surface circulation in the Pacific in the total amount of energy that's involved and so on, my point is that the ocean gathers it beautifully. The wind system is such that the ocean carries it right to the western Pacific, and the record mainly from COADS and the record from the solar records, as I say, just agree beautifully. And, as a corollary to what I'm saying is that the IPGC - the Intergovermental Panel . . .

BS: *International Panel on Global Climate.*

JF: This is mostly baloney. I mean, my point is that 35 years ago, Monovi in weatherology at UFDL did a one dimensional calculation of the effect of doubling CO2 and they came up with three degrees Centigrade.

BS: *That's if you double it.*

JF: What?

BS: *That's it you double it.*

JF: That's a doubling.

BS: *We haven't doubled.*

JF: From 300 to 600, roughly. No we haven't. But, my point - that was the result we came up with. Being a one dimensional calculation, that's assuming that everything else remains constant or that all the feedback loops cancel, neither one of which . . .

(400)

BS: *It's only atmospheric.*

JF: It's only atmospheric, yes.

BS: *It doesn't take into account the ocean absorption of heat.*

JF: But, my point is that today, with expensive GCMs and big computers and so on, they get the . . . the GCMs get the same result. Now that says the same thing, that either all these feedback loops cancel or everything else remains constant, both of which we know is not so. So, what I'm saying is that the GCM, I'd call it almost a spurious result. It really has no credibility in Nature. But, the argument in the WMO has been, we know so little about the dynamics of climate that we have no other method of estimating it.

BS: *Throw money at it.*

JF: Throw money at it, exactly. And you know, it's been for the last 20 years, it's been obviously grossly wrong, but it's not until now that we have an alternative explanation that makes sense and is credible and can be verified with observations. And so, the whole damn international community has adopted this point of view of saying well, we have no better way of doing it, so just run the GCM and accept the results for making international policy. And now Al Gore and Tim Worth are trying to beat all the other countries in line into get them to agree to mandatory emission standards in the year 2000, roughly aimed at the 1990 levels. And two things you can say. One, it's impossible that we can succeed with this, but they can create a hell of a lot of commotion in doing it and Two, it's unnecessary because CO2 is not that important, by any means. The real importance is the process that I described.

BS: *It's absolutely fascinating. I understand.*

JF: Yeah, this is what I'm wrapped up in now.

BS: *Is this a personal project now?*

JF: Yeah, it's a personal project.

BS: *When did you retire?*

JF: What?

BS: *When did you retire from NOAA?*

JF: Oh, about 4 years ago, '92. Yeah, I retired at the beginning of '92.

BS: *And your NOAA career or your government service career is as long as your Air Force career.*

(450)

JF: Yeah, roughly. But, it's going to work. My point is that when I got out of the Air Force, I adopted this problem. And I remember at the time that my wife and I had some long talks about this where I said, I would like to finish in a dead heat. I would like to be around when this problem is solved, but not too much. I want the problem to be solved by the time I die, but not too much before because I'd like to work on it for the rest of my time.

BS: *But, basically what you're saying is that there's not a whole hell of a lot that we can do to affect it.*

JF: That's right.

BS: *And all the worry about the carbon cycle and global warming due to increased carbon dioxide is actually a minimal . . .*

JF: Completely unnecessary.

BS: *Except for places where it gets thick enough to breath, and then you want to reduce it, like Beijing and a few places like that.*

JF: In fact, I subscribe to an opposite point of view, you might say. I've researched this question of the biological aspects of the carbon cycle and so on, and my conclusion is that the best world expert that I know of is an employee of the Department of Agriculture. He's the Chief Scientist at the Agricultural Research Institute at Phoenix, Arizona. A guy named Izzo. And I've talked to him on the phone. I've never met him. But, I read his book. He wrote a book and he has written several other things and a few months back, I phoned him. Got in touch with him and you might say that's the first time we've been acquainted, even to talk. And he sent me, since then, a couple of his more recent publications. But, briefly let me give you his point of view, which, as I say, he's the guy I would turn to for information of this sort, because I think he knows more about it and has looked at it more thoroughly than anybody else that I know of. And he would say, I'm putting this in my own interpretation now, if you look at other planets, for example, you find that there are quite a few planets that have gaseous atmospheres, but most of them are dominated by carbon dioxide. Take Venus, for example. Or, you can give all sorts of other examples. Well, only on Earth do we have the combination that we have here. The Earth's atmosphere is, as you know, 21% oxygen and 78% nitrogen and within the one remaining per cent, you have all the other gases of which carbon dioxide is a minute quantity, only about 300 parts per million, which means that it's a fraction of one hundredth of one percent instead of being 80 or 90%. And why is this? Because there has been time in the evolution of the Earth, for the biological activity on Earth to reach a balance and if, for example, plant life on Earth, which generates oxygen and which devours carbon dioxide, if plant life dies on Earth, it would take something like 100,000 years in which oxygen would be a trace gas. Then it would be a largely dominated carbon

dioxide atmosphere. So, it's the balance between these voracious life forms which constitute the vegetable kingdom which consume carbon dioxide that has both generated oxygen and reduced the level of carbon dioxide.

Now, it's Izzo's premise that he explains in his book is that these voracious eaters of carbon dioxide, namely the plant kingdom, have reduced it to such a level that certain types of plant activity have, in fact, been extinguished and some have been retarded and if it's decreased much more, there will be a drastic breakdown in the plant kingdom. So, you're close to a dangerously minimal level right now. If you doubled the carbon dioxide from 300 parts per million to 600, the plant activity would increase by somewhere between 30 and 50%, but probably closer to 50% than 30%, which would be a very beneficial thing. Now if you combine that with the factors that I have been talking about, increased evaporation, increased rainfall and so on, then it makes 50% increase much more credible. And so, according to his arguments and his analysis, there's nothing but gain in an increase in carbon dioxide levels.

BS: *In other words, man is contributing to the atmosphere beneficially.*

JF: Yes. Yes. And there are . . .

BS: *I believe you're right. I've studied this a little bit.*

JF: There's quite a lot of observational evidence to support this point of view, too. And my point is. . .

BS: *A bunch of those plants are in the ocean.*

JF: A great deal of them, too.

BS: *Well, they produce more oxygen in the ocean than you do on dry land.*

JF: Let me put it in perspective in this way. When you have heating, whether it's from greenhouse effect or whether it's from solar constant, there are three important negative feedback loops. One is the dynamic loop that I have described which says that if the size of the warm pool increases, the strength of the circulation increases, evaporation increases, precipitation increases and that's one negative feedback loop which surpresses the warming effect. A second one has to do with the radiative exchange due to clouds and a guy named Romanathon, you may know him. He used to be at NCAR. He's now at Scripps.

BS: *I think I do know him because . . .*

JF: He's an India scientist. Romanathon is the expert on this. It used to be that the GCM modelers would claim that all three of these feedback loops were positive rather than negative, but now it's pretty well established that they're negative. Romanathon has shown using the (Erbie) data that the effect of cloudiness and the cloudiness is generated by deep

(End of Tape 4 - Side A)

(Begin Tape 4 - Side B)

JF: . . . are so low, would be greatly enhanced by addition of CO2 and that's in two parts. One part is the uptake by land vegetation and the observations of CO2 levels over the last several years. That's one of the conclusions of the NOAA laboratory. One of the ERL laboratories that's based here at Boulder, monitors this and they have a big sink in the biological sink, put it that way, that they haven't been able to account for and by process of elimination, it must be due to a

much greater uptake by plant life than anybody had thought. But the other part of that is what you're talking about, the enhancement of plankton growth in the ocean which, in turn, takes up CO2 for the plankton growth, but also generates dimethylsulfide which gets into the atmosphere as aerosols and then becomes a part of the cloud radiation, if you like . . . modulation.

BS: *Decreases clouds?*

JF: No. It increases.

BS: *Increases.*

JF: And that is a negative feedback.

BS: *You mean it acts as a catalyst.*

JF: No, it acts as a cloud nuclei.

BS: *That's what I meant. A nuclei. I'm sorry.*

JF: Condensation.

BS: *Condensation nuclei. OK. I understand. How about the effect of ozone depletion? Ray Smith tells me that from his experiments in Santa Barbara that it's . . .*

JF: Let me put it this way. I feel confident of what I've been telling you. I'm much less confident on the subject of ozone because . . . but there are certain things about the ozone theories that I

would question. I don't question the chemistry. I think the definitive experiment was the NASA U2 you know, the Antarctic experiment.

BS: *Yeah. Susan Solomon, from here?*

JF: Well, the air ---onomy lab was running the science. I talked with the science director on that experiment in some detail and one of the fascinating things is, they flew out of Punta Arenas and made a lot of *in situ* measurements where the reaction was taking place. And this is in the Antarctic peninsula region. In this region, they found, indeed, they had the stratospheric clouds and they took *in situ* measurements and they found it a very highly layered structure. They found that these clouds are like 85-100 feet thick, extend over vast areas, and in between, you had clear air layers of 1000 or 500 feet thick, very laminated in that sense. In their measurements, they found that in the clear area parts, there is no ozone depletion. In the clouds, it's almost 100%, like somewhere between 95% and 100%.

BS: *All the ozone in those cloud layers.*

JF: In those layers . . . see the chemistry, the theory of the chemistry has been that the effective reaction has to occur on the surface of the droplet and this appears to be true because in the regions where clouds are present, the depletion is almost complete. As I say, it's between 95% and 100%. But, in the clear areas in between, it's very small. And that raises some interesting questions. One thing that it raises is that, what if the chlorin content were twice as great as it is now? You're not going to get twice as much depletion if you're already getting nearly 100%. You follow me?

BS: *Um-hum. I do.*

JF: Or vice versa. Suppose it were half as great. The same kind of question. The chemists have no answer to this. Now, where I have some questions about assessment theory is that the assessment theory is that the temperature has to be very low, below minus 80, roughly.

(50)

You have to have the stratospheric clouds present because the chemical reaction occurs only on the surface of the droplets. And thirdly, you have to have the catalyst which is normally assumed to be chlorin. I can be another halogen like bromine and so on, but chlorin is the dominant one. Those are the three conditions. Now, what all the chemists have assumed is that the meteorological conditions are a constant and that the variables are the amount of chlorin and the temperature which can reflect meteorological factors. But, in my estimation, I would question that assumption because as I mentioned before, if you look at the behavior of the system as reflected in the COADS data set, for example, the most variable feature is the strength of atmospheric circulation.

Now the stratospheric clouds are like the sea air wave and like the waves that occur over Boulder. They're orographically induced and that is why they occur mainly in the Weddell Sea region where you have westerly circulation and you have the extension of the Andes in the Antarctic peninsula and you have orographic stimulation of these stratospheric clouds. As the strength of the circulation changes, it's manifested throughout the system. You see it in the surface wind that I've been quoting about, but it also has got to be reflected in the strength of the upper level winds and the clouds. In the northern hemisphere, that is why Scandinavia has always been the home of lenticular clouds, for example.

BS: *I was going to ask you about lenticular clouds.*

JF: Well, you've got Greenland as the big orographic feature that's upwind and the clouds are well developed. The North Atlantic is pretty narrow at that point, but Norway and Sweden has been where, you know, stratospheric clouds have been studied and so on. Because that's where they occur and the reason they are so prominent there is because of the orography in the presence of Greenland. In Antarctica, it's the Antarctic peninsula which is the main feature. But, my point is, because they have no observing system that observes the time variability of this, they assume that that's a constant. Whereas, just looking at the dynamics, I would expect that that's the most dynamic factor. That that's a hell of a lot more variable than the amount of chlorin. As a matter of fact, the amount of chlorin in the atmosphere mainly from fluorocarbons in recent decades, in the decade from the mid-'70s to the mid-'80s, that was almost constant. But, the Antarctic ozone hole started about 1980, or '79 I guess was the first noticeable year. And the amount of chlorin didn't change during that period. And I would guess that the thing that changed, really, was the presence of the amount of stratospheric clouds. So that's one aspect that I'm very suspicious of.

BS: *But, didn't the volcanic reactions of Pinatumo and others also contribute to the chlorin?*

JF: Oh, I think yes.

BS: *And how much of the chlorin is natural and how much is man induced, percentage-wise?*

(100)

JF: I've read about this and I've asked this question and I think the argument that the chemists make is that man is responsible for almost all of the stratospheric chlorin, put it that way. Besides chlorin, there are certain oxides. NOX, can play the same role in ozone destruction. And I question this part a little bit too, because NOX is generated - after all, 78% of the atmosphere is nitrogen and the NOX products are developed by lightning, mainly. And so you would expect

the amount of NOX generation to be proportional again to convective activity because that's what generates lightning. This is totally ignored by the ozone community. Put it that way. So, it is just another question that I think needs to be sorted out.

BS: *Well, very interesting. Would you like to go to dinner? May I take you to dinner? I have no idea where to go.*

JF: Make it easy on yourself. You have a family and you're visiting.

BS: *Oh, I'm going to be here for another week and I've been here for a week already. My family doesn't expect me back until later.*

JF: All right. I can go out to dinner. As a matter of fact, one of those phone calls that we had was to invite me to dinner and I said, well no, I'm planning to go with a friend and they said bring your friend, but I didn't accept that. I thought it better to stay separate.

BS: *Well, we can do that, if you like, too.*

JF: Well, I wouldn't want to call back now.

BS: *I understand Well, this has been fascinating. It's certainly a lot broader than Polar stuff.*

JF: With Polar, before we go, let me finish by showing you a couple of these things.

BS: *I'm going to shut this off, OK?*

JF: Yeah.

(End of Tape 4 - Side B)

END OF INTERVIEW