American Meteorological Society
University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview of Joseph O. Fletcher
19 March 1991

Interviewer: William W. Kellogg

Kellogg: This is an interview of Joseph O. Fletcher being conducted by William W. Kellogg on March 19, 1991, in the Kellogg home. We will take it from there.

Joe, you have had so many different things in your life that it is hard to know where to start so I thought maybe the best thing would be to just simply start at the beginning. When I first knew you, we were together in the Air Weather Service.

Fletcher: And that was nearly a half a century ago.

Kellogg: Yes, it was a long time ago. We were working for the Signal Corps actually although we were Air Force officers.

Fletcher: Army Air Corps.

Kellogg: By that time maybe it was Army Air Force.

Fletcher: No, not then.

Kellogg: It was still Army Air Corps?

Fletcher: Oh, yes indeed.

Kellogg: That was going back a long time. Now, Joe, what were you doing with the Signal Corps as an Air Force officer?

Fletcher: That is a story that goes back a year or so before. We both went to flying school in that span of time. In my case, I had gotten there by a kind of circuitous route because I went to met.school at MIT in the class that began in 1940 and ended in 1941. That was a class that included Ken Spengler, Tom Malone and a few other oldies that I think are still around

but it was a full year course and one that was organized in anticipation of the war so it was a greatly expanded class.

Kellogg: You were a cadet at that time when you went through.

Fletcher: No, oddly enough I was on a fellowship that was offered by the Department of Commerce, as was Tom Malone. Tom came stopping out of South Dakota and I came out of Oklahoma. It was a special thing that was sponsored by the Department of Commerce for people who had taken the civilian pilot training and they were encouraging to take up meteorology. I spent that year there and then having taken ROTC I was called up first in the Horse Artillery because that was a kind of ROTC unit we had at Oklahoma, so I started at Fort Sill in the Horse Artillery but then they moved me to the Ordnance Department, and I spent the summer of 1941 at Aberdeen and we were supposed to be forming a maintenance company for heavy tanks but we never really got around…none of us knew anything about tanks so we were sort of using the time to prepare basics, but then I managed having gone to the meteorology course I managed to transfer to the Air Corps and suddenly I was the station weather officer at Montgomery, Alabama. I also applied right away for pilot training and so at the end of the summer I was accepted in pilot training and spent the next eight months or so going through the Air Corps training.

Kellogg: I have known you all these years and I have not heard that story about you in the artillery. When you were working with the Signal Corps labs, we were both at the Evan's Signal Lab. What was your particular interest and your duties at that time?

Fletcher: I will tell you how I got to the Signal Corps because that I think it is an interesting story. When I got out of pilot training, my first orders were to a P-38 fighter squadron in Australia. So I headed west but in those days you had to wait a couple of weeks at San Francisco before they had room to carry you on the airplane. While I was waiting I got called back. They changed my orders and sent me to Langley Field, Virginia where I was assigned as a pilot to the first sea search attack squadron. This turned out to be a very interesting assignment because it was the only squadron in the world that was equipped with microwave radar on airplanes. Because there were only ten airplanes, well we started with 12 and most of the time had ten airplanes, they were used only for special missions. At that time…

Kellogg: That was 1941?

Fletcher: That was the summer of 1942.

Kellogg: '42 by that time.

Fletcher: Yes. At that time the German submarines were completely dominating the east coast. They were sinking ships right in Chesapeake Bay. It is hard to remember all of that now. The whole east coast was strewn with sunken ships. The American ships were trying to stay close into shore to stay away from submarines and the submarines were coming in after them so a lot of them were only half submerged. So, it looked almost like a junkyard up and down the east coast. The upshot was that they mounted a kind of a crash effort of adding submarines, marshalling all the airplanes they could muster, but most of them were not equipped with anything more sophisticated than eyeballs.

They get a lot of sightings during the daytime patrols and then the most credible ones would be marked and the first sea search attack squadron would go out and fly a night search from about nine o'clock at night until three or four o'clock in the morning. We would go out and fly an expanding rectangle around the report because they had to come up at night to charge their batteries. They could only operate on battery and they had to run the diesels for which they had to be surfaced. So, they did that at night and that meant then that they presented a radar target during that time so our job was to fly with everything turned off except the microwave radar and do an expanding square around the reported location and try to locate them while they were charging batteries. In this process we got a lot of interesting things. One of the first things, of course, is that you find that microwave radar sees weather phenomena very nicely.

Kellogg: Those are very early microwave radars weren't they?

Fletcher: They were the first airborne microwave radars.

Kellogg: APQ 13 was it?

Fletcher: Oh no this was much earlier than that.

Kellogg: Much earlier than that?

Fletcher: When the production set came out I think it was called SCR 517, I think that was the designation, but at that time they had been hand installed by the Radiation Laboratory at MIT. That is kind of a side story because in the quarrel between the military, who had decided not to fool with microwave radar, had gone with 400 megacycles, which is about as high as they had used, and the National Defense Research Committee, NDRC, who had sponsored the MIT Radiation Lab. They had used the British invention of the cavity magnetron to develop a whole menagerie of

microwave radar sets. There was this rivalry between the NDRC and the Signal Corps

The first airborne sets were installed directly at East Boston and that was early in 1942 so they started out with 12 airplanes. We lost two and we never knew what happened, they just didn't come back. We figured that they may have made an attack run on a waterspout or something like that. So in order to try and investigate this I wanted to be able to take humidity measurements from the aircraft, for example, and we were being paid a lot of money like a $100 a month so I wanted to go down to the Radio Shack and buy some meters and things but you couldn't do that in those days. You may recall because everything was controlled you had to have a permit to go down and buy a galvanometer.

I went up to the Pentagon looking for somebody who could give me a permit and I ran into Ken Spengler. Ken said, "You're just the person we were looking for because there were some people in here yesterday from MIT that were looking for someone who had an electronics background, and who was a meteorologist and a pilot, and we weren't able to come up immediately with someone but we didn't think about you. How would you like to go back to MIT?" So I said, great I'd like to go and Ken took care of the rest and very quickly I was on my way back. I moved into my old apartment at the grad house and I spent a very busy next half-year or so working in the Radiation Lab in the propagation group. There I got equated with what microwave radar could do for a bunch of things; seeing weather phenomena, measuring upper air winds as the two most conspicuous.

Kellogg: By tracking a balloon?

Fletcher: That's right, by tracking radar reflectors that you could put on a balloon. So I was trying to interest some of my people, or the people who had sent me up there. This was the Air Weather Service, or what became the Air Weather Service, it had a different name. There was Lieutenant Colonel John Ackerman, who you probably know. Remember him?

Kellogg: Yes.

Fletcher: He was the chief of operations, I guess, for what was then the weather section or something in the Pentagon. He was the person I directly reported to and I was trying to interest them in using microwave radar because there were a lot of microwave radar sets already deployed around the world. Well he used to get his flying time by flying up on Saturday and chewing me out for not doing what I was supposed to be doing instead of fooling with the radar sets to see what they could do for making weather measurements. But among other things he steered Colonel Duffy

up to the Rad lab and Duffy came up and I took him around and gave him my arguments. I think he wasn't too impressed the first time but he came back again a second time and decided to really take some strong action to…

Kellogg: Is Colonel Duffy then the liaison officer to the Signal Corps?

Fletcher: Duffy was the…that's right, at that time the Weather Service several years earlier had been transferred from the Signal Corps to the Air Corps, but that did not include the development of meteorological equipment and supplies which remained a function of the Signal Corps. So that meant that the Weather Service per se was sort of half in the Air Corps and still partly in the Signal Corps and Duffy was the link that they had with the Signal Corps. So anyway, Duffy was getting one set of stories from his Signal Corps colleagues and he was getting a different story from me so he decided to have a test and he was able to exert enough influence to borrow one of the early fire control radars, the SCR-584, which was at that time called the XT-1. He brought me and the XT-1 down to Fort _____ and set it up alongside a triple theodolite system operated by the Signal Corps to find out what it could really do and that turned out to be such a resounding success for microwave for the XT-1.

Kellogg: In this case the application was tracking a balloon with a…

Fletcher: Wind finding, that's right. Duffy took me and went to the head of the Weather Service who was Colonel Senter, Oscar Senter, and he got fully behind it and said, well fine he would like to support the idea of using the microwave radar that was already spread around the world for fire control, for search light control, for early warning to weather purposes and would do whatever was needed in the way of support to implement such a system. So, Duffy immediately had me pulled out of the Rad lab and brought down to New Jersey to try to put together a program for doing that. Our approach, of course, was to use the sets that the Signal Corps had on hand in the back lot, to use each of them, find out what they could do and the best way of utilizing them and to write up a set of instructions and a set of manuals and train people who could go out in the field and put them to weather uses, which they were not being used for when they had been deployed for anti-aircraft and other purposes.

So that is the long answer to your question, what was I doing there and how did I get there. One anecdote I should say: the squadron that I got pulled out of at Langley with Ken Spengler's good offices was deployed only a month or two later to North Africa to cover the approaches to Gibraltar. Within six weeks we were wiped out so it probably saved my bacon in the process. You started out asking me what are the things that stand out most; it is my incredible good luck over the years through no

fault of my own, in simply having the good luck to not be in the wrong place at the wrong time.

Kellogg: Well, that was fortunate indeed. Just to set the record straight, also the Signal Corps was developing another wind-finding equipment, the SCR-658, which was an entirely different king of a thing. It was not a radar at all it was a direction finding system. Were you involved in that one to, because that, of course, was a lot cheaper than the big radar?

Fletcher: Well, you could look at it two ways. The answer to your first question: No, I was not involved in the development of the 658. That had been the Signal Corps approach to develop wind-finding system. In fact there was a period in which there was a fair amount of competition because I was advocating using existing radar equipment that was already deployed in the field in which case the only thing you need to add is the radar target, the balloon and the effort involved in organizing the use of the radar, where as the 658 was a complete system in itself and required all the things that go with it; batteries, transponder and that sort of thing. The upshot was that during the war years we actually utilized the radar approach because there just wasn't time to build and deploy 658's. The 658 came into use about the end of the war or shortly after, but during the war the upper winds that we got through overcast was really done on the makeshift bases. There were, at that time, some 500 fire control radars, for example, already in the operating theaters. So what we did was to train a bunch of second lieutenants and send them out with an armload of instruction manuals and a crate of reflectors and expect them to organize this service in an operational theater. It is really, I think in many cases, quite amazing. How successful they were able to do that.

Kellogg: It wasn't always possible, for example, to find the hydrogen to fill the balloons so that was another part of that effort wasn't it, to generate hydrogen in China for instance?

Fletcher: This is where Colonel Duffy and Colonel Senter, Oscar Senter, especially, came in, because they were so fully committed to being behind the program that through Oscar Center, as head of the Weather Service, they were able to send the right communications to at least give these audacious second lieutenants the best possible introduction in getting into the theatre.

Kellogg: It's a part of the World War II R & D effort that probably isn't very well known but it was brilliant, I think, and I remember being close enough to it to see how resourceful those people were that developed that system.

Fletcher: I think that is true and you are right, it is almost like a blackout period because everything was so classified there were no written records so later

after the war whenever radar meteorology began to develop but it appeared to be from scratch. Some of this has been covered in the recently published AMS book, **Radar In Meteorology.** Have you seen the book edited by Dave Atlas?

Kellogg: I know about it. I haven't actually read it, no.

Fletcher: It only came out last year, but they resurrected a number of people who had been involved in this including me and Colonel Duffy and Don Swingle and a number of those people from Spring Lake. So that is the only written record that I know of the early developments during the war years.

Kellogg: That was the book, I think, dedicated to Lou Battan wasn't it?

Fletcher: That's correct. Lou was one of the things that Colonel Center agreed to that we had proposed to him very early was to build a cadre of trained people by sending ten new weather officers a month for ten months, a total of 100 of them, through the radar school at Harvard and MIT in order to provide a highly qualified cadre for the future. Lou Battan was one of those, Dave Atlas was one of those…

Kellogg: Was Roscoe Braham one of them?

Fletcher: Roscal Braham was one of those. Who is your chap at NCAR now who has been developing radius?

Kellogg: Vin Lally was one of those; I mean the list goes on and on.

Fletcher: Yes, well that was quite an effort then. Obviously a great legacy was passed on and we are still working on it here in Boulder we have the Wave Propagation Lab which is observing the atmosphere in three dimensions to a whole new level but those early views of the atmosphere in three dimensions were real eye openers. It was the first time we had a network which could get a three dimensional picture of the atmosphere.

Kellogg: Well that's true.

Fletcher: Well, Joe, let's push on with you. After the war you went to UCLA?

Kellogg: Yes, at the end of the war, I first was detailed; well in the latter part of the war I guess one of the more interesting episodes in my experience was that while at Spring Lake when we got the system going, that I have just described, in which we sent people to all the war theatres around the world to organize with the regional weather officers in those theatres a system

for utilizing in place radar, for both weather detection and especially for upper winds.

At that same time the need for weather reconnaissance was developing. The first weather reconnaissance squadron, which was B-17's commanded by Karl Rock, was formed in Panama. That one was sort of done by boot strap you might say; that is the design of a aircraft configuration for weather reconnaissance was not done ahead of time, they sort of put the instruments on one at a time, but it was a very successful squadron and operated through the war years. Then there was a demand for a lot of other weather reconnaissance squadrons. The second weather reconnaissance squadron was B-25's, as was the third weather reconnaissance squadron. The second squadron was formed by Brad Baker. Do you remember him?

Kellogg: Yes.

Fletcher: They formed up at Meridian, Mississippi, but at any rate, I was answering to Colonel Duffy and I was detailed to put together a design of the aircraft configuration.

Kellogg: That is the instruments that had to go on a reconnaissance aircraft?

Fletcher: That is correct, including radar.

Kellogg: And including the recording of this.

Fletcher: Yes, information processing, data processing and the rest. Now that is where the APQ-13 comes in because in that case what we did was to take the APQ-13 which was the most advanced microwave radar, 3 cm radar, it had been developed for use as the bombing radar on the B-29 and it was in a retractable dome. What we did is to stick it in a fixed dome on; you'll remember a B-25 had kind of a high fuselage in the rear, and a lot of other things. We loaded it up with so much specialized gear that we had no weight left for guns. They didn't even have a machine gun, which turned out to be a very good thing, I think.

Brad Baker took that first squadron from Meridian, Mississippi to Burma, which was their first operation. Then we did a second squadron that also went to the China-Burma Theatre. Then came two squadrons of B-24's and in that case you will recall a B-24 had two bomb bays and what we did is close off one whole bomb bay and make an operations room out of it and again we used the APQ-13 and put it in a retractable dome in the B-24. So, it had modern radar, and I think it is fair to say that those weather reconnaissance airplanes; the B-25 squadrons, the B-24 squadrons, had

better radar capabilities than our most modern weather reconnaissance airplanes today.

Kellogg: You have been recorded on that statement.

Fletcher: I think that is true. It is just that the APQ-13 was a good and advanced set. The radar that is used…that of course does not include the P-3's which have special radar, but the weather reconnaissance C-130's, for example, have really only the navigational radars, which are not nearly as capable as the APQ-13.

Kellogg: Yes.

Fletcher: And that's what I mean. So they were quite advanced for their day. That first B-24 squadron was formed by Nick Shevas at Oklahoma City. So, I was working very closely with these people since I was responsible for the technical configuration and there are a lot of anecdotal stories about those squadrons and getting them equipped and off to their missions. After the two B-24 squadrons there were two P-61 squadrons. That was the Northrop night fighter but it had a nice pod in the middle and it had radar, which came with it, which was for the night fighting mission but was also very good for weather detection. So, in that case we could actually keep the same radar in the airplane and equip it for the other missions.

We even had one squadron, a P-47, that was to be deployed to Iwo Jima, and so it was a single seater but the idea was that the P-47 had enough range to get to Tokyo for target reconnaissance. It never got deployed because the war ended before it got out there. The two P-61 squadrons never got deployed to the Philippines but they were fully equipped and trained at the time the war ended. Then the airplanes went into storage but we pulled them out again for the Thunderstorm Project in Florida.

Kellogg: Oh, yes. That is where the P-61's came from.

Fletcher: At the end of war, I was detailed to the All Weather Flying Division out in Ohio and one of the things that formed there was the Thunderstorm Project, Horace Byers and others at Chicago and Harry Wexler were pushing. So, we all collaborated in trying to put together a plan for doing this and part of the plan was to pull these P-61's out of storage down in Texas and use them as the aircraft fleet for that. It turned out that they were really ideal for the use.

Kellogg: They were pretty high performance aircrafts, of course.

Fletcher: Oh, it was a high performance airplane, and as I say, they had excellent radar and they were already equipped for weather measurements and there was really not too much more that you needed to do.

Kellogg: Well, Joe, we brought you up to the end of the war and the thunderstorm project.

Fletcher: I guess I skipped the part…I was only in New Jersey for about a year. Duffy brought me out of the Rad Lab and we did the activities I described but then my involvement in the weather reconnaissance squadrons led me to being transferred out to the Pacific. So, I ended up in the Marianas…

Kellogg: Really.

Fletcher: …helping Nick Shevas get his 53rd squadron deployed there, and by the way…

Kellogg: That was one of the B-24's?

Fletcher: Nick had B-24's. But that squadron was there until about two years ago.

Kellogg: Really.

Fletcher: It was the longest lasting weather reconnaissance squadron. They went through B-24's, B-29's, B-50's and C-130's and back in the 70's I stopped at Guam and went out and found the squadron and paid them a visit. Only last year they were brought back to Keesler Field, Mississippi and turned over to the reserves.

Kellogg: Where they do the hurricane reconnaissance still?

Fletcher: That's right. They are doing hurricane reconnaissance operated by the reserve. In the meanwhile while I was in the Marianas then I had the job of well…General Lemay was just taking over command there from General Hansel and he was reorganizing everything. He pulled half of the wings out of combat and put them in a training mode in the Marianas because he claimed that their bomb accuracy wasn't good enough. He put on a lot of pressure to improve the Weather Services and they were running into the jet stream. If they were going upwind into a 200 mph wind, they were moving awful slow, and if they were going downwind they were going over the target so fast they didn't see it. So, there was a lot of emphasis on target reconnaissance.

Kellogg: Sometimes it has been said that that was the discovery of the jet stream.

Fletcher: I think it was the first recognition, really, of what was going on. So anyway, part of the job I had out there was to try to organize an interim reconnaissance system and the only aircraft that could reach Japan were the B-29's so we organized a system where we had a shuttle flight. We would pull…we had a sort of improvised set of weather reconnaissance observations to be made from the operational aircraft and the aircraft would be pulled from the regular bomb units and we would send the mission every seven hours around the clock so that the appearance of a target reconnaissance would not signal a coming strike. At any rate, that system then operated for the rest of the time that the B-29's operated.

Kellogg: What period are we now? Are we right up to the end of the hostilities with Japan?

Fletcher: No, it wasn't. I was there the second half of 1944. But then I got pulled back again to…well actually I got pulled back first to help Colonel Duffy because they were already planning what would happen after the war and among the things that Duffy had the foresight to sponsor was to sit down and try to write the requirement specs for weather radar sets that could be developed for weather purposes. Everything that went on during the war was simply adapting things that had been built for other purposes. So that is when the specs were written for weather detection, which turned out to be the military CPS-9, which was when adopted by the weather bureau, became the WSR-57, and that was the first generation radar, NexRad. I mean that is what we are getting rid of now, it was being replaced by NexRad. So NexRad is second generation.

Kellogg: And the…

Fletcher: And this is 40 years later.

Kellogg: I was going to say about 40 years. That is a long time for a system to continue to operate.

Fletcher: Of course in addition to the detection radar there were 3 or 4 other radar sets; cloud detector, which looked only in the vertical and some wind finding equipment. Those are described in the Lou Battan memorial volume by some of the people who stayed with the Signal Corps and were deeply involved in developing these things in the 40's and 50's.

Kellogg: That was a period of great development and suggests that if you were to draw a curve of the rate of increase of instrumentation to observe the atmosphere in three dimensions that was definitely the fastest period of development.

Fletcher: That was a step function.

Kellogg: Yes, it really was.

Fletcher: Oh yes. And as I say much of where we are today really did develop rather explosively during the war years. As I say, the NexRad we think of is the next generation radar but you don't always stop to think if this is the second-generation radar of radar design for that purpose.

Kellogg: As we go along through the later part of your career, Joe, we might come back to this subject and think a little bit of what is happening in your lab right there at the Wave Propagation Lab in looking at it from the point of view of a somewhat outsider I feel this is another step function almost where the indirect remote probing of the atmosphere is allowing us to see the atmosphere in three dimensions without having to send a balloon up.

Fletcher: That's true. Interestingly enough if you read Don Swingle's chapter in the Lou Battan volume, he recounts the next 30 years of development at the Signal Laboratories and among the things that he recounts is that, for example, acoustic temperature measurements, which we are now, we think, bringing to fruition as an operational system. We are not only much discussed at Spring Lake, and some rudimentary experiments were tried, but some of these weather officers, or radar weather officers that were trained at Harvard and MIT in the immediate post war years, tried to develop these techniques and it didn't lead very far because the technology just wasn't there to support it.

I have asked Steve Clifford, who now heads our Wave Propagation laboratory, what was the key in making it really feasible to the generation we are looking forward to now. One of the keys, there are two or three, but one of them is simply the computer, I mean it is the signal processing of being able to separate signal from noise, which was just wholly beyond the technological capability at that time, which is an essential part of the systems that we are developing now. So, some of these things simply have to wait until the technology is there to make practical.

Kellogg: These are enormously complex systems that we have now. Well, Joe, have we reached the end of the war yet.

Fletcher: Yes.

Kellogg: Can you go back to UCLA?

Fletcher: Yes, at the end of the war, I spent so much time in the states; I was at the Air Weather Flying Division that I didn't have enough points to get out. You remember that you got so many points for overseas time and so many for stateside time. I happen to run into Colonel Moorman in the elevator

and he said, "what are you going to do?" and I said well I am going to get out as soon as I get enough points and he said, "well what are you going to do then?" and I said I am going back to graduate school and he said, "all right, go." That was before we had any regular program for sending people back and he said, "we'll think of something to call you." So I gathered up Ed Isvan and Judd Tibbits and George Austin and all four of us went out to UCLA and enrolled.

Kellogg: I've got to tell you, Colonel Moorman called me into his office, I have to be down there at the Air Weather Service Headquarters, it was in Asheville in those days. He called me into his office and said, "I understand you are planning to get out. You know if you stayed in we could send you back to graduate school."

Fletcher: That's right. About a year, well less than a year, about a half a year later they organized a regular program for doing it and a lot of other people started coming in under that program.

Kellogg: So, you were about a year at UCLA then?

Fletcher: A year and a half. I was there three semesters.

Kellogg: Of course I was there too.

Fletcher: Yes, you were there.

Kellogg: I was a teaching assistant on $90.00 a month. Wondering why you guys were in and I was out.

Fletcher: But we were both having fun.

Kellogg: Oh yes, it was a great time and of course it was a great department. I gained a great affection for Jac Bjerknes.

Fletcher: So did I. I gained a great affection for him then but even more so many years later when I came back to Rand and got better acquainted with him.

Kellogg: After UCLA, what then Joe?

Fletcher: Well let's see. One reason I interrupted my time there was that Colonel Duffy came out and more or less recruited me back to the Wright Field. At that time he was at Wright Field in what later became the R and D Command but it was then called the Engineering Division of the Air Material Command. At any rate he brought me back there as part of his staff where I had the job that was called Chief of the Research Plans

branch. We were involved in a number of things. One was setting up the Cambridge Laboratories, the Geophysical Laboratories.

Kellogg: Oh yes, the old GRD.

Fletcher: Another project was the computer project in which von Neumann and the _____ development. I should say that one of the most visionary people, I think in my experience, was a person whose name is not very much heard in the history books and that is the Signal Corps Colonel Carl Mayer who was the head of the Signal Corps labs in New Jersey. So, he was the Signal Corps counterpart that Duffy worked with there and whenever…

Kellogg: This is the interview with Joe Fletcher and we were just talking about Colonel Mayer who was the Signal Corps…

Fletcher: Colonel Mayer was the Signal Corps colonel who was deputy head and then head of the Signal Laboratories and at the end of the war he transferred to the Air Corps and came back to Wright Field as director of plans. Plans included a lot of things and a real visionary in the role of head of plans can put a lot of wheels in motion. He was just the person to do that including the supersonic wind tunnel, the development of sponsoring of computers, which Air Force did in a big way, setting up the Geophysical Labs by pulling together elements of the Radiation Lab and some other elements and a whole CIRES of things which in later decades I have been impressed more and more by the vision that led to the initiative to do that. In many such cases he got crossways with his superiors, most of whom had their roots, they were pilots, in the Air Corps and in the upshot he resigned from that role which is one reason we don't hear his name very much and took a job as vice president of Fullman Company in charge of research and development but he left the seen from that Wright Field role. I am often reminded, even here four decades later, of a lot of things that are going on today which I recognize that he really sewed the seeds of.

Kellogg: We're talking about Colonel Marcellus Duffy.

Fletcher: And Mark Duffy answered to him at Wright Field.

Kellogg: Colonel Mayor he was talking about then.

Fletcher: I was talking about Colonel Mayor, yes.

Kellogg: Colonel Mayor, ok. I remember him too.

Fletcher: One of the things was they teamed up with allies in the Navy and the Weather Bureau to sponsor computer development. We spent six months arguing about whether to go analog or digital which seems ridiculous in retrospect but at the time it seemed like a very reasonable argument. They formed the liaison with von Karman to kind of oversee the development which led to the _____ and at the same time through mainly Harry Wexler, who had gotten out of the Air Corps and taken a job as chief of research for the Weather Bureau, mainly through Harry's initiative put together the group that formed under von Neumann at Princeton.

Kellogg: Jule Charney, Phil Thompson.

Fletcher: Jule Charney, Phil Thompson, Norm Phillips. Who is our Norwegian friend?

Kellogg: Eliassen was it?

Fletcher: Yes.

Kellogg: That was a brilliant group. All of them went on to do further work in that field. Amazing group. So that was Colonel Mayer, then Colonel Duffy were part of the Air Force group that pushed that.

Fletcher: They were still Air Corps in those days but yes.

Kellogg: Still Air Corps. I think it was Army Air Force by then wasn't it?

Fletcher: I think the Army Air Force must have formed. I don't know I guess it was about 1947.

Kellogg: Toward the end of World War II or shortly after it became the Army Air Force. Then, of course, it became the Air Force. Well anyway, so that was an exciting time. Then what happened to Joe Fletcher?

Fletcher: From Wright Field I went to Cambridge where I was director of the GRD, the Geophysical Research Directory. I think you had your reserve assignment with GRD and I used to see you.

Kellogg: Not only did I have my reserve assignment back there, but Rand would send me to sort of keep track of things and had a big travel budget so I would get back as a Rand employee to GRD. That was where the action was. It was a very interesting group. Under your leadership it grew very fast didn't it?

Fletcher: That's not due to any credit of mine. I would say that under Colonel Mayer's sponsorship he managed to get the funding and the people that allowed it to grow.

Kellogg: Are we talking about 1947?

Fletcher: Yes, that was 1948-49. In 1949 again having spent a lot of time in the U.S. I was still short of overseas time. There was a lot of pressure in the Air Force to back to the cockpit. At any rate I was reassigned to Alaska as C.O. of weather reconnaissance, B-29 weather reconnaissance squadron, replacing Karl Rock who had taken the squadron up to Alaska.

Kellogg: That was based in Isleson was it?

Fletcher: That was based at Isleson which is…well it was based really at Lad Field which was at Fairbanks and then about the time I moved up there we moved from Lad to Isleson which is 26 miles out. Lad Field had a runway that abutted the river in both ends so it couldn't be extended. It wasn't long enough for the B-29's. Meanwhile they had built at that time, I guess, was the longest runway in the world out in the boondocks in what was to become Isleson. But they had 16,000 feet of runway and nothing else actually. There was a small holding attachment of less than 200 people out there.

The Air Force by then wanted to develop Isleson into a major base and in order to do that…well two things. One, they wanted to get the B-29's out of Lad because it wasn't big enough for them because the runway wasn't long enough and secondly they wanted to get an operational organization at Isleson so they could develop it. I moved in on top into Isleson with 12 airplanes and 600 people and there was really nothing there hardly except they had about 200 people who were operating in minimum facilities. When I left there, a little more than two years later, we had 5,000 people because in the meanwhile the Korean War had started, money had started to flow, they were able to build a lot of buildings and they brought in a bunch of Army units to "defend" the place.

Kellogg: It was important also, strategically, to have a forward base from which bombers could fly.

Fletcher: That's true. It became a refueling stop for the Strategic Air Command.

Kellogg: What was it like in those early days flying weather reconnaissance in the Arctic?

Fletcher: It was an experience I will never forget. The first winter especially because we moved into very austere facilities. We had one small hanger which could house airplane but since we were required to fly daily missions at a set takeoff time that hanger had to be used to preheat the airplane or else, at least with that equipment, it was impossible to meet scheduled takeoff times at 40 to 50 below zero. That meant in order to do maintenance it had to be done either outside in the cold or we did have four canvas nose docks that you could pull the front of the airplane into and have some heated space to work on them. So, the first winter especially turned out to be a nightmare of maintenance problems because most of the people were just rotating. The people that Karl Rock had brought up from California, their tours were up and they were all going home.

Kellogg: They might have been happy to go home certainly in the wintertime.

Fletcher: They were happy to go home and to compound things the Korean War began in 1950 and this meant that there was a big influx of people so there was a lot of untrained or minimally trained people spread over a much larger Air Force effort. It was a real struggle.

Kellogg: The story of how your aircraft spotted that big ice island, of course it has been told many times, but I would like to hear it again anyway because it is an important part of your story.

Fletcher: Let me say something first about the aircraft. When I was flying from Guam in 1944, there was at that time the maintenance officer for the 20th Air Force out there was a Colonel Irvine, who later became three or four star and head of the Air Material Command. At any rate, I do remember vividly that the average engine time for a while there was 25 hours installed on the airplane. When you got four engines on the airplane and you've got 13 hours to get to Japan and back so that's 4 X 13 which is 52 hours of engine time just for one mission. With an average life of 25 hours you can figure your odds of getting back on two engines or three or one.

Kellogg: Those were very good engines.

Fletcher: I am just illustrating. In fact most of the losses were simply going down in the ocean. There wasn't much loss from enemy action over Japan but there were quite a few losses simply from losing engines.

When I was in Alaska flying B-29's, when we finally got the average lifetime up to 400 hours, we had a big party that lasted two days and we invited the commanding general come up and join in, which he did, which was General Kepner by the way at that time. It was a big event. When I stopped at Guam many years later and visited the squadron they were flying C-130's and I went out and looked at the _____. The average time on the installed engines was 12,000 hours. The high time engine, I think, was 18,000 hours.

Kellogg: What accounted for the enormous extension of the lifetime of an engine that you were able to achieve there in Alaska? What accounted for it?

Fletcher: The B-29 was developed in kind of a hurry and that engine just had a lot of weaknesses especially in the lubrication system. This was the R-3350 engine and it was a troublesome engine. They were in a hurry to deploy the B-29's first to India and then to the Marianas. It just took a long time for them to work the bugs out of it. In fact when I first went to Alaska they were redesigning the engines to try to make it better and because our reconnaissance squadron had a very high priority in the

bureaucratic mode, we were the first to get the new engines. It happened to be in the wintertime.

Nevertheless we changed 50 engines in the middle of winter and then when we started flying the new engines they started blowing up. I lost 12 engines in the first 100 hours of flying as a new squadron commander. At any rate, we didn't know what the problems were so I grounded the squadron and that got the attention of Wright Field and all of the sudden I had people swarming all over me up there. It turned out that among the improvements that they had made there actually was a mistake made in the factory. They had put in floating valve seats, changed the valve overlap and a bunch of things, which increased the pressure inside the cylinder greatly and allowed you to get more power because you needed more power. The airplane was underpowered for the uses that it was in. Well, at that very time they had made a mistake apparently in assembling the engines in the factory and they left a little bit too much tolerance between the wrist pin and the connecting rod to the cylinder. The upshot was that the bearings were failing because of the excessive tolerance and once you had a failed bearing it wiped out the entire engine; all of them. The whole row went out. We ended up changing 50 engines to the new improved ones and when they turned out to be defective we changed 50 more back to something that we could use. That's an unforgettable experience for, I think, all of us who were involved at the time.

Kellogg: But you finally did get up to 400 hours per engine, which was really remarkable.

Fletcher: Yes, and it was just done with careful attention to maintenance practice.

Kellogg: Did you have a very good maintenance man or some good mechanics there who really understood that engine very well?

Fletcher: The head of my engineering section was a first lieutenant, named Klemper and he was excellent, yes. Mainly it was just hard work on the part of the people.

Kellogg: These are some of the stories and some of the parts of the account that you don't too often hear about, the problems that you had flying that early Arctic reconnaissance.

Fletcher: Actually our mission was half on the Arctic side and half between Japan and Alaska. We flew 50 miles off the coast between Japan and Alaska on a shuttle and then would come back on a separate mission two days later on alternate days off the coast of Northern Siberia and then a dogleg up to the pole.

Kellogg: One of them was called Ptarmigan flight?

Fletcher: That was the Arctic side and the other was called a Loon flight.

Kellogg: Loon flight _____ Japan.

Fletcher: Actually there were a couple versions of each.

Kellogg: Now, what about the Ptarmigan flights over the ocean? At what point did you see this precarious feature in the ice and decide to go and look at it more carefully?

Fletcher: Well actually, this curious feature in ice had been detected a couple of years earlier but the information was kept classified and the piece of ice that had been originally detected had drifted off of the track. What we did was to institute some special procedures to try to really make an effort to spot something of this sort if we came across another one. So, T-3, in fact we came across T-2, which we called Target 2, as a second such object and then T-3 as the third such object. T-3 turned out to be the one that we eventually put a station on. It turns out that the kind of radar return is really quite different then what you get from sea ice so once you really recognize the ways in which it is different and have people alert to watch for those characteristics it was a pretty straight forward thing before it started to show up.

Kellogg: As I recall you stayed on after you were relieved as commander of the reconnaissance squadron so you could lead this expedition out as T-3 is that right?

Fletcher: Yes, we had tried to convince the theater air commander who was General Old and the folks back in the Pentagon too who had approved to give us permission to put a drifting station out on the ice and there had been a lot of discussion of putting such a station on sea ice but sea ice is such a precarious platform that it would have to be a temporary kind of thing. So, what we were trying to sell was the idea of establishing a station on an ice island where it would be secure from the elements and it would have enough permanence so that you could invest in the kind of things that you need permanence for and you could have a secure air field and place to get in and out of at least during the season when it's not melting.

Kellogg: They've actually had laboratories in comfortable housing and so forth on it.

Fletcher: At the time I finished my tour as squadron commander General Old got behind it and agreed to support it and he sent me back to the Pentagon and it turned out that General Yates, who was by that time head of the Weather

Service, and General Senter, who had become a brigadier. They both were supportive people that I had worked with in former years and Burt Balkan, who had been commander of the rescue squadron in Alaska had been a close colleague until he had gone back to the Pentagon, was also supportive. So, there are enough people who were supportive so that they gave us a green light and said go ahead and do it. There were a lot of people…

Kellogg: I think did it require a bit of salesmanship on your part?

Fletcher: It required a lot of salesmanship because there were also a lot of people who were predicting disaster and who were very much opposed to doing. But nevertheless we did get permission to go ahead. It was kind of a window of opportunity because the people who were saying yes were also transient and they might not be there six months later. In any case, I wouldn't have been there six months later. So, you were confined by those kind of factors and you're also confined by the annual cycle of weather because my assessment was that…first of all starting in May but through June until it gets dark in the fall you've got maybe 80% low cloud cover and through half of that period you've got melting at the surface so because of the cloud cover and because of the surface conditions that period is out of the question for conducting such an operation.

Kellogg: Too wet on the surface.

Fletcher: Especially low visibility. That continues right into the dark period. Then things freeze up and by lets say November or even October, you are frozen again at the surface but the weather is bad and it's dark so if you want to wait until you have some light you have to wait until March. That's the coldest period of the year. But then from the time it gets light until about the end of April, you have a period of relatively good seeing conditions and it is gradually getting warmer but you are getting more and more cloudiness. It was my assessment that we learned how to deal with the cold. The really controlling factor is predictable clear weather so the approach was to go in as soon as there was enough light to be able to see what you were doing which means about early March or the middle of March; in fact we went in the 15th of March.

At that time the ice island was close to about 120 miles from the pole and that's pretty far from Alaska so in order to get there we had to deploy through Greenland and Elsmere. Feeling the constraint of time you might say do it now or it will be never. You didn't really have the option to delay because of these physical reasons that I described. It was put together in kind of a hurry. It so happened that the England Field was doing a cold weather test of a C-124 at the Cold Weather Test Center at Lad Field and I got quite friendly with the test crew and they very kindly

agreed to use the C-124 to haul all our supplies over to Thule. So I got two flights out of them to haul all of the stuff that we wanted to get out there from Fairbanks to Thule and that was absolutely essential.

Kellogg: Mostly fuel.

Fletcher: Oh no, we could get fuel at Thule. But this was the other kind of equipment that we wanted to get out. We could get all of the fuel we needed there at Thule. But then there was the problem of getting out to the location.

Kellogg: What kind of aircraft were you going to use to get up there?

Fletcher: All we had was ski equipped C-47 and C-54 without skis. So, you could use them in combination. The C-54 had a lot more endurance and range, of course. The old DC-3 or C-47 "Goony Bird" was pretty limited in what it could carry or how far you could go. That's why from Alaska it was about 1,000 miles out there. From Thule you were a lot closer, it was more like say 700, maybe 650. But that was still too far for the C-47.

My plan was to stage an alert, which was a joint weather bureau Canadian weather station and they had a strip and they had some drum fuel if you needed it. It had been our routine practice to land as part of our survival training to land at the Canadian stations. The one thing I bureaucratic _____ that I had overlooked was that I had taken for granted the potential use of Canadian strips, actually they were joint U.S. Canadian but they were in Canada, and I had done the sales job back in Washington but I hadn't taken the trouble of doing a sales job in Ottawa, which in retrospect I realized was a mistake. The upshot was that when I had my stuff all over at Thule and meanwhile we had made the request, which is normally taken as a proforma notification, that we were going to land and take off at alert with the C-47. Back came the answer from the Canadian government saying no, we recommend that you postpone for a year, that you undertake coordination discussions with the Canadian technical agencies and a bunch of things like that. My point is that since it was now or never this simply wasn't feasible to do. So we had a very tense couple of days, or a few days at Thule.

It turned out that the liaison officer for the Danish government was there as well and they were in discussion with the Canadians because the nature of the operation had attracted the attention, by that time, of the Canadian government and the Danish government and as bureaucracies normally operate they wanted to be part of the plan and participation and all of that which is fine, we would have been pleased to do that except it just wasn't feasible because we had this narrow window of opportunity in which to do

whatever we were going to do. The dilemma is what to do in that circumstance.

What I did was set up a cash of fuel on the sea ice on a re-frozen lead off the northern tip of Greenland almost half on the dividing line between Greenland and Elsmere but just barely within visual sight of the northern tip of Elsmere and the northern tip of Greenland so that we could actually triangulate visually on landmarks to identify the location very accurately. Because of the time of year we could depend very much on clear weather. We set up a fuel cache flying out of Thule on the pack ice and you see this is under nobody's jurisdiction. It is not a Canadian jurisdiction and it's not Danish jurisdiction and if they don't know where it is they can't even claim it is. So we didn't tell anybody where it was either. Then in order to get enough range from the C-47 we landed and refueled on the pack ice in order to get out to the ice island.

Meanwhile we had sent the C-54 out actually 2 1/2 hours earlier because they had a radar. The C-47 had no radar at all. The C-54 then could fly search pattern, locate the ice island and give us a homing signal so that we could go directly to it and not waste any fuel that way. That would give us a chance then to land and then if things were feasible the C-54 could drop enough support equipment so we would have enough to get a foothold. That was the plan.

Kellogg: The C-54 could fly from Thule and back without landing.

Fletcher: They had a enough fuel to go out and fly, to do a search pattern, to linger while we landed, to drop a few things and then go back.

Kellogg: So that was the first time you set foot on T-3. Did you go back then or was that it, did you stay out there for the summer?

Fletcher: No. You see there are a lot of things we didn't know first. Could you land a wheeled airplane, for example, and if you could the idea was to use a C-54 to haul stuff in. To cover the contingency, I had rounded up as many parachutes as an alternate as possible from Lad Field and containers. The paramount consideration is safety and the best way to improve your safety chances is to keep the actual operations to the minimum required.

The idea was first to get out there and make a landing and then give us time to _____ and assess the situation. So, for the first two weeks it was just me and three other people; our radio operator, Dr. Rodall and Captain Breneger who was an expert in surface operations. To give us time to scout around on skis and decide whether it would be feasible to have wheel landings. It was pretty obvious that wheel landings operations were not feasible that we would really have to start with an air drop.

For the first two weeks there was nothing to keep them away. The only contact with the mainland was that my old B-29 Ptarmigan crews would come over every second or third day and we could communicate with a walkie-talkie and I could send messages back. So then that meant revising the plan to adjust to the physical circumstances. But then things were complicated because then the second landing we brought in the short-wave radio and a power supply so that we could have continuous communications if we wanted. It turned out that the continuous communications were very good. Although we had only a 1,000-watt transmitter we had 200 feet high dial electric ice, fresh water ice, between us and saltwater ocean so that it was like having an antenna array 300 feet in the air. So we strung out an enormously long _____ antenna on the surface which gave a very narrow but highly concentrated beam. The upshot was that I could communicate perfectly with Alaska, with Washington D.C. and with places, if they were pointed in the right direction, really all over the world with a very low power.

The biggest complication was that at about this time the Navy who had an oceanographic expedition, using ski equipped P-2V aircraft, paid us a social visit. They had a winter operation that spring working out of Point Barrel. The upshot was that they lost an engine on takeoff.

Kellogg: Going back from T-3?

Fletcher: Yes, it was about 40 below or so. They stayed longer. The P-2V has the same R-3350 engine. It is a different model but it is the same basic engine as the B-29's. That engine just can't stand sitting in cold soak very long. We had learned from experience in Alaska what you can and can't do with that engine and one thing is that you can't let it sit at 40 below for four hours and then start it up again and expect it to function well. Sure enough they lost an engine. Then I had an airplane with about 30 people stranded out there.

Kellogg: Thirty people on this P-2V.

Fletcher: It turned out that, how do you get a new engine in there to change the engine because the engine is too big and heavy for a C-47 to carry. You couldn't bring it in with a C-47.

This led eventually to a C-54 wheel landing. We were able to clear a strip, a very short strip, too short.

Kellogg: By hand?

Fletcher: It turned out that we hauled a new engine for the P-2V and with the C-54. No, not by hand. We could carry a very small tractor in the C-47 so we hauled a little tractor in. The C-54 actually landed on the hard snow but as I expected it broke through. It didn't wipe out the gear but it was mired there and no possibility of getting it off. This meant that we had not one but two airplanes stranded.

Kellogg: The P-2V and the C-54.

Fletcher: So, to get the C-54 out we hauled a small tractor out in the C-47 and cleared a narrow strip just long enough so that the C-54 with minimum fuel, as light as possible, had a chance of getting off. This was piloted by Lieutenant Colonel Jack Streeton, who was the commander of the transport squadron in the Alaska. They all took it as good sports and Jack managed just to get it off but he left a couple of creases in the wheels at the end of the runway that hadn't been cleared. So, the cleared area was just barely what he could do. So, there were a few unexpected developments in the plan.

Kellogg: So then they finally got the P-2V out.

Fletcher: Yes, we changed the engine on the P-2V and he got off but of course he had skis. He didn't need a hard runway.

Kellogg: You were, of course, on the island all that time yourself.

Fletcher: Yes, I was there the first three months and that's all. I was never able to visit there again.

Kellogg: You never got back?

Fletcher: Never got back.

Kellogg: I was there several years later, of course by that time it was pretty well established. It was Fletcher's Island by that time it was called was very close to Point Barrow at that point so it didn't take but just a short hop from Point Barrow out to T-3 Fletcher's Island.

Fletcher: It ended up making several big circles around the Arctic basin. It was first occupied in March of 1952 and I guess it was about 1985 it finally came…that is 33 years…anyway it finally came down the east coast of Greenland and rounded the southern tip and back up into _____ Bay and broke into pieces.

Kellogg: I gather somehow it was traced all that way.

Fletcher: Almost. It was an actual operation as a research base for about 25 years but there were a number of years in which it was not occupied.

Kellogg: It was grounded for a while wasn't it?

Fletcher: That's right. There was a time in which it wasn't moving hardly at all over near Greenland. At the time of the IGY we actually had to mount a search to find it again because we had lost track of it at that time and we wanted to use it as one of the IGY drifting stations.

Kellogg: Joe, there is another aspect of your Arctic experience and that is your close contact and friendship with some of the Russian scientists, I am thinking particularly of Boris Sankoff and the man who is head of Leningrad Polarists was it?

Fletcher: Tereschnikoff.

Kellogg: Tereschnikoff, yes. They, of course, landed and set up camps on the ice itself did they not? On the very furthest north pole stations?

Fletcher: That's right. They had done, that is the Russian's, dome extraordinary pioneering work in the late 30's and early 40's. In 1937 they had established a drifting station on ice they called North Pole I.

Kellogg: 1937?

Fletcher: 1937. It was occupied by a four-man team as it drifted across the Arctic and down the east side of Greenland where they were picked up by an ice breaker nine months later. They were on the Asian side and the drift is sort of straight across there. The team leader was a guy named…(tape end).

The second Russian activity which impressed my very much was their development of landing and taking off from the pack ice. In 1941 they conducted an extraordinary expedition. The number of the airplane was N-169 and the chief pilot was a guy named Chairdevichny. In the spring of 1941 he did an extraordinary CIRES of landings and takeoffs at different places off of pack ice using mostly wheels, using mostly _____ and leads. At that time with the equipment that they had available, I think this was really a remarkable feat. They returned…they didn't come immediately back, in June of 1941, which you will recall was the month that Hitler attacked Russia. So, ordinarily they would have had a national celebration over the returning expedition but there was no time for that and they were fighting for their lives.

Then there was a hiatus. They, of course, had no energies or attention to devote to that kind of thing until many years later. But in 1951, or maybe it was 1950…no it was 1951, they deployed another drifting station mostly northwest of Barrow, which was called North Pole II but it was a secret operation. They didn't inform anyone. It turned out quite by chance one of my Ptarmigan crews flew over in the dark, it was during the dark part of the year, but they looked down out over the Arctic ocean several hundred miles out and low and behold they though they saw runway lights. They were at 18,000 feet, 500 milibars, and the aircraft commander decided…first of all he sent what is called a service message that is the one that gets everybody out of bed in the chain of command all the way back to the Pentagon…and he departed from his flight plan and started letting down. So they were able to keep it insight and it wasn't until they were going through about 9,000 feet that the light suddenly went out. I was the first one that they got out of bed, of course being the squadron commander, after me came the Alaskan air command headquarters and so on. The upshot was that nobody would believe it.

When they got back I interrogated every member of the crew very carefully and I could only come to the conclusion that they had seen runway lights but it was such an outrageous…it seemed so incredible that I couldn't even convince my own superiors in the Alaskan air command that it was real. They had all kinds of theories about seeing a reflection of the moon in a lead and things of that sort. The upshot was that we were never able to establish credibility for that.

Kellogg: This was pretty close to Point Barrow?

Fletcher: It was several hundred miles out. This was one of the things that made it incredible.

Kellogg: It wasn't very far away though.

Fletcher: No, not terribly far away. My point though is that years later I was able to sit down with Tereschnikov and Federov who were the two leaders of North Pole II and North Pole III and it turned out that they were both there at that time. They remembered the incident and indeed they did turn off the runway lights. But they, of course, were very pleased whenever we occupied T-3 and established our own drifting station because it gave a great impetus to their activities in convincing their superiors to provide the support that they wanted to do the things they wanted to do out there.

Kellogg: How many North Pole stations were there? Are they still putting them out there?

Fletcher: Not continuously from the early 50's but from the late 50's after T-3 it was enough impetus so they were able to establish as a policy decision that they would maintain two drifting stations continuously and they have done so. They sometimes have three but normally have had two drifting stations at all times right up to the present day and I don't know what number they are up to. They are up to about North Pole 30 by now.

Kellogg: Did you ever get to visit a North Pole station?

Fletcher: No, I have never been to one of theirs. When I went to NSF many years later as head of polar programs Tereschnikov and I tried to establish closer links, which would include exchange of people. We were still in the cold war period and there were a lot of reservations really on both sides so we agreed that we would do it outside the territorial jurisdiction of both countries, which the drifting station is.

In fact under this understanding one day a whole plane load of Russian scientists from the Arctic and Antarctic institute showed up at T-3, which at that time was being operated by the Navy mainly for ONR. They idea was that it would later be a visit of American scientists from T-3 over to them. ONR not only didn't object, they were so enthusiastic that somebody decided to load up all the Russians and take them into Barrow, which they did. That, of course, excited the interest of the state department and the authorities. They all informed them that they were there at the invitation of Joe Fletcher at NSF and for a brief time we had some very interesting interaction with the State Department who were very understanding actually and resourceful under the circumstances because they commissioned the local judge at Point Barrow to be a counselor official. He gave them temporary visas and they spent a week tooling around Point Barrow seeing the sights before they went back out again but the whole affair was fraying the Russian nerves of their bureaucracy enough so that they put a clamp on it and decided not to further develop that avenue of exchange.

Kellogg: That's a wonderful story. Then in the end the Americans didn't get to join the North Pole stations. That part of the exchange didn't go through I gather.

Fletcher: Actually there was a rather minor visit. I forget the name of the chap but there were some Navy folks that did stop in and pay him a visit but they didn't go into the mainland they just stopped at the drifting station for a while.

Kellogg: It's another world out there in the Arctic ice. One other question I was going to ask you about. There was a period there where you were putting on your Air Force hat very eager to get the Air Force geared up to Arctic

operations and it seems to me that as I recall you made some progress in that respect but I don't know how much.

Fletcher: Yes, after I came back one of my duty tours was with the Air Weather? War College down in Alabama where I was on the faculty there for three years. One of the jobs I had was to…well I was sort of a designated Arctic consultant for air operations. One of the themes that I tried to preach was that operating in cold weather can be done and in fact the Russians were very good at it. So you can't assume that just because the weather is cold that people can't operate because with careful planning and understanding of what you are doing they can not only operate efficiently but in many cases even better. For example, the range because it is so cold gasoline doesn't take up as much volume and you get about a 20% increase in range just from that factor alone. Moreover, jet engines love cold weather and when the Bison and the Badger came into operation, that was their counterpart of the B-47 and the B-52, operations are very feasible both for them and for us. So, this was a theme that I was involved in for a while and trying to improve the quality of our air operations.

Much of our experience had been developed from reciprocating engines, for example, and with very ill preparations. When I was at Isleson, for example, Isleson was also the staging base for the B-36 wing from Fort Worth. The B-36 you may remember it…six reciprocating engines and they would come up there from Texas in the middle of winter and there were so many fuel leaks because of the changes of temperature that it was a dangerous operation and in fact they lost some of them just getting between Texas and Alaska. In one case it went down over Queen Charlotte, Ireland off British Columbia in which everybody bailed out and some of them were rescued, some were not. But my point is that the advent of the jet engine changed everything because one thing is that the aircraft systems involving fuel, lubrication and so on were vastly improved and the other was that the development of the jet engine just made an enormous difference.

So, there was a period there in which the U.S. was relying almost entirely on SAC and SAC was severely limited in range that is why they were building American bases in Morocco and other places in Europe and other places around the world. But the most direct routes, or course, are across the Arctic.

Kellogg: The most direct route for both ways of course.

Fletcher: Yes, of course. It was during this period that the U.S. was very much concerned about defense against…

Kellogg: We say it's the DEW Line for one thing.

Fletcher: Yes, it was during that time when I was down at Maxwell that the DEW Line was conceived again by an MIT study group. At any rate, as the Arctic consultant…I got pulled out and was made project officer. President Truman presented with the DEW Line proposition decided to…you might say _____…not build a whole DEW Line but to build a section of the DEW Line and make sure that it functioned as it was supposed to function so they decided in 1953…in fact in November of 1952 I had just come back from Alaska in June of 1952 from T-3. In November the decision was made to build the Alaska portion of the DEW Line, which involved seven main stations and about 15 intermediate stations.

At any rate they pulled me out of my assignment and made me the Air Force project officer for this operation, which was then contracted out to the western electric company, part of AT&T, and they wanted the thing operational. They wanted it installed and operational by September of that year 1952. The point is that making a decision like that in November when everything is frozen up, it's dark, there are no exits except by air to the north slope, there is no sea access possible until July or August at the earliest and so the question is how do you go about such an operation with the prospect to try to make it operational by the end of September. But we did, and again this was done with many of the same people who had participated the year before in the T-3 operation.

Jack Streeton and his air transport squadron no longer had C-54's. They had gotten the C-124, which you remember was a monstrous big thing underpowered but could carry a huge load, and this made it possible to airlift an awful lot of stuff up during the spring to the north post. The Navy and the USGS had been working the petroleum reserve on the north slope for exploration for oil. They didn't discover prude oil but they had been doing a lot of exploration and they had a lot of sledge and _____ and things which were useful and they were about to abandon that operation so they turned all of the that stuff over to me. We were able to do enough during the spring…we were able to airlift enough stuff to work with plus what we could take over from the Navy to get things ready so that with the big sea lift in the summer of 1953, and an accelerated construction schedule, we actually got the stations in and they were operational.

Kellogg: By that fall?

Fletcher: By that fall. And then there was a hiatus of a couple of years before the decision was made to build the rest of the DEW Line across Canada.

Kellogg: I remember visiting the one that is near Point Barrow, very impressive station.

Fletcher: One of those DEW Line sets modified, that became surplus later, was picked up as surplus and is the experimental radar in our National Severe Storms Laboratory down in Norman.

Kellogg: The radar was brought back. I don't know whether we should pursue these things chronologically, Joe, but on this Arctic theme, or the polar theme, you became the head of the Division of Polar Programs of NSF. I'm not quite sure what happened in between your DEW Line experience and your Maxwell Field experience and your becoming head of the division of polar programs but then, of course, your Arctic experience came into use again didn't it?

Fletcher: Yes. For one thing during the time I was flying in Alaska I would say that was really the original awakening of my interest in climate. Even though I had been director of GRD, I didn't know enough about the subject to be interested. It was something that had never really sunk in to me. But flying over the pack ice for a couple of years and realizing that this huge part of the globe is covered by a thin film, maybe eight feet thick on the average, and that it's presence or absence could have such an enormous impact on the heat budget and perhaps the atmospheric circulation, really starts you thinking, you might say, about the fragility and the dynamics of our climate system. This was really the thing that got me to reading some background stuff about climate. The first book that I got a hold of was [CEP Books]? book called, **Climate Through the Ages**. You must know it don't you?

Kellogg: Yes, a yellow book.

Fletcher: I found that absolutely fascinating and after reading books I, of course, starting reading Lamb and that developed interest that continued to grow, although there wasn't any opportunity to pursue it for a few years.

Kellogg: When you went to Rand, of course, you did pursue it. You had time to pursue it there.

Fletcher: That was sometime later but after the DEW Line operation I went to the Navy War College for a year and then went off to Norway for three years…

Kellogg: Oh, that's right.

Fletcher: …as head of a military systems group working with the Norwegian Air Force. When I came back from there and went to the War College I did

my last tour in the Pentagon as head of the long range plans group before I retired and went to real work for Rand. But at Rand, the reason I went to work for Rand is because it did afford the opportunity, and I know you made the opportunity available as head of the division at Rand, but it was an opportunity to pursue an interest in climate. That led to a lot of other things.

Kellogg: You were very energetic. We happen to have here some of the reports that came out of Rand during your time there having to do with weather modification and another impressive report, which I am reaching for now, "Ice extent on the Southern Ocean and it's relation to world climate, J.O. Fletcher." The date of that was 1969, quite a bit later, but you obviously had a chance to really dig into this.

Fletcher: It wasn't an obsession with the polar regions per se, so much in the fact that the polar regions were a little bit more attractable problem to look at then the rest of the global system, mainly due to the fact that the surface conditions are relatively homogenous over huge areas. In the Arctic, you have the whole Arctic Ocean and it has a lot of similarity. In the Antarctic, you have the whole Antarctic continent surrounded by an ice-covered ocean, which is ice-covered most of the year. So, if you're looking at the dynamics of how the thermal forcing of the global atmosphere changes with time, on the annual cycle and on the longer time scale, the polar portion of this baroclinic portion between tropics and the polar regions, is a little bit more attractable then is the rest of the system.

That's why I started when I went to Rand, it was mainly an experience of learning and it gave me an opportunity to do a lot of reading and thinking about the problem. You had to write reports to earn your pay but it was trying to look at the time variability of the forcing of the global system. So, I studied the Arctic first and then the Antarctic. Once you get me outside those regions at that time I found you were up against a brick wall because there is no data, or at least I though there was no data, for the ocean areas. Some years later I realized that there was data and this today we know as the ?World Data Center.

Kellogg: Let's jump ahead in time. Continue this train of though about the polar regions and the _____ Marine Data Center which you and Ralph Sluuts were able to work with much later.

Fletcher: As I say, during the time at Rand I studied what was available on the time variability of the thermal forcing on the cold side in both hemispheres but I didn't realize that there was enough data from the ocean areas to map fields and really extend this into the mid latitudes. In fact it turned out I at that time made a few trips up to Monterey where Paul Wolfe was building the Navy Fleet Weather Central and among the things that Paul helped do

was to generate a project which would try to collect all of the Marine data that was available and this was picked up by the Navy, by ONR, by NSF and by WMO and it resulted in a lot of marine data being brought together.

In later years, that is in the last 15 years since we have been here in Boulder, we brought this all together. It turned out that as a result of the collective efforts of the NSF, the Navy, the Europeans and the stimulus that Paul Wolfe was able to provide from Monterey. Different countries took on responsibilities for collecting this data. The German government took responsibility, for example, for the Atlantic. The Netherlands took responsibility for the Indian Ocean. The U.S. took responsibility for the Pacific. All together they brought together some hundred million Marine observations taken since the 1850's. It turned out it was unusable because it was in a form of some 600 tapes plus a number of other data sets from whaling fleets from Japan, from Norway, from South Africa and so on. It turned out to be like 1,000 reels of data full of errors we eventually found. Some of them had been introduced since the beginning so this wasn't feasible to use.

What we were able to do here in Boulder is bring all of this together, try to create a homogenous data set by correcting the multitude of errors, identifying biases and purifying the data set as much as possible. So that today it turns out that we really do have a record of the feels of most of the surface variables. Williams is probably best represented. Pressure is well represented since about the 1880's. William goes back to the 1850's and other surface, like sea state, cloudiness and some other things. This is really…

Kellogg: What about temperatures?

Fletcher: Temperatures of course, sea surface temperature and air temperature. This is the record of how the climate system has behaved over the last century and one-third. When I say the, I mean there is no other. Our observational stations are so sparse and so concentrated in Europe and the Eastern U.S. that you can't begin to map fields, for example. They can be used for cross checking and verification and a lot of useful purposes but my point is that it turns out that the marine record is really the only observational record we have of how the dynamics has changed. Now that attention is being given to the global dynamics and climate change this, I think is turning out to be enormously valuable. The hero of that story is, of course, the guy who set up the system that created this data set which is a fellow named Matthew Fontaine Mowrey who was the head of the Hydrographic Office in the 1840's and 1850's, and who managed to organize the international effort that standardized the time and method of observations and stimulated all the ships to take observations and to

archive them. This is accumulated over the century and, you know, they didn't even have wireless of a lot of period and they weren't even able to utilize them anywhere outside of the local ship that was taking the observations.

Kellogg: So in order to assemble that data, they had to go through all of the ships logs, I suppose.

Fletcher: Oh yes, of course and when we collected all this data and brought it together it had lots of problems of errors in format but just the sheer volume of data was such that it was about $80,000 worth of computer time just to read through it once without processing anything. Since it has been processed, it is now today for $20.00 any graduate student can access any field he wants for the last century and one-third in greeted form for the whole global domain.

Kellogg: And was that refinement of the data done here in Boulder?

Fletcher: It was done jointly by NCAR, NOAA and the climate center at Asheville, that furnished much of the basic data. We did the formatting and programming largely with a NOAA group with the university. NCAR very kindly furnished the Cray to do the big computer processing. Asheville helped gather the data from sources in Europe and around the world.

Kellogg: This ____ marine data set has been invaluable and it's been used now to get officially the global temperature changes as you, of course very well know. The University of East Anglia has factored it into the land stations to get a better picture of what has happened to the global temperature. The question, of course, being have we warmed up the world. We won't get into that.

Fletcher: That's a many faceted problem, all right but it's a very important one.

Kellogg: Tell me, back just a little bit, a very important time in your career when was you were in charge of the polar programs of staff, at a time when they were trying to open up the Antarctic in particular. I gather that you were at least one of the people influential in getting to equip C-130's [ski-130's?] brought into the picture for _____. Is that correct?

Fletcher: Yes, well I can tell you the story of the C-130's, all right. After the T-3 experience in my experiences in Alaska, of course I was terribly sensitive to the limitations of our existing fleet of aircraft. I came back from Alaska in 1952 and one of the first things I became aware of being detailed to the air research and development command in Baltimore, was the C-130 which was not yet in production. Well, it was in production but it was not

yet being delivered at that time. But when I saw the specifications I became very excited because it was the ideal airplane for cold weather operations. First of all, it was simple, as simple as you could be and still have a sophisticated airplane. It had turbine engines, which was the enormously important thing. It was self-sufficient in that it had heaters and starters contained in the airplane starting with a putt-putt, an auxiliary, you had all the where-with to warm up or to start in even the most severe conditions.

Most important, with the landing gear in the fuselage, it was configured so that you could fit it with skis and have them retract. You see all of the big airplanes up to that time had the landing gear in the wings and there is no way you can fit skis to them without them interfering with the propeller. You know it just couldn't be done which meant that big airplanes automatically were confined the wheels. So as soon as I saw the specs on the 130, I started campaigning to try to get a ski configuration developed for it. I had a very terrible time getting the Air Force to be interested. In fact, I had no luck at all. I tried to use the extension of the due line as an argument, for example because there are a couple of stations up on the Greenland ice cap. The obvious way to do that is with a heavy ski airplane. At the time even that argument didn't sell so when I was down at the Air War College I was continuing to push this idea and I even wrote a couple of articles in the "Air University Quarterly Review" and so on, extolling the need for an Arctic logistics airplane. Burt Balkin was trying to help me in this campaign but even with Burt's help we didn't really get it done until we finally enlisted the support of a three star general who was commander of the Northeast Air Command, a General Barkis. With his help, we finally got the Air Force to approve the development of a ski configuration. Burt and I were on the mock-up board for that development which was done by Lockheed at the Marietta. It turned out to be enormously successful.

It is still today, 35 years later, the best airplane in the world by far. You could almost say the only really heavy airplane that is really capable of operating in the Arctic or Antarctic, go to the South Pole or anywhere else in the Antarctica. The upshot was one squadron was equipped, the first squadron, and it was deployed down at Ardmore, Oklahoma, which was ideal for the kind of operation.

Kellogg: What kind of operation? What was going on in Oklahoma?

Fletcher: Well, nothing was going on in Oklahoma, but I mean the IGY type of operation. Now the IGY was setting out to put a station at the South Pole. The C-130 is perfect for that and there were 17 ski-equipped C-130's sitting at Ardmore during the IGY and were never used. The reason was that there was apparently some rivalry between the Navy, you know, the

services like to have it a single show and the Navy really had the responsibility, the Air Force had the C-130's and they never were able to get together. The plan for putting the station in the South Pole was to use a sled train.

Kellogg: All the way from McMurdo?

Fletcher: All the way from McMurdo to both the South Pole and to Byrd Station. Well they did use the sled train to Byrd Station but the plan failed for South Pole station so there had to be a substitute plan instituted in the middle of the operation. The substitute plan involved getting the C-124's. An Air Force wing from Tennessee brought down a batch of C-124's. They, of course, couldn't land at the South Pole but they could make a huge air drop and so that's the way South Pole Station was established. I was not involved in that because I was detailed by the Air Force to the National Academy who was responsible for the IGY operations but my responsibility was for putting in the Arctic program which was mainly two drifting stations and I was fully occupied with that and not able to participate in the Antarctic Operations.

So when they decided to continue the South Pole Stations, the plan for the IGY did not include …

Kellogg: This would have been 1958 or 1959?

Fletcher: 1960.

Kellogg: The IGY was 1958?

Fletcher: 1957, '58 was the IGY and it was not at that time planned to be a continuing operations.

Kellogg: At the South Pole.

Fletcher: It was going to end. In fact, the IGY effort, those stations were going to end but it turned out that they decided that they would continue. So there was another supply emergency in 1960 and this time instead of the C-124's coming down, well they came down too, but the Air Force sent C-130's down and they were such a great success that at that time the Navy procured their own C-130's which have continued to be the backbone of the Antarctic Operation ever since.

Kellogg: They say they are wonderful planes, having gone down to the South Pole in one, I'm impressed.

Fletcher: Yes, well it's as I say to get around the kind of problems that plague operations most at it's time, the early 50's, it was almost the ideal vehicle.

Kellogg: You lost some of the C-130's and I remember the heroic effort to recover them. Did some of these efforts take place when you were the head of the division or was it later on?

Fletcher: We did recover one from Dome Sea but there was another one, which we decided not to try to recover but which in recent years, they with enormous effort, did recover but they lost a new one in the process. It turned out to be a wash.

Kellogg: It established the law of diminishing returns at that point. Yes.

Fletcher: It is fraught with some hazards. I haven't made a count in the last ten years but up to the mid-70's, there had been about 60 airplanes lost in Antarctica.

Kellogg: Are these just U.S. airplanes?

Fletcher: U. S., that's right. About half of those, were helicopters and about half of them were fixed wing. I went through all the accident reports during the time I was responsible for the program and I would say probably three-fourths of them were what you might call a result of technique in Arctic flying or pilot error or poor judgment. That's what I mean by technique. Operating under visibility conditions in which you ought to not operate or something of that sort but my point is that very few of the accidents were unavoidable mechanical failure.

Kellogg: I hope they have done something about that by now having had a lot more experience.

Fletcher: I think it is going much better.

Kellogg: But, of course, the fact is also that the Navy pilots they rotated in an out, they don't have a chance to get a lot of experience.

Fletcher: That's a big problem and continues to be…(tape end)

END OF TAPE

Kellogg: Ok, you went to the University of Washington.

Fletcher: I went on leave of absence from Rand for a year to the University of Washington to set up a project, which was another drifting ice station, which we called AJAXER, Arctic Ice Dynamics Joint Experiment. This was an idea to measure all of the parameters affecting the movement and deformation of pack ice over _____ scale, like 100 miles setting up a triangle of three stations and so on. Well, my family liked Seattle and the University so much that we extended for a second year and I never did go back to Rand because I took the job at NSF as head of Polar Programs and that was three years, but didn't like Washington and Lynn didn't want to live in Washington so I looked for another job. Instead of going back to Santa Monica I went to work for NOAA as deputy to Bill Hess who is director of the NOAA Environmental Research Labs. I was Bill's deputy here for several years and then when Bill left to become director of NCAR, I was director. Bill went back to Washington for a year and then he became head of NCAR. At any rate, during that time I was the director and then after that I left the government and retired from civil service and spent a couple of years at the University. It wasn't until …

Kellogg: That was the University of Colorado?

Fletcher: Yes, the University of Colorado.

Kellogg: At CIRES, weren't you?

Fletcher: At CIRES, yes that's right and it wasn't until 1983 that I rejoined the Government as Assistant Administrator for Research at NOAA and that job was back in Washington. I ended up spending five years in the role, which was much longer than I intended to.

Kellogg: And this was the period that was incredible to me how you could have one foot in Boulder and have a job in Washington.

Fletcher: Well, we had full laboratories in Boulder, which there was quite a lot of reason to give attention and be out there. Yes, I had one foot in Boulder and one foot there but at any rate, when Lynn became ill; I came back to Boulder full time. It just turned out that it was convenient to come back as Director of Environmental Research Labs.

Kellogg: What year was that, Joe?

Fletcher: 1988.

Kellogg: So you have been director of ERL since 1988 again, your second round.

Fletcher: Yes, it's deja vu.

Kellogg: Being director of a big lab like ERL must be an enormous administrative job but you also have obviously, knowing you, been very much involved in the scientific program too, as you have been shepherding some of the programs. Which of the programs have you had the most fun pushing and looking in on and encouraging?

Fletcher: The climate program, of course, I think. Because, I think, there I have been able to call on the resources I developed at Rand. Rand gave me the opportunity to do enough reading and studying so that I had developed a lot of perspective on the problem so that there is a lot of homework that I already had under my belt. Therefore, I was able to devote energies to organizing and trying to sell the program and other things.

Kellogg: What was the relationship between you ERL and the National Climate Program that was also based in NOAA under Alan Heck originally and still based in NOAA, I guess.

Fletcher: Actually goes back much earlier than that when I was at NSF, we really tried to sell a national climate program and we put together, in fact, all the background studies and necessary paperwork and I think came very close to getting a Presidential executive order in 1974 that would have set up a national climate program. I was head of Fuller programs and Gene Bierly was head of Atmospheric Sciences, which Ed Todd had at that time. We came very close to putting that across but we failed. It wasn't until several years later that actually George Brown and the Congress really took the initiative but it did have some of its roots taking root at that time.

Kellogg: If it would have been set up by Executive Order, do you think it might have worked better?

Fletcher: No, in retrospect, I was terribly disappointed that we didn't have more success at the time. We did succeed in getting a climate dynamics program established at NSF. That was the only new initiative from NSF that year. But Guy Stever was very supportive. We didn't succeed in getting a national program, but I should say, in retrospect, I'm not sure that is all bad because frankly, I don't think the community was ready for it. It think that had we developed faster with a big program there would have been more mistakes and probably it would not have developed as soundly as it did on a slower pace.

Kellogg: When Rob Swear and I are writing our book called **Climate Change in Society,** under Aspen Institute sponsorship, we went to see George Brown

whom I'd met several times before and I thought I'd have about twenty minutes to talk to him about his view on the climate program. He spent over an hour with us and one of the things that stuck in my mind was his saying that he thought that the National Climate Program would be one inter-agency program where there would be no squabbling. The agencies could really work together subject to climate, after all. There can't be too many vested interests in the climate system and he was very disappointed at that stage, which was 1981, that the various agencies had not learned to pull together under the National Climate Program. There was still squabbling.

Fletcher: I think that's a fair comment and this is part of what I mean that I'm not sure that the community was intellectually ready for rapid development. I would say in the last several years, under the auspices of OSTP and the Committee on Earth Sciences, there is developing a much better mechanism for agents from government wide…

Kellogg: Interagency…

Fletcher: coordination and that probably it took a few years for the ground to become fertile enough to have that develop.

Kellogg: Well, there has been a certain amount of rivalry, for example, DOE and EPA rivalry, that kind of thing. I'm impressed by what is happening in the National Climate Program.

Fletcher: Yes, I think much of what has developed today was outlined in the proposal for the Regional Executive Order, you know. But my point is that even had that succeeded in getting an Executive Order, I'm not sure that the agencies or the community was ready to be able to implement.

Kellogg; You're probably right. Returning to ERL, not only do you have the climate effort here, which, of course, I can understand why you are particularly interested in that, knowing you. What about some of the other things that are going on in the ERL that excite you; the Wave Propagation Lab, for example?

Fletcher: Well, the Environmental Research Laboratories are 11 laboratories, most of which are not in Boulder but in other parts of the country. You know, from Princeton, GFDL at Princeton, to Miami to Seattle where the two main oceanographic laboratories. It turns out that we do have five of those laboratories in Boulder.

Kellogg: There are six others in ….

Fletcher: There are six others that are elsewhere and they are all quite different and all of them are doing exciting work. I think some of the new frontiers are, for example, space weather. This is something where the Space Environment Laboratory and NCAR have worked closely together, as in many other areas, for that matter. Well, if you read about the development of the Weather Service which as I say have read recently in the "Thor's Legions," AMS ?Chronicle, which I found very fascinating to read because I recognize so many of the characters. I see the same thing developing in being able to observe, understand, and predict the space environment between the earth and the sun, which we're increasingly operating in both manned and un-manned satellites. I think one of the aspects of this, of course, is better understanding of the sun's influence on climate variability.

Kellogg: When I came to Boulder to work under Walt Roberts who is a great enthusiast for the solar weather relationship, I was rather skeptical and was for a long time that there was really anything that you could get your teeth into in that area. But recently there have been some breakthroughs that you probably know that indicate that there really is a solar influence on the weather. I am thinking particularly of Karen Labitzka and Harry Van Loon.

Fletcher: Oh, Yes. Walt and his colleagues like Roger Olson and others were doing empirical studies. It is in the nature of that kind of investigation that you discover relationships that you don't understand and were not able to explain mechanistically, which I think was the case. It is in the nature of the prudishness of the scientific community that until you can explain the mechanisms they tend to pooh-pooh it.

Kellogg: You are so right. The Labitzka-Van Loon relationship which holds up very well statistically; it's such a strong correlation that you can't deny it, there is still no mechanism.

Fletcher: Well, I'll stick my neck out if you want.

Kellogg: OK, you've done it before you'll do it again.

Fletcher: For the record. No, climate change has become a big subject now and it is fair to ask a number of questions among them are what changes. What is it about the global mechanism that changes in the process of climate change? What fields are the most robust in exhibiting change? What regions are the most robust in exhibiting change? What is the coherence in time and space of change and so on?

To summarize my conclusions from looking at available evidence, much of which comes from COAD's data set goes something as follows.

Number one, the most robust signal of climate change is simply the strength of the global atmospheric circulation, the wind strength. It's very robust because the vigor of the circulation has changed surprisingly much during the period of record, which I mean the period of the COAD's data set, which is back to the 1850's. The most robust field is wind.

Kellogg: Can you say how the wind circulation has changed?

Fletcher: Yes, you could ask the question how do things change. Do things change suddenly or gradually so you have to _____ in the statistics to find a signal and the answer is most of the variance is represented by sudden change; three or four adjustments to the atmospheric circulation which you can think of as adjustments in the wave number of statistics and the planetary ways, for example which represent most of the variance of the last century. The points in time in which these adjustment have occurred have been the mid-1870's, around 1990, around 1930 and not so strong in the early 60's. The general pattern of change in the strength of the wind is that it was more vigorous in the 1860's than it has ever been since. There was a decrease ....

Kellogg: Just a stronger circulation, the winds were generally stronger over the ocean's in the 1860's.

Fletcher: Yes. There was a rather abrupt weakening in the mid-1870's that was most pronounced in the high latitude in the northern hemisphere, especially the high latitude. There was a pronounced weakening around 1900, which was most conspicuous in the southern hemisphere, especially the Indian Ocean sector. There was a minimum in the 1920's and 30's, the thing that characterized the dust bowl years was that it was the period of weakest circulation of the global atmosphere of the last century. There has been an increase since the 1930's but not enough to approach the levels of the 1860's. Now, the pressure field has to reflect the changes in the wind field. The reason I emphasize the wind field is because that's our best data set. There are more observations and they go farther back and better distributed but there is enough pressure data so that you can map the pressure fields not back to the 1850's but you can do it pretty well for the 1900 decades and a little bit back in through the last century.

So then let's go to the next question. What changes because the wind has to be responded to changes in the pressure field and what is it that changes and I think the answer to that is that the most robust feature of change in the pressure field is the depth of the sub-polar trough in winter specific to season specific to the feature. Now that says in the Northern Hemisphere it's basically the deepness of the North Pacific glow ……

Kellogg; The Aleutian glow …

Fletcher: The Aleutian glow and the Icelandic glow. Now in the Pacific sector, there is not much change over the century in this sub-tropical high. In the Atlantic sector, there is significant change in the sub-tropical high as the Icelandic glow deepens the sub-tropical high strengthens. But the most robust feature is the depth of the sub-polar trough in winter. Nearly all the variance of the other fields as I say can mechanistically be derived directly from that. Now that's narrowing the problem because the next obvious question is what could influence the depth of the sub-polar trough and then you start marching through the possible forcing mechanisms, solar irradiance and so on. Solar irradiance, you know, can be a factor influencing tropical ocean temperatures and amount of convection and so on and I think that's probably a significant factor but when you look at the observational data, it's hard to think that that's enough to account for the changes that the fields represent. When you look at the sub-polar trough, another interesting feature that even though the energetics of the atmospheric circulation is much greater in the Southern Hemisphere than in the Northern Hemisphere. If you look at change, the influence of the Northern Hemisphere seems to dominate.

Kellogg: It changes more, in other words.

Fletcher: Yes, it changes more and seems to dominate even through the trade wind zone of the Southern Hemisphere, which is something that has to be explained and there is an explanation, I think. There are hypothesis that's fairly obvious or straightforward but what I'm coming to now is that most people when they talk about solar influence think of the irradiance as the main factor. More and more, I am coming to suspect, I say suspect rather than conclude, but I think it's something that can be tested and thought of that irradiance is probably significant but maybe not the most important solar influence because there are other solar influences which can fit the data better specific to the region, specific to the season, specific to the field and feature. For example, inner-action with the solar wind and by that, I mean basically proton precipitation. Now among the hypotheses that are worth testing is simply the simple one, namely the solar wind is able to precipitate into the near earth only at certain magnetic latitudes …

Kellogg: In the auroral zone …

Fletcher: In the auroral zone, that's right, that are specific to the energy of the particles. Different energy, different magnetic latitude. Now in the Northern Hemisphere and in the process of doing this various mechanisms have been proposed which involve chemical changes in the atmosphere or electrical changes to ionization which can influence condensation in the formation of thin cloud layers, for example. The amount of energy directly is not enough to heat the atmosphere but if you can do these other

things, either chemical or the formation of cloud layers, you can then change….

Kellogg: You are changing the electric circuit in the atmosphere.

Fletcher: Well, through either of these, you don't have to do much to alter the irradiative exchange in the vertical as you know enough to be important in heating troposphere so the mechanisms that are in question are is it possible that the precipitation of these charge particles can influence the temperature of the troposphere through the formation of cloudiness or chemical changes which also modulate the irradiative exchange in the vertical dimension.

Now the reason that we have not had anything conclusive is because we have had no data to test these hypotheses but there are some fairly obvious empirical ways of going about this. For example, why should the Northern Hemisphere be so influential compared to the Southern Hemisphere. Well, if you just look at the geography, one thought comes to mind and that is that if the auroral circle and the sub-polar trough in the Northern Hemisphere are almost coincident. If this is an influential mechanism, it would be very effective whereas in the Southern Hemisphere, the auroral circle, which is an ellipse and the sub-polar trough which is almost a circle are at a considerable angle so that they intersect and only certain longitudes. If through either chemistry or nucleation processes, these mechanisms are operative. Therefore, they should be operative only at the longitudes of intersection. Do you follow me? That's something that can be tested.

In order to test that what you need is the special and temporal distribution of the auroral density that is the particle density and you then compare directly with the behavior of the atmosphere which we have for the last thirty or forty years in the file at NCAR. Well, one of the things that we are doing is SEL, I've been leaning on SEL and we have been flying now for thirteen years a proton, well, in fact, a space environment monitor which in about five different energy ranges is measuring the density of the solar wind outside the atmosphere. We can now grid this in time and space and in a manner very similar to the way we have the archive of the behavior of the atmospheric circulation and it's a pretty straightforward thing to simply test some of these hypotheses and see if they apply or not. One of the things we are doing now is pulling together these thirteen years of direct measurement of solar wind in order to be able to do this.

If then the evidence appears to be very strong that this kind of thing is taking place then the next thing you do is organize some _____ measurements. You've got to get up to where these things are happening and find out if you can observe them actually happening and if you can

observe the tropospheric heating that would be the result. If you can, that that's just about the nail that puts it together.

Now back to Walt Roberts. What Walt and his colleagues were doing was at the other end of the problem. They were observing the deepening of the sub-polar trough.

Kellogg: The Aleutian low specifically.

Fletcher: Specifically, that's right. I have been calling it the sub-polar trough. But my point is that through their approach they did identify where are things happening; what is the feature that is most robustly reflecting change and they weren't able to go much farther than that but at least that, I think, is a very important discovery.

Kellogg: Very convincing statistics. The thing, which is also fascinating, is that while the rest of the world has been warming up the North Atlantic in particular it has been getting colder and wetter.

Fletcher: Not just the North Atlantic, the Pacific as well.

Kellogg: The Pacific has been getting colder also.

Fletcher: Every part of the Northern Hemisphere ocean is cooler in the 80's than it was in the 70's. Cooler in the 70's than it was in the 60's except the Gulf of Alaska. You have had such persistent southerlies there that you have a big warm anomaly but nearly everywhere else in the North Pacific and the North Atlantic is cooler.

Kellogg: It appears that the Icelandic glow…

Fletcher: Why is it cooler?

Kellogg: I have been looking at the Pacific so much as the North Atlantic. The Icelandic glow has apparently migrated westward.

Fletcher: And deepened.

Kellogg: And deepened.

Fletcher: And the subtropical high has strengthened.

Kellogg: And that could account for the cooling of the North Atlantic couldn't it?

Fletcher: Well that's right. It has stronger circulation, more evaporation and more heat loss by the ocean. That's right. All of these things do fit together.

This is why I have found the current debate over global warming quite frustrating because much of it I consider so downright inane. One of the things, I think, is using global surface temperature as the index of climate. That is one of the poorest indexes that you could use. It would be far more meaningful to talk about the strength of the circulation, to talk about the surface wind, to talk about the pressure field and almost anything would be more meaningful than what is being talked about. To talk in terms of global averages instead of regional phenomenon simply compounds it.

Kellogg: You think we should talk about the regional phenomenon more?

Fletcher: First of all you should talk about the more fundamental things such as the strength of the circulation and changes in the surface heat budget. Global surface temperature is one of the most misleading things you could possibly talk about.

Kellogg: I guess it is used so much because of the greenhouse effect deals directly with temperature whereas the strength of the winds is a secondary thing, which we haven't quite pinned down.

Fletcher: But in fact the global surface temperature is dominated by the wind field.

Kellogg: Certainly the regional temperature would be, not necessarily the global temperature though.

Fletcher: The global temperature is the composite of the regions. We were just talking about the Northern Hemisphere. My point is that all this, the Northern Hemisphere record, is simply reflecting the changes in the wind field and the Southern Hemisphere record as well.

Kellogg: I think your right though that the wind field has not been studied adequately. The temperature is the one that people look at including myself.

Fletcher: Moreover, let me put it another way and say, take just one term in the surface heat balance for the ocean. If you integrate over the ocean domain…if you look in a textbook it will say, evaporation 65 or 70-watts/square meter, actually I think that is grossly off, it's actually more than 100. I would say somewhere between 100 and 120 would be a more realistic figure at least for recent decades. It may be that the lower figure was derived from the All Weather Bureau historical map CIRES or something like that, and it might have been lower. But what I am coming to; if the mean evaporation term is, let's say, 100-watts/square meter to

make the arithmetic easy, doubling of CO2 changes the heat balance through radiated processes by, let's say, 3-4 watts/square meter, that's 3 or 4% of just the evaporative term. Now the evaporative term is directly proportional to wind speed and in fact in the _____ record we have a record of evaporation computed from individual observations with wet and dry observations, which is not too bad for recent decades. The point I am getting at is if you take the decade of the 30's when the circulation was weak and compare it with the decade of the 80's when the circulation is much stronger, the change in the evaporation term alone is several times bigger than a doubling of CO^2.

What I am saying is that the dynamics are enormously important and you could rephrase the current global discussion with a simple question. In an enhanced greenhouse world will the wind blow stronger or weaker? If it blows 5% stronger…let's say it blows 5% weaker, it will more than double the greenhouse effect. On the other hand if it blows only 5% stronger it will more than cancel it. Therefore to even talk about what it will be, you've got to address the question, is it going to blow stronger or weaker? Now, if you ask the modelers that question, I find Jerry Mawman and Sue Key, I have talked to them several times, they give the arm waving argument that they think the answer is weaker and that therefore the greenhouse effect would be augmented because the evaporative term would be decreased.

Kellogg: That conventional wisdom because the idea that the polar regions warm more than the equator so you have less equator to pull temperature difference.

Fletcher: That's right. I think that is probably wrong. If I had to bet, as I do when I buy a stock, I would bet on the other side of that, although I recognize that it is an open question. The reason is couple fold. One is that it fly's in the face of our observed record. What we actually see is that if you look at the strength of the global circulation and compare it with sea surface temperature, if you try to compare it to mean global sea surface temperature you don't get anything meaningful out of it and there is no particular reason why you should. If, on the other hand, you compare it to the area within the 29-degree isotherm then you find there is a very close good correlation, which makes sense because within the 29-degree isotherm you have intense deep tropical convection, the Joanne Simpson-Malkus type. So my point is that's really the index of how the engine in the boiler is burning. That isn't the same as the mean surface temperature because in the mid-latitudes and much of the area, surface temperature is not triggering the intense atmospheric response.

Kellogg: I was just reading a paper by Herman Floehn last couple of months who says almost exactly what you are saying. He says that the most sensitive

indication of any global change or any greenhouse effect would be in the tropics and tropical moisture.

Fletcher: That's right. I would say specifically a good index is the area within the 29-degree isotherm because it is a very non-linear.

Kellogg: Is this something that has been looked at?

Fletcher: I've looked at it only in a cursory way. I am going to look at it more closely in the coming months but my point is that I have looked at it enough to…I think I can say that the feature that correlates with the intensity of circulation is the area within the 29-degree isotherm and that isn't at all the same as the mean sea surface temperature.

Kellogg: No, it wouldn't be.

Fletcher: If you stop to think about it, if you have more deep tropical convection that's mass transport upward. Some of that is compensated by mass transport.

Kellogg: More importantly it is transport of moisture upward.

Fletcher: Yes, of course, both. But it also means that mass transport has to come down somewhere too. Some of it is in the subtropical highs and if you look at the amount of cloudiness, and I admit the record is very shaky, because the observational record, but to the extent it is good enough so I think we can say that over the last few decades the amount of cloudiness has been increasing, so has the strength of the circulation been increasing, so has the area within the 29-degree isotherm been increasing and at that circulation, I think, dominates the strength of the global circulation. It dominates the other _____.

In fact if you look at Suki's own model you find that his 10-degrees, or whatever it is warming in the Arctic, is all in the surface layer. In fact, below the inversion where it has little influence on _____, if you look at the mid-troposphere where it matters, he has about a 1-degree warming in the Arctic and if you follow it to lower latitudes you find that he's got about a 1-degree warming in the tropics in the mid-troposphere. So, even if you take the bottle results at face value the change in overall _____ is well within the uncertainties of the model. On the other hand the forcing of the Hadley circulation through tropical convection is very direct and the observational record is very consistent. What I am saying is that if I had to bet, I would bet that it is the tropical forcing of the Hadley circulation that dominates in this. If you try to warm the global system, what you'll do is speed up the circulation and this dynamical buffer will be just that, a buffer that it will greatly reduce the other features like the

warming of the ocean because what happens is you speed up the wind, more evaporation, you are cooling the ocean through evaporation.

Kellogg: Which the monitors were calling negative feedback.

Fletcher: That aspect is negative, yes.

Kellogg: Whereas the extra tropical convection would be a positive feedback.

Fletcher: Yes.

Kellogg: A big step forward, of course, in the _____ and Kurt Bryan's work is having a circulating ocean for the first time, a really good circulating ocean coupled to the atmosphere and then being able to run it with a progressively increasing forcing by the greenhouse gases and NCAR, Washington and Meehl have done the same thing. They have a coupled ocean atmosphere model and they run a dynamic experiment. The results are quite different, as you would, I'm sure, expect. When you do that kind of experiment vs. the old equilibrium experiment where you just took the difference between the _____ run and a controlled run.

Fletcher: I agree but I have been mostly following Suki's GFDL experiments and my point is that first they have got huge warming occurring around Antarctica and then they do another run, they have got huge cooling. My conclusion is very simple and that is that this is progress that needs be encouraged, it needs to be enhanced and all of that but when you get such dramatic differences from one experiment to the other, why take any of them very seriously as a representation of reality.

Kellogg: I think reality is very complicated and now the models are getting as complicated as reality. The point is, Joe, that I haven't been following Suki's work as closely as I have the NCAR work of course. What you find in the dynamic experiment is, for example, from mere 20-30 you get something quite different from what happened up to year 20. In other words the ocean circulation in particular became perturbed and led to this anomalous North Atlantic and North Pacific cooling in the model, only at one period.

Fletcher: We have always known that the formation of deep water is episodic. Some years it happens and some years it doesn't. We know that it is very localized. Only a portion of the Greenland/Norwegian sea has the vertical stability conditions for…

Kellogg: That really is a very sensitive part of the climate system.

Fletcher: I'm not so sure. My point is that we have known that it is sensitive in the sense of the supply of deep water. What we don't know is that that is important to forcing climate system on the time scales were interested in. Before that deep water will interact again with the surface is not a decade or a century but a millennium.

Kellogg: That's not the point though. If you don't have that sink for the surface water and you don't have any downward transport of water, first of all you don't get as much carbon dioxide taken up because the rate at which carbon dioxide is taken up by the oceans is limited by that sinking. Cut off the sinking, the carbon dioxide stays in the atmosphere and, of course, the _____ record shows that in the last couple of years the rate of increase of CO^2 has been going up faster than you predict.

Fletcher: That's true the exchange between the surface ocean and the deep ocean is a factor in the rate of uptake of carbon dioxide, indeed. On the other hand this is another area that I am very skeptical about and many people seem to make the blind assumption that the concentration of carbon dioxide is really the crucial factor and I am not convinced that that's the case at all. The fact that we have seen the high carbon dioxide levels during warm periods, I find it just as plausible to explain that.

END OF INTERVIEW