Johnson.D.1353_8_23_2013

Donald R. Johnson
Louis Uccellini
Troy Reeves
22 August 2013
Atmospheric, Oceanic and Space Sciences Building, University of Wisconsin-Madison

DRJ: That I think need to be discussed.

Troy Reeves: OK. Today is Friday, August 23, 2013. This is the fourth and final interview with Don Johnson. Also present is Louis Uccellini. My name is Troy Reeves. I'm with the UW-Madison Oral History Program. So just before we turned this on, Don, you said you want to spend a few minutes talking about some things that you feel you missed in the previous sessions.

LU: I guess we should note that John Stremikis has arrived and will be taking some pictures during this session. [transcriber's note: Stemikis, John R. Spectral characteristics of the orthogonal wind components near the ground. University of Wisconsin-Madison, 1973. M.S. thesis.]

TR: OK.

LU: OK.

TR: Alright. Don.

DRJ: I want to go over very briefly some relations with Jerome Namias, who was a colleague of Rossby. [transcriber's note: Jerome Namias (1910-1997), US meteorologist] [transcriber's note: Carl-Gustaf Rossby (1898-1957), Swedish, US meteorologist, pioneered explaining the atmosphere in terms of fluid dynamics] [I knew *] him quite well, as well as several other people [however before discussing Jerome Namias *], I'm going to show you a representation of the Hilldale lecture [*] which was given by Paul MacCready. [transcriber's note: Hilldale Lectures are a series of free, public, annual lectures begun in 1973 and sponsored by the Hilldale Fund of the University of Wisconsin-Madison] I was chairman of the research committee for that year [transcriber's note: Hilldale lecture 1982-83, Paul MacCready, President, AeroVironment, Pasadena, California. Impractical airplanes, practical visions] and served on it for three years. This is the UW research committee, graduate committee. I got to know Paul MacCready even before that because of my connections in atmospheric science and traveling to Caltech and places like that. He would come to the AMS meeting. [transcriber's note: American Meteorological Society] He was quite, of course, a well-known person even by this time because he won this award for having [designed *] a man powered air craft flying across the English Channel. And so I invited him for that annual lecture that the graduate school had each year. Then he came, spent some time with him as well as shortly thereafter. So that's the first

individual. I mentioned that I knew Jerome Namias who was a colleague of Rossby and there's a fascinating story here about isentropic ways of doing analysis versus the isobaric way of doing analysis. Airplanes fly on isobaric surfaces essentially because they have altimeter settings and [from *] setting the altimeter they'll fly on that isobaric surface, not a constant height surface. At the same time back in World War II they were transmitting information on the teletype from radiosondes both in terms of temperature and isobaric surfaces, but then they [also *] transmitted the information [concerning the *] Montgomery stream function and temperature on [*] isentropic surfaces. Now that lasted essentially through World War II but then when [I asked Namais *] were you better doing analysis on isentropic surfaces or isobaric surfaces. [He stated *] in one of his texts that as long as they understood what was going on, he felt that they did really quite well looking at things in isentropic surfaces. And if you go back to those years you'll find a lot of analyses on isobaric surfaces. You'll find a lot of papers on isentropic surfaces. You'll find a lot of papers [on the delineation of *] mass types. Rossby had come up with this classification, as well as others, on air mass [*] types. Maritime tropical, continental tropical, polar air masses, cold, warm, etcetera. [Then you also find *] a great deal of literature on [such analyses *]. In fact Rossby has a [special *] diagram showing air masses types relative to the water vapor distribution and isentropic structures. What happened then was that at the end of World War II, there was a communications overload on the teletype services and Jerome Namais says an army lieutenant decided to eliminate this transmission of the isentropic data at that time. That brought an end to essentially analysis of isentropic surfaces out in the field at that time.

0:04:48.4

LU: Was that tied up also with the _____ value being computed as two separate terms?

DRJ: That, of course, was one of those things

LU: creating one of those errors that

DRJ: Right, but I believe that Ed Danielsen had [done *] something to solve that [problem *] [transcriber's note: Edwin Danielsen, Pennsylvania State University and National Center for Atmospheric Research (NCAR]

LU: Right. So Ed Danielsen did it as an integral

DRJ: Right.

LU: and showed that, you know, inherently it was correct. He said the geostrophic wind didn't work

DRJ: Right.

LU: in the isentropic framework.

0:05:11.0

DRJ: Now if you compute the geopotential energy separately from the entropy or internal energy and add those two you get what's called the Montgomery stream function. You'll get a lot of error as you go higher in the atmosphere.

LU: Which is what was done

DRJ: That's right.

LU: in that period.

0:05:25.9

DRJ: Now alright, you just brought up Ed Danielsen. So just a few more things about Ed Danielsen. While I had gotten to know him more at Penn State, and then his time at NCAR, there came the emphasis on the Southern Hemisphere, stratospheric vortex and diagnostics of that because of the chemicals that were destroying the ozone layer. And one of the people involved here was Adrian Tuck [who *] in '85 had a special workshop in Great Britain, England. [transcriber's note: Adrian Tuck, Royal Meteorological Society] He's an English scientist, and I was invited there. Ed Danielsen was supposed to be there but for some reason he didn't show up. So I got to know Adrian Tuck at that time. Adrian Tuck later came to this country and worked in Boulder NOAA labs. But along the way there was this effort to try to get a counterpart program in Europe, like a counterpart to MONEX. [transcriber's note: MONEX, the Monsoon Experiment, part of FGGE during 1978-1979] And so there was an effort to do that. Just prior to that however there was a person by the name of Roberto Frassetto who was a Italian scientist, quite well known internationally who had Ed Danielsen come and lecture with others at Venice for summer school to potential Italian scientists. [transcriber's note: Roberto Frassetto, Italian climatologist, oceanographer, hydrologist] Italy at that time didn't have a meteorology department [*]. So then the following year Ed Danielsen couldn't come, so Ed Danielsen suggested my name [and *] I ended up going to Venice to lecture at this workshop to young Italian future scientists, primarily ones who had their degrees in physics and that sort of thing.

0:07:42.7

LU: And what year was that?

DRJ: Be '74.

LU: '74

DRJ: spring of '74. That's not true. It was a couple years before that probably.

LU: OK.

DRJ: In any event Roberto Frassetto was a international scientist in his own right and at that time there was a concern of saving Venice because of the storm surges interacting with the Adriatic Sea and causing these surges into the city with water four or five feet deep in the areas there where the main square is. But [*] the thoughts were to develop a breaker so to speak that would rise up when these things threatened to shut off the channels that [would protect the harbor of Venice *]. That was always in the future. But at that time then being a guest there, of Roberto Frassetto we were shown Venice to no end, although I had been there before, nice, got boats so to speak, and drive us around, look at the various things in Venice, [such as a *] place in Venice which was one of the old Venetian mansions [*]. And then there was this effort to develop ALPEX [transcriber's note: ALPEX, the Alpine Experiment, part of FGGE during 1981-1982] a year or two later. We had a meeting in Sveti Stefan of Yugoslavia so we would fly into Dubrovnik for example and drive down to Sveti Stefan to attend an initial meeting there. Winn-Nielsen was there. [transcriber's note: Askel Wiin-Nielsen 91924-2010), first director of ECMWF in 1974] Charney was there. [transcriber's note: Jule Gregory Charney (1917-1981) American meteorologist, faculty at MIT] I give a presentation with many other scientists and out of [these efforts came *] the ALPEX experiment.

0:09:41.7

LU: At that point was

DRJ: That'd be '74.

LU: That'd be '74. Was there Bologna group, with the Speronza and Treveston [transcriber's note: spelling?]

DRJ: If

LU: and Buizza now involved [transcriber's note: Spelling? is this Roberto Buizza of ECMWF?]

DRJ: They followed. They were those young scientists who came there from Italy

LU: OK. OK.

DRJ: And then they went to one of the universities. They developed _____

LU: Because Buzzi became a person who promoted the isentropic approach

DRJ: Right

LU: you know, in later years. And worked with Black at bit

DRJ: Right.

LU: _____ Black [transcriber's note: name? spelling?]

DRJ: Right. So they, they essentially got exposed to that probably with Ed Danielsen at first and then of course I focused on that sort of thing.

0:10:20.1

TR: And what's. Is ALPEX an acronym?

LU: ALPEX is the Alpine Experiment

DRJ: Experiment. Yeah.

LU: Yeah. Alpine Experiment. So it was a focus on the, on the mountains in southern Europe and the role they played in the nature of the cyclones that formed in the Mediterranean and affected southern Europe, Italy and then into Yugoslavia.

DRJ: Right.

LU: It's a very complicated sequence of events that lead to these major storms.

DRJ: Well, you had an interaction with the Saharan Desert, sirocco winds and the Alpine winds that came up over the Alps and created these blows so to speak in Europe itself. The, and ALPEX was considered quite a success because it brought together the countries in Europe and particularly the Balkans and central Europe and even France and England to some extent, but not so much towards England.

0:11:29.4

TR: So non-communist countries?

DRJ: They. I'm trying to think now exactly what stage were we at in terms of

LU: Yugoslavia was still a, you know, was still a larger country. It hadn't disintegrated into its ethnic parts. You know, from a world communism point of view, it was one of the countries that was somehow able to remain independent of the Soviet Union. And there was obviously an influence but Tito who was the head of Yugoslavia emphasized the, their independence and basically nobody told him what to do. The Soviet Union kind of respected that.

DRJ: Well, I guess prior to that, just to give you some additional information on travels. I was invited back in the days when [*] strictly communists were in charge, to go to Kharborovsk that's clear over in the Eastern Soviet Union. And so, you know, I flew out of New York, Paris, Moscow, clear across in Russian airlines to Kharvorovsk from Moscow. Along the way they lost my suitcase. [laughter in background] I got there and didn't have a suitcase the whole time I was there. It came back three months later, safe and sound. [laughs]

LU: Probably, probably a little microphone in there _____

DRJ: Well, you know that was what was interesting because once I was in Kharvorovsk and you'd go out and walk, you'd have a communist person following you all the time. Then I was running at that time, so there was always somebody at my tail. [laughs] [laughter in background] And it was a meeting, of course, of scientists there from the Soviet Union in some ways dealing with the forerunning of FGGE [transcriber's note: FGGE, First GARP Global Experiment (1978-1979), SSEC was selected to archive satellite wind vectors from cloud heights, Suomi played key role.] [*]

0:13:37.6

LU: Could, just with respect to Yugoslavia. I think there's one other name I'd like to just bring up is Janjic [transcriber's note: Zavisa Janjic, Serbia/US meteorologist, awarded the 57th International Meteorologial Organization prize in 2013]

DRJ: Sure.

LU: Did you meet Janjic during this period as a student? He was a student then and of course he went on to do a lot of modeling work that we referenced as we worked.

DRJ: I probably did, although his, because his major professor, a person who did the Steppe Mountains. Who was? What was his name?

LU: I don't

DRJ: Mesinger? [transcriber's note: Fedor Mesinger, Serbian atmospheric scientist]

LU: Was that his major professor?

DRJ: I think. I believe that's right. Or they worked together at that time.

LU: They worked together.

DRJ: Right.

LU: There's a tremendous competition between the two of them now.

DRJ: That's true. When they came here to this country.

LU: Right. Right.

DRJ: Mesinger was the one who did the ["step" mountains *]. [transcriber's note: step-mountain coordinate]

LU: But Don is the one who did some work on the development of the hybrid model and the pressure gradient. Don introduced me to Janjic's work. And then you moved forward in time

and Janjic actually now works at National Centers for Environmental Prediction, part of the Weather Service. He just got the WMO prize. But my first introduction to his work was through Don. So.

DRJ: Well.

0:14:48.2

TR: He works for you?

LU: He works for the Weather Service now. Yeah.

DRJ: He worked for Louis. [laughs] [laughter in background] _____ at NCEP. [transcriber's note: National Center for Environmental Prediction]

LU: He's a, he's a kind of individual where you know

DRJ: Well, he's an intellectual giant.

LU: Yeah, he's an intellectual giant so you just make sure that you create the environment that he can get his work done.

DRJ: And of course he had a tragic accident

LU: Tragic, broke his back.

DRJ: he broke his back in Yugoslavia, broke his back and is confined to a wheelchair.

LU: Right. Right. Yeah.

DRJ: That, so there was these links with Mesinger and particularly, and to some extent Janjic.

LU: Right

DRJ: when I was there for that four years, half time, quarter times, always talked to him.

LU: Right

DRJ: And his wife.

LU: Right. Georgia.

DRJ: Georgia.

LU: Right.

0:15:36.2

DRJ: And we exchanged congratulations to him on the awarding of this prize. And he responded and his wife did, too. Then another thing happened, well getting back to Roberto Frassetto. He and his wife came by here and I would see them every once in awhile, but they visited me here in Madison at the university. And he became very active in saving Venice from air pollution and that part of the world because there was all the deposits of acidic air pollution on all our famous cathedrals and what have you and that was one of his main activities. OK, I won't go into that. There was another meeting in Bulgaria, which I attended when communists were still in power. But there was no effort in some sense to follow me like they did when I was in Khabarovsk. Why I guess that was just natural when you get into the deep part of the Soviet Union. There was a fascinating thing that happened in Khabarovsk. First of all, one of the leaders or organizers of the conference took us around to see [*] the Amur River. You could look over the Amur River and then they'd say you know we have this territory to the south that we're struggling and fighting over with the Chinese. And we don't know how that's going to work out. Well, two days later I went on an official tour with just a representative from that area and I said well I understand you have this problem with struggling with the Chinese. Oh no, that's Soviet Union territory. That's absolutely Soviet Union territory. [laughs] [laughter in background] So there was an official version and then of course there was what people did actually realize and understand.

0:17:50.2

TR: Is that were the term party line came from? [laughter in background]

LU: Again, Bulgaria was a more independent of the countries that were within the communist sphere at that time. And with, you know, you get to know more about these people after the Soviet Union collapsed, you realize how fiercely independent they are.

DRJ: Well, they weren't, Soviet Union didn't invade them and take them. They just said you're part of our country.

LU: Well, they, yeah, they. I think there was more

DRJ: Not like Poland though. [laughs]

LU: Right. Right. There was more a respect for and they just didn't want to over reach I think into the Bulgaria, Yugoslavia domain.

DRJ: Well, Yugoslavia has a mixture of various ethnic backgrounds. It's amazing.

LU: Yep. Right.

0:18:43.7

TR: Don is there anything else you want to address before we get into today's topics.

DRJ: Well. Well let me go back to Namias and his colleagues there. Chester Newton and his wife [, Harriet I knew *] real well. Chester had been and was at NCAR so we would visit with him. And eventually [*] he transferred the editorship of the Monthly Weather Review to me so I took up a three year term as a chief editor and I had three colleagues as associate editors. [transcriber's note: Johnson was editor for 1978-1980] And John Hovermale and John Cahir and now I can't think of the third one for the moment. [transcriber's note: James C. Frankhauser] So that lasted for three years and that's where John Stremikis was involved because he was working with me and making sure that we were able to carry out the functions of being the chief editor. [transcriber's note: John R. Stemikis was Assistant to the Chief Editor] That then led to eventually to [being the *] chief editor of the monographs of the AMS and of course there were several monographs which emerged at that particular time. [*] I wanted to cover those things to [ensure that those names were not lost *]. The relationships with Chester Newton were particularly warm, who was out of the University of Chicago and I'm sure his Ph.D. advisor was Rossby. You knew Chester

0:20:24.6

LU: Yes, and Chester was the editor of the Monthly Weather Review when posed my first paper on the gravity waves.

DRJ: OK.

LU: Which Don and I had an interesting interaction with as I moved up [laughter in background] and Chester was the type of, I mean he's written books and well known synoptic dynamic meteorologist. He, the reviews on my first paper were mixed towards negative because it was a very controversial area. And he took it upon himself to read the whole paper and told me here are the seven things you have to address. If you address them, you get them published. And I sat down and addressed all seven of them. Some editors don't do that. They see one negative review and they just throw it out. OK? So Chester Newton was the type that got very involved in the work that he did and intellectually involved and I always viewed him as one of the fairest individuals in the whole field.

DRJ: Well that was very true. When I was editor, if I ever got a mixed review and ended up after the second go around still a mixed review. I'd publish it because I feel that those type of questions should be out there. As long as they were

LU: Well posed.

DRJ: good questions. Well posed.

LU: well posed and yeah.

0:21:45.0

DRJ: Then, I guess now we can go ahead. One thing about Adrian Tuck, who I mentioned I became familiar with in '85, we kept contact all the years and he led this work on the Southern Hemisphere stratospheric vortex and diagnostics of the circulation modeling of it, etcetera, etcetera. He's recently published a book on atmospheric, call it atmospheric turbulence because he's had very high resolution measurements of temperature in the vertical and note that it's not always indicative of a static stability. [transcriber's note: Tuck, Adrian. Atmospheric turbulence: A molecular dynamics perspective. Oxford University Press, 2008] He currently is back in England, taking care of a ninety six year old mother. I presume he's still there. That was true, let's say, over half a year ago. One of the results of being involved with that [effort *] was that [both Dorothea and I *] got to spend two weeks in Christchurch [*] during the field aspects of that particular program. Now I've been to New Zealand, been to New Zealand many times for various reasons. Maybe not quite as much as Australia, but just a little side light, if we did not have any relatives or friends in this country of all the places I'd move to New Zealand. [laughs]

LU: I've heard that from other people, too. [laughter in background]

DRJ: The people are delightful. We, we actually had a small caravan. We traveled around and they would always help you. Ended up having to start out on this week adventure traveling in the caravan and we had purchased a night's lodging south of Christchurch, I can't think of the city right now but it's a city named after somebody, someone in Ireland. In any event, we [went *] there and they says you know we're sorry but we don't have room for you because we have a family here that's having health problems and they can't move now. But they said we have alternate lodgings for you, reserved, you know, several blocks away. Went there and actually, yeah, they did. But they hadn't transferred the money [for the reservations *] that we had, but the people just accepted our word that we were covered and we stayed there. Now you don't find that any place. [laughs] You know that's one of the reasons, too, is because the people are very open and gracious. And I guess we can now turn to the earth system science education.

0:25:01.6

TR: Great. So that was one of the things we want to talk about, was your involvement in the acronym ESSE, Earth System Science Education.

DRJ: Well, I noted in this master plan for the earth observing system that maybe there was a problem that they hadn't really laid out anything.

LU: And this is the one that was

DRJ: This is a Bretherton [transcriber's note: Francis Bretherton]

LU: Bretherton report

DRJ: report. Right.

LU: And Bretherton actually succeeded Suomi as the director of the Space Science and Engineering Center for a period of time. [transcriber's note: Verner E. Suomi (1915-1995) joined the University of Wisconsin-Madison Department of Meteorology in 1948, father of satellite meteorology]

TR: OK. So is it this report here? The one on touching our changing planet or was it another.

DRJ: Oh, it's all part of the same sort of thing

TR: OK. All part of the same sort of thing.

DRJ: But here is the whole report. And it, you know, covers a diverse area of many disciplines that were tied up essentially with the earth system. And this one page, I'm just going to, that's the one page

TR: Oh, then where the green is?

DRJ: green

TR: There maybe

0:26:08.8

DRJ: And there's only one paragraph here [in the Bretherton document *] on education. So by this time I [was *] pretty well involved with USRA as a chairman of their earth science activities. [transcriber's note: Universities Space Research Association] There was a president by the name of Paul Coleman who was quite well known in scientific circles. [transcriber's note: Paul Coleman, UCLA, emeritus professor of space physics] He had been head of the Alamos Laboratory. [transcriber's note: Los Alamos national Laboratory] He had gone back to UCLA to head a space science venture there and Caltech, oh, UCLA, sorry about that. These things are hard to sort out sometimes because they're closely tied to each other. But knowing Paul, etcetera, eventually he wanted me to join USRA because by that time we had written a proposal to NASA to have them fund [an education effort *] for various universities. The universities themselves would have to write proposals [for only two courses they were required to offer *] but there was money there for support of a TA for courses. They were required to offer a survey of earth system science, let's say [*] at the freshman, sophomore level, but it had to be taught by at least two professors from different disciplines. And then they had to also offer a senior level course, [in which the *] emphasis would be on attracting scientists, mathematicians as seniors in undergraduate work to be exposed to earth system science [with the *] hope that some of the them would eventually pursue graduate work in these areas. Originally NASA was going to [provide *] funds essentially for five universities but because of Shelby Tilford in particular and others in that office they [funded for some twenty universities *] over a five year period. We'd bring them on, so many each year. Now after the first five years there was a second five years which [were for another twenty universities *] and then a third five years [was for *] were

another twenty. So there's about a total of sixty universities. Now you end up having a fair number of faculty from different disciplines, a broad variety of disciplines brought together that way in workshops once or twice a year. [The program *] was really quite successfully, at least it was considered to be a success I think it, it might be say [the forerunner of *] the STEM program. We didn't call it that. STEM wasn't being labeled at that time, Science, Technology, Engineering, Mathematics, but towards the end of that after working with NASA and both NASA directors of various laboratories and the university, then we had a meeting at NSF because they were quite familiar with this work going on. [transcriber's note: National Science Foundation] And sitting around the table one of the other people [*] was John Snow. There was this thought that well, we should, NSF should take up that sort of thing and the community as a whole. And that really I think was the beginning of STEM. When you first did the four letters from the various disciplines it was SMET, Science, Mathematics, Engineering, Technology, but they couldn't have that, so they changed it to STEM and Mathematics got reduced [to the end of the acronym. *] [laughs]

LU: Acronyms rule. [laugher in background]

0:30:26.9

DRJ: [laughs] That's right. Now that of course has been, what should I say, expanded tremendously, but in the letter in which I was nominated for the AGU award, they noted that we had over a hundred thousand students that had been exposed, those classes were still going on in most of these universities. [transcriber's note: American Geophysical Union] They were really quite well accepted and that led eventually to an award by the AGU for education. I'd received one I think that was brought up yesterday from the AMS some years earlier. And they, you know, that just fit within my background naturally, that [in *], really one of my primary interests, always was and [always will be in education *]. I take some satisfaction in the fact that I had these opportunities to work in this area all these years in education in this university and among the larger community.

0:31:48.9

TR: Oh, go ahead.

DRJ: Now some of the people who were involved with [the ESSE program *]. One of the first individuals, University of Michigan, was Tim Killeen. [transcriber's note: Tim Killeen, director of NCAR from 2000-2008]. Tim later became the director of NCAR. He went on to lead the fairly large effort in NSF and now is in some place in New York.

LU: He's in Albany.

DRJ: Albany.

LU: I don't know which, they have a number of institutes there now associated with the university.

DRJ: And he's heading that.

LU: Right.

DRJ: So, his wife was Roberta Johnson who did a lot of work in education but from the solar space physics side. I got to know both of them quite well. And haven't seen them for a couple years, but they were always a delight to visit [*]. He once said in a lecture at NCAR that his experiences with the ESSE program was really what brought him into this area. And there are other cases like that. The ESSE program, as I said, prospered. We had workshops in Alaska. We had workshops in Boulder. We had workshops all over the country. I don't have a record of those here, but they were at least once every year or twice a year. And then the people who were coming there would relate their experiences to the students in the class room on subjects that they were emphasizing.

0:33:34.7

LU: I was curious as to how Russ Schneider linked into this. Wasn't his, part of his interaction with you as a support person here at the university out working with you on these education

DRJ: OK. A precedent to this which really happened within Space Science with the capabilities to use computers to portray the evolution of imagery, both in terms of satellite images as well as predicted fields from atmospheric models. We set out with NSF support from Gene Beirly in particular. [transcriber's note: Eugene W. Bierly, president of the AMS in 1984] I should mention Gene. I will. The effort were to use a VCR and produce these evolutionary fields on tapes and we had Russ [Schneider *], John [Stremikis *] here. The other [person *] involved was Chuck Wash. [transcriber's note: Carlyle H. Wash. A momentum circulation budget for an extratropical cyclone. University of Wisconsin, Department of Meteorology, 1975. M.S. thesis. And Diagnostics of observed and numerically simulated extratropical cyclones. University of Wisconsin Department of Meteorology, 1978. Ph.D. thesis.] There's a list of them that worked with me. At that time, they were usually my senior graduate students and or post-docs. And they were then being supported through creating these VCR tapes. I don't know how much we've talked about Tom Whittaker, but I think he did come up, but he had been my programmer

LU: He had been there awhile.

0:35:18.1

DRJ: Well, he had been programming for all this scientific analysis that we did in terms of cyclones and planetary circulation, and taking discreet information from radiosondes and surface and actually creating fields before we even did that with numerical models. And he got attracted to using McIDAS as it was being creating and essentially then when I joined SSEC, he became

an employee of SSEC. [transcriber's note: Man-computer Interactive Data Access System, developed at SSEC, beginning in 1972] [transcriber's note: Space Science and Engineering Center, University of Wisconsin-Madison] He moved off on his own in many ways with much more freedom [than he had *] in working with these focused efforts on angular momentum of the cyclones and/or evolutionary fields of models. We ended up producing five tapes. And they were, I think, quite popular. We had over a hundred universities from this country and abroad actually buy them. They were nominally cheap, only the cost of the tape itself, thirty dollars or something like that. And we had many favorable, positive reviews because this is the first time they had those capabilities to show that in the classroom.

0:36:36.7

LU: The reason I brought up Russ Schneider's name, I think if he wasn't your last Ph.D. student then he was one of the last.

DRJ: Well, he wasn't the last, but he was near the end.

LU: And

TR: couple

LU: Yeah. And he is now working in the weather service for, he is the director of the Storm Prediction Center down in Norman, Oklahoma. So Russ and I are actually going to be, we're cohorts in the opening presentation at the colloquium that we're having for Don this coming February [transcriber's note: American Meteorological Society Annual Meeting, 94th, Atlanta, Georgia, 2-6 February, 2014. The Donald R. Johnson Symposium is one of the planned conferences and symposiums that are scheduled for this meeting]

0:37:15.8

TR: OK. And one thing that either you gave to Jean to give to me or Jean just gave it me, was I think it was like a ten year recap of college and university, the essay program. [transcriber's note: Jean Phillips, librarian, Schwerdtfeger Library, Space Science and Engineering Center, University of Wisconsin-Madison] It's like you co-wrote it with someone?

DRJ: Right. Martin Ruzek. This was a summary of it. We have a paper published in the AGU, that's not quite right. But there is a journal for geoscience education. Yeah.

TR: So this is a summary of a larger article that appears

DRJ: Probably.

TR: OK. Because this says IEEE

DRJ: Oh, that went in, no, that's different. That, we've always put out every year or two something like this to the various journals and what have you, to keep this effort in the forefront so to speak of what we were doing.

TR: Right.

DRJ: So there's a whole list of those [involving *] Martin Ruzek who was from Wisconsin and still has a family up here in central, northern Wisconsin, still working for USRA at Columbia, Maryland. He was the [*] key, sort of the key individual that make everything happen on a day to day basis and let us maintain the relationships with all the people. I was involved certainly, but you do a lot of traveling and everything [while *] you're still trying to keep a research effort going on here in terms of development of a hybrid isentropic model, plus everything else, being president of the AMS and the things that follow that over a four year period. You have to have someone in that organization there, located at Columbia University, Columbia, Maryland, so that this effort moves on within the headquarters located there. And of course, I never moved to Columbia.

0:39:21.9

LU: But that's where I live as it turns out.

DRJ: [laughs] now

LU: which I didn't even realize USRA was located there until Don came to NCEP and you know was part of that effort that we talked about yesterday. The other, you know, he's talking about splitting his time and he was splitting his time with USRA and I realize some time into the appointment that he was actually, that the USRA headquarters was right down the street from where I, where I live. So if I ever do retire, Don, and [laugher in background] I get involved in USRA I could roll out of bed and go to work. Right? [laughs]

DRJ: [laughs] Right. And they'd probably welcome you. [laughter in background]

LU: Instead of these long drives that I've been blessed with.

0:40:09.8

DRJ: I don't know I want this in the record, so I'm just going to talk about it and then you can [decide *]. For example, USRA would sponsor the programs for the various NASA centers. They'd write proposals and now we're talking about the effort where they would take care of the administration and contact the universities to get post-docs and scientists involved, as well as scientists who were located at the NASA centers, but on the payroll of USRA. Of course, the money coming from NASA. And we were successful [in maintaining that effort at Goddard for let's say ten years but *] that's not the ESSE. [I was *] quite active with Milt Halem and others [at Goddard *]. [transcriber's note: Milton Halem: currently holds an Emeritus position as Chief

Information Research Scientist to the Director of the Earth Sciences Directorate at the NASA Goddard Space Flight Center] He was a scientist there. And of course we got to let's say [the year *] 2000 or something like that. We put in another five year proposal. It was twenty one plus out of twenty one questions actually they [judged positively on *] the proposal on an evaluation. And we lost it because it was interference you might say of some congressional, senator who wanted it to go to University of Maryland, Baltimore County. They only had fifteen positive. We objected and of course it was turned down. Now I guess it turns out they have it back, that is USRA has it back.

LU: Right.

DRJ: It's one of those things you can't actually change things like that. It happens.

0:42:15.7

TR: Right.

DRJ: But it's about that time, too, then I get this chance to become affiliated with NCEP on a half time basis. However I'd better finish our efforts in ESSE. They did continue and as I said, you know, we had workshops particularly at the key places that attract people. One of which was in Alaska, Fairbanks, Alaska. University of Alaska was involved with [the ESSE program *]. And so there was this thrust, in fact we wanted to focus on the Arctic Ocean, too, as one of the key things that people would be working on. Then of course we lost the support for the ESSE programs and these things never happened at that time. What else happened under ESSE? I got to know a large number of faculty from different universities with different backgrounds and it was a real delightful experience to be able to talk about things like I had originally studied in chemistry or mathematics, etcetera.

0:43:42.8

LU: You mentioned John Snow.

DRJ: Yeah.

LU: who has obviously become a more prominent figure within the larger enterprise from many different angles, education, some research, his, the links that they have with the private sector, etcetera. So anybody else in the academic community that, you know, you want to just mention? That you really linked in with, based, you know, through this program?

0:44:15.3

DRJ: Well, I need to get the list of names out to be honest with you. There were several others and one of them was Bob Ford who is out in California. But we have lost contact the last five, six years, although he [wanted me to *] sign up for linkin or whatever it's called. [transcriber's note: Linkedin, a web based social connection site for profession occupations] [laughs]

[laughter in background] I make a practice of never signing up for either Facebook or Linkedin. [laughs] [laughter in background] But the gentleman also who was at Penn State, went to NCAR and became the director and then off to FSU [transcriber's note: Florida State University]

LU: Oh, Eric Barron. [transcriber's note: Eric Barron, director of NCAR from 2008-2010. Became 14th President of Florida State University, Tallahassee, Florida in 2010.]

DRJ: Eric Barron. Right.

LU: Right.

DRJ: And I never was that close to Eric Barron but we certainly knew each other.

LU: He was a very articulate, he still is, very articulate person in research education and it's no accident, I didn't think it was any accident that he became a university president.

DRJ: No.

LU: So, he has that kind of gravitas.

DRJ: But he was a graduate of FSU as I understand it, I believe originally, that was one of the reasons. No, that wasn't the reason he got the position, no doubt about that but it didn't hurt.

LU: It was one of the reasons he was probably attracted to

DRJ: To, that's right

LU: he had for going back.

DRJ: And you know I spent a fair amount of time at Florida State University on, during the period of FGGE. I'll mention an individual there by the name of Krishnamurti who is quite a well-known figure in numerical modeling. [transcriber's note: T. N. Krishnamurti, professor of meteorology at Florida State University] Actually Krishnamurti got his Ph.D. at the University of Chicago and while I was still [in *] the last year or two of my graduate education, I was chairman of the graduate school student association. [*] And Bob Fox was my colleague as vice president or whatever it was. Bob Fox became the executive director after Tom Haig left and was associated here in SSEC for many years with Verner Suomi. Briefly I'll just also mention this, that when it came the time for Tom Haig to think about stepping aside and Verner Suomi was over in the University Hospital with heart surgery, there was [a question of *] a who are we going to get to be the next executive director. And I and Tom [Haig *] talked about Bob Fox who had retired from the Air Force as a colonel. But [earlier *] he had come here for his master's degree and then he eventually came for his Ph.D. I got to know Bob both times of course. [During his master's studies *] he worked with me to get Krishnamurti to come here to give a special lecture on this tropical jet as an outflow from essentially the deep convection that

occurs in the western Pacific. And so I got to know Krishnamurti well, as well as later on his wife Ruby [while visiting Florida State *] and we had interesting exchanges on those years. And anyways Bob Fox was a name that both Tom and I agree on [to come to SSEC as Executive Director *]. Bob was one of those individuals, he came here to do his master's in the Air Force and he worked on so he actually had passed his language at that time for a Ph.D. Then before this event of getting him to come here to be an executive director, I had written Bob and said when are you going to come back and do your Ph.D. because we had kept in contacts someway. And then he came back to do the Ph.D. while he was in the Air Force yet. Finished it in record time and went back and then retired. Well, then he was on the verge, I guess, of [deciding *] where he could retire. So we suggested [Bob Fox *] to Suomi, here's your new executive director, Bob Fox. And basically Suomi accepted that and after Bob Fox, even though he didn't know him that well. Bob and Suomi worked very well together. So Bob Fox has been, is a key figure here in this umbrella so to speak of what went on in SSEC.

0:49.15.5

TR: So executive director of SSEC

DRJ: Yes, that's right.

0:49:18.9

LU: So let me just say a few comments from the perspective of a student during this era. What always amazed me about SSEC and I didn't know Don's full role with respect to Bob Fox, so I feel like I need to bring this up. As a student we were, I interacted with the folks from SSEC. The students of Vern Suomi, like Fred Mosher, Don Wylie and others. And I always marveled at Suomi, you know, being this, you know, idea guy. You know, he had an idea a minute. And there was always stories about him coming back from conferences and, you know, another device that he felt needed to be brought into SSEC and they'd figure out how to pay for it later or something. But the executive director. So you think of Suomi as the velvet glove and then the executive directors were viewed as the hammer. You know, they ran the day to day. They made sure that they, you know, they operated to the bottom line. They were very good at that. And you know everybody talked about well you can ask Suomi but you're going to get your answer from Haig or you're going to get your answer from Fox. So you know, now that I'm in a top administrative position I understand completely the dynamic and why that's necessary, but as a student it was just like, you know, people had trouble sorting out those differences. They all wanted to run to Suomi and, you know, deal with him in that regard. So Haig, Tom Haig and then Bob Fox were viewed as, you know, the hammer running the organization. And making sure that Suomi didn't have to worry about, you know, what it took to keep these six floors going other than keep those ideas coming. Keep the money flowing and we'll manage it for you and keep the people in line. And they certainly did that. They did actually in the background and they did it very well.

0:51:31.1

DRJ: Well I think there's one thing you can say about these individuals. They had been in the Air Force and essentially in positions of authority and either as colonels when of course they were in command of a fair number of activities and people underneath them, both of them. They excel at these things because of that experience.

TR: So. We have roughly forty-ish minutes. There's, there were three things we talked about yesterday that we want to talk about today. So, three things left. Joanne Simpson, you want to talk more about your students, and then if possible we want to talk more about these awards that you received. So which, it's your ball game. Which one do you.

0:52:21.3

DRJ: Well I don't know what else to say about the awards, just somebody puts you in for an award and you either succeed or not. So in the education arena that was two successes. There were a couple other, but they're awards that you sort of naturally get in some sense as you move along. I don't know what they were, but they're listed there.

TR: Yeah. It think there's one from USRA. One from AMS. And you just mentioned one from

LU: AGU

TR: AGU

DRJ: Right.

0:52:56.7

LU: Right. So the AMS and AGU meeting awards are really the pinnacle of recognition from the educational teaching perspective. I know, I know Don was particularly happy, I was there when he got the AMS award, he was particularly happy about getting the award, the teacher's award there. And I think that traces, the happiness is sincere, and it traces back to what we heard in the early part of this oral history in terms of that's where he originally was going until he got called in by his teacher. You know, he got the Air Force letter.

DRJ: Well, but you look at it this way that the Air Force gave you this background to deal with the weather and circulation, atmospheric circulation in an unusual way that you do the analysis yourself. So you have this insight of how it's applied in a practical sense and what people have to sort out. And then of course you get into the university where you have to teach others about those things and it's a background that I find that has served well. I might go back to mention Miss Anna Marm. [transcriber's note: Anna Marm served as professor of mathematics for 33 years at Bethany College; in 1962 a new female residence hall was named in her honor] I think I've mentioned her as one of the key individuals who said if you learn your mathematics you can do anything you want to do. And that was a thing she would say from the beginning to the end.

She got me into the meteorology by this Air Force thing and then of course when we would come back to town years later after being in Europe, we were invited over to her house for tea. She never married. She was a beauty queen at KU so she was not from that stand point sort of off by herself, but she had made it her life's mission to teach mathematics and educate. [transcriber's note: University of Kansas] And they have a building named after her at Bethany College. So you know those, those are the type of people you, you enjoy having time with and I'm just a student of hers.

0:55:22.8

LU: Do you want to talk about Joanne now?

DRJ: Sure. Well Joanne Simpson was an interesting individual in the fact that I think she was the first Ph.D. woman in meteorology.

LU: In the United States.

DRJ: In the United States.

LU: Because we found that Lettau's wife, what's her name, was [transcriber's note: Heinz H. Lettau (1909-2005) professor, University of Wisconsin-Madison, Department of Meteorology from 1958 to 1980] [transcriber's note: Katharina Lettau (1910-2008)]

DRJ: OK

Voice in background: Katie

DRJ: Katie?

LU: Katie Lettau had a Ph.D. from Germany

DRJ: Right.

LU: in the mid-'30s.

DRJ: In meteor, atmospheric?

LU: In meteorology

DRJ: Is that right? I should have known that.

LU: According to Ralph Peterson, that was

DRJ: Probably right. Yeah.

LU: So, anyway

DRJ: Yeah. OK. She, I believe, was [Joanne Simpson *], a student of Rossby

LU: Yes.

DRJ: At University of Chicago. She, well had a couple different husbands along the way, so she was an attractive woman from that stand point. Delightful personality, but she was serious in the way in which she approached atmospheric science. And she went on to UCLA where she excelled in dealing with tropical meteorology, convection, its impact on atmospheric circulation. She had, you can go and find a very nice figure of the vortex roles in the PBL that [*] form by shear in the boundary layer and then turn upward to be the vortex of [*] tropical cyclones for example. [transcriber's note: Planetary Boundary Layer] She really could actually put these things together in a marvelous sort of way. Now her husband's name at UCLA was Malkus [transcriber's note: Willem Malkus] [transcriber's note: according to her obituary in the Washington Post Joanne Simpson had also been married to Victor Starr. Her marriages to Malkus and to Starr ended in divorce.]

LU: Right.

DRJ: I believe that's right. And then she married a fellow by the name of Simpson who was a key meteorologist in his own right. [transcriber's note: Robert Simpson, meteorologist, head of the National Hurricane Center] So then they were together for many, many years. Now she passed away. He's still living I believe.

LU: Yes, he's going to be one hundred and one this November. I was at his hundredth birthday party last year.

DRJ: Well, I probably got to know Joanne, I'm hesitating to say here. She would come as I said I think earlier, to the University of Wisconsin because she had a son here who was on the faculty in engineering, I believe. [transcriber's note: David S. Malkus, emeritus, College of Engineering, Engineering Physics, University of Wisconsin-Madison] And that would be an annual or biannual visit. And because she know Verner Suomi so well she's always come over here and give a lecture. And that was probably when I first met her, [before she *] moved from UCLA to Goddard. [*]

LU: Actually she went

DRJ: Oh, Virginia

LU: She went from UCLA down to the NOAA lab where she was one of the leaders of the Storm Fury with Bob and I think that's when she met Bob. [transcriber's note: Project Stormfury, experimental program on hurricane modification (1962-1983) by the Hurricane Research Division of NOAA] And then they wound up at the University of Virginia.

DRJ: That's right.

LU: together. Right.

DRJ: But then she went to Goddard

LU: Then she went to Goddard Space Flight Center, the year after I arrived there.

0:58:50.1

DRJ: OK. And since I was going to Goddard at least probably twice a year, maybe even more frequently, certainly three, four times. There was another person there by the name of Dave Atlas. And Dave Atlas and Joanne got along well. I had known Dave Atlas for some time because he worked in [the development of *] radar and was a key person in using radar for watching atmospheric circulation. Dave Atlas had gone to Boulder to be part of NCAR and I met him there, too, also talked to him. And he enjoyed that. He decided that he wanted to move on to someplace else where he had more freedom on his own right. And so he moved to Goddard because they, I think, [*] they were now going to have really an atmospheric science program.

LU: Right. This Goddard Laboratory for Atmospheric Sciences and he became the first director of that. Basically spun it up. With Milt Halem coming from New York City [transcriber's note: Milt Halem, currently emeritus Chief Information Research Scientist to the Director of the Earth Sciences Directorate at NASA Goddard Space Flight Center]

DRH: Right

LU: running the computer branch and then he eventually recruited Joanne to come in and run the severe storm branch which is where I was located. So that's how I got connected to Joanne.

1:00:30.9

DRJ: Well, in my visits there we would, I'd get a chance to talk to her some, not that we have any really [*] close a connection at that time. However she became president of the AMS and then there was Jim Mahoney [after which *] I became president. And there was an overlap now between the presidents because you have to serve on the executive committee for four years.

LU: Four years?

DRJ: And there was a, some sort of overview or let's say what's going to be the new direction for the AMS and Joanne and Jim had a strong hand in determining how that played out. And I got involved to some extent [*] at the tail end of it. And coming in as president then you had these various themes that you might emphasize. The thing I emphasized was water [at a *] special annual meeting, that was the special emphasis. And I invited people like Tom Malone and also there was George Benton. All of these [had a *] strong connection either with the academy and or NOAA itself. George Benton service [in NOAA *] actually helped me.

LU: He was, he actually got up to I think the, into the Secretary of Commerce office.

DRJ: Right. Eventually.

LU: And then he was also one of the leaders on the GARP, you know, doing the whole that spin up of that whole first global program that involved this worldwide effort that we discussed yesterday. [transcriber's note: GARP, Global Atmospheric Research Program, a research program organized by the World Meteorological Organization (WMO) from 1967 to 1982] He was one of the leaders in helping to pull that together.

DRJ: But there is still another individual [Robert White *] who moved eventually from NOAA to be the briefly a president of UCAR for a while and then, well we can find that.

LU: Yeah _____

DRJ: So there was this interaction with some of the key people that I was able to enjoy in this role of being part of more of less of the USRA activities now, visiting all these various labs including Los Alamos, a laboratory out [near *] San Francisco, and Goddard and then the one down in Virginia.

1:03:18.0

LU: You must have made the impression on Joanne because that letter that you showed yesterday that she wrote to you after you wrote the Advances Geophysical Sciences paper on the general circulation, was a very good letter, a very nice letter. I think a very important letter in terms of her and what she gained from that article. Was there more interaction after you wrote that, that report with Joanne with respect to general circulation? She was very active in TRMM. [transcriber's note: Tropical Rainfall Measurement Mission, launched in 1997] She was the science leader of TRMM. Probably the project leader. Was there any more interaction on that?

1:03:57.9

DRJ: Well there was always an interaction as long as Joanne was in Goddard. I mean, you know, we'd go there and so, I mean, who knows exactly the timelines of what we discussed at that. But, in going to Goddard for example I showed essentially these video images of atmospheric circulation in isentropic coordinates and Milt says well did you really [visualize them *]. And I say, yeah. [laughs] [laughter in background] because they're, they show you [from the simulation the evolution of an extratropical cyclone, as a *] sort of mountains [*] where the entropy pokes itself up, valley's where it's down lower in the atmosphere and then they swirl around this front, etcetera, tied up with the angular momentum that we now deal with as vorticity but the two properties are essentially so closely related because [*] vorticity is a measure of the angular velocity and angular momentum is likewise a measure of angular velocity. [*]

LU: Dave

DRJ: Dave

LU: Dave Altas.

DRJ: Dave Atlas, right. You always gain something.

1:05:40.7

LU: And now from the, as a student of Don, who actually, you know, encouraged me to go to Goddard and I mean I was looking at, I was here for one year post-doc after I finished my Ph.D. with Don. And I was a post-doc, actually I was officially a post-doc within Space Science and Engineering Center for one year.

DRJ: Yeah, sure.

LU: And then this opportunity came up to go to Goddard and to go to a new place rather than going to NCAR which was becoming a little bit more you know siloed [transcriber's note: siloed?], I guess is a good expression to use. I went there. I was very chaotic. I was hard to hold on that first year. But then Joanne came and I recognized her as an important person in the field. I didn't know that much about her work and I certainly didn't know about her abilities to organize and lead and defend a group of scientists, and you know provide the top cover. As I said later she filtered the high frequency noise from Dave Atlas and above.

DRJ: [laughs]

LU: But when I reflect on my career, working for Don and then working for Joanne for what turned out to be ten years. You know, I worked for two giants. And it was very productive research career and she turned out to be very encouraging to me when I decided I wanted to go over to the weather service and have a better understanding of the operational world because I was very interested in the research to operations, societal benefits and things like that. And there were people within Goddard who would say no, no, stay here, you can work your way up here. And so, so here are these two individuals, Don who encourages me to go to Goddard, and Joanne who once she realized what I wanted to do was fully encouraging to have me go over. These links, I guess it doesn't happen by accident but I'm finding this very interesting actually.

TR: Two pretty good mentors it sounds like.

1:07.52.0

LU: Oh, yeah. Yep. Yep. And Joanne actually just being the first Ph.D. here in the United States and working for Rossby who was actually dismissive of her research topic, when she tells, she told the story before she died and she wrote it in a few of her memoires that Rossby, when

she told Rossby she wanted to work on the tropical clouds and the turrets of the transport of energy and momentum and he said well that's a nice topic for girls, you know, clouds.

DRJ: [laughs]

LU: And he was kind of dismissive about it. It really, it more than annoyed Joanne. I think she took that, you know, because she, and she was an influential person in the field in getting more women involved right from the get go. So anyway, very, very influential person.

1:08:43.8

TR: Well it looks like we have about twenty minutes left. I know yesterday you a couple of time pointed at your sheet of students. I think you wanted to speak a bit more to that.

DRJ: OK.

TR: Do you want me

DRJ: Give me another five minutes to finish up.

TR: OK.

DRJ: I'd like to add some things.

TR: Yeah, that's fine. We could do.

1:08:59.9

DRJ: Speaking of Rossby. There's an interesting history for this particular person dealing with his relationship to the Bergen School of Meteorology. [transcriber's note: mathematical and data analysis approach to meteorology founded by Vilhelm Bjerknes, et al, in 1917] Many of them will claim that he, his major professor was Bjerknes, the second Bjerknes. There's three Bjerknes. There's a grandfather. There's a father and a son. So this would be V. Bjerknes I believe. [transcriber's note: Velhelm Bjerknes (1862-1951) Norewegian meteorologist] [*] That's not true, because he was doing his [graduate *] work [*] in Sweden, probably Uppsala, I'm not quite sure. I'll have to check that out. And here he's getting close to [*] finish his Ph.D. So he got an appointment essentially at the Bergen School of Meteorology which is where now the intellectual giants that created the extratropical cyclones, global circulation, etcetera, resided. And he spent a year there but then he went back all of a sudden because there had been some sort of conflict that happened. This is related by Sverre Petterssen and then he went back [to Sweden *]. [transcriber's note: Sverre Petterssen (1898-1974) Norwegian meteorology] Well when you actually look at Rossby's ideas and the fact that we have this middle latitude wind maximum from two different aspects, one has to do with conservation of angular momentum coming from the tropics moving poleward and presumably if you had it conserved entirely you get a thousand meters per second by the time you got to fifty, sixty degrees. The other had to do with mixing

other vorticity which [in mid-latitudes *] leads you to a different profile like this and this combination of the two give you a wind maximum. Well of course, it's really neither one but [that likely *] didn't come from Bjerknes, that came from his mechanical engineering and mathematics education as far as I'm concerned from Sweden. Because he's one of the first people who actually put together this as a combination of different processes, the way in which [circulation moves *] around. Just though I might add that. He of course emerged then by coming to this country prior to World War II and working in Washington. He developed these connections with [now NOAA, U.S. Weather Bureau now the National Weather Service, [*]. And then of course he went on to the University of Chicago. He was an intellectual giant, no doubt about that because he came up with these things about air mass types

LU: Ocean circulation _____

DRJ: ocean circulation

LU: He was

DRJ: But I don't see that way in which they brought circulation in like [*] the Bergen School.

1:12:12.0

LU: Now it's interesting, I think it's worth noting here why Bergen in Norway. A number of these people including Bjerknes actually were establishing themselves in Germany before World War I. And then of course World War I happened and folks, the intellectual giants of the Scandinavian descent especially, moved up into Norway, went back home essentially. And then spun up the Bergen School immediately after World War I, or during and then after especially. So it became the focal point. There was a real focus of the Bergen School on cyclones. One of the drivers for it was can they predict cyclones over the open waters because they were losing so many fisherman to these storms. It was really almost a national crisis, the number of people that were being lost. And that, Petterssen got interested in that from that perspective. And they clearly focused on that on a smaller scale. Maybe there was some, I don't really know, this is the first I'm hearing about Rossby coming and going, but Rossby always thought more globally. Maybe that's what he wanted to do there and they were still focused on cyclones scale.

1:13:39.5

TR: So I'm hearing two B words that sound a lot alike. So the Bergen School is different though than Bjerknes.

LU: Yes. Vilhelm Bjerknes was a leading scientist at the turn of the last century. So in the late 1800s, early 1900s. Vilhelm Bjerknes, B J E R K N E S was an international giant, you know, came up. _____ was also another one of these people who, you know, from an energy perspective, from _____ momentum, angular momentum perspective, devising the circulation concepts mathematically and their applications to the atmosphere. And

then his son Jacob became more prominent post-World War I, focused on the cyclone and he eventually wound up at UCLA. [transcriber's note: Jacob Bjerknes (1897-1975) Norwegian-American meteorologist] So that's Bjerknes. Those are the scientific leaders. And then they congregated in Bergen, Norway which is right on the west coast so a beautiful location. And we had a celebration of the hundred, I guess it was the hundred year anniversary. It was in 199 …

DRJ: 1995

LU: 1995. So yes, they must have spun the school up just prior to the conflicts that were developing in Germany. But it really took off after World War I.

DRJ: It was in the '50s. That was the fiftieth year, that's when they. In 1995, fifty years before that would be '45 and that's during World War II.

LU: Two, but they actually were doing their work in 1920 on cyclones.

DRJ: But that was done in Germany I think.

LU: No, I was, that was up in Bergen. That was the Bergen.

DRJ: OK. You may be right.

1:15:32.4

LU: Yeah. So the whole idea of a front, the fronts came in after World War I and that was Jacob Bjerknes characterizing these boundaries and there was some flak about that, you know, because World War I left such a deep psychological scar. And what happened along the fronts,

TR: Right.

LU: the battle fronts.

TR: Right.

LU: And then he started calling these cold fronts, warm fronts and there was that whole discussion about that. So anyway.

1:16:00.8

DRJ: Well the, the, you're right, very much so that the motivation for that was getting support. They're going to save fishermen. [laughs]

LU: Right.

DRJ: So that was one of the practical efforts.

LU: And again, I mentioned this last night. The idea of bringing in forecast, Bjerknes who was, really knew his stuff, you know, mathematically and the physics. Built this in, conceptually into the ability to forecast from a, and using mathematical and basic physics to do that. Ran afoul with many people in the science community that this was not the way to go and Nobel was one of those scientists who or engineers or whatever his expertise was, and that's the Nobel of the Nobel Prize. And meteorology is not considered a category for the Nobel Prize for that reason. So.

1:17:03.3

DRJ: The, one other thing though you should, we should point out that J. Bjerknes while he worked on cyclones he came up with the emphasis of deep convection over the Pacific creating this

LU: Yes

DRJ: Hadley Circulation.

LU: Right. So, and he was one

DRJ: And I heard that lecture.

LU: Right. And he was one of the first. He actually influenced Namias.

DRJ: Sure.

LU: And the two of them were actually the first to show and postulate and then show to the extent that they could that the oceans and what goes on in the oceans and the ocean temperature contrast could drive circulation regimes both from a global and a synoptic perspective. And looked at things, you know, started the ball rolling towards El Nino/La Nina that we now just take for granted as part of the general basis of the way the Pacific Ocean influences the weather here for example. So they, these, that has links back to J. Bjerknes.

DRJ: One point now I want to be sure we get in and that's the influence that Gene Bierly in this arena, too, because Gene went on to NSF. When I say he went on to, because he originally was working for the AEC. [transcriber's note: AEC, Atomic Energy Commission] He had been in the Navy during the Korean War and he got involved then with Danielsen's work on stratospheric extrusions and bringing ozone into the troposphere through these extrusions. So he was familiar in some sense with Danielsen's concepts. And after he left AEC and went to NSF. I don't think there was anything in between, he also was a president of the AMS some years before me although we're the same age. But since I had maybe had some connections with Danielsen. He understood some things about isentropic perspectives. When we came to doing these [educational *] modules here locally and later on in much of the research that I had, he was above Ron Taylor and I think he was also sympathetic to the way in which we were approaching

the view of the atmospheric circulation using isentropic coordinates. And so [*], I've always had someone there that I could also [discuss ideas *]. Now Danielsen did, used conservation of absolute potential vorticity to track over some twenty days [radioactive *] of material that had actually been inserted [into the stratosphere *] by a nuclear explosion, [which was *] now brought back into the troposphere. [That led *] to essentially the cessation of free atmospheric tests of nuclear bombs. [*] Once the radioactive debris got into the troposphere and rained out and it actually led to contamination of the grass which the cows would eat and they could detect this in the milk.

LU: Strontium 90.

DRJ: So that was part of the history, too, of what was going on at that time.

LU: Yeah. I heard Ed Danielsen give a lecture out at NCAR on that whole work back _____ in 1974. It was really fascinating.

1:20:57.8

DRJ: My students.

TR: Yes.

DRJ: Well, it's hard to say how I'm going to talk about these various people because I always took pride in what they did.

TR: Right. I wonder if you maybe start with what your, when a student would come in. Was there things you wanted, was there sort of a protocol you followed with them? And I don't know maybe if Louis can. Because I mean, there's bunch of students there. We could obviously spend the next half, several hours talking about them so I don't know if highlighting a few of them. You've already sort of done that. I guess as an oral historian I'm sort of curious, perhaps maybe even change over time. Did the graduate students under you differ

DRJ: Well

TR: as your time teaching them went along.

1:21:52.3

DRJ: I really can't say. You know they were set out to [study a certain topic *], this student, that student. I let them chose their subject. I think actually I would lay out a range of possibilities. And John here can answer that. And then you know, well here's what we're working on over this range of areas which one are you interested in. They had, as I said once before, I required [them *], if they're going for a Ph.D., [to acquire a *] minor in mathematics or statistics or computer science. And I really hadn't said that much about my work in statistics but I did get tied up with a professor by the name of Grace Wahba and we published several papers

together. [transcriber's note: Grace Wahba, professor, University of Wisconsin-Madison, Department of Statistics] That had to do with the fact that we both served on this graduate research committee and walking back from one of our meetings we talked about some things that were outstanding problems in meteorology. Because I had decided years before then to do this minor in statistics, I got exposed to people like G. E. P. Box. [transcriber's note: Professor George E. P. Box (1919-2013) established the Statistics Department at the UW-Madison in 1960] In particular he came from Great Britain and had this marvelous capability to bridge real practical problems in the sciences with statistics. And he would provide us with a background material on essentially why your various PDF distributions were created for what particular problem and that was always very interesting to me. [transcriber's note: Probability Density Function] [Grace Wahba *] became a member of the academy eventually. We worked probably for four or five years and I even provided support for mine and several of her students who published a few papers together. [Just *] another dimension, I thought I would mention. That one of the reasons I guess if you're going to work in this area from uncertainty and statistics and the physical and dynamical aspects of the equations of motion, you need to have this background in mathematics or statistics [*]. So all of these students essentially in some ways or another when we came to talk we would talk about things like either the planetary circulation and what are the [processes *] that actually drive the planetary circulation or what are things that actually drive the secondary circulations and down to the range of topics including hurricanes and vortex roles and how they relate to each other. I don't really think I had a general philosophy of what I required my students to do other than to simply say let's talk about you might be interested in.

1:24:59.4

LU: I think my first conversation with Don. I hadn't had him for any classes. I was exposed to the isentropic jet stream type thing through Frank Sechrist who Don helped bring here. [transcriber's note: Frank Sechrist, University of Wisconsin-Madison, Department of Meteorology, professor from 1967-1987] It was, there was a temp, I had been doing my master's under Charlie Anderson who gave me complete free rein in the gravity wave world but I became more and more interested in this jet stream. [transcriber's note: Charles Anderson, University of Wisconsin-Madison, Department of Meteorology, professor from 1966-1986] And I sized up Frank as being somebody who was a little, too volatile for me. He could be, you never knew which mood he was going to be in, brilliant guy but he was just a lot like Danielsen I guess. So when I talked to Lyle Horn who was really a mentor for the undergraduates, even graduate students and told him about my dilemma because I really wanted to get into the jet stream and I knew of Don's work. [transcriber's note: Lyle Horn (1924-1989), University of Wisconsin-Madison, Department of Meteorology, professor from 1960-1989] I did two things. One, I took a reading course under Don. And then started discussing the, that my interest in coming up and doing my Ph.D. under him. And we did get to discuss what I would like to do and there wasn't, I didn't want to work the cyclone problem. There was so many people working the cyclone problem. When I brought up the jet stream and jet stream aspect he

immediately lit up and I still remember one of the first things he said to me is that it was one of the, one of the forecast challenges that you had when you were in Europe in the Air Force and what these jets were doing crossing the Atlantic and whether they were in some kind of quasi-steady state mode or whatever. So it was very clear he'd be interested. You know, I think the research proposals that you were doing that I was funded under had a real focus toward cyclones but he didn't use that to say hey you have to work on cyclones or you, you know, you're going to have to find somebody else to do this under. He immediately lit up and we started discussing. He just mentioned the, I think the key word was the secondary circulation. Because, you know, jet streaks have their secondary circulations and smaller scale than what he was looking at with respect to the general circulation and cyclones at that point but they were there. And so

DRJ: Well they're

LU: how I wound up coming up.

1:27:47.9

DRJ: Well they're intimately connected and I think I knew that that particular time. Actually Bob Gall, did this work on the quasi-steady propagation of jet maximums in a channel. [transcriber's note: Gall, Robert L. Prediction of a quasi-steady propagating jet core with an isentropic numerical model. University of Wisconsin-Madison, Department of Meteorology, 1972. Ph.D. thesis]

LU: In a channel

DRJ: And there you can write out a set of equations that if you've cast them in this sort of quasi-Lagrangian motion and everything relative to that, then you can write out a set of equations where the inertia terms get turned into essentially an absolute vorticity and divergence of the kinetic energy. So we knew that that was part of the problem and then as you say there are the secondary circulations. One is a direct circulation that drives the jet. And there's an indirect circulation that extracts its energy and puts it back into the cyclone. So Louis selected one of these topics, somewhat the key one in terms of dealing now in a numerical way of the jet streak,

LU: And in

DRJ: in a channel

LU: And in the case study.

DRJ: In a case study, right.

LU: Just to show you how, you know, and since naive a student can be. You know, I'm so ingrained in this and so accepting that, you know, direct and indirect circulation and of course everybody would just fall over for this. Just before I did my Ph.D. defense Brian Hoskins came and visited Don and Don had me sit. [transcriber's note: Brian J. Hoskins, British dynamical

meteorologist and cliamtologist] Brian Hoskins is a world renowned meteorologist, dynamicist and all that

DRJ: Well, he became one.

LU: Yeah.

DRJ: When he first stopped

LU: He'd actually, he actually had just got his Ph.D. under Bretherton.

DRJ: Right.

LU: And was making his name for himself at that point.

DRJ: Yeah, that's right.

LU: And I got through my, I just described everything, and he leans back and says I don't believe that these indirect circulations even exist, there's no need for them.

DRJ: Yeah.

LU: Don and I both kind of like whoa this is going to be interesting.

DRJ: [laughs]

LU: [laughs] So I spent ten years, you know, doing these additional studies after I got my Ph.D. and if Hoskins was in the audience I'd say here it is, here it is, here it is. He finally cried uncle, but

DRJ: [laughs]

LU: so you know, it turned out to be a more controversial area than I assumed it would be, you know. I knew the gravity wave work was controversial but I really didn't think this was. But that was my experience as a student coming up, you know, and transferring over to Don.

1:30:22.0

DRJ: Well, it's interesting that you recounted, you recounted Brian Hoskins visit because he denied that these things happened. It really came [in part *] from essentially an [inertial *] instability of the jet [core*]

LU: And he's referring back to _____ work with, there has to be a mean direct circulation because friction takes care of everything else, you know, turbulence.

DRJ: But of course, later on Brian Hoskins, I got to know him well, visited Reading even when he was a member of the faculty there.

LU: And a leader. He's a knight now.

DRJ: Oh yeah?

LU: _____ knighthood.

1:31:08.4

DRJ: So, and when they did the FGGE analysis, he had to then put together an isentropic data set for that. I never did really use that because by that time I was getting out of the business of suggesting these various topics for the students. My last student, I might point out was, well it says Zhuojian Yuan I guess that's it, in 1990. [transcriber's note: Yuan, Zhuojian. On the forcing of the meridional circulation within cyclones: An isentropic study. University of Wisconsin-Madison, Department of Meteorology, 1990. M.S. thesis. And Yuan, Zhuojian. The role of diabatic heating, torques and stability in forcing the meridional circulation with cyclones. University of Wisconsin-Madison, Department of Meteorology 1993. Ph.D. thesis] I had students coming from China. I had a fellow by the name of Bob Lo. [transcriber's note: Lo, Robert Chin-Tsan. An investigation of cloud distribution from satellite infrared radiation data. University of Wisconsin-Madison, Department of Meteorology, 1968. M.S. thesis. And Lo, Robert Chin-Tsan. An inference model of cloud distribution from satellite infrared radiation data. University of Wisconsin-Madison, Department of Meteorology, 1971. Ph.D. thesis.] I believe he came out of Taiwan though. I'll talk a little about Zhuojian Yuan who [*] because of this work here and visiting to China, I had a couple of different professors come from various universities. One of them was Huangchuan. I don't remember the name of the individual but she came here as an interpreter for this professor. And her interesting background is that she was not a young student at that time, maybe twenty-five, thirty, but she was served as the interpreter. Her father had been the editor of the key newspaper in Canton and when the Red Revolution came along they took him and killed him. And she was sent to the fields to work [in the fields *]. That's what happened to people at this time. She always kept her interest I guess in actually science or learning or what have you, so when she came with this [professor *]. What I was impressed by her was her ability to grasp these concepts because she literally took and studied all these things that we had done and when she left she was pretty familiar with it. So later I brought her back as a graduate student, supported her and she did a master's and a Ph.D. Now she actually worked on a version of the hybrid model in a channel and really she did something that has not been done yet. She put it completely in entropy coordinates instead of potential temperature. And we got better results with that than actually with our version of the way which we did the coupling to the PBL. I just point that out but that's a topic that would be nice to pursue. Too late now.

1:34:00.4

TR: Don, we're over an hour and a half. But I want to give you space to make sure if there's anything else you want to say.

DRJ: Well, in any event, she went back to China and she worked at the university there and retired. That's all the other people have done well I think, pretty well. And I hesitate to go into details but I guess when you think about people like John Zillman and Louis here, Rick Anthes and Bob Gall, etcetera, even Bette Otto, Chuck Wash, all these people, they've all done well.

LU: And Russ

DRJ: And Russ Schneider particularly. Yeah. Ron Townsend, I did mention him previously. He's, I said, a CEO now of a billion dollar organization. [laughs] What else can I say? [laughs]

1:35:07.6

TR: Louis, is there anything else you want to make sure we get on?

LU: Well, I'll, I'll just say with respect to the students in where we're organizing this colloquium, there's a number of students and colleagues, John and John Dutton, John Stremikis who is sitting here helping to organize this. One of the things that's a marvel is not just the intellectual nature of these students' accomplishments but somehow we've all gotten, a number of us have gotten into positions where we are managing, leading organizations which takes a certain skill. I think one of the things that Don brought to that, I always viewed was, you know, when you're frustrated with dealing with reviewers or something and would take extra time to get something done, he'd say look you know it's not who's first it's, who's right. You got to do this right. He kept on emphasizing and I know I've taken that with me in what I've done, whether I was working in the laboratory or leading operational units or now leading the Weather Service. It's a very methodical approach I think he conveyed to the students. And I know Russ and I have talked about that as well, who's leading the Storm Prediction Center. It's not easy when you're trying to deal with this research to operations and you want to maintain your fidelity, your credibility within the science world as you're working your way into the operational community, which has its own set of challenges. So I just find it interesting, fascinating that you look at these students, the accomplishments that have gone with science, but beyond science. And the education world as well. We've all been big supporters of the education and whether we go that directly from Don or through osmosis, it can be debated but he clearly had an influence on it all.

TR: Well, I want to thank you both for your time these last few days. I appreciate it. So this ends the oral history with Don Johnson.

End of audio

1:37:15.6

Transcriber's note. Bracketed text marked with * are edits made by DRJ, July 2014 and January 2015 and April 2015.