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Louis Uccellini
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Atmospheric, Oceanic and Space Sciences Building, University of Wisconsin-Madison

Troy Reeves: Alright. It is still August 22, 2013. This is the third interview with Don Johnson and Louis Uccellini. We are still here in the, on the twelfth floor and I'm still Troy Reeves. So, as we turned off the recorder we decided on some topics for this interview. The first of which we were going to start off by talking about the importance of an acronym, E_C_M_W_F.

LU: Yeah, so that's the European Centre for Medium-range Weather Forecasts and they work on and provide global model forecasts on a daily basis. And currently they are the premiere modeling center as measured by the accuracy of their forecasts.

TR: OK. And Don, you wanted to talk specifically about this group and its importance, so

DRJ: Well, there were at least three people that I had some relations who were tied up with ECMWF. Even Askel Wiin-Nielsen who was the first director of ECMWF, Lennart Benngtsonn became the second director but he was a key scientist under Askel Wiin-Nielsen. [transcriber's note: Askel Wiin-Nielsen 91924-2010), first director of ECMWF in 1974] [transcriber's note: Lennart Bengtsonn, Swedish professor, senior scientist. Head of ECMWF research 1975-1981; director of ECMWF 1982-1990] And then Tony Hollingsworth who is the following premiere theoretician, jack of all trades in terms of modeling so to speak. [transcriber's note: Anthony Holingsworth (1943-2007), Irish meteorologist]

LU: and was their chief of their scientific division.

DRJ: Right.

LU: Under Benngtsonn and then beyond Benngtsonn as well.

TR: OK.

DRJ: Well, [*] at the end of World War II all the meteorological community's services in Europe were in a state of disrepair. And eventually they decided to form this consortium, if I may call it that, of countries who would sponsor the medium range forecast center in Europe. The person selected to head this was Askel Wiin-Nielsen who had come to this country from Sweden originally to be part of the NCAR. [transcriber's note: NCAR – National Center for Atmospheric Research, Boulder, Colorado] And [after *] he was formally established [at NCAR

*], he became chairman or head of the department at the University of Michigan and called the meteorological department although I don't think it was exactly called that. [transcriber's note: Askel Wiin-Nielson was the first head of the Department of Meteorology and Oceanography at the University of Michigan in 1963] That's where I really first met him. Actually he visited here because Reid Bryson tried to get Askel Wiin-Nielson to come here as a faculty member and bring essentially his expertise to this department, but Askel chose University of Michigan because he had control now of a department there that developed into a very nice one. But they also did planetary studies and what have you so it was considerably broader than just meteorology. John Dutton and I wrote this paper, sort of book you might say, Advances in Geophysics, where we emphasized isentropic ways of looking at things and Askel objected to it at a meeting of the Geophysical Fluid Dynamics that was being held annually or even semi-annually among the universities in the midwest. [transcriber's note: Dutton, John A. and Johnson, Donald R. The theory of available potential energy and a variational approach to atmospheric energetics. Advances in Geophysics, v.12, pp333-436, 1967.] And then we had communication with him, both verbal and by correspondence and we reached a nice understanding. Well actually he invited me to come there and talk to a group of people that [who *] would come once a year. They were actually meteorologists from the military and the Navy and what have you. But I was to go there and talk about infrared radiation structure of the atmosphere using Suomi's radiometersonde which had become my Ph.D. thesis as a way to use that type of data to look at profiles of infrared radiation through baroclinic phenomena. [transcriber's note: Johnson, Donald Ray. The role of terrestrial radiation in the generation of available potential energy. University of Wisconsin-Madison, Department of Meteorology. Ph.D. thesis. 1965.] [transcriber's note: Verner E. Suomi (1915-1995) joined the University of Wisconsin-Madison Department of Meteorology in 1948, father of satellite meteorology] And, you know, [he invited me *] to his house [and *], so we got to know each other [and through *] our correspondence ended up being quite congenial. It's one of those unusual cases where you have a meeting of appreciation of what each can offer. And Askel was one of those people. So when he then became the head of ECMWF, they became a central figure in the efforts on FGGE and prediction and data assimilation. [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] [And so because I was involved *] in this global weather experiment, I would end up going there probably twice a year. [*] The key person I would see there at that time [would be Lennart Bengtsonn *] because he was the head of essentially of everything under Askel. Askel being the head and having to deal with administration [was not that *] heavily involved you might say in the day to day operations. The thing that was so impressive was that you would go there and you had three different groups at that time at least. One had to do with the analysis and understanding of weather. One had to do with the numerical predication. And then one had to do with the theory behind it. And so when I would go there, Lennart would always drag out these computations of available energy and what have you and show how their model was doing on those things. And we got to know each other as well as got

to know his wife. To give you some idea, some years later when we were looking for a person to lecture in honor of Suomi, Suomi had passed away by this time, we invited Lennart Bengtsson to come here. He brought his wife Karen. Then Dorothea and I took [them to *] northern Wisconsin and on into Minnesota to look at Lake Chisago. [transcriber's note: Lake Chisago, Minnesota is where the Swedish author Vilhelm Moberg had a home during the summer of 1947] Lake Chisago is an actual lake up there and it was a place where a Swedish author by the name of Moberg wrote four novels of Swedish immigrants coming here. [transcriber's note: Vilhelm Moberg (1898-1973), Swedish author, wrote series of novels "The Emigrants".] There is the situation at home. There is their journey here. Their ability to establish homes here, primarily dealing with agriculture, use of oxen, and caring for the land and working with the Indians at that time. And then there's one called Last Letters Home, where essentially now that they're so established here, three generations apart, people forget their ties back to their homeland. So it's a very fascinating set of four books. Moberg was quite a well known, call him a novelist because they're in some sense real life but they are a novel about a particular family and the life of that family. And if you go to Sweden you can go to Moberg's residence and see all of what they have there, including the genealogies essentially of people from Sweden tied to this country, too. So Lennart brought me these books and then of course we had this very nice time together over the four or five days. And this was a follow on the fact that I had gone there previously, and I'd get treated pretty nicely as a visitor from this part of the country tied up with these endeavors. So that in many ways accounts for this link. And later on Tony Hollingsworth became the key [scientist *] as Lennart moved up to be the head of ECMWF. [transcriber's note: Anthony Hollingsworth, European Centre for Medium Range Weather Forecasts (ECMWF)] Tony became a very key individual there. Tony has visited here, spent time with Tony to some extent, too. I guess one of the things I always remember about Tony is that he and Rick Anthes were very good friends, too. [transcriber's note: Richard Anthes, president of UCAR (1988-2012; M.S. and Ph.D. degrees from University of Wisconsin-Madison] And Rick asked him, you know, why don't they actually go about and build an isentropic model and Tony simply said well you know we [haven't looked in this matter *]. In any event they of course had their direction they were going and you'll find in most cases where you have a certain model being developed there is a certain set that they'll continue to work with. And they of course had to do it like this. The beautiful thing about this community and this effort in relation to the rest of the European scientific community is that it was set up so that young scientists could go there for five years to work. If they were good they might get extended another five years but then they had to go back to their home country. And they of course are ones that [after spending their early years at ECMWF returned to their home counties*] and became key figures you might say in the development of modeling in these various meteorological centers for the various countries. But they also always had these ties among themselves because of their relationships, that you had somebody from Italy coming there and you had somebody from Germany and somebody from Sweden. Well, Lennart Bengtsonn is from meteorology of Sweden so he had that [heritage *] in common. Tony Hollingsworth is from Ireland and was great.

0:20.59.4

LU: singer

DRJ: singer [laughs]

LU: [laughs] especially with a few beers.

DRJ: [laughs] Oh, yeah, right.

LU: If you went out to dinner with Tony or went out to a party with Tony by the end of the party you had to be singing with him or you aren't getting a ride home. [laughs] [laughter in background]

DRJ: And then of course I had these times when Tony and I could talk some or he visited here, not that many, maybe a couple times a year, probably less. But I always remember at the fiftieth anniversary of founding of NCEP I did give a presentation. And at the end of it I had shown how you should go about actually assessing the accuracy of models. And Tony's remark on the floor at that time, every model should have, be required to undergo these assessments. And of course it wasn't too long after that that he actually passed away. But we had good discussions. So that in some sense [his passing ended my endeavors with ECMWF *]. Later on in traveling in the '90s, actually I would end up going to Denmark and I had actually stopped and saw Askel Wiin-Nielson because there was a key individual [*] who went [back *] to Denmark to do modeling. I don't remember the name of the individual [from Goddard who had moved to Denmark *]. There was always that opportunity once or twice, maybe three times, to stop in and see Askel. By that time he had left the ECMWF, had gone to be the head of the scientists for World Meteorological Organization. And it's also true that one of my students became the president of the World Meteorological Organization, but that's different from [being *] the executive director. OK? And that student was John Zillman so it comes to a time when we should talk about the connections to the Australian group, too. [transcriber's note: Zillman, John William. Isentropically time-averaged mass circulations in the Southern Hemisphere. University of Wisconsin-Madison, Department of Meteorology, 1972. Ph.D. thesis. Served as president of the World Meteorological Society (1995-2003)]

0:13:26.0

TR: Alright. We could talk about that now. I do want to, since we've, since you've touched, actually physically touched these books a couple of times. I'd like to at least get them into the record. So these are

DRJ: Well that's, I was head of committee, the organizing committee so you can find my name in here.

TR: So that's the proceedings of the first

DRJ: Right.

TR: national workshop

DRJ: And of course I

TR: on global weather experiment. OK. [transcriber's note: National Workshop on the Global Weather Experiment: Current Achievements and Future Directions, 1st, Woods Hole, MA, 9-20 July 1984. Washington, DC, National Academy Press, 1985. 3v.]

DRJ: It's three volumes here [*]. This shows all the various people in the various committees but here you find the FGGE workshop.

TR: OK. And so FGGE is F G G E, is the acronym

DRJ: First GARP Global Experiment

TR: Right. And GARP is the Global

DRJ: Global Atmospheric Research Program.

TR: OK.

DRJ: Global Atmospheric

TR: And so again there's a variety of folks on this. Some different universities in this one than the other one I see.

DRJ: They're from all over.

TR: The University of Utah, Florida State and then again places around the country. Again showing sort of the community that has to be built

LU: And the growth, too. So you start seeing during this period, now you're into, is this in the '70s? When was this workshop?

0:14:35.3

DRJ: Late '70s, early '80s.

TR: It came out in, the workshop was '84 actually.

LU: '84, so during the '70s into the early '80s you have, you're starting to get the growth of these schools like Utah. Florida State was there, but you know it was certainly becoming more global oriented and you start seeing a broader collection of people.

DRJ: You see a lot of new departments being organized.

LU: Yes.

DRJ: in the ‘80s.

LU: ‘60s, ‘70s, but mostly you know ‘70s, ‘80s.

TR: OK. And do we want to talk about the tie to Australia now? Or do we want to talk about research?

LU: Oh, you get two, two

DRJ: Well,

LU: key students

DRJ: [laughs]

LU: and one of them became the director of the weather services in Australia and then wound up as the president of the World Meteorological Organization.

DRJ: I’m one of the few people [that *] probably has sponsored

LU: Two directors

DRJ: two directors [laughs]

0:15:36.1

TR: Well, let’s go ahead and talk about that then.

DRJ: OK. We had this tie of Schwerdtfeger with the Antarctic Meteorological Research Center in Australia. [transcriber’s note: Werner Schwerdtfeger (1909-1985), faculty, University of Wisconsin, Department of Meteorology, 1962-1980] And he would go there and then he would arrange for NSF to sponsor students, young scientists who finished Ph.D.s to go there and spend the year. And for example, Doug Sargeant [transcriber’s note: Douglas H. Sargeant, faculty, University of Wisconsin-Madison, Department of Meteorology, 1965 to 1972] was one of those people [as well as others, Dave Martin and Ben Bullock. I don’t remember the exact details who was the other person *] who had suggested that Zillman might come here and study with me. But before that Bill Downey had actually come to work with Verner Suomi. [transcriber’s note: Downey, William K. The angular momentum budget of a mid-latitude cyclone using a quasi-Lagrangian isentropic co-ordinate system. University of Wisconsin-Madison, Department of Meteorology, 1970. B.S. thesis] Downey, William K. The dynamics of the extratropical cyclone : an angular momentum approach.. University of Wisconsin-Madison, Department of Meteorology, 1972. Ph.D. thesis.] And the nice thing I guess I would call it, if you got into Australian meteorological service you never got in there by studying meteorology, your first undergraduate degree was in physics and mathematics and then they had their own school [of

meteorology where *] they actually educated you to become a meteorologist. Well Bill Downey had taught in this school and he decided then I guess somewhere along the way to come here and pursue a master's degree to begin with Verner Suomi. Well he wasn't here very long and he found it very difficult to get to Verner Suomi, so he transferred to Sechrist. [transcriber's note: Frank Sechrist, University of Wisconsin-Madison, Department of Meteorology faculty, 1967-1987] [But *] then he actually ended up doing it with me I guess because I was doing things maybe that was more attractive to [him. We wrote a series *] of three papers on looking at the angular momentum and there is a fourth paper on the available energy, although he's not an author of that which following these phenomenon along and being able to study it from you might call it the perspective of angular momentum in a moving element *]. And he stayed, got his master's. [transcriber's note: Downey, William K. The angular momentum budget of a mid-latitude cyclone using a quasi-lagrangian isentropic co-ordinate system. University of Wisconsin-Madison, 1971. M.S. thesis.] Along came Petterssen, liked it all. [transcriber's note: Sverre Petterssen (1898-1974) Norwegian meteorologist] Bill was supposed to go back [to Australia *] but Petterssen writes a letter to his friends in Australia that, [*] he should stay here, and pursue his Ph.D. And so they let him stay. You have to understand that Petterssen as head of the Department at the University of Chicago had very strong ties with Australian meteorologists so they were always there working with him and they had [maintained *] these ties. So he could reach out and talk to the director of the Australian meteorology, Bureau of Meteorology or whatever it's called, and they actually allowed Bill to stay and pursue the Ph.D. and he even stayed here beyond that. I worked with him as a project associate, research associate, not an academic appointment but a research appointment here. In the meantime John Zillman contacted me about coming [*] here with Australian support, from the Australian meteorologist community to [pursue a Ph.D. *] He came here with a [meteorology background including a master's degree *] at the University of Melbourne. And it was probably the best degree thesis, I might say that laid out the climate of the Southern Hemisphere at that time and probably even still today. In any event he came and within two years he had finished his Ph.D. and then he went back. When he went back he became [practically instantaneously *] the head of the meteorological services in Australia. Now all these [government weather services *] are part of the World Meteorological Organization and so Zillman would be their representative. It wasn't long thereafter that they actually voted him to be the president of the WMO. So here now I had [sponsored these two individuals, one who headed the meteorological services of Australia and became the president of the WMO. This international activity took quite a bit of time, so Bill Downey *] became his executive director, associate director in the Bureau. So here I have both of these people high up. Now some place in here I have a picture of being in Australia and I'll drag that out later sometime and show you the nice office they would give me when I go there. [laughs] [I would go down there at least a couple times a year because we continued our work. *]

LU: You were finishing up, it wasn't just his thesis, it wasn't just Downey's thesis, it was that you were working on that into a set of papers.

DRJ: Yeah, right. The three papers.

LU: Right. And I remember, this was just about the time that I came on, the proposals that you needed, you had a connection within NSF going back to the Air Force again.

DRJ: Well,

LU: Yeah. I thought that's what you said that the Ron

DRJ: Ron Taylor, but he wasn't in the Air Force.

LU: He wasn't?

DRJ: Ron Taylor came out of UCLA. He had been in Antarctica. He had done isentropic analysis [*]

LU: Ah, that was the connection.

DRJ: That's the reason why. [*]

LU: So you were, you were able to get extended through this interest in NSF, you were able to get support.

DRJ: Support.

LU: And Don had one of the unique things as I found out from when I went out and dealt with people coming from other universities, he actually engaged his students in writing the proposals. And so I got, I got more associated with the cyclone work through helping with the proposals and Downey was part of that. And Chuck Wash was part of that. And I remember you going down there a couple times where we were sending you stuff from the proposals as well as you working on the papers. So it was clearly an ongoing effort. There was a reason you spent three weeks down there. [laughs]

DRJ: Oh sure. It was, I mean I was accused of being on the beach. [laughter in background] They have a solstice party here where they'd have these pantomimes of various faculty and what they were doing.

LU: Yeah, yeah.

DRJ: So they actually, one of them, the students, and I wasn't even here at the time. They set him in a chair and here I'm in a chair on the beach [laughs] [laughter in background]

LU: Australia [laughs] [laughter in background]

0:22.34.5

TR: Don, if there's anything more you'd like to say about Australia, I want to give you space to do that, but Louis I think you just sort of opened the door here for something else. Not necessarily the research but the relationship you two had as you became his student

DRJ: Now let me finish a little on Australia

TR: Please do.

DRJ: [This relates to Sverre Petterssen *] and everything else he had access not only to the Australia meteorological community down there, but the CSIRO which [involved *] scientific endeavors across a broad range of disciplines. [transcriber's note: CSIRO: Commonwealth Scientific and Industrial Research Organisation] And key people who were in CSIRO had been tied up to Petterssen and we had good links and a lot of them went down there from Great Britain after World War II because [of *] this strong link between Great Britain and Australia.

LU: Yeah. And didn't, wasn't John Zillman in his role as the director not only a director from the point of view of operations but also of research.

DRJ: Oh yeah.

LU: Yeah. So that's a different role than I have a director of the National Weather Service which is more operational focused. We have a research to operations component but I've been, since then I've been on a review team for, international review team for the entire enterprise down there in Australia. And the person in that position has much more broader responsibility in terms of research and operations.

DRJ: OK. And there's even a person by the name Joanne Simpson who we should bring in here at some point. [transcriber's note: Joanne Simpson (1923-2010), first woman to receive Ph.D. in meteorology, known for work in tropical meteorology] But we don't have to do it right now.

LU: Yeah. Because she had

DRJ: Because she had ties to us.

LU: She had ties to Australia, too.

DRJ: Very much so.

LU: And just happened to be my boss after I left Don. [laughs]

DRJ: [laughs]

LU: so it's an interesting connection.

TR: OK.

0:24:19.6

LU: Let me just say two things here with respect to the Australians. Both Downey and Zillman were still here when I transferred up from Charlie Anderson who I got my master's degree under as we noted earlier. [transcriber's note: Charles Anderson, faculty, University of Wisconsin-Madison, Department of meteorology from 1966-1986] I was much, I became much more interested in synoptic aspect and the isentropic framework, so I really did _____ Ph.D. under Don. I talked to him and he invited me up. When I got up there I mentioned that I had to take the advanced calculus courses and one of the other things I had to do was I had to take the seven twelve general circulation course which he was a professor of. And I was very nervous because this is a very high, let's put it high octane level course from a mathematical perspective so I worked very hard in that. And I saw, and two things happened. One, I was really impressed by the fact that these two Australians were in this class and they, they, I didn't know that they'd already taken this course about two or three times already and practically sat in it every time Don taught it so they could get to understand it. And I, but they always seemed to ask the right questions. So I mentioned to John Zillman several years ago that I was really impressed about how, you know what they brought to the class. And he goes what, are you kidding? He says, Bill and I we'd talk about this young, young student Don got, me, and what and the questions he was asking. So and he, and so then John even brought up this, this incident that happened where there was the twelve week exam and Don gave this question. It was an essay question for the whole twelve week exam. About how, you know, just considering the force of friction as the only external force operating, describe the general circulation. Which, you know, I looked at that question and I said oh boy. So I say here goes. And I wrote an essay that Don ultimately copied for the rest of the class. I wrote this thing in the whatever time frame was allowed for the test, I think a two hour time frame. And with equations. Used, I actually used colored pencils to get the southerly flow and the northerly, you know I got it all worked out. And Don remembers that and John remembers it as a student and he actually, he actually brought it up and Don, Don actually mentioned it in an interview a couple, well when I got my job. And Zillman, and both Zillman and Downey they just still treat, I could tell they treated their experience here as such a fundamental part of their growing up, you know, within this community. And they, like me, they absolutely, they absolutely loved their time here and their attraction with Don. I, Don, Don, you know I said this thing about the essay

DRJ: Yeah, no.

LU: So. I actually sent, I actually scanned the original version of it and sent it to him. [laughs] So anyway the connection there was great. I mean, as I came up these were the people that were there. There was other graduate students of course but having this whole international community, you know, as part of this graduate level set of students was just an amazing, I just felt I was a part of an amazing group of people.

0:27:54.9

DRJ: OK.

TR: So. Go ahead.

DRJ: There's one thing here we haven't covered. I'm just going to touch it very briefly. It looks as if I never did anything in the university itself, but when I finished my work here I became an assistant [then *] associate [and then full *] professor [*] I was quickly appointed to the graduate research committee of the graduate school [*] under Dean Bock. [We had *] responsibilities to go out and listen to proposals from the various faculty throughout the campus to get some initial support to start research. [transcriber's note: Robert M. Bock, University of Wisconsin-Madison, Dean of the Graduate School from 1967 to 1989] And one of the members of the committee was Phillip Certain who later became the dean [of L and S *] here. And I actually was named chairman [of that committee *] for one year. And that was a three year appointment. And so I did get to know a fair number of people on the campus and in the various disciplines including chemistry, physical chemistry, geology, or what have you. And that never hurt either because I did know a few people. Once this started, time did not permit, nor was I inclined to move up in the administrative ladder rather than being just a plain old professor. I was chairman of the department three years. I was asked to be chairman again some years later but I declined because I said you know I've other things [that *] I'm doing. And by that time I had become fully integrated in SSEC. [*].

0:29:47.2

TR: Right. Well in that, so I just want to make sure we clarify, too. So you were involved in this campus committee but then as your work more nationally and internationally became important to you, you saw that you didn't have time for both.

DRJ: Yeah. I was asked to go and be dean some here and dean there, not on this campus but I didn't even go interview for it.

TR: OK. So did you ever interview for a position outside of UW-Madison?

DRJ: Eventually I interviewed briefly for a position at the University of Michigan towards the end because of this relation with Askel Wiin-Nielsen. [*] He was already retired and he wanted me to go and try to be there, part of the revival [of the department *] at the University of Michigan. And I had a nice experience but withdrew because they took too much time to actually decide what they were going to do. They actually hired a key person who was heading NASA _____

LU: Fisk? Was it Fisk at that? [transcriber's note: Is this Lennard A. Fisk, chair of the Department of Atmospheric, Oceanic, and Space Sciences (1993-2003) at the University of Michigan. Formerly an associate administrator at NASA.]

DRJ: I don't

LU: Fisk?

DRJ: Yes. Yeah. Fisk. Yes. So I didn't, I had withdrawn before they actually made that decision. I could see they were hesitating and I wasn't interested in waiting around.

TR: Right.

DRJ: Beside, I was becoming very much involved with the University Space Research Association and it's because of that involvement that I actually got attracted to Michigan because their associate dean was a key part of my work in the efforts in the earth system science endeavors that USRA [transcriber's note: Universities Space Research Association] had with the various NASA laboratories.

0:31:30.7

TR: So

DRJ: Go ahead.

TR: Don, is this a spot where we can, one of the things that Louis wanted to make sure we touched on was your research and I know we've sort of alluded to it here and there and I don't know Louis if you want to point him in one specific direction to start off with or

LU: Well I think you, you need to get into the general circulation research and then downscale it from there. That's cyclones and then the mesoscale and even down to turbulence. They're all connected of course, but I think it might be best to categorize it in that _____

TR: OK.

DRJ: Well, probably the key thing I did in writing something was this, the bulk of this book which I have essentially the portion from what? [transcriber's note: Sound of turning pages in background] Let's say page thirty, ah forty, yeah forty, forty, forty-three to the end which is three hundred and some pages. OK? Three hundred and twenty nine. [Pages forty three to pages three twenty nine *]. So essentially this is a summary of my general circulation research. [transcriber's note: Johnson, Donald R. The forcing and maintenance of global monsoonal circulations: An isentropic analysis. Advances in Geophysics, v.31, 1989, pp43-316.] It's not a summary of the research on the cyclone. But there are two letters here. You probably don't want to take time to insert them, but you should look at them and you should probably look at them, too.

TR: So the book is called Advances in Geophysics.

DRJ: That's right. And the editor of it at that time was Barry Saltzman. Barry Saltzman came out of MIT. Chief scientist under Victor Starr originally. [transcriber's note: Victor Starr, faculty at MIT from 1947-1972, known for work in atmospheric general circulation] And was

sort of the premiere person involved in many of these activities because he was the editor of a whole series of some twenty [books entitled Advances in Geophysics *]. And so I sent him the manuscript and he immediately accepted it. It should have been a book by itself but it wasn't, because it was sent to this activity. In any event, later year later, well actually that's not quite true. I sent a copy of this to Arndt Eliassen and also brought a copy to Joanne Simpson who was, as Louis said, was his boss. [transcriber's note: Arndt Eliassen (1915-2000), Norwegian meteorologist, pioneer in use of numerical analysis] _____ She was your boss

LU: Right.

DRJ: when you went there.

LU: When I went there.

DRJ: Well, Joanne not only did I have this link to Goddard in the FGGE but she had a son who was on the faculty here and she would come out here every year or two to see them. And she was also very, a good friend of Verner Suomi. And so we would get to spend some time together when she would come out and visit and also when I would go to Goddard. So she writes something pretty nice about this book and also Arndt Eliassen when I sent it. This is at the end of Arndt career obviously because he probably is at this time seventy five, seventy seven something like that. And he didn't read it [in detail *]. Said he didn't understand it all but what'd he say at the end? Very impressive or something to that effect.

LU: Yeah.

DRJ: And of course this really is an outgrowth of all the work in isentropic ways of looking at things that began in some sense with John Dutton here and things were carried on by students, etcetera. People like John Zillman and Bill Downey, etcetera. So, that, let's put it this way, as far as I was concerned I was in an ideal situation here, enjoying myself.

TR: So, did you end up then using this book in your teaching, in your classroom.

DRJ: Well, I used a lot of the material. I don't think [so since *] the book was near the end of [my teaching the class room since*] I actually was getting close to retiring. Was that seven, was that '89?

0:36:18.8

LU: This is 1989.

TR: OK.

DRJ: See, I retired in '94

TR: Right.

DRJ: from the classroom. But it was unique.

TR: So, it was used

DRJ: But I had a secretary for twenty years who wrote out the notes and we would hand them out. [transcriber's note: Judy Mohr]

TR: So it was used not under the auspices of the actual book but

DRJ: No, not so much. But everything that's in there, because a lot of it had to do with material from student thesis and in particularly a person by the name of Ron Townsend. [transcriber's note: Townsend, Ronald D. A diagnostic study of the zonally-averaged global circulation in isentropic coordinates. University of Wisconsin-Madison, Department of Meteorology, 1980. Ph.D. thesis.] Ron Townsend is CEO of Battelle now which is a activity that controls, well that carries out a lot of the support for the DOE labs. [transcriber's note: Ron Townsend leads Battelle's Global Laboratory Operations which includes six national labs for the Department of Energy] He has an exceptional career in his own right. But he came here because of the fact that he was at Offutt. [transcriber's note: Offutt Air Force Base, Nebraska.] I think that I mentioned this maybe before. And one of the Air Force people told him that well you should go and think about studying for your Ph.D., Air Force sponsorship at the University of Wisconsin. And so he came here with a express purpose of pursuing a Ph.D. under me. He had gone to Bethany College for two years. He had gone off to K State back in those day of the Korean War, or later maybe Vietnam War, I guess that's right. [transcriber's note: Kansas State University] And he had done his work in mathematics and then he went into the Air Force and then decided to come here and pursue the Ph.D. He had [received *] a master's from K State in mathematics. He did so many of these computations. I found a story that Bart Adrian told me, only a couple, only the last year or so that is interesting. [transcriber's note: Is this Bart Adrian, meteorologist for WITI-TV, Milwaukee (1982-2010), now teaching at UW-Milwaukee?] There were these rooms where my students sat and worked. One was adjacent to my office upstairs. And when [Ron Townsend *] arrived he was sort of asking about me and [someone *] said well Don's been in the Air Force, he says I know I can work with him then. [laughs] [laughter in background] And as Bart says he would be there at 6:30 in the morning, work throughout the whole day. He did that all while he was here and he finished his Ph.D. in two years, so. We had help with other students in the background stuff we had done and what have you, but on the other hand, with his mathematics background, he was able to really turn out all of these computations that we used to describe transport concepts of the planetary and secondary circulations. So we've kept in contact. He sends us Christmas cards and we've stayed at his home a long time ago, some ten years ago. He retired from the Air Force. He became head of the AFIT which is the Air Force Institute of Technology in the Air Force as a colonel. And I guess he didn't make major general because, general because, who's our, who's the person that, Jack what? There were the two Air Force people that you had in NOAA?

LU: Oh, Jack Kelly and Jack Hayes. [transcriber's note: spelling?]

DRJ: Jack Kelly and Jack Hayes.

LU: Right.

DRJ: Well Jack Kelly was, Lew had worked with him and got support from me to come on and be a half time scientist at NCEP. [transcriber's note: National Center for Environmental Prediction] This is, now we're getting into the '96, '97, '98, maybe even the year 2000. And the idea was to bring in some I guess blood from outside the NCEP to NCEP. I had met Jack Kelly some years ago at the AMS meeting and he came up to me and asked me, he says you remember who I am? I said you're General Kelly. Head of the US meteorology activities in the Air Force. And he says, yes, but I was your student in your class taking statistics at Penn State. [laughs]

LU: And you, did you remember him as a student?

DRJ: Not really. No. I had twenty, twenty five students.

___: Right.

DRJ: Probably, possibly. I don't remember, but they were in this classroom and I only taught one course there that year I was there. And that was for one quarter. But so I would have to write a report of my activities to General Kelly for the first two years of that appointment, at the end of every month I guess.

LU: Yeah.

DRJ: And you know [when *], I'd go [to D. C. *] he'd let me come and visit him once or twice, three, four times I guess over that period of time. In any event as we moved on those things came to an end, too, because he was retired essentially even from NOAA. Well we've got ahead of ourselves. I'm supposed to go back and talk about now my students I guess. Right?

LU: Or your research itself in the general circulation.

0:41:41.8

TR: Right. And we have a couple of specific questions and if you've already addressed these, but they're specific questions about global circulation. And one was development of the theory for the forcing of the global meridional?

DRJ: Meridional

LU: Meridional

TR: Meridional mass circulation and isentropic

DRJ: Isentropic

TR: isentropic coordinates

DRJ: Isentropic coordinates.

TR: Well, you can tell I'm not a meteorologist

DRJ: [laughs]

TR: Do you, have you already addressed that? Or would you like to?

DRJ: Well, we can carry on a little bit with that because it goes back to all of these questions, questions were being raised by the MIT general circulation work tied up with Victor Starr. When I was somewhere in the early '70s, probably, or late '60s, Lyle Horn went for a year's leave at Dartmouth. And I went to visit him because we were doing this research together. At the same time I had a invitation from Lorenz to come and spend the day at MIT. And so I was a guest of Lorenz and we talked some. By that time he knew of this work that John Dutton and I did, but not this [other works *].

LU: Not this one here.

DRJ: Not this one here.

_____ [Voices in background]

DRJ: And so we had some good discussions. And then I got invited to this dinner at his house that night. But during that visit he had me go and spend an hour with Victor Starr. Now Victor Starr was the premiere scientist in general circulation theory at the time. Still is to some extent to many people. And Victor Starr spent the hour talking to me about the importance of water [vapor in the circulation of *] the planet as a whole. Guess who I saw as I was walking down the hall at MIT? Rasmusson. [transcriber's note: Eugene Rasmusson, faculty, University of Maryland, Department of Atmospheric and Oceanic Science] He was now being sponsored by NOAA to get his Ph.D. from MIT under Victor Starr. And of course we are old friends so you know we could actually talk to each other with ease and what have you, although we didn't spend a lot of time then talking to each other because they had me tied up talking to various faculty and other things. Anyway as I said, Victor Starr spent a hour talking about water vapor and of course I didn't have, I didn't take much time to think about available energy because he had gone beyond that. In any event it was primarily a one way conversation. Now just before he passed away it's interesting. By that time we had done these sort of things

LU: Are you talking about Victor? Just before who passed away?

DRJ: Victor Starr.

LU: Victor Starr.

DRJ: Yeah. Right. He had [some years later *] gone in for surgery and had complications and he didn't actually survive that. And I mentioned in here, I'll talk about it, and just before he had done that he did write me a letter. And he had finally paid attention in many ways to the angular momentum of storms. [I *] probably possibly could find that letter. [*]. He passed away and there were two colleagues of his, young scientists, who was, Salstein [transcriber's note: David Salstein] was one of them. [And Rick Rosen was the other *] [*] Now what's interesting in going to Lorenz's house for dinner that night is that we didn't talk about meteorology but his wife is a very personable individual and we talked about some stories from World War II, the Okinawa and what have you. And he told this story, I don't want to go into details about Reid Bryson. And Reid Bryson has told stories about Lorenz [since both were stationed at Okinawa *]. They're kind of, they're fascinating, about winds and all that sort of thing. The fascinating thing at Lorenz's house was his wife had been a ferry pilot, women, not officially in the Air Force, but they ferried aircraft from Nova Scotia over to Greenland because you could never fly directly all the way [to Europe *] with the aircraft at that time. And she [could talk freely *]. She was a personable individual so that was a very nice experience. But after that we never had [*] a lot of connection with Lorenz [*] any more because he had gone on to [*] a means of describing in a mathematical sense the uncertainty of meteorological circulation. Lorenz [received the Crafoord Prize the equivalent *] of the Nobel Prize but they didn't offer [the Nobel Prize *] to meteorologists or mathematicians at the time. [transcriber's note: Edward Lorenz and Henry Stommel won the Crafoord Prize in 1983 for their work in geophysical hydrodynamics]

LU: They still don't.

DRJ: Huh? They still don't.

LU: They still don't.

0:47:36.5

TR: So, he won a Crafoord Prize.

DRJ: Right.

TR: Which is a close, sounds like the closest thing you can get for a Nobel from

DRJ: Maybe even better [laughs] [laughter in background], considering what they've awarded Nobel prizes for these days. [laughs]

LU: becomes more, it's more a political thing than

DRJ: That's right.

LU: Yeah.

0:47:52.9

TR: Do you want to continue, is there any more you want to say about that or the next thing on the list is the monsoons, development of the theory for the existence and forcing by global monsoon circulations.

LU: Yeah. I would just say that and it's in this volume thirty one, Advances in Geophysics, where Don got two hundred pages. Right?

DRJ: huh?

LU: You got that part of the chapter you have in there is about two hundred pages.

DRJ: Three hundred.

LU: Yeah. And it's a, the issue here is the meteorological community basically went in to the isobaric sigma coordinate system to try to explain the dynamics of the atmosphere including the general circulation as numerical models were being developed. So we're talking the '50s, '60s, '70s timeframe. And what Don's work is in a very general sense, an overview sense, I'm just trying to get him going on this, is you go back to the original concepts of the turn of the last century and people dealt with basic physics, you know, momentum, mass, energy. And meteorologists up through the first third, maybe forty percent of the last century, worked to a certain extent in this isentropic framework which is a more, from an entropy perspective, this allows you to deal with the conservation principles a lot more effectively. So his presentation of all the dynamics and conservation issues in the description of the circulation from equator to pole is a different perspective than what you get from sigma and _____ which has it broken up into more sections. So it was controversial, this idea of a circulation from equator to pole. And this article written in 1989 that he was referring to letters from Joanne and Arndt Eliassen lays out, you know, that not only lays out how these differences can be actually explained mathematically and conceptually, but also in terms of the degree of contention within the field that so he talks about the letters, was between _____ and Starr.

DRJ: Yeah. Yeah, that's right. There's a very interesting controversy.

LU: and the exchange, so it's a very controversial area and he plopped himself right down in the middle of it. And it's still, the isentropic framework is, is uncomfortable to many people who got their formal education within this other framework. And especially, especially and I'll say this, especially those at the MIT school who, in which many of these general circulation concepts in the sigma pressure coordinate system was developed and mathematically developed and actually became the basis for the numerical prediction system as we know it today. So it's a contentious area. And yet you see these letters that people getting insights now once they, once

they understand that there's a mathematical reason for these conceptual differences and they can be explained. Did I capture?

DRJ: Very well.

LU: [laughs]

0:51:42.3

DRJ: I'll go back a little bit further though and because of my background in physical chemistry and chemistry major you quickly get exposed to the idea of entropy in the first chapter of any good physical chemistry book. And having that background which provides a key part of dealing with a gaseous atmosphere, the relationship of the internal energy of the gas as the random molecular motion of the kinetic energy of the gas in relationship to temperature. In other words the temperature as internal energy is nothing more than the root mean square of the random molecular motion of kinetic energy. And that goes back to the people like the Sir Napier Shaw and his mentor who is [Maxwell. *] [transcriber's note: Sir William Napier Shaw (1854-1945) British meteorologist] But it also goes to another quite famous mathematician by the name of Caratheodory. [transcriber's note: Constantin Caratheodory (1873-1950), Greek mathematician] Caratheodory is the first one who really sets up the relationship for what is considered to be reversible and irreversible processes. Reversible process is one [involving expansion and contraction *] where essentially you have a volume element with surfaces that are impermeable and you expand it. The internal energy inside will change because it's less compressed. Compress it and [the internal energy increases *]. As soon as you introduce the idea that now molecules can move through that surface, actually called deformative coordinates, then of course that's the irreversible part enters because you can't go out and capture and bring them back in. You have no way to control them. Guess what deformative coordinates are used for these days. They're used in the portrayal of cartoon characters, the outlines of cartoon characters. You can actually go on the web, you'll see deformative coordinates, cartoon. So you're, you're following the outline of the cartoon characters. Now we never really had bought in to that idea in many ways in the atmospheric sciences except the person by the name of Chandrasekhar at the University of Chicago who is quite a well known world renowned theoretician, came out of India, studied in England and then went on to the University of Chicago and has written, you know, six, seven volumes on various parts of the dynamics of solids, stars, everything else. [transcriber's note: Subrahmanyan Chandrasekar (1910-1995), astrophysicist, University of Chicago, author of many books in his field] He is just phenomenal. But he would take a subject for several years and study it and then write a book on it. So you have to go to books like that if you want to understand some of these [*] things. And he certainly focused on Caratheodory and said that's the only way to actually deal with the subject of entropy, is to think of the reversible and irreversible processes. Now there is a kind of interesting twist to this in the sense that Louis knows Tom Bogdon and I got to meet Tom Bogdon. [transcriber's note: Thomas J. Bogdon, current president of UCAR] He would come here.

0:55:19.7

LU: He's now the president of the University Corporation for Atmospheric Research, succeeding Rick Anthes. Just now over the past two, three years.

DRJ: But he was head of your

LU: Space. Space Weather Prediction Center.

DRJ: Under you.

LU: Under me. Right.

DRJ: Right. At Boulder, Colorado. So I had met him before he actually ended up coming here. But he comes here to visit an aunt who lives here in Madison. [*] When I found out he was to be the UCAR director, let's say a year or two ago, it was at the AMS meeting, I introduced myself and then found out he was coming here, so we have gotten together on a couple occasions here just for dinner, or lunch and what have you, to talk about University of Chicago and Chandrasekar who was a professor there. And there are real, some real interesting stories about Chandrasekar who, when he would come in to a seminar and he would ask the speaker, what's your subject, what's your approach. And if he didn't like it, he'd walk immediately out. So they were the people as [Bogdon *] said were in some ways lived in fear of Chandrasekar. But you can go and study this man and he is one of a kind. [As well as *] Caratheodory who was a world renowned mathematician at his time. So, these are the type of things I enjoy studying and try to understand a little. I'm not able to grasp everything they say at all, nor begin to, but then apply it you might say to atmospheric science. And the work that Louis just described the many ways in dealing with global monsoons, and monsoonal circulation have a basis in the underlying theory being put forth here, including Sir Napier Shaw. So that provided, just trying to read those authors, use it in applications to deal with analysis of atmospheric circulation, isentropic coordinates, after Chandrasekar, I should say Sir Napier Shaw and Caratheodory more so and with a strong twist from Chandrasekar, formed the basis of it.

0:57:56.5

LU: So you emphasize the entropy aspect

DRJ: Right.

LU: I think it's worth also saying a few words about the different perspective that you bring with respect to and going back to the fundamentals of angular momentum.

DRJ: Right.

LU: versus the concept of vorticity that the meteorological community is essentially embraced over the last sixty, seventy years. Because Don's work not only brings us back to the entropy

aspect but also the use of angular, the mass angular momentum of conservation aspects. And it's a common feature in his angular, in his general circul work, circulation work but also in his cyclone work as well. So.

0:58:44.6

DRJ: Well when you get into angular momentum you quickly focus the velocity of the flow onto the horizontal and we're talking about horizontal motion on a sphere now. [And then you separate out *] vertical motion which is always an adjustment involving the irresolvable scale of adjusting gravity and internal energy in the column. When vertically integrated they got to be equal to each other [that *] is internal energy plus RT the work of expansion, must be equal to gravitational potential energy. The gustiness of the wind out here like this morning, with the convection, this adjustment going on because when you release of latent heat which is saying I'm heating this, I've got to adjust my gravitation energy potential *] and essentially we can explain essentially that the bursts of momentum, angular momentum, are really bursts of angular momentum tied with the relationship of rotation. And we never really made that real connection in the MIT version of it because they're dealing with angular momentum about the earth's axis rotation in a different way than when [you look at how everything moves along tied up with the velocity of the radius vector. Entropy enters in here right front and center. While I didn't realize all things, we did realize that when it comes to transport processes that the thing you want to do is an isentropic analysis because it just comes out naturally in doing the diagnostics of it. The diagnostics isolate certain co-variances of the incremental mass and the angular velocity and the radius, in entropy coordinates [which *] gets lost in doing it in isobaric or spherical coordinates. I'm still struggling with that to some extent and writings some papers on it. Hoping I get them finished and get them published. [*]

LU: So

DRJ: And that's in here.

1:02:27.5

LU: So, so in terms of the research aspect and you just alluded to papers you're still working on. Clearly there's still, I would say within the meteorological community, a, almost an overwhelming view that it's, you know, we can proceed with the vorticity approach, a sigma pressure coordinate that people feel comfortable with. What Don's pointing to are fundamental weaknesses in that approach that are not accounted for in the basics with respect to entropy, angular momentum, mass, conservation principles. So I just wanted to high light that and bring that out here. There are some that are working the isentropic modeling. We have a few people within the Weather Service. There's a major effort going on out in Colorado under Sandy McDonald and Stan Benjamin. [transcriber's note: Alexander 'Sandy' McDonald, director of the Earth System Research Laboratory, NOAA, Boulder, CO] [transcriber's note: Stanley G. Benjamin, NOAA, Earth System Research Laboratory] And it is a model that's being considered

and it needs to be evaluated within the context of the diagnostics Don just pointed to. And Tony Hollingsworth's comment is something that has reminded me that we basically said the same thing at one of the workshops. That there should be a standard package that uses these diagnostics to measure the conservation principles and whether the models are living up to it or not. So I just wanted to make sure we got that out from the research perspective.

1:04:07.0

TR: OK

DRJ: Maybe I'll put it a little different way, a little more blunt way. Any meteorologists dealing with models are just putting [observations and parameterizations *] in a black box and then [determining a *] result, but not knowing what's going on inside the box.

LU: Right. And coming from a numerical modeling perspective, there's the general consensus that the modeling within these frameworks that we just talked about are good enough to make it a black box.

DRJ: Increase resolution and you'll

LU: Yeah, increase the resolution

DRJ: you'll get the right answer

LU: within that black box and torque the physics and that's fine. So it's, you know, there's still this reaching back in to the Sir Napier Shaw, the mathematicians Don has pointed to, Caratheodory, does have its basis for getting, getting us back into some of the more fundamental parameters that other physics, other physics disciplines actually rely on, are on entropy, angular momentum, but not the meteorological _____ .

1:05:30.0

TR: So are you saying, Don with your work you're trying to figure out, you're trying to figure out what goes on in the black box? Is that?

DRJ: Well, I think we understand a lot more than what goes on in the black box through the work that we've emphasized. And I've just handed Louis a letter that I got four or five years ago from, what is it, the Chemical Engineering Society?

LU: Yep.

DRJ: And they wanted to honor me

LU: It's the American Institute of Chemical Engineers.

DRJ: for writing one of the top papers of some two hundred over the [past *] decades and that sort of thing, in, that came, you know, that came out of physical chemistry. Well, it turns out that to be a member of this society and to be awarded, you have to had a major in physical chemistry. And I write a letter to him and I say well I'd be delighted to be honored by this institute. I suspect I'm not the Donald R. Johnson you may be seeking. Once upon a time I did major in chemistry at Bethany College in Kansas and was even accepted for graduate school in chemistry at Kansas State. However on entering the Air Force and attending the University of Washington to study meteorology my connection with chemistry had been through the development application of global modeling and weather and climate employing hybrid isentropic sigma coordinates as well as theoretical studies of isentropic perspectives of global and secondary scientists who also engaged in NASA and DOE and NOAA. [*] However, but these are somewhat distant from engineering chemistry. And so, you know, I have to say I enjoyed reading thermodynamics, Hirschfelder, who was a key professor, was at the University of Wisconsin, Sommerfeld, Chandrasekhar. [transcriber's note: Joseph O. Hirschfelder (1911-1990, founder of the Theoretical Chemistry Institute, professor at the University of Wisconsin] [transcriber's note: is this Arnold Sommerfeld (1868-1951) German theoretical physicist] And then I get a letter back that says you know, well you know we've made a mistake. And so [*] the fact that I got this I guess means probably as much to me as any award I ever [received *], or more so because [laughs] you [*] never really talk to any of these [chemical engineers *] directly, but they've read your work. [laughs]

TR: And what group, I'm sorry, what group was that?

LU: That's the American Institute of Chemical Engineers.

TR: OK.

DRJ: I was always mystified, because I'd always [received *] an invitation to join this group every year. But I never did join them.

TR: Will we have

DRJ: You see I'm clear outside the area of typical meteorology.

1:08:16.1

TR: Right. We've got about twenty minute here. Louis, from the questions that were provided about research, is there anything else? There's a section here about numerical modeling and data assimilation.

LU: Yeah. The, the thing is with Don he mentions the diagnostic aspect and basically unraveling the, the intricacies of the developing circulations, whether it's from a general circulation perspective, and again having that more of the global Hadley circulation [transcriber's note: named after George Hadley (1685-1768) English amateur meteorologist] as

viewed from _____ which is now being recognized. I know that Lenzen [transcriber's note: spelling?] who had doubts about it in the late '70s in one of the talks you gave at Goddard Space Flight Center that I sat in the audience is more accepting of it now. And in the diagnostics with respect to the circulations as they develop in cyclones which is remained more outside I think the generally accepted vorticity perspective that people have used to describe cyclones. But still, you know, couched in these basic principles. Don took it another step further and in fact I was involved in one of the steps in this. Is he got into numerical modeling of the, of the circulation in a hybrid, hybrid system which is isentropic for most of the atmosphere but sigma right near the value layer. And provided evidence both from a general circulation perspective and then you know to show how these could be actually simulated in numerical models, if you select the right coordinate systems. And then got into the real time. Actually set it up here with us group of post-doctoral students, support scientists, to actually run this model in real time. So you know Don can, you know. build more into it but I just want to point out here a lot of people who deal with the theory, theoretical aspects of it don't necessarily get into models. And as Don has pointed out those who have gotten into models have approached them all like a black box and haven't gotten into the unraveling the theoretical mysteries.

1:10:57.0

TR: OK.

DRJ: Well, I'd just say Louis was involved with the development of this model, particularly dealing with the flow over orography also in the PBL [*] one of those things you have to develop in your model as you go along the way, you take on certain parts of the problem piece by piece. [transcriber's note: is this PBL: Planetary Boundary Layer?] If you put everything in the black box you'll probably get a mishmash, but it became very clear that you could deal with the transport of water vapor much more accurately if you put it in [isentropics coordinates base *]. Now here we're jumping, but you can look at this figure up here and I could show you a lot of figures upstairs of how nice and clean you put in a tracer so to speak and can follow how it's carried along with a great deal of accuracy. And this actually came out of our model and when the people who were doing GEWEX saw that and wanted something for their brochure they actually took that figure and put it in this brochure. [*] GEWEX is Global Environmental Weather Experiment? [transcriber's note: GEWEX: Global Energy and Water cycle Experiment] I guess. [*] [Well it's weather, water vapor, and water vapor transport combined. *]

TR: Is it Global Energy and Water cycle Experiment?

DRJ: There you go. That's right.

TR: So we're looking at a pamphlet, Global Energy and Water cycle Experiment, and on their cover is a model based on your

DRJ: Right.

TR: OK.

DRJ: And GEWEX still goes on because of the importance of the water vapor. And just briefly, when I was president of the AMS, that was the theme that I select to emphasize, water. At that time, '93 or something, they just had the Rio conference which had to do with the environment, very broad view, aspect of earth system sciences so to speak. [transcriber's note: is this the United Nations Conference on Environment and Development (UNCED), Rio de Janeiro, 3-14 June 1992?] And at the same time there was a counterpart conference in Ireland, I believe, on just dealing with the water dimensions of it. So that was just a natural thing to emphasize because water and prediction require being able to understand how we deal with transport of water vapor as a gas and when it's realized as a liquid in terms of convection and precipitation, etcetera. [The simulation of water vapor _____ *] was always a first interest of mine. That is a part that was carried out with colleagues here, upstairs, started in some sense with various people who had been post-docs even going back to, well, went back to such people like Rick Anthes. He didn't do isentropic modeling, [*] he had an interest in these things. We, he was interested and we did some work on this energetics of the hurricane which is in the theory of available potential energy. Theory of available potential energy paper. [That is the end of my teaching at the UW *]. Well, I retired in '94 [with *]thirty years of teaching. Seven years in the Air Force before and so I was [*] getting 65 years which is the normal age for retirement. And in the meantime, I had been tied up with USRA going back to Tom Haig. He actually had me become the representative to take over from him to USRA from the University of Wisconsin. And within that activity there was a person by the name of Paul Coleman who was the president and for somehow some way we had a good relationship. [transcriber's note: Paul J. Coleman, Jr., president of USRA from 1981-2000] Paul had been a head of a NASA laboratory, DOE laboratory, [*] in Los Alamos. And then he had gone back to the UCLA to head a planetary institute that they had there, but at the same time he was the president of USRA. And so he ended up [at USRA and *] I ended up being chairman of their atmospheric earth science dimension of USRA because they dealt with satellites and space and everything else. And so I had a group of people with me [*] that I knew from primarily from FGGE and that, that joined this activity in this advisory capacity. [We would *] meet once or twice a year, we did a lot of visiting in various laboratories of the DOE and NASA. That then led to this idea when I retired, [that is they actually encouraged me to retire to come on and have *] an eighty percent appointment with them. And so I had an eighty percent with them, twenty percent with SSEC.

LU: Not exactly the definition of retirement.

DRJ: No. No. I was [kept *] very, very busy [laughs] [laughter in background] because now you had two things or three things before that.

LU: And then we brought you on to NCEP and you did a fifty fifty, right?

DRJ: Right.

LU: Fifty USRA, fifty NCEP. Was that the way that worked?

DRJ: Well, it was fifty [percent *] of that eighty percent with USRA.

LU: OK.

DRJ: You had fifty percent of that eighty percent I believe. You may have had fifty percent of my salary.

LU: I think I got more than fifty there. [laughter in background] I think I got more than fifty _____ but that's OK. [laughter in background]

DRJ: You're probably right. You got fifty out of [laughter in background] but you know, I don't know, I didn't approach you for the appointment.

LU: No. Jack. It was Jack Kelly

DRJ: Jack Kelly did.

LU: Yeah, yeah.

DRJ: Jack Kelly

LU: talked to you and then talked to me. Yeah, he was

DRJ: Alright

LU: very interested in doing this.

DRJ: So I said alright as long as I can in that appointment I can spend fifty percent here, my time.

LU: Right. And fifty

DRJ: and fifty percent, one week out of four [*] in NCEP.

LU: Right.

DRJ: Stay there, have an office and talk to the people.

LU: Right

DRJ: Delightful experience.

1:17:47.7

TR: And NCEP is in DC?

LU: Well, the national centers is actually spread, the principle centers and the modeling center is in, is now at the University of Maryland research park, it moved there. But at the time it was at Camp Springs. And there were five centers. So you got to deal with the climate, principally with the climate center, the modeling center. I think those were the, and then you had some interaction with some of the other people.

DRJ: Well I visited.

LU: Right. Right. Getting back to the research for just a minute. I see we have

TR: I think we, yeah we lot about ten minutes, twelve minutes.

LU: Just real quick. We had mentioned the general circulation into cyclones and again when I came up there was, there was an inclination that perhaps I get involved in the cyclone work but I didn't, other than the contributions I made during the proposal process. And what I understood interacting with Downey and Chuck Wash [transcriber's note: spelling?] I would help. But what I did get involved in, also involved in, the importance of isentropic framework was very, was a basis for my Ph.D. thesis under Don in coupling these lower level jets to upper level jets in the pre-convective storm environment which up to that time were treated as separate entities that, you know, if you read all the papers and the books before then somehow came together by accident, you know, primed an area for convection by having moist air brought in a low, the low and then had drying, cooler air brought in aloft. It enhances the convective instability. And by using the dynamic equations within the isentropic framework and focusing in on the inertial and isobaric components they're able to show that the lower level jet is in fact coupled to and indirectly transfers circulation that fills the troposphere. That's induced by the upper level jet. So that the development of the lower level jet at a significant angle to the upper level jet, which is a key factor in the pre-convective environment, is in fact a coupling mechanism that was really made clear by doing the dynamical approach. Modeling it within this hybrid model that we talked about. That's where I got involved in the hybrid modeling and using the, coupling the boundary layer to the top in a way that allowed mass to flow in and out of both domains. By using that model and by actually applying the diagnostics to a case study, we were able to show that this tropospheric mass adjustment related to these transfer of circulations which were very cleanly and simply diagnosed in an isentropic framework, show that these entities were actually couples, that they were not independent. So in a way I know there was other work that Don was doing with vorticites with Frank Nickelson [transcriber's note: spelling?] and relating it to hurricanes and other smaller scale, but this was a foray into, you know, we kept on downscaling this use of isentropic coordinates and the dynamics processes now into a subsynoptic scale. So even though they were different applications, you know, there was always that connection back to the basics, you know. If it's all consistent with all the available potential energy and kinetic energy work that Don did with Dutton and was certainly applicable, oh let's say, that way to the equations that were being derived for the cyclone. The one thing I didn't do, I didn't take the quasi-Lagrangian approach. I didn't follow [laughter in background] the jets. If I would have

stayed here longer as a post-doc I probably would have gotten into that quasi-Lagrangian approach for the jet stream like he did for the cyclone. I never did get into that. I did discover the hydrodynamic stability thing which we need to follow up on I think. But it, the point I'm trying to make here is that the basics were still the same.

1:22:20.2

DRJ: There's this coupling that always is so evasive to find.

LU: Yeah. People just want to treat things but what's interesting in the field of meteorology and I was warned by John Young when I went out I would be dealt with harshly so be prepared to defend this. Which we did, you know, I mean it was kind of interesting. But there were two aspects, the fact that we did it in isentropic coordinates, to me it simplified things. To other people it confused them. How could something at three hundred millibars influence what's going on at eight hundred and fifty millibars in such a way. So we had to make that, had to make that point. And but now it's just accepted. You know, we kept on following up on this and now it's become, I could tell you the forecasters use, use this coupling. And we've applied it to east coast cyclones and things like that as an important part of the whole cyclone problem. So

DRJ: Well that's good news because I, you know, I'm not out there on the field these days to find out what's accepted and what's not accepted.

LU: They, they use it. Forecasters do use it. And

DRJ: As a

LU: It's not, it's not part of their job description

DRJ: Russ Schneider [transcriber's note: Schneider, Russell S. Quasi-Lagrangian diagnostics of the 9-14 April 1979 Great Plains extratropical cyclone and subsynoptic scales. University of Wisconsin-Madison, Department of Meteorology, 1986. M.S. thesis.]

LU: Russ was actually, OK, so Russ was another one of Don's students who towards the end of his teaching career here. And Russ is now, actually works in the weather service. He worked directly for me within NCEP. He's the director of the Storm Prediction Center. This would be our storm center down in Normal, Oklahoma. Russ is a very careful reader of the science. And he, if I have any questions about sort of this genetic linkage from one paper to another, all the way down the line on almost any topic, I turn to Russ first. And Russ spent a lot of time in the '90s building up training modules on this and he still tracks it very carefully in his role as the director of the Storm Prediction Center. He calls me up occasionally to let me know this is still there. And it's actually even in the second or third generation of students and forecasters since we did this work, you know, people are still learning it and using it. And they _____ into their discussions. So

DRJ: Great.

LU: it's work.

DRJ: There's a picture of Russ shaking hands with the President of the United States here not too long ago.

LU: Yeah. He met, he met Obama down on the tarmac

DRJ: After the Moore tornado

LU: After the tornado [transcriber's note: Moore, Oklahoma hit by a devastating EF5 tornado on 20 May 2013. President Obama visited the city on 26 May 2013.]

DRJ: episode

LU: outbreak

1:25:00.1

TR: This is Russ Schneider?

LU: Yeah, Russ Schneider.

TR: And. Alright, we're at one hour, twenty five minutes. Don is there anything on your mind that you want to discuss before we wrap up today?

1:25:12.5

DRJ: No. I guess I want to get into earth system science education. And why that became a new [endeavor in which I was *] always heavily involved in, as far as in viewing the [circulation of the *] atmosphere and the planet earth [*]. But now it was a time in which [*] this thrust that emerged [*] called the earth observing system which was to take satellites and apply it, now not just to atmospheric science. Very quickly when Suomi retired there was a [group *] of NASA key people that came here [*]. Suomi [and others *] wanted to do what was called SGGE, the Second GARP Global Experiment, because they hadn't solved the water vapor problem. And [NASA *] wouldn't buy it. It had to be much broader than that. And they had what has called a Bretherton report, and that's in here and the thing you find in the Bretherton report, who actually came here after being head of [transcriber's note: Francis Bretheron]

LU: NCAR

DRJ: NCAR, that you find that there's only one page on [with *] one paragraph on education and the value of, you might say, in education. And so [we got to develop that whole education *] area through NASA support in the USRA. So I want to be sure we spend [time on this effort *]

TR: So we'll start tomorrow with earth system science education?

DRJ: education. Yeah. That seem reasonable, Louis?

LU: Yep.

TR: Louis, anything on your mind that

1:27:02.2

LU: No, I, I think we covered the research part. I think we had mentioned Joanne Simpson and I think that we can bring a little bit more into that because she certainly was a important force here. And the last thing is the, I think, you know, the students who

DRJ: I need to talk more about students.

LU: Yeah, the students

TR: Yeah.

LU: that came out under Don. Don actually got the teacher's award from the AMS about, about six, seven years ago. And I know that the letter that was put together that had all the students sign it, that came out with the, when the review committee saw that the discussion ended. And so he's had quite a run, you know, with the students that he's produced. So

TR: OK.

DRJ: Well I got the AGU award and when they actually saw [this letter about Earth System Education *], the fact that we'd reached well over one hundred thousand students in sixty seven universities involved, I guess quickly solved that, too.

LU: So you're talking about your role in USRA.

DRJ: Well, that had to do with the AGU and USRA, that's right.

LU: Right. So you got that award

DRJ: but

LU: and then you got the AMS award, too, on what, the type of students

DRJ: Right.

LU: that you produced and the leaders that they've become. So

TR: Right.

DRJ: But you have to realize these were connected.

LU: Yeah.

TR: Yeah. Right. OK. So we will start there tomorrow. Thank you both for your time. This concludes the third interview.

1:28:33.2

End of audio

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