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

Troy Reeves: Alright. Today is August 22, 2013. This is the second interview with Don Johnson. Also present is Louis Uccellini. And this interview is being held of the twelfth floor of the Atmospheric and

DRJ: Oceanic

TR: Oceanic Science

DRJ: Yeah. AOS.

TR: AOS. [transcriber's note: Atmospheric, Oceanic and Space Sciences building, AOSS] My name is Troy Reeves. I'm with the UW-Madison Oral History Program. Louis, you wanted to lead off, sort of summarizing what we learned from yesterday.

Louis Uccellini: Yeah. So I just thought there were two things that struck me. One was the connection with Ed Danielsen [transcriber's note: Edwin Danielsen, Pennsylvania State University and National Center for Atmospheric Research (NCAR] earlier in your career than I realized, that he was an instructor at the University of Washington when Don was, had his first meteorology foray, that opportunity he had to go out there. And the reason I say that is that I wondered where Don's connection to isentropic analysis, when it came into his work. I assumed that the connection really was solidified when he went to Penn State later in his career as a visiting, I think you were visiting a professor there or scientist for one year, maybe

DRJ: Visiting [associate professor *].

LU: Right. And I thought that's when the connection happened but it happened much sooner, much earlier and I think this is important given the nature of Don's work, whether it's in the general circulation or the cyclone or even getting down into the mesoscale when we did the jet stream, jet stream work. So that was one of the aspects that I took away yesterday, this connection to the isentropic world. And the credit that goes to Ed Danielsen, I guess is helping to spread the word because clearly that approach has its, has its advantages, important advantages. The other aspect was the importance of the Air Force in Don's career. As a student, he didn't talk about that much. You know, you'd occasionally get a comment from him, oh yeah

I forecasted in the Air Force. That was it. [laughs] And you know, whether he just did his time and got out or you know, it was not made clear. And what was made clear yesterday was the fundamental importance of his experience and his growth through the Air Force and obviously just like I think his teachers in Kansas provided him a basis for moving forward, that experience and opportunities that the Air Force provided, I would say as an outside observer, was just as important. And as a student I didn't realize that.

0:03:19.5

DRJ: Well, let me just add a few words because this question of what was my connection with Ed Danielsen and did that really impact the fact that we pursued our diagnostics and even later modeling using isentropic coordinates. I guess I'll look back and say that really [wasn't at *] the forefront because what happened in graduate student experience here. John Dutton [transcriber's note: Dutton, John. The relation between atmospheric entropy change and available potential energy. University of Wisconsin-Madison. Ph.D. thesis. 1962] [became *] interested in available energy theory, ala Lorenz, and Lyle Horn did, too. In fact Lyle Horn did his Ph.D. thesis [transcriber's note: Horn, Lyle Henry. A statistical analysis of the relationship between the kinetic energy and potential energy of the atmosphere. Madison, University of Wisconsin, Department of Meteorology. Ph.D. thesis. 1961.] on looking at an alternative to the quadratic of the temperature distribution because he felt it really was the geopotential energy [was important *]. If you go back and look into Lyle's thesis you'll find that he has set out forth how this connection with the geopotential energy part. See, there's the [geopotential energy due to lifting of the mass against gravitation *] and then there's the energy due to the thermodynamic unrest of a gaseous medium and that has to do with the variance of temperature as the basis of kinetic theory of gases. But in being Lyle's student and listening to this and then trying to sort out the link of available potential energy as it's pursued in a diagnostics and isentropic coordinates, that blackboard experience with Al Carasso [transcriber's note. Carasso, Alfred S. Certain aspects of energy transformations in the Northern Hemisphere. M.S. thesis, 1964] and other students in the room [with *] Lyle and then [with *] John Dutton [we *] established this connection and John's interest in this area. That really was probably the tipping point where we moved into [the extensive diagnostics in isentropic coordinates *].

0:5:25:0

LU: That's interesting, so when you went then to Penn State

DRJ: Oh, we already had worked out the details

LU: You'd already worked

DRJ: The Advances in Geophysics.

LU: So when you, but then when you went to Penn State, you met Frank Sechrist, [transcriber's note: Frank Sechrist, University of Wisconsin-Madison, Department of Meteorology faculty, 1967-1987] he was a student there, right? At the time? Do I have that right?

DRJ: That's right. And Ed Danielsen.

LU: And Ed Danielsen was actually his major professor at

DRJ: Major professor at Penn State.

LU: Right. And so,

DRJ: My going

LU: it got so when then Frank came here as a synoptic professor, he solidified isentropic [analysis *] within the synoptic lab as he was teaching the students.

DRJ: Yes.

LU: You were, you were pursuing research in isentropic coordinates.

DRJ: Right.

LU: And it was, so there's still this, and John Dutton became a full professor at Penn State. He had gone to Penn State. In fact you were probably visiting John when you went there, right?

0:06:21.1

DRJ: Well, that's because of this link that John and I had.

LU: Right, exactly.

DRJ: He invited me there.

LU: So there's this, sort of this collection of things coming together and I'll just point out here from a personal perspective that getting exposed to, actually my first exposure to it was through Frank Sechrist who Don brought here after he went to Penn State. And one of the reasons I transferred up to Don was because of the research being done in this framework. So there was this sort this connection being made.

0:06:50.5

DRJ: Well I had been appointed as an assistant professor and actually I made associate professor in two years which was kind of unusual at the time. And that in another year or two I got invited to come to Penn State because John Dutton had moved to Penn State from the Air Force's ETAC in Washington, DC and we were continuing pursuing these concepts from a theoretical standpoint. However, when I got to Penn State I probably didn't realize that Ed Danielsen was

there. Walked in to the lab work and here was Frank drawing diagnostics of jet streams, the kinetic energy maximum of a jet core moving within the upper flow. And it was fascinating so we immediately could connect.

LU: Right.

DRJ: [We *] meet Frank and his wife, and [were invited by *] the Ed Danielsens for a dinner one evening. [*] So all these things just came together.

LU: came together. Right.

DRJ: because of our mutual interests.

LU: Right.

DRJ: The critical thing was that experience of going to Penn State and seeing Frank Sechrist. Alright, so we invited Frank Sechrist to come and lecture during the summer school in meteorology. He liked it here and then of course we made him an offer. And people marveled at his work in synoptics, there's no doubt about that.

LU: Right.

0:08:26.3

DRJ: And he stayed although there was a very interesting episode that happened that when he was here. [Just *] before the semester was to start when Frank was going to be a faculty member. He says I'm going back to Penn State. [laughs] [laughter in background] So Lyle Horn and I [laughs] got him down to a bar [laughs] and talked to him and told him why he should stay here because you [have *] so much resources here, that a new dimension you might say with meteorology with the satellite work [is feasible *].

LU: And McIDAS was being talked about and all that. [transcriber's note: Man-computer Interactive Data Access System, developed at SSEC, beginning in 1972]

DRJ: And so Frank did agree to stay and his wife came here and we were good friends then. We've crossed paths many times after that, including even when he eventually ended up in the Navy out at Monterey [transcriber's note: Monterey, California] as part of their big research effort. But he [*] always had this desire that he wanted to be at Millersville [transcriber's note: Millersville University in central Pennsylvania] and so that didn't work out so that lead him to go to Monterey. By that time he had [also *] gone to NASA.

LU: He worked _____ at NASA for a year. Right.

DRJ: but he was always [had a sort of *] unrest in him wanting to look to something new. He'd actually been flying when he was in the Navy and flew over hurricanes, hunters, reconnaissance so he had this really interesting background.

0:10:05.6

TR: I'm. Alright, so let's continue the chronology, or at least as we've been trying to do it. I think when we left last night, you were here as an assistant professor. It sounds like you sort of quickly became an associate professor. My first question is just basic logistics. I don't think this building was built when you were here, so where were your offices when you first got here?

DRJ: Well, we were in Science Hall and we were on the fourth and fifth floor. That of course was the Geology Geophysics building and Reid Bryson, [transcriber's note: Reid Bryson (1920-2008), in 1948 became the first chair of the Department of Meteorology, University of Wisconsin-Madison]. [He *] had come here in ['46 *], that's right, as an assistant professor in the geography department. [There *] were some interesting tensions in the geography department between Trewartha [transcriber's note: Glen T. Trewartha (1896-1984), faculty, University of Wisconsin-Madison, Department of Geography from about 1923 to 1966] and Reid Bryson. [Then of course *] the university was expanding and Reid went to the dean and they organized this department of meteorology in '48. And at the beginning he actually drove down to the University of Chicago [in 1949 *] and brought Suomi and his wife back to join the faculty here, even though Suomi didn't have his Ph.D. at the time. [transcriber's note: Verner E. Suomi (1915-1995) joined the University of Wisconsin-Madison Department of Meteorology in 1948, father of satellite meteorology] He had become quite a well established atmospheric scientist in the design of instruments and things like that. So Suomi finished his Ph.D. here when he was in Wisconsin, although it was awarded from the University of Chicago. [transcriber's note: Suomi, V. E. The heat budget over a cornfield. University of Chicago. Ph.D. thesis. 1953.]

0:11:51.4

DRJ: Now there's about a ten year period here before I arrived on this scene. In the meantime the department actually acquired Lettau so there were three professors here, Suomi, Lettau and Bryson. [transcriber's note: Heinz H. Lettau (1909-2005) professor, University of Wisconsin-Madison, Department of Meteorology from 1958-1980]

LU: Was Wahl there by that time? [transcriber's note: Eberhard W. Wahl, University of Wisconsin, Department of Meteorology, faculty from 1963-1984]

DRJ: No, no. There was [*] I'll think, Businger, Josst Businger [transcriber's note: Joost A. Businger, Professor Emeritus, University of Washington, Department of Atmospheric Sciences] who came here and was an assistant professor, although as an established scientist, he moved on eventually to University of Washington and became quite a well established person there. In any event, when I arrived [*] these were the three main professors, although Robert Ragotzkie who

was a student of Reid had just joined [*] the faculty as an assistant professor in that ’59 or maybe the ’58 year.

0:12:41.7

TR: OK. So then the three when you got here were Suomi, Lettau, and Bryson?

DRJ: Right.

TR: But there were a couple others, but those were really the main.

DRJ: No. That was it.

TR: OK. So Businger was

DRJ: He was here a couple years before, but he went on to the University of Washington.

0:12:59.0

TR: Alright. I’d like to talk about. I’d like to start talking about teaching today. And there’s some other things I want to talk about but that’s one of the sections that we have some good specific questions about. So the first teaching question is, and this is on, if you want to look, it’s on the flip side.

LU: Oh, I think it’s one page at a time. It’s right here, teaching career.

TR: So maybe we start with, look at this from the higher level so the overarching teaching or educational philosophy you came, you came here with.

0:13:40.0

DRJ: Well I had a minor in education from the Bethany College, not that that made any difference once you get your Ph.D. But it certainly didn’t hurt in some sense, since you always had in the back of your mind that I was going to pursue teaching except the Korean War came along and that essentially is the reason I [joined *] the Air Force. And I told you about the experience of the math professor making this announcement and that was the key thing that changed the direction of my professional work. [*] When I came, one of the reasons I think I was offered an appointment here was that the synoptic lab was considered one of the keystones of teaching meteorology, that’s where you actually go into the practice of doing analysis of all observations and forming the weather patterns that we use [for *] actually both understanding and forecasting at that time. At that time we did not have computers actually making numerical weather prediction. From the period, when I [arrived as *] graduate student [*] there was only three [professors *], but by the end of the time I finished my [graduate studies *], they had brought in Professor Schwerdtfeger and Professor Wahl. [transcriber’s note: Werner Schwerdtfeger (1909-1985), faculty, University of Wisconsin, Department of Meteorology,

1962-1980] And I'm not quite sure how the connection happened with Professor Schwerdtfeger, although he had been in the German weather service and had been a brigadier general heading the forecasting for the Nazis at that time, or I should say the German military. He had over two thousand [reconnaissance flights *] over the North Sea because during World War II. They always wanted to know what's happening over that area because that's where the weather was [*] coming from. And he had actually gone to Argentina and had become quite interested in the meteorology of Southern Hemisphere. Somehow, some way there was that link that was made and there was some things that we were doing I guess in the Southern Hemisphere. They brought in Schwerdtfeger because of his experience and expertise of the weather in South America. [transcriber's note: along with his many scientific papers, Werner Schwerdtfeger was the editor of Climates of Central and South America, with is volumn 12 of the World survey of climatology series.] Lettau was always quite interested in the weather of South America, particularly the Andes and the

LU: Peruvian desert.

DRJ: Peruvian desert and the Southern Hemisphere anticyclonic circulation which was so tied to the [northern *] flow of the ocean as an equatorial current along the Andes [*]. And then of course [this current forms *] the Humboldt Current that goes out across the equatorial region from the Americas over to Asia. There were these natural links that [*] brought Schwerdtfeger here to do synoptics. Along the way there was this link to Eberhard Wahl who had been interested in space [*]. He was, I believe, a pilot in the German [*] military. [coughs] Excuse me.

0:17.38.3

TR: So at least

DRJ: They wanted somebody to teach synoptics, to help Schwerdtfeger so to speak, because Schwerdtfeger wasn't always interested in teaching synoptics. He wanted to teach other things, so I think that's one reason why I got the offer. I had this Air Force experience. [laughs] But [the offer came *] from Professor Lettau who was chairman at the time and of course the other faculty had a [*] saying who they're going to invite or who they asked to be a member of the faculty.

0:18.08.2

TR: So, you came in and immediately started teaching synoptics?

DRJ: Synoptic meteorology.

TR: Is that the only class you were teaching or where there

DRJ: Well, the first year I'm sure it was, but then Lyle got busy and I was asked to teach the theoretical courses, too, in meteorology, let's say the second or third year using his notes and others I had gathered from Professor Lettau as I sat in his courses. And so I started teaching you might call it general circulation theory at that time.

0:18.49.3

TR: So, so Horn was, was Horn on faculty here?

DRJ: Oh yeah.

TR: OK, so

DRJ: He was an associate professor now I imagine by this time.

TR: So we've mentioned at least six now when you actually started teaching, Suomi, Lettau, Bryson, Schwerdtfeger, Wahl, Horn.

DRJ: Right.

TR: OK. And this expansion, you mentioned this last time, that this expansion was because of research and if you had research dollars that UW would match it? Is that

DRJ: Well the whole university in many [of *] the science areas was expanding and the dean at that time actually and I guess probably with the blessing of the chancellor and everybody else, if you had and could bring in research dollars, then [*] half of your faculty salary was on research dollars, they only had to put up half for the teaching. So we actually taught more for that half time than most people do clearly. I even taught two courses sometimes on a half time faculty salary.

0:19:58.9

LU: OK. I just wanted to interject, when you mention the six, you also mention two other names besides those six: Ragotzkie and Wahl. And it's not clear exactly when, so I think it went from three to four when Ragotzkie came on, right?

DRJ: and that, and he actually joined before I came here as a student.

LU: Right.

DRJ: Or that first year

LU: That first year. So, mentioned the three and then Ragotzkie would make four. And then you have Schwerdtfeger and Wahl.

DRJ: Schwerdtfeger came before Wahl.

LU: Right.

TR: And then Horn

LU: And then Horn.

DRJ: Well, Horn came in as a faculty member before Schwerdtfeger got here.

LU: OK, so it sounds like you had the three, then Ragotzkie, then Horn.

DRJ: That was probably right. He became a faculty member before I actually finished my Ph.D.

LU: OK. And then you bring in

DRJ: So that would be roughly in ’62, ’63.

LU: And then you bring in Schwerdtfeger

DRJ: Right.

LU: and then Wahl

DRJ: Right.

LU: OK.

TR: Alright.

DRJ: Now there was, I can’t think of his name right now. They brought in a young assistant professor. I’d have to go back and look, who lasted here for three years but he didn’t make tenure so [he left *]. Deland, I believe he was. [transcriber’s note: Raymond J. Deland]

0:21.17.4

LU: OK. There was another connection that I had heard about and this probably traces back in to the late ‘40s, early ‘50s. When Suomi came here there was a professor named Tanner? [transcriber’s note: Champ Bean Tanner (1920-1990), University of Wisconsin-Madison, Department of Soils] But he wound up instead of in meteorology he wound up in Agriculture?

DRJ: Tanner was a soils scientist.

LU: That’s right.

DRJ: [So as a professor he was associated with the Department of Meteorology and I guess was also on a joint appointment as a professor with his salary and everything coming from Soils. *]

LU: OK. So and that's were Suomi started, was doing his radiation studies on different farm fields, and whatever and corn fields. They've got him walking through a corn field with a radiometer.

DRJ: Yeah. Out here by the hospital.

LU: Where the hospital is now.

DRJ: Yeah. Well, down in that marsh area where they actually grew corn at that time.

LU: And, and Tanner was involved in that because I think there were picture of him.

DRJ: I don't know.

LU: And when they, when Lettau was here, starting up and doing his boundary layer studies I guess they had the bushel basket experiment out on Lake Mendota to get the roughness of the lake. You went from smooth ice to these bushel baskets and then he could, they could [study *] the wind profile and come up with the roughness estimates [used to estimate frictional dispensation *]. So that kind of really basic field programs literally was taking advantage of the surrounding areas here in Madison. And ultimately Suomi's work grew into radiometers and then space designed instruments. And I guess Lettau became a major force in the Great Plains experiment for

DRJ: even before Suomi got into space.

LU: Yeah, right. So but the idea was that they started with these local type of experiments that then grew into more of a national, into a national basis.

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DRJ: Well, Lettau had his national, international connections and so he would be able to get research for planetary boundary work.

LU: Right. Right.

DRJ: And in so doing they established a lab [here near Lake Mendota *]. There was an old house out on the north [shore of *] Lake Mendota [that *] is on this side of Lake Mendota but it faced to the north, [overlooking *] that open area, whatever bay it's called, to the west. And there was another person here became a faculty member, I think even after I did, but he had been here as a researcher by the name of Charles Stearns, [*] Chuck they called him. [transcriber's note: Charles R. Stearns (1925-2010), University of Wisconsin, Department of Meteorology, faculty from 1965-2010. Pioneer in polar meteorology, considered the father of the US Antarctic Program's Automatic Weather Station system.] Chuck had worked very closely with Suomi in his work in that area of, working on planetary boundary layers and radiation what have you. And he was also heavily involved in Suomi's first design of the heat budget [satellite *] that

went up to measure the balance, the energy balance of the atmosphere, that was Suomi's first satellite. But along the way he transitioned to Lettau and worked closely with Lettau in making that laboratory work [near Lake Mendota *]. Lettau was always interested in a key problem at that time, [just *] was how does the surface of the earth [impact the *] flow of the atmosphere over the earth, [that is how do they *] link to each other. And you have the dynamical link, you have the radiation and other means by which you exchange energy but the dynamical link had to do with getting into concepts of the frictional stresses in the gaseous medium and how that momentum would be transferred [between *] the atmosphere and the earth. He talks about the bushel basket, bushel basket experiment which was a unique one. They would have an ice covered lake and then they would saw out so that you would get a slab of ice here that would be free from the rest of the ice of the lake. Then they put [put the bushel baskets on that ice part and place tension gauges to determine *] how the tension that would come from this frictional coupling of the bushel baskets causing the turbulence in the atmosphere. Of course one of the key areas of Lettau was turbulence. He wrote the first book on atmospheric turbulence in '39. [transcriber's note: Lettau, Heinz. Atmosphärische Turbulenz, Leizpig, Akademische Verlagsgesellschaft, 1939.] That was in German but still recognized as the initial book on atmospheric turbulence. So [these experiments went on for a decade. *]

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TR: So you've mentioned at least one person who was Tanner, who was in Soils Sciences and one of these questions is the importance of a multidisciplinary approach for engaging all the disciplines. Is that something that was important to you, both in your research and your teaching?

DRJ: Well, along the way in my research I became interested very much in statistics and the theoretical work involved here. So [*] in my third or fourth year of my graduate work there was a visiting professor from Princeton who was really, had my first course and really got me intrigued in statistics. So I ended up for my minor in the Ph.D. being statistics and took courses from G. P. Box who was considered and was a world recognized statistician here. [transcriber's note: Professor George E. P. Box (1919-2013) established the Statistics Department at the UW-Madison in 1960] [His courses *] were simply eye-opening as to how you link up statistics and importance of models in some physical, dynamical or any process and then looking at the uncertainty that comes about from trying to apply it. And he really taught me many things of which I, you find in meteorology is that we have all this uncertainty [that *] comes from [predicting that much precipitation occurs *] and as this front came through.

LU: We talked about that last night. [laughs]

0:27.52.5

DRJ: Right. But because of what you would call the lifting instability, convective instability, the moisture in the atmosphere that was released and what they forgot in some sense and what

they don't really use in TV meteorology that much is the looking at thermodynamic diagrams and really deciding [if there is *] convective instability by release of latent heat or is it by lifting by the dynamical convergence of the atmosphere along fronts. You see that when I was in the Air Force we would come in the morning and you'd have all these maps of the flow at the various levels and then you would have the thermodynamic diagrams, what's called the Skew-T Log-P, and you had to do analysis of what type of air mass it was, what type of instability was there, and we had all these categories.

LU: Level of free convection.

DRJ: That's right.

LU: Whether you could get to it that day or not.

DRJ: Yeah. And so that even when we taught [*] synoptic meteorology we had maps [and soundings *] like that always on display [*] on the fourteenth floor. So it was kind of interesting this morning, obviously this type of instability was released. [While *] we might have possible rain showers, [*] but it was not likely [*] to end up here raining in Madison because it was going to the northeast or southwest.

0:29.34.1

TR: I told Louis before you got here that having two meteorologists in the room, we were going to talk about today's weather [laughter in background] so I'm glad that you, glad that you brought it up and I didn't have to ask directly about it.

LU: I'm glad he brought it up, too. [laughter in background]

DRJ: Yeah, he's responsible for talking about the weather. [laughter in background] I can sit back and kibitz.

LU: Well, the one thing I tell folks is that one thing I can promise people we will never have perfect forecasts.

DRJ: That's very true. Profound. [laughs]

TR: And that's something that never changed over time, right? And there's no perfect forecast.

LU: What's really fascinating is that it appears, you know, there will be exceptions to this, too, but that we do a better job on the extreme events now than we do on the light to moderate events which really tests the limits of predictability. Not the extreme events, which is not true when I was a student and in that time frame in fact there are memos that say do not predict. You know, even when Pierson [transcriber's note: spelling of this name?] wanted to do the first tornado warnings, he was told by upper level management we will not predict or warn for tornados because we will just scare people. This notional thing of predicting rare events traces back to

them being relatively rare, so if you have extreme event they're relatively rare. Do we have the capability of predicting rare events? And answer back in the '50s, '60s, and '70s and before that was no. So now you see extreme events actually sticking out, you know, eight days, seven days, six days, five days, four days, because on a larger scale the factors playing in there. You get a light to moderate events over small event like we had last night here, the notional aspect of convection was certainly predicted. But the details of when it would die, if it would reform in the morning, or wait until the afternoon over Chicago rather than reform over southern Wisconsin in the early morning was something that wasn't connected, so, or covered. So those are your challenges. Those are your, we'll always have challenges.

0:31:50.8

DRJ: We, we could tell an interesting story here about story here about Lorenz who is considered to be, I guess, the premiere theoretician in atmospheric science in this area. [transcriber's note: Edward Lorenz (1917-2008), American mathematician and meteorologist, pioneer in chaos theory] Ed Lorenz got into meteorology just like I did and many others, but that's World War II. He nearly had finished his Ph.D. at Harvard and in his final year he was taken into one of these programs because you know you either went into these programs or you went into the Army. So, he became a meteorologist. Both Reid Bryson and Ed Lorenz ended up on Okinawa in this final years of the war and were [*] predicting for the B-29s that were bombing Japan. [Under the leadership of *] General Curtis LaMay [headed the *] SAC Air Force. Anyway, so they actually knew each other. Now Reid, of course, excelled in climate and the broad aspects of dealing with the earth as a system, being geological he actually had a master's I believe in geology before he [entered the military *]. Ed practically had a Ph.D in mathematics. So when he came back to MIT, he finished his Ph.D. under Victor Starr who had been at Chicago but moved to MIT. [transcriber's note: Victor Starr (1909-1976), MIT faculty, meteorologist] And eventually because of this, call it the uncertainty, he formed the theory of chaos which has become quite a popular subject among many different disciplines. Which brings uncertainty from various forms, but you do get a mathematical relationship dealing with hyperbolic functions which you look at and you get certain patterns that are repetitive and then all of a sudden it slips into something else. And that he developed from the fact that he was using computers, he would quit at the night, evening and put his numbers down, come back the following morning and try to put them back in the computer to get the computer to start up where he left off. But it would always would go in a different direction. He just didn't have enough information to get out to having a precise level. But there is always at this end here regardless where you are, uncertainty out there that you can not actually [eliminate the uncertainty *]. And so there will always be [uncertainty *] in meteorology, as Uccellini was saying. Now, I'll come back to it, how I got tied up with Ed Lorenz to a certain extent later on, but in the meantime I guess we should talk a little bit more about this relationship that got established between Penn State and Wisconsin.

0:34:52.7

TR: Certainly. Yeah.

DRJ: OK. Well once actually John Dutton went to Penn State and he'd been there maybe a couple years the head of the department, and they had heads there instead of chairmen. [*] Al Blackadar was head of the department and went on leave to Europe, Germany I believe. [transcriber's note: Alfred Blackadar, Penn State Department of Meteorology head from 1967 to 1981] John became the acting head and that's when he invited me to come to Penn State. And then of course we stayed [one year *]. My relationship [*] with Frank Sechrist and even Ed Danielsen who were established at that time, getting invited over to Ed's for dinner. [*] Because he remembered me as a student maybe, but we had this mutual interest in doing isentropic analysis by now, so we just had endless things to talk about in many ways. The, when I was there they were expanding their department also and there was a person by the name of Denny Thomson who came out of Wisconsin. [transcriber's note: Thomson, Dennis Walter. Airborne measurements of tropospheric structure relating to transhorizon microwave propagation. Madison, University of Wisconsin, 1968. Ph.D. thesis.] He really was in physics and I guess atmospheric science, and he had been my TA. Denny wrote to me and said you know I have written to Penn State, Blackadar and never really got a response. So [*] then I went to John Dutton and others there and told them Denny Thomson was really superb. He had been given a support [as a *] post doc to work in Europe. And so they invited him to be an assistant professor just based on my word. This lead to the fact that John Norman went there [for some years *], too, from Soils and Atmospheric Science [*]. John came back and became a professor [at the UW *] here, too, but Denny Thomson always stayed [at Penn State *] and is quite well known for his instrumentation work. [*] Now by this time I had graduate students, one of whom is Rick Anthes. [Rick had actually *] started in [an undergraduate *] reading course that he took from me as he was a senior and then became my [graduate *] student. He got interested in predicting hurricanes and so he was developing a numerical model to simulate hurricanes. Toward the end of that, [as he was *] doing his summer work [with *] the National Weather Service and the hurricane lab had money for students to come down there. [He *] decided to go and work there for this final year of his Ph.D. developing the hurricane model. And OK, so along come let's say February or so of the year I was in Penn State and Rick [*] provided a draft of his Ph.D. thesis. I invited him to come up [to Penn State *] and we'd go over it. And when he did come up at that time, the fact that he was such a good scientist, I invited the faculty over, my wife and I did, to spend an evening with Rick. And then that opened up this fact that [when John Dutton *] became the acting head or somewhere along that way, it was John who invited him to come for one year as a visitor to Penn State faculty. So Rick went to the head of the hurricane lab at that time and they weren't going to let him go to be on a one year leave. So they refused and Rick said well, you know I'll just leave the hurricane lab and become a member of the Penn State faculty which they also quickly or already had in mind. And then Rick became a very well established scientist at Penn State, leading in many ways being the key individual in developing hurricane models.

0:39:43.1

LU: and the whole regional approach to model

DRJ: numerical, but that came later in some sense. And he had a whole series of students that became, got their Ph.D.s and many of them ended up at NCAR UCAR. [*] Once Rick got so established they invited him to come and head NCAR and then he moved up to be the UCAR president which he was for some twenty years. So all that time of course, we had maintained in many ways a link so I was always welcomed if I ever went to UCAR NCAR to come over and have dinner with Rick and Susie. And we did that on numerous occasions. And of course then I quickly [related with *] Rick's students who were out there at the time, too. Bill Kuo and what was the other person that you, he passed away here not too long ago. [transcriber's note: Ying-Hwa Bill Kuo of UCAR]

LU: Oh, Tom Warner.

DRJ: Tom Warner. Right.

LU: Yeah.

DRJ: And these were leaders in the development of regional models at NCAR.

0:40:56.2

TR: So that year in Penn State sounds like it's very important for not only the cross pollination between UW and PSU but also a lot of interesting meteorological work.

DRJ: That's very true I think.

LU: And actually the growth, the growth of the departments, you know, you talk about Denny Thomson going out there and Rick going out there, They became two very important people within the context of Dutton and others who were there, you know, building up the department. And I think it's something that I've seen at Penn State is that they have people from the department rise within the ranks of the university so they become deans and you know, vice, I guess they call them presidents or vice presidents there. I, they have a

DRJ: Yeah.

LU: different terminology for the structure, but what it meant, what it translated to was that they were always able to sustain the growth and the quality of the department because as professors came and went they were always able to fill in behind because of that. And Dutton was a classic example of that. He made sure that people, when he rose up within the university that he had people coming in behind him that could fill those positions as he moved on. Right? So, it was really an interesting connection here.

0:42:17.7

DRJ: Yeah, the, particularly when he got involved in the earth system science modeling _____

LU: Barron

DRJ: Eric Barron. [transcriber's note: Is this Eric J. Barron? President of Florida State University, former director of NCAR, former professor of geosciences at Penn State]

LU: Eric Barron. _____

DRJ: The, there's another aspect here also we need to cover and that has to do with when Frank came here, because Penn State had regularly provided meteorological forecasts through their [radio *] station for years going back to Charlie Hosler and what have you, who became a dean, vice president, etcetera. [transcriber's note: Charles Hosler, credited with transforming television weather forecasting into a more accurate presentation based on science, which in 1965 led to Penn State's own VHF television station, WPSX] But anyways, Frank had this interest and brought it here and of course through the efforts of SSEC [transcriber's note: SSEC - Space Science and Engineering Center, University of Wisconsin-Madison] and the department to some extent when I was chairman and Tom Haig who was the executive director of SSEC, we actually provided the first views of weather in motion using the geostationary satellite information that was brought [on PBS at Madison *] and I don't know exactly the year, that is probably '68?

LU: Ah, yeah. I think, you know, this is, Suomi was a force behind that, too.

DRJ: Oh, yeah.

LU: McIDAS

DRJ: Well,

LU: I think, that didn't really happen until the early '70s, J.T. Young

DRJ: You're right, '73.

LU: Yeah, J.T. Young actually became the project manager under Suomi in SSEC, but what Suomi and J.T. Young did was they engaged Frank Sechrist.

DRJ: Well

LU: You probably played a role in that, too.

DRJ: Oh, yeah, well. [laughs]

LU: but they got him interested in this so he eventually wound up, that's how he eventually wound up on TV because he had this weather in motion to bring, so it was, you know, this thing sort of, you know,

DRJ: Well, that happened, that happened because of Tom Haig.

LU: Tom Haig

DRJ: Tom Haig and myself.

LU: The connection with the

DRJ: That's right. We got the funding to do that and the link to WHA [transcriber's note: Wisconsin Public Television channel, based at the University of Wisconsin-Madison] and I had my graduate school connections at that time because I served on the graduate school research committee and all that. So then we got money to support the help for Frank including bringing Kelly here.

0:44:41.2

LU: Terry Kelly [transcriber's note: Terry Kelly, degree in meteorology in 1971, television weather forecaster, co-founder of Weather Central]

DRJ: Terry Kelly who became quite a well known person in Wisconsin, particularly in Madison. But once that happened there's another dimension to that. [I have been supporting Tom Whittaker in *] doing the mathematics of mapping meteorological information into what's called McIDAS, Man Computer Data Access System. He got so interested in this that Suomi and people here quickly, Tom Haig in particular, quickly let him work in that area.

LU: Help build the system

DRJ: Well, help do this software development. And I gradually, actually of course encouraged that because I was providing support to him [through NSF funds *] and he was doing all this work for us but I mean this was just beautiful work.

0:45:35.3

LU: So you see how the accommodation of people you have somebody like a J.T. Young who is more hardware oriented.

DRJ: Right.

LU: you have Tom Whittaker, who is more software oriented. You get the science folks here. You get somebody like Frank who had the personality to be on TV and make it exciting. And he eventually wound up on, I think it was W ___ H Chicago. [transcriber's note: WLS-TV, Chicago]

DRJ: Chicago, right. Weekends.

LU: on weekends. So but the whole idea was could, can you get this data in real time onto a system like that. Can you display it in such a way that you're extracting the information. Can you make it understandable so that, you know, the general public, it can be used in terms of a weather cast. I mean. And people in this department, you know, were able to sort of help put all that together.

DRJ: Well,

LU: I know Suomi was interested in that, too, because he clearly needed to show from a satellite perspective that he could do all this stuff in real time. It wasn't just a matter of somebody writing a paper on it.

0:46:38.3

DRJ: Well, he definitely was. To give you some idea of [the interactions *] when I was [the chairman of the department for three years *], we established these relationships between the department and SSEC. There had been some sort of stress in the meantime. But Tom Haig and I got along well. This link to Suomi of course, with Tom Haig and Suomi was at the heart of it. And once you had this capability and demonstrated it through WHA, the information was taken to the next AMS meeting [transcriber's note: American Meteorological Society] and that exploded because everybody saw this visualization that you could actually put in a computer tape and show at a meeting was magic. [transcriber's note: First public demonstration of McIDAS. The video titled, "Processing and Display of Satellite and Conventional Meteorological Data for the Classroom" was presented at the 57th Annual Meeting of the American Meteorological Society, Tucson, Arizona, January 1977.]

LU: What was the experiment in the late '70s, the FGGE?

DRJ: Well, FGGE, the First GARP Global Experiment [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]

LU: Was that the one, the field, where Suomi had real time, you know, he was doing it real time around the clock here with graduate students you know or paid assistants. So he really, really, I'm sure people kept on saying, like the wind, those winds look good but if you can't get it to us in real time, you know, how can we use it? So the system now became sort of an engine for not just visualization but for actual product generation and

0:48:12.8

DRJ: You really have to bring in the international dimension to this [event *] particularly. Because [*] a couple years before what you're talking about, they had decided to have a global

weather experiment and the first part of that was the first GARP global experiment. [transcriber's note: GARP, Global Atmospheric Research Program, a research program organized by the World Meteorological Organization (WMO) from 1967 to 1982] Suomi was one of the four people who headed [the effort *]. I don't remember all the other two, but one of them was from MIT who did instability, theory of numerical instability, who would have not

LU: Was that Charney?

DRJ: Charney [transcriber's note: Jule Gregory Charney (1917-1981) American meteorologist, faculty at MIT]. Yeah, and two others. I believe Tom Malone was one.

LU: Right

DRJ: but I don't remember the other, the fourth one. But so these four people set out a GARP global experiment that we're going to [*] create information for the global atmospheric circulation using satellites. And there were two types of satellites by that time. Along came the geostationary satellite that Suomi put up but in the meantime. They had what are called polar orbiters where you actually have a camera on a polar orbiting satellite and the difficulty with that is that you have to put these strips together. So Suomi was having problems with putting these strips together because a person by the name of Fujita [*] at University of Chicago was able to do this more efficiently, in many ways better than the people here although he was doing it, but he got more, more publicity on it. [transcriber's note: Tetsuya "Ted Fujita (1920-1998), University of Chicago, famous for his work with severe weather, tornados and hurricanes] So Suomi [*] sat at home and watched the play back of a football game and he says you know I'll put a satellite [at *] geostationary altitude, and we can do that same thing with satellites. So you're looking at the same place on earth so to speak. That led to a whole host of geostationary satellites. There was, Britain had one. We had several. We had three in fact. And [the *] Soviet Union was supposed to put up one. So there was supposed to be five satellites to do this FGGE experiment. OK? And lo and behold the Soviet Union didn't get its ready on time and so we're going to be missing one of the geostationary satellites over India, the Asian continent. And so by that time there were three geostationary. One up there was sort of a backup for the others because we had one in the east and west here of the US and then Suomi was able to convince them to move this spare one over the Asia part, which [some *] people in this effort said well we don't need that. [This satellite *] proved enormously useful for [studying *] monsoons. And one of the[*] global weather experiments [*] was the monsoon experiment [called MONEX *]. [transcriber's note: MONEX, the Monsoon Experiment, part of FGGE during 1978-1979.] You had a couple others. One of them, the final one was ALPEX [transcriber's note: ALPEX, the Alpine Experiment, part of FGGE during 1981-1982], which was the European one and I [was *] heavily involved with all [after *]. But you know by that period of time I had [*] ended up being chairman, Suomi invited me to join SSEC as a associate director, even though you had an executive director. But Bill Smith and I were then associate directors because [of our links really to SSEC which *] had happened while I was chairman. [By now *] the bulk of my research was

being done under SSEC. Suomi asked me to join it and I says well you don't tell me what to do and I won't tell you what to do, [laughs] and that was perfect. [laughs] [laughter in background] And we had an unusual relationship, even after he retired, even on his, when he went to, on his deathbed so to speak.

0:52.47.2

TR: So we've gone to geostationary satellites and weather in motion. I'd like to

DRJ: Here's the thing that came out of FGGE that we now had capabilities to actually map and use computers and satellite to give us global data sets. And the global data sets were very useful because you could look at weather, the internal thermodynamics and dynamics of the atmospheric circulation. Not only weather but essentially all scales so to speak, except when you get down to the micro scale. And so people then realized the relevance of these global data sets and as, once we had done all, many of these diagnostics for the various things there was a person by the name of Richard Reed at University of Washington [transcriber's note: Richard Reed (1922-2008), faculty, University of Washington, Department of Atmospheric Sciences] who was senior to me in many ways but we realized the importance of it and so together we wrote a proposal to the Academy and I ended up being the person who chaired that, bringing together ten leading scientists at that time who [endorsed such efforts *]. Now we're talking about Earth Systems.

0:54:28.9

TR: So we're looking at a book published in 1991, Four dimensional model assimilation of data: A strategy for the earth system science. [transcriber's note: National Research Council. Four-dimensional model assimilation of data: A strategy for the earth system sciences. Washington, DC, National Academy Press, 1991.]

DRJ: And in there you'll find the name of Tony Hollingsworth who was as Louis will say, an exceptional scientist. [transcriber's note: Anthony Hollingsworth, European Centre for Medium Range Weather Forecasts (ECMWF)] You'll find a whole bunch of names here that were involved.

LU: Who also played a role in

DRJ: Played very much important role

LU: in my career, too, Tony did.

DRJ: Pardon?

LU: He played an important role in my career, too.

DRJ: Oh, yeah. He was, you might call the premiere theoretician and practitioner in the European ECMWF, European community meteorological forecasts.

0:55:12.5

TR: So these are ten people including yourself from ten different places, both campuses and governmental centers?

DRJ: Yes.

LU: So you see Eugene Rasmusson. [transcriber's note: Eugene Rasmusson, faculty, University of Maryland, Department of Atmospheric and Oceanic Science] Remember when we talked about him out of Kansas?

TR: Right.

LU: He's there. Tom Warner, a Rick Anthes student in the modeling community is there. [transcriber's note: Thomas T. Warner, Pennsylvania State University] What's interesting about Roy Jenne is that he's more of a data type so it all starts with data. [transcriber's note: Roy L. Jenne, NCAR]

DRJ: What he was able to take these data that were coming in and form data sets that we could work with.

LU: Right. So you know it's an interesting combination of people and Anthony Hollingsworth is there who was leading the science division of the European Center which was by the late '80s had become a premiere modeling center focused on extending prediction capability. Michael Ghil is a mathematician type, very much more theoretically oriented who I also had interaction with at Goddard Space Flight Center. [transcriber's note: Michael Ghil, University of California, Los Angeles] And then John Bates was more a modeler type. [transcriber's note: John R. Bates, NASA, Goddard Space Flight Center] But it's interesting that Eugene, Gene Rasmusson is on that list.

TR: Right.

LU: So you see how this starts

TR: Right.

LU: weaving and that's why I knew that Gene Rasmusson and he were connected further back in time and they still remain so as they

0:56:48.0

TR: So did do something like this it, I mean the cliché is that it takes a village, but these are, you know, five different universities, five different national centers

LU: Or international

TR: or international centers.

LU: Right

TR: So to do this well, it takes a lot of different people and a lot of different minds.

DRJ: That's right. Coming from different backgrounds. That's a very important thing to remember there because if you go and try to understand how meteorology as a science or atmospheric science would have developed all these things. Our expertise and the formative things of instrumentation and models and everything else really comes from outsider disciplines.

0:57:27.6

TR: So again, it's this, what we were talking about earlier, the idea of cross, it's

DRJ: community.

TR: not just meteorology.

DRJ: No

TR: Cross community, not only on this campus but cross community nationally and internationally.

DRJ: Oh yeah. But you have to realize because of Suomi here in particular in this area, he was one of the key people involved in this effort and lead the way.

LU: He was a magnet in many ways.

DRJ: Oh yeah.

LU: I'll never forget it was at one of the meetings leading up to FGGE. He had all of the national program leaders within the various government agencies that actually bring the money with them. OK. And they all met here in one of the important meetings to literally like, the way I would interpret it now is probably one of those final steps. Like, ok, here's the money you're going to bring to this and here are the project plans. And they basically, and that meeting was in early February of that year and it was probably one of the coldest periods that we had had that year when I was a student, I can't remember the exact year. But I'll never forget the folks saying Well nobody can accuse us of being on a boondoggle for coming to Madison, Wisconsin in the beginning of February. And I thought, as I was there as a student, and this was one of the great things about this school because they included the students in these activities, in many different, in the formative ways Suomi had us write proposals. We had two credit courses in helping to write proposal on some of these major activities on satellites and stuff. And I'll never forget that. That, you know, they came here. I interpret that as being because of what was going on in

this building and principally with the satellite aspect, you know, Suomi clearly had an important role and he really was a magnet.

DRJ: Oh, he was.

LU: There was just not question about it. And I'm talking about every, NASA was here, NSF was here, there were folks that were doing academy studies that were here. You know, it's not a given that these experiments are going to happen. That an agency pulls out at the last minute or a country doesn't step up so we might have to put more money on the table to get things going. It was a very important meeting. And it was cold. [laughs] That was not the time you wanted to be in Madison.

1:00:06.4

TR: Go ahead.

DRJ: You have to understand the relevance of Verner Suomi to this campus in terms of his link to the international dimension and when they build this building for example, they had money from NSF to [build the *] first eight floors, eight stories. Well, the university said here, this is valuable real estate, you've got to have more stories, because they had in mind the geology geophysics building. And then they had in mind actually south of that, classroom building for us all [*] called the geoscience area. And so here now they've put a roadblock to the building of this building. And Suomi goes off to NASA and he gets the funding for six stories, so that's how we got the fifteen stories. They, NASA didn't put up funding for the equipment to furnish the six stories so the university did end up doing that, but otherwise all the money came from a combination of NSF and NASA because of Verner, well primarily [became *] feasible because of Verner Suomi. The department had put together a nice strategy where they were going to emphasize the climate, the planetary boundary work, planetary boundary work and everything to go to NSF and that is in a document that the department has, a brochure like this that you know lays these things out in these four different areas. And that really I guess originally is why NSF funded the first eight floors.

1:01:54.7

LU: So in 1998, when they had the fiftieth anniversary of the department and they went through the history of it, one of the things I learned during that was the role of Ragotzkie who I guess was, he might, I don't know if he was the chair or not, but he was obviously very good at the interactions with NSF and there was a meeting that was discussed and it's got to be documented in there about how he had to fly off to Washington on a moments notice because NSF got a little weak in the knees at one point or something and he had to basically resell the whole, the whole project and it was. Yeah, so I come here as a student in '68 and I'm walking into a new building in '69. A one year old building and you know, you just thinking things like this happen, you know. [laughs] So

1:02:49.4

TR: Now, Louis you transferred from somewhere to come here, is that correct?

LU: Yeah, I actually, I grew up in, you want me to get into it a little bit.

TR: yeah.

LU: Yeah, just, I grew up on Long Island and was in New York state and I, where as Don is very clearly had an incredible run of it even though high school and all that, I'd be classified as a late bloomer I believe. So, to the point, and I grew up with parents who didn't go to college or didn't, you know, emphasize education but didn't have that connection. So I was basically on my own, relying on teacher, guidance counselors and nobody told me when to apply so I applied to Penn State and Florida State and Chicago and I applied late, so I didn't get in there. The only school I got accepted to was a New York State school, Syracuse. And I was told by a guidance counselor that there was meteorology there and I assumed that translated into a meteorology department which was not the case. So I show up and I get a geography counselor who tells me that there's no meteorology department there. But, what was really interesting was that one meteorology undergraduate course was taught in geography was taught by a man named Hammond, who had just came back from his, a one year visit here or two years, working under Trewartha, Don mentioned Trewartha, and it turns out he had worked with Lyle Horn. And so I got into this geography course, always interested in meteorology. I did my tests with color pencils, I had the whole color. These guys had never seen a freshman student like this before, so my TA brought me into Hammond's office and Hammond says you got a real knack for meteorology. Basically asked what am I doing here at Syracuse. He got me into the one graduate level course under _____. And but his main message was you have got to go to the University of Wisconsin. And he did not only mention Suomi, but he had mentioned that they were building a building here, that this department was really a growth department and I remember I was not going to be late this time. I remember applying in October and they wrote me a letter back saying well you know you don't even have your grades yet, you know, you'll have to refresh your application when you get your grades. But I was not going to be late, OK. And it was just the second time I heard Suomi's name, because I'd gone to a weather service office in '67 when I was going through high school. My aunt brought me there. Everybody in my family knew I was interested in meteorology. And there were three people who talked to me. Two people told me there was no future in meteorology, do not go. And one guy who I think was in charge of this, it was in Garden City, said if you go into meteorology you've got to go to the University of Wisconsin; there's this guy named Suomi out there who is developing all these satellites and satellites are the future of this field. So now it's two for two. So I didn't apply any place else. And I transferred here as a sophomore. OK? And then I walked into this building as a junior to take my first course in meteorology. And I had Doug, I actually I had to have an advisor, an undergraduate advisor, and I was assigned to Doug Sargeant who was, I think, an associate professor here at the time. [transcriber's note: Douglas H. Sargeant, faculty,

University of Wisconsin-Madison, Department of Meteorology, 1965 to 1972] He was just transferring to Washington. He wound up joining NOAA. But he doesn't remember me walking up to him at the desk, [laughs] you know, but just one of probably a hundred students there that walked up to him that day, but. So I came here at the time when this building literally being filled, 1969.

1:06:53.1

DRJ: There is one additional point I want to make here. [*] At that time prior to essentially the emergence of the University of Wisconsin satellites. [*] There were three main schools. [The others were *] MIT, Chicago, and UCLA. And University of Washington was relative new but when I went there was three professors and, you know, a few more by this time. But Wisconsin had this dimension of satellites and a whole, [which *] was exploding. So we had all kinds of publicity you might say about how Wisconsin was now the, sort of not competing with these, in fact they set out purposely not to compete with these other schools but to build a new vision for the atmospheric sciences.

1:07:48.3

LU: And this growth of professors, actually the growth curve on the number of Ph.D. students were astounding. I mean

DRJ: Right.

LU: it was, you know, the number of graduate students per year, were like a hundred students.

DRJ: We had [over *] one hundred and ten graduate students here.

LU: On an annual basis, on an annualized basis.

DRJ: Right.

LU: and you know, the other, you walk into a reading conference and you mention you're from the University of Wisconsin and you got a hundred and ten graduate students and a lot of these schools had ten, five.

DRJ: Yeah.

LU: You know.

DRJ: Maybe fifteen. [laughs]

LU: Maybe fifteen. [laughs]

1:08:26.0

TR: So almost as many UW graduate students as probably the other major program combined?

LU: I would, they were growing fast. This, Washington grew fast because of the connection to MIT, so the dynamics that came out of Charlie's group

DRJ: That came a little later.

LU: Yeah. But Holton was at Washington, was writing, had published his book. John Young came here out of MIT, so the Holton's book came out in the mid '70s. [transcriber's note: question: does this refer to Holton, James R. An introduction to dynamic meteorology, New York, Academic Press, 1972]

DRJ: Right.

LU: He was, and that started that gravitation, you know. So other people got their niche. Oklahoma exploded with the radar and severe weather connections. So, you know, but that was later. That was definitely after.

1:09:11.9

DRJ: Well they had Holten and they had Wallace and several people

LU: Right.

DRJ: [out there, they formed what *] I'm going to call them the second MIT.

LU: Right.

DRJ: because they were carrying on the type of work that was really started at MIT.

1:09:29.9

TR: So, Louis moves here. This new building, but this is also all going on during the Vietnam war.

DRJ: Right.

TR: And I told you two that I was going to

LU: bring that up.

TR: force you to talk about this.

1:09:46.0

LU: OK, so one of the interesting things, and I was a student. It was, it's interesting because Don had a flat top hair cut during this period, too. And one the things, I heard he was in the Air

Force or something like that, I said boy that's interesting, they've got professors here from the Air Force I wonder what they think about what's going on. I came here, when I was in Syracuse, I went there in '67, '68. The anti-war demonstrations were just starting to kick up in other universities. I wasn't aware of, like the Dow, they had a Dow riot here [transcriber's note: anti-war protest at the University of Wisconsin-Madison campus on 18 October 1967 protesting Dow Chemical, the makers of napalm. Tear gas using for the first time in an anti-war demonstration], this was before I got out here. I wasn't, it's not like, I wasn't even aware of it, you know. You heard about Berkeley, you know, the demonstrations going on in Berkeley or some of the New York City schools, because it's on the east coast. I, my brother had gotten out of the Army. Within the context of my family I'd say it was a pretty conservative family. One of the things, one of two things happened early on here with respect to me as a student coming here in '68. Number one was watching the Democratic convention, the Chicago convention with my father. We watched the conventions pretty religiously together as a family, you know, it was just something we did. My father was a very Republican family. It's not like today where everybody, you trash the other parties. This was a sociological event that was there in front of you and it had an impact on me. And my father was pretty disgusted with the way the police were beating people. OK? Whether he believed that they precipitated the riots or not, it never really was discussed but the anger of Mayor Daly and the recklessness of the police was what the media captured. When I came to school, so when I was at Syracuse I saw some anti-war demonstrations going on, especially in the spring of that year and then after Martin Luther King was assassinated in April in '68 there was a demonstration on campus at Syracuse that I did not participate in but I was, you know, aware of it and concerned about where this country was going with Martin Luther King getting assassinated. And then Robert Kennedy got assassinated and then you had the Chicago _____. So my whole mind set was certainly being influenced by this. The second thing that happened was when I came here I got assigned to Bashford Tripp dormitory which is one of the older dorms by the lake and the reason I'm bring this up is that the, you went to eat across the street at Carson Gulley. You collected in the lounge. These were four, these were relatively small, you know, they were large dorms but they had small houses. You were in Bashford, you were in Tripp but the Bashford house. And we would collect in the lounge before we went to eat. And it got to be pretty good routine. This was, some of my closest friends that I still talk with, still interact with _____-. And I'll never forget, it was like the second or third night, there were people, there were people in our dorm that were actually in the Chicago riots, that were actually there. And the anger that these people had was, astounded me. It just, I, I, I, somebody of my, my, maybe I had a protected life, you know, I was on a farmette , not a farm. Middle of Long Island, large family, large Italian family, and my mother's German family was close by. You tend to be a little bit. So I see this and now you start seeing some of the demonstrations here, you have the black strike [transcriber's note: The Black Peoples Alliance at the UW-Madison organized a strike to demand recruitment of more minority students, Feb-March 1969. See UW-Madison Archives and Oral History] and then the spring anti-war things in '68. I was more, I was more interested in what that view. And then of course

as when Nixon came on, it became clear that he did not have a peace plan and if he did it wasn't obvious to us as students, to us as draft-eligible people who, you know, would have to go fight a war that we were increasingly not believing in. So this campus got pretty hot. The black strike I think made it hotter than the anti-war demonstrations and I bring that up because the professor who actually captured me first here was Charlie Anderson [transcriber's note: Charles Anderson, University of Wisconsin-Madison, Department of Meteorology, faculty from 1966-1986] who was African American and was sort of, more than sort of probably, but the university created a black studies program and he was the first chair of that program even though he was a quote unquote a meteorology professor. So I talked to him a lot, you know, as an undergraduate student and I wound up getting my master's from him, under him. But as a student you see this and then there was some anti-war demonstrations and I wasn't saying I was running around closing buildings down but I was out there. I was very intrigued by what was going on, by, I was very intrigued by the fact that the local press was saying that these were all out of state agitators creating this. And my father is writing me letters saying that where did you go to school, you know, you're in a school that, you know I knew there were a bunch of liberals out there, you know, like he was blaming the school for what was happening in the state. And the people in the state were blaming the out of state agitators. And of course, I'm from out of state, so I'm sort of like. I mean, all of this is really fascinating to me and I became increasingly against the war. I participated in what I would still today call a peaceful demonstration _____. The change moment came with the Cambodia invasion in 1970. And then the Kent students being shot that were protesting that. And then the Jackson State students, shot and killed, four in Kent State and two in Jackson State. The campuses erupted. And what was really interesting there. I remember we, there was going to be a night time demonstration down in the other part, down towards the State Street area. And we were actually walking down there. There was a group of us, you know, who had collected in Bashford, you know I was still there, I was there two years. And we were walking down the lakefront path and there were people coming back and they said you don't want to go there. And we said why. And they said it's really going to get dangerous, the crazies are running. And we didn't. We turned around and came back. And the whole place erupted. And cops threw tear gas and some of the tear gas drifted into the, if you read the papers, the tear gas drifted into then, where the fraternities and sororities were, into the dorms, into the new dorms. And kids who were not even interested in demonstrating just poured out into the streets. It got to be a really ugly scene and that was in May. And then in August of that year is when and meanwhile now I'm working here. I'm in a department. I'm becoming more focused. I'm really, this was like a learning experience for me here in the department. Charlie Anderson mentored me. I had him as my first core course. And then he got me into cloud physics and told me that. I wasn't even thinking about applying for graduate school. He's the one who said you're applying to graduate school. Don't bother applying any place else. And you know he had me, he had an RA for me before I even applied kind of thing. So I spent more time in the building. And Charlie actually sent me to a conference in, he wanted me to see what it was like. I was an undergraduate and we went out, this was in August of 1970 and we went out to

Colorado State. [transcriber's note: Conference on Cloud Physics, Colorado State University, 21-28 August 1970] That's where this conference, this conference was. It was an AMS conference at Fort Collins. And it was there that we heard on the news that the bomb went off here. That somebody had backed a truck up into the load dock area of the Army Math building and blew it up and killed a Ph.D. student [transcriber's note: Sterling Hall Bombing, 3:42am, 24 August 1970. Target was the Army Mathematics Research Center (AMRC) in Sterling Hall] It was 3 o'clock in the morning. They had made this assumption that there wouldn't be anybody in the building and they killed, his name, I still remember the name, his name was Robert Fassnacht. [transcriber's note: Robert E. Fassnacht (1937-1970), physics post-doctoral researcher] And I also the next day in the paper it, the hospital used to be right across the street from it. It turns out that my future father-in-law was the director of hospitals and being in the medical school at the time, Peter Eichman. [transcriber's note: Peter Eichman, University of Wisconsin-Madison, Dean of the Medical School 1965 to 1971] I saw this lonely figure. I'd already been going out with Susie, Susie Eichman, and I saw this and this was in a Colorado paper, this figure walking around the rubble at the hospital. It was taken from where the bomb went off into this, through this enclave and then in an open area. And it turned out that was my father-in-law, my future father-in-law. And one of the things, and I mentioned this to Don once, is that when I came back in the end of August now the first thing that struck me was how different the campus was. There was a mood here that was somber, completely different. That in September when the students came back and there was this call for an anti-war demonstration instead of twenty thousand showing up, I think two hundred showed up. It was over. It was basically over in terms of demonstrations on this campus.

1:20:02.0

DRJ: Yeah, that bomb made everybody very sober _____.

LU: Yeah. And so, one of the things, I talked to Lyle Horn about this, who was, became a mentor for all the undergraduates. Everybody, everybody will tell you that Lyle Horn, I'll say it right here, he was the heart and soul of the department when I was an undergraduate and a graduate student here in many ways. And I talked to him, he died in 1989, he died the same year my father did, and they both died of cancer. And I saw them both in hospitals as they were dying, very similar. And before he got really bad, I talked to him about this era, the late '60s and '70s when they had the anti-war demonstrations and what he thought of it in terms of the classroom experience because there are people who claim that it interrupted the classroom experience. And my sense as a student and it was his sense as a professor that it was actually an exhilarating time to be in the university because the students actually felt emboldened to ask lots of questions. And it carried over. He went into this, the courses that were being taught here, and I felt like I had every right to ask the questions. And that it was almost expected. And what I sensed when I came back here on visits in the '80s and '90s was that I'd give a talk and [laughs] there would be no questions. This is why I started talking to Lyle about it and he said that it, you know, I was here during the time and he enjoyed it, where people asked questions. We had

raging debates about the weather in front of the maps on the fourteenth floor. That he and Frank actually looking back on it helped initiate by just throwing the questions out there and then watching us scurrying. But then we challenged them, too, you know. And so it was really an, the new building? It was a growth here. As an under, Charlie gave me my own office as an undergraduate student because there space on eleventh floor that they hadn't quite filled yet, from a cloud physics perspective and he goes here. And I wound up writing a senior thesis that won an AMS award. It was just an amazing time to be here. It was growing. The faculty was fairly young, you know, I think a new interdisciplinary. And we were in an environment where you could ask lots of questions and it was almost expected that there were going to be lots of questions. And so I didn't look at the anti-war demonstrations going on on campus as an inhibiting event. I looked at it as something that actually contributed to my learning experience. Of course, look when I came into meteorology I didn't even know the mathematical basis for it. I learned that as, it was meteorology's, the application of Newtonian physics in meteorology that got me interested in Newtonian physics [laughs] not the other way around. And then I'd have to go back and fill in the blanks. When I started, when I came up and worked for Don the first thing he said to me was OK welcome aboard and you signed me up for advanced calculus [laughs] [laughter in background]

1:23:32.6

DRJ: Well I made all my students either minor in mathematics

LU: or computer science

DRJ: or computer science.

LU: I got my minor in computer science and I was there when all the numerical analysis which I didn't have a clue about two years beforehand. But it just was, I was just like a sponge. I had, you mentioned Lettau, I had Lettau for my, I had Denny Thomson for my second core course before he went to Penn State. And he was the guy who during the anti-war demonstrations, he kept us busy. I think he did this on purpose. He kept us busy enough that we would have to come here at nights and work because we had the Hewlett Packard desk top computers that were just delivered that could actually, you could, you could in machine language program your equations and actually see a charts come out in front of you rather than have to wait two days to get them from across the street. He would only teach that after dinner. [laughter in background] So we were here from like seven to nine o'clock at night and then we'd come up to the map room and we'd get, make sure we're getting all the maps for the next day, the zero Z cycle [transcriber's note: What is this? Not sure I am hearing this correctly]. And Lyle Horn would be working with a student and Frank would be there. So it was like the education did not stop at five o'clock and I'm looking back on it. If, if Frank were alive today and Lyle were alive today, did they do this on purpose almost to get us in here and keep us off the streets during these demonstrations. Especially in May, when that Cambodia thing happened we actually went up on

the roof a couple times to watch what was going on from the roof and then Thomson would say OK. And Thomson was very, everybody was very upset about what happened at Kent State, you know the professors I dealt with, they were very upset about that. And we'd have discussions but then it was time to get back to work, back on the. So I think I was here at the right time. New building, the growing department, it's always best to be here during growth. And what these, the commitment, I mean I worked on Denny Thomson as being as you said he was a TA for you. I put that down as a connection. Tony Hollingsworth, you know, when I did my work for model based diagnostics like we used to do off these maps. With Dan Keyser [transcriber's note: Spelling? Is this Daniel Keyser, meteorology professor at Penn State?] and I and others were shown, you know, and this group I had at Goddard that you could actually look at fifteen minute output from the models and start getting it to cause and effect. Tony invited us over the European Centre, gave a talk and Dick Reed started complaining about it. Tony got up and said this was a tour de force. And was a big, big fan of me and the work we were doing and this was extremely important for keeping our group moving forward. By that time Tony had become a tour de force himself and was really important. So what I'm finding is all these connections here [laughs] but being here in the late '60s and the '70s, I left in '78, I think was a prime time. And not because I was here, I'm talking about the nature of the department and the newness of this building and the level of excitement that it brought here which was just amazing.

1:27:07.4

TR: Don, we have a couple more minutes till we hit our hour and a half mark here. Anything that Louis said or anything in general about this time period particularly as it pertains to.

DRJ: Well, I guess I'd like to insert some things, my views of this whole thing but they have to go back to the fact that there's a World War II, a war that we seemingly had to fight regardless. And then of course it ended up with getting into the Korean War and the threat of the Communists in the world as a whole. But my hero of course was Eisenhower and I like to point out that as far as I'm concerned Kennedy made the first mistake by sending observers into Vietnam. The French had pulled out and we start to talk about, you know, this world as a whole, why did we send in those observers in? And of course Johnson created this effort that lead to the actual outbreak of the war. You know that

LU: It's amazing you say that because I just got done, did you read the book Ike? [transcriber's note: Korda, Michael. Ike: An American hero. 2008] It just was

DRJ: No

LU: It just came out about five years ago

DRJ: Well, Eisenhower

LU: it's an interesting, his influence in the '50s is pretty interesting. It's underestimated, let's just put it that way.

DRJ: Well, yeah, I mean he

LU: He got us out of Korea.

DRJ: _____ There was a PBS show on it recently but I had followed Eisenhower for many years. Being the fact that his home is in Kansas

TR: I'm sure. Right.

DRJ: and we've gone and visited it. He had this view where he brought in for his cabinet members from the Democratic Party and it was a very peaceful time. But there were many things that he did that we don't, aren't aware of. He actually had a special ambassador to go to the near east to try to get a situation where the Syria would quite blocking up the river there that goes down to Jordon with dams because [*] now the tension was starting to develop.

LU: The Golan Heights areas.

DRJ: The Golan Heights and the

LU: And the Suez Canel, he

DRJ: That's right.

LU: He pulled the plug on Great Britain and the shenanigans they were pulling on the Suez Canal. He didn't mince any words or actions on that.

DRJ: No, he didn't. And he kept, he had a different perspective on war than what emerged, that got us into the Vietnam and

LU: So I mentioned the thing about you know when Don, so when we were in the anti-war mode, mind set, you know, people who were in the Air Force, the Army, and the Navy. I had relatives in all, and just the mere fact that I was talking anti-war that you know that or in '72 I'd be thinking in terms of McGovern versus Nixon in my family I was like the only one. I got this sort this pound, pounded feeling when I went home for, I mean everybody loved each other but it wasn't like this, I mean you know you just you know in today's environment you know people can't even talk about having a difference of opinion, it breaks families up even you know. But it's, it was interesting from that point of view. The one thing about Don that always struck me is that he never, he never voiced and neither did Horn nor Thomson. Thomson was probably the most explicit in voicing an opinion. When I look back at it. Lyle's was more, you know, more philosophical about things without trying to alienate or say one side was right, one side was wrong. And but when I was coming out of my home atmosphere and I was almost afraid to tell

Don that I was against the war because I just wasn't sure where he was at. I didn't want to get in the way of

DRJ: But you didn't have any reason to

LU: No.

DRJ: believe I was for it at all.

LU: No.

DRJ: [laughs]

LU: No. I'm not saying. It's just like I didn't even want to go there.

TR: break, break

DRJ: If we'd have talked about it we could have gone into the role of Eisenhower, he went over and got us out of the Korean War.

LU: Well, one of the things that my, one of my father's friends was against the war which was interesting, at his generation was one of the few, that was Eugene Bellinni [transcriber's note: spelling?] He was the one who told me to read the Best and brightest which is not a very complimentary book. [transcriber's note: Halberstam, David. The best and brightest. 1998. Book about how American because involved in Vietnam.]

DRJ: No, right.

LU: so it's Kennedy's

DRJ: He brought in McNamara and this Harvard contingent and they were now in control of the world.

TR: we are over an hour and a half so let's wrap up for this session. So this ends the second session of the interview with Don Johnson. Thank you both.

End of audio

1:32:01.3

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