American Meteorological Society
University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview of Joost Businger
29 August 2000

Interviewer: Kristina Katsaros

The transcription was difficult due to many foreign names. Therefore, the two of us have gone over the whole transcript together on several occasions and have edited to remove extraneous material. We think the flavor of the discussion is retained and the material has been substantially improved.
The transcript is turned over to the archives at NCAR in January, 2008.

TAPE 1, SIDE 1:

Katsaros: Tape 1 of an interview project for the American Meteorological Society managed by NCAR of Dr. Joost Businger. He will tell all about his career and background on this tape. I am the interviewer, I am Kristina Katsaros, who was one of his first students at the University of Washington in an undergraduate class in 1958 and we have been friends and colleagues for years. We will get started by telling a little bit of Joost's life history.

Welcome to Whidbey Island, to our home in this wonderful place. The date is August 29 in the year 2000.

Businger: Thank you.

Katsaros: So, do you want to tell us about your early life and growing up in Holland?

Businger: Yes. I remember when I was a Boy Scout that I was surprised in March, one March day, that the weather was all of the sudden mild and warm. This surprised me so much that I thought I wanted to know why this happens and ever since then I have been interested in weather.

Katsaros: That's great.

What did your father do?

Businger: My father was a dentist. He was really an artist and a dentist in his spare time.

Katsaros: It doesn't hurt to be an artist if you're a dentist.

Businger: That worked together well.

Katsaros: I met your father and I remember your mother also very clearly. I met her a couple of times, a very impressive woman.

Businger: She lived to a good old age. She died at 94 and she was still visiting here when she was 90 years old on Guemes Island. That was the last big trip she made. Anyway, the interest of meteorology continued through high school and when I was 16 years old the war broke out in Holland in May of 1940. After that there was no weather information because that became secret. So, I kept my own records of weather and tried to forecast for the neighborhood what the weather was going to be.

Katsaros: Cute. You were 16 at the time?

Businger: Yes, I was 16 at the time.

Katsaros: You were through high school then or not yet?

Businger: No, I was sort of in the third grade.

Katsaros: Of high school.

Businger: Yes. I finished high school in 1942 and then went to the university. The question came up, what to study. There was no program in meteorology or atmospheric sciences at any of the Dutch universities. The University of Utrecht was closest because they had in their geography program a course of synoptic meteorology, which I was very interested in. In talking to my astronomy teacher and physics teacher (it was the same person) what would be the best way to approach meteorology and he suggested that I study physics first. Physics and mathematics were better than taking geography. I have appreciated that advice ever since because it was clear that people with a geography degree have trouble with the physical problems that atmospheric science presents.

Katsaros: You saw that many times with graduate students. They would come in with weak understanding of physics and math and they had trouble.

Businger: They had trouble. So, I was lucky to enter the university in the program of physics and mathematics with a minor in astronomy; meteorology was not part of the program. It was a small group of very talented students. Half of them obtained PhD's and became professors. I was very fortunate to have studied with such a select group of people.

Katsaros: I think it is true at any level; it is very important for your learning and growing.

Businger: So, the astronomy professor wanted me to specialize in astronomy and there was of course a lot that was similar to meteorology, but I wanted to understand turbulence and the weather formation, clouds and rain. There was some work by Chandra Sekhar on turbulence. I felt that I really was not interested in Chandra Sekhar's work in astronomy. I had good friends, who loved to watch meteorites. But, then there was a special program that was very useful at the University of Utrecht. It was not an institute; it was called "Warmte Stichting" (Heat Transfer). It was dealing with heat transfer for practical purposes. It seemed to me that a lot of what's happening in the atmosphere has to do with heat transfer so that was an attractive field in which to become a specialist. I learned about ventilation of houses and radiative transfer, convective transfer from hot surfaces to the environment etc. There was a teacher who was brilliant in the way how to deal with the problems in dimensional analysis. His name was van der Held. There I learned to actually get the basics of similarity and similarity approaches to difficult problems of turbulent transfer etc. This institute of Heat Transfer, which was part of the physics department of the University of Utrecht, offered me a job. That job paid enough, just enough, to get married.

Katsaros: And you were anxiously waiting.

Businger: I got my bachelors degree in 1947, married in 1949.

Katsaros: You were still working on your PhD then.

Businger: I was working on my PhD.

Katsaros: That's how they do it in Europe; they give you a job while you are working on it and then it can take quite a while actually.

Businger: So, I got my "doctorandes degree". There has been no real master's degree in Holland but there is an exam that qualifies you to go on for a Ph D. After that all you have to do is write a thesis. When I was working at the institute, somebody came to the department who was looking for a person with my background and he persuaded me to come to Wageningen. The deal was that I would get the research position doing work on greenhouse climates and I would get one day off a week so that I would have a three day weekend to work on my thesis.

Katsaros: No day for holiday. When you say greenhouse climate you don't mean it in the way we talk about greenhouses and climates now? True greenhouses!

Businger: True greenhouses, right.

Katsaros: A good story of your life.

Businger: I went back to observations that were interesting. So, I worked on a thesis and I remember commuting from Utrecht to Wageningen, which is about 25 miles and I often did that on a scooter.

Katsaros: Was it a motor scooter?
Businger: A motorized bicycle was what we had then, a mobilette.
Katsaros: Yes.

Businger: I forget whether I stayed in Wageningen over night but anyway after a few years, about three years, we did move to Wageningen because we bought a piece of land and built a little house that we moved to and that was closer to work.

Katsaros: What most Dutch people would have been doing in the early 50's. Nobody had cars then. What were your research studies there?

Businger: The research topics were greenhouse climate. We also did an interesting experiment in frost protection. Frost protection was done by sprinkling the crop while the temperature near the surface was below freezing. The latent heat from freezing the water that had been sprinkled on the crop kept the temperature near the freezing point and the crop would not be damaged by the frost. In the greenhouse, the various techniques of heating the greenhouse were studied, and tomato plants were used as an indicator of how well they liked what we did.

Katsaros: They are pretty delicate, I think.

Businger: It turned out that with convective heating, where you moved the air, the crop would mature the quickest. When you had just infrared radiation it would be slower.

Katsaros: Would that have to do with carbon dioxide flow?

Businger: Yes, probably with carbon dioxide exchange.

Katsaros: Interesting.

Businger: We didn't go into great detail.
Katsaros: Probably because there wasn't anything to measure carbon dioxide concentration and its variation with photosynthesis at that time.

Businger: Yes. One of the deals of that job was that after I got my PhD I shouldn't quit right away but stay on for two more years to pay off the debt. So, I got my PhD in '54 and in '56 I was free to move away from the Institute of Tuinbouw Technique.

Katsaros: Strike out.

Businger: To strike out. Since there was no program in meteorology yet in the Netherlands I applied for a job in the U.S. I wrote to six universities and the Weather Bureau. The Weather Bureau answered that I didn't get the job because I wasn't a citizen, but the universities said it would be ok. Then Heinz Lettau told me that U. of Wisconsin had a job. I had also written to Wisconsin and they offered me a job. It was a Research Associate which is basically a post-doc.

Katsaros: That's pretty early, '56.

Businger: I came in '56--- in August '56. I was just too late for an experiment they had done in the Great Plains in Nebraska, where someone had developed a sonic anemometer. It was my job was to analyze the data obtained with that instrument.

Katsaros: So, that's how you started in the business, I never knew that, interesting.

Businger: We arrived in Wisconsin and met Vern Suomi, who was his exuberant self and he stimulated me right away. We spent weekends in the laboratory and he showed me how sonic anemometer data were to be analyzed. He said in two weeks you can have this done.

Katsaros: That is remarkable. I have seen students work on sonic anemometer data for years.

Businger: It took me two years to complete the work and he was still appreciative that it was so fast.

Katsaros: When you came to America, were you here on permanent status or was it a temporary post doc position?

Businger: I applied for my immigration Visa when I got the job offer from Wisconsin and I thought that was a very easy thing because I had a job, but not so. I had to go through the immigration thing and it took half a year before I had immigrant status and that is why I missed the experiment in Nebraska. It was my intent that if I liked to be in the United States that I wouldn't have to come back and apply for an immigration Visa. So, I didn't want to be there as a visitor with a temporary position.

Katsaros: How about Holland loosing a PhD scientist so soon after your degree?

Businger: It was still the policy in Holland to encourage people to emigrate and in fact since I had this job there was a fund to help me pay for the travel. I think the airfare was paid for by the Dutch government. I had very little money at the time so that was very welcome.

Katsaros: It seems odd today that they would really want to encourage well educated people in an important field like agricultural greenhouse science to leave, but I guess they were still picking up after the war.

Businger: Right. I had so little money in Madison, Wisconsin the first month that I remember walking a mile to save a 3-cent stamp.

Katsaros: Wearing 3 cents worth of shoes out from it.

Businger: That came later because that was…

Katsaros: That was an investment for the future.
And you had at that time a wife and two children?

Businger: Two children.

Katsaros: Two boys. Ferdi and Steven Businger in that order.

Businger: Yes.

Katsaros: How was it working with Vern Suomi? Did he work closely with you or did you have to be very independent?

Businger: He was very stimulating. We would often have lunch together and with other people. Suomi was a person who every day had a new idea. All you had to do was filter his ideas, because not all of them were good. Sometimes he would come with an idea and a day later he'd say, "I have a better idea." That often turned out to be a worse idea, if you really thought about it, but he left you totally alone on the work and stimulated me to do some electrical engineering to learn about analog computers and data analysis, which I had no background in. That was very helpful.

Katsaros: Was it a large group at the time? How many people involved?

Businger: It was a very small group. I was working by myself on the sonic anemometer data analysis and there was a graduate student, a PhD student, Chuck Stearns, who helped with other things. We shared an office with the secretary of the department. There was Dean Staley, who was an assistant professor at the university, and then there was a Chinese guy --- his name, I think it was Chien. He was also a PhD student and he had some Chinese slave labor organized. He was an illegal immigrant. That was all bizarre. Then there was another person…we were all in one large office.

Katsaros: One big room with all those people?

Businger: One big room with a secretary, yes. There were four or five people.

Katsaros: Not terribly efficient.

Businger: Not terribly efficient but it seemed to work O.K.

Katsaros: Lots of communication. No problem with communication.
Was the department the strong department that I know it is now? Were there a lot of good professors there? I don't know about the early days there.

Businger: There was Suomi and there was Reid Bryson and Dean Staley that were the professors.

Katsaros: Meteorology in general was not all that strong yet.

Businger: It was a small department. In 1957 a great deal of excitement with the IGY (the *International Geophysical Year*) came about. Sputnik turned the science around. You might say that Suomi was so excited about satellite meteorology that all of his ideas, which were very profuse and many were very good went into that direction; many discussions about measuring the radiative balance of the earth from satellite and that sort of thing. At that time he traveled to Washington DC almost every week, so much so that he couldn't teach his course of physical meteorology and asked me to do that. That's the first time I had to teach a course. My English was not terribly good, so what I did was I framed the sentence in Dutch and then translated it for the students. It was a very slow process. There were some very bright students in that class; among them were John Dutton, Grant Darkow, and Claude Duchon.

Katsaros: You had some early teachers who advised you to study physics and math. Who was your PhD advisor in Holland? Was that a person who also had a strong influence?

Businger: His name is Wouter Bleeker and he was the director of research of the Royal Dutch Meteorological Institute, KNMI (*Koninklijk Nederlands Meteorologisch Institut*). He was very helpful in getting literature and was interested in the work, but he was not really an expert in heat transfer or energy transfer. Those subjects were really the scientific basis of my thesis and van der Held was the advisor for that. The formal advisor was sort of in the periphery, but he was very helpful. The idea was that his name would be helpful, because he was well known in synoptic meteorology. He had also studied with Bjerknes in Norway for a year. So, he was well known and his introduction was very helpful.

Katsaros: And his signature on the thesis.

Businger: Yes.

Katsaros: So then Wisconsin was maybe still a little small for you---I'm guessing? You were, may be, ready to move on, because I know it wasn't too long before you went to the University of Washington, where we met in '58. So, what caused that change to happen? What was the reason emotionally?

Businger: That's an interesting question. I think it was maybe late in '57. Although the IGY and everything was going strong, the grant that I worked for was from the Air Force, the Air Force Cambridge Research Laboratory, and they decided that they would cancel the grant. They actually cancelled the grant and my job ended in mid '58. I remember the secretary in the department, because we shared an office and she asked "you're not upset", and I said, "No, why should I be upset, there are other possibilities." And there were because in '57 there was a meeting in Toronto where I met Bob Fleagle and we shared a tour to the Niagara Falls. We exchanged a lot of information and in a very short time we became good friends. It turned out that this was not an accident. He actually was looking for a new faculty member for the University of Washington. So, not much later after that I got an invitation from Phil Church from the University of Washington to come out there and visit him. That was in January of '58, I think. I was shown around and treated with great hospitality. It was a mild rainy day, which reminded me of Holland and there was this beautiful country, it was Holland and Switzerland combined, I thought this was a more exciting country than Wisconsin.

Katsaros: There are better times to come than in January.

Businger: No, I like the cold winters in Wisconsin very much, I enjoyed the skating on Lake Mendota, but the combination of the Washington mountains---the Northwest was so attractive that I accepted the offer.

Katsaros: And the department has some very special faculty. Perhaps you were not aware of that in the beginning.

Businger: The name's Bob Fleagle and Dick Reed were of course names that I already had heard.

Katsaros: You were all young then, of course.

Businger: Vern Suomi and Reid Bryson had good reputations too and not only that when I was about to leave Heinz Lettau came to the University of Wisconsin and Reid Bryson said, don't you want to work with strongest program in boundary layer meteorology in the world or something like that.

Katsaros: You would have but there is a fork in the road; you'll never know what the other road would have been like.

Businger: That's right. It seemed to me that Heinz Lettau and I worked in the same field; why should we work together? So, at that time there didn't seem a great deal of attraction. Reid Bryson mentioned that they were not going to continue with Dean Staley and his position, because he would not get tenure at the University of Wisconsin. I thought that was a great mistake because I really learned to appreciate him very much, and I did not want to take up his position at the University of Wisconsin.

So, when I was at the University of Washington I got offers from the University of Wisconsin.

END OF TAPE 1, SIDE 1

Interview with Joost Businger

TAPE 1, SIDE 2

Katsaros: We have just gotten to the point on the first side where Joost has accepted the job at the University of Washington and I knew him from that first arrival at the university when he taught an atmospheric physics course for juniors in the fall of 1958 and I had just started majoring in that field that summer so that was my second quarter. I enjoyed the course and it was really a very nice real physics. I remember it being sort of real physics.

Businger: I had to fill in for Bob Fleagle because he had a sabbatical leave from England; in fact I lived in his house. That was the first year and I had to teach his courses. One of his courses was a senior course in dynamic meteorology.

Katsaros: I didn't take that. I was still too young. I was just beginning in meteorology.

Businger: The first course that I remember was just one with the Air Force students.

Katsaros: I was just beginning; I had another whole year and one-half before I got my degree. I was just beginning, it was '58, and I didn't graduate until March of '60.
: Anyhow, I remember you standing by the black board waiving your crayon and you had a real vivid image and it doesn't seem to me that you've changed that much. A little less white on beard and gray on hair but it's a long time ago, it's 42 years ago.

Businger: I didn't have a beard at that time.

Katsaros: They came and went, the beards. All the scientists at the University of Washington, all those male scientists, and I was the only girl student for a while. I didn't pay attention to beards, didn't seem to notice. It was people working in the Arctic, who had beards. They would go to the field and they come back fuzzy and then it would go away again.

Tell me what it was like to be with all those wonderful people that I think of as my professors when I got introduced to meteorology and my special friend Konrad Buettner. What was your first memory of Konrad?

Businger: That was, of course, a mixed impression because Konrad had been a German officer in the *Luftwaffe*.

Katsaros: He had been. He came to America right after the war.

Businger: I remember one occasion that I liked him as a person. He was very friendly and very easy going. We often went to the swimming pool for a little swim, and he had his long officer's coat on and I remember I got goose bumps. I said,

"Konrad, it is impossible for me to walk with you, if you have that officer's coat on."

Katsaros: He didn't understand the symbolism?

Businger: He understood. He never wore it after that.

Katsaros: I see. Internally he was not associated with that regime in his heart and his mind. So, for him he probably couldn't understand that anyone else would think that he could be.

Businger: There was something in him that was still there, but he wanted to make a great deal of effort to remove that from him. He understood and did not wear that coat.

Katsaros: He still probably was proud of Germany. This is a terrible hurt for many people-- even for the Dutch to have had been closer to that nation at one time. But, to work with a colleague, whose nation had been in conflict with your nation as you grew up would not have been that easy.

Businger: Right. So, that was with Konrad, but we had many things in common as far as our background and education. We wrote a few little notes together, I think, on terminology and heat transfer and had some arguments about definitions which was very useful. By and large, the department was very friendly and very congenial. I came in '58 and I was the junior member for years after that. I remember that when there was a promotion or something important, all the members of the faculty would go out to dinner together and celebrate. When I got promoted from assistant professor to associate professor, I invited everybody to a party like that. It was tradition at that time, but could not be maintained when the department became larger.

Katsaros: What year did you become associate do you remember?

Businger: 1961.

Katsaros: 1961, that was quick.

Businger: Yes, I was lucky. The character of the department was actually formed, I would say, by Phil Church. Phil Church had a way of taking care of his faculty members and he would show how to be hospitable. He would just do everything to make life comfortable for the other faculty members. That atmosphere of congeniality he made possible by excluding himself as a scientist, who had to contribute. He would identify with the other faculty members and their contributions and took pride in the fact that they made major contributions. That was extremely stimulating for the faculty members. That atmosphere that Phil Church created has pervaded in the department up until now I would say.

Katsaros: Yes, I think it is very true this characterization, because I was a young woman applying and they were not used to too many. There might have been one or two before, but he sat me down in the chair in his big office, Phil Church, and invited me to start. He was the father of four girls himself, so I think he had a special background for helping me. The wonderful congeniality of that department that you mentioned helped a lot in the formative years, for both you and me. We were at different phases of our careers, but it was still very valuable for us both. Those were the exciting years with all the Arctic research.

Businger: The Arctic research, yes. They had a program with ONR that dealt in a general way with problems in the Arctic. Frank Badgley and I would take turns every year to go to Pt Barrow and supervise the research that was going on for a few weeks. I found every trip an interesting experience.

Katsaros: I bet. Later on you did your own experiment.

Businger: Yes, we had an experiment later on in '71, '72. That was with Clayton Paulson, his student Ed Andreas and my student Ron Lindsay; Frank Badgley also participated.

Katsaros: The experiment was about heat transfer, I believe.

Businger: It was heat transfer over an open lead in the Arctic Ocean. So, what would happen to the atmospheric surface layer, if you had measurements up wind and down wind from an open lead? Up wind from an open lead the temperature is -30 degrees C and the water is about 0 degrees, it is a fantastic contrast and fog forms right away and it is very difficult to measure anything. Andreas was measuring the drops and the particles and the profiles down wind, and our station was up wind.

Katsaros: Up wind was better, you didn't get your instruments wet. But then I remember the lead had different currents to their stations. Up wind and down wind did not remain lined up on opposite side of the lead after a while.

Businger: No, therefore the stations would only stay for a few hours. It was technically a rather sophisticated experiment because the experiment could be set up in an hour. A helicopter would pick up the hut with all the equipment in it, then set it up at one side of the lead and then the helicopter would pick up the other station for the other side of the lead. In a few hours it was set up and it worked only for a few hours and either it would freeze over or, as you say, change with a different condition. Most of our observations were made with an artificial lead. We just made a pond and then measured upwind and downwind from the pond.

Katsaros: In general the atmospheric science department was much more oriented to field work in those days- wasn't it? Outdoor field work rather than laboratory work?

Was there any discussion about these things? Was there a conscious decision, or was it just funding driven?

Businger: No, I think we preferred fieldwork. Bob Fleagle also liked to do field work but Dick Reed did not. Konrad liked experimental work.

Katsaros: In general, that was more the way people worked. Computers weren't even really operating that much yet. So, I guess theory would have gotten some attention. You did some yourself.

Businger: A little bit, yes.

Katsaros: Maybe we should go back to the 60s, when you first came and talk about what kind of experiments were carried out, the ideas you worked on then and some of your students from that time.

Businger: Yes. My first student was Chandran Kaimal. I had brought a design for a sonic anemometer from the University of Wisconsin. He knew enough electronics that he could build it and he would test it. It took about a year, I think. One thing was very interesting about him, that I very much remember. He had it all put together and was ready to go, but he couldn't turn it on. He was afraid to turn it on because it might not work. It took two days before he turned it on and then it worked. Another student, Mike Miyake, and I could not understand why he wasn't anxious to turn it on. Mike would have turned it on long before. So, they had very different approaches.

Katsaros: Maybe in the Indian philosophy, it would have to be just right… or the mood had to be right.

Businger: It was a little bit of that.

Katsaros: He was from India wasn't he?

Businger: Yes.

Katsaros: I remember Chandran Kaimal and Mike Miyake in a little tiny basement office with no windows and mostly electronic wires coming out of the door, because it was so full of stuff and they were in there all the time working hard. They were really hard working guys.

Businger: With very different personalities.

Katsaros: Wonderful. But then you had a sonic anemometer of your own for turbulence measurements.

Businger: Yes. We did measurements with it and Chandran got his thesis finished on the basis of that and the first measurements.

Katsaros: And he devoted most of his career to sonic anemometry at NCAR.

Businger: No, it was at the Air Force Cambridge Research Laboratories (AFCRL). They developed the Kansas experiment, which was carried out in 1967/1968.

Katsaros: Did you have any involvement in this experiment?

Businger: I had some involvement. I came late in '68 towards the end of the experiment and I had another student by the name of Shelby Frisch who had organized a small plane to measure the temperature and the radiation of thermal plumes that form over warm surfaces and what happened to them over irrigated surfaces.

Katsaros: That became his thesis.

Businger: Yes.

Katsaros: The Kansas experiment became the heart of turbulence research. That and the Wangara experiment from Australia. Those two were the basis for a lot of interpretation of boundary layer fluxes and profiles.

Businger: Yes, the Australian and the Kansas experiments.

Katsaros: When was the Australian experiment?

Businger: The Australian experiments were earlier. In fact I was in Australia before the Kansas experiment. My first sabbatical leave was in 1965/1966. I went to Australia to work with Arch Dyer and Bill Swinbank. That was a very interesting year. There we got ideas about profiles from the literature. Arch Dyer and I formulated more or less simultaneously the same idea about profiles which are now known as the Businger-Dyer or Dyer-Businger profiles.

Katsaros: Stratification effects on the profiles.

Businger: Yes, the stability effect on the profiles. It was all on the basis of some data that Swinbank presented to me. I got the idea from a plot of the Richardson number effect on the profiles. That plot according to me was a little wrong because we had made the assumption about how the profile should be. There is a little correction for that, which I calculated and the plot, when remade, showed that the z/L and the Ri number was a one to one relation in the unstable surface layer. It was a mind-boggling thing. It changed the profiles into a very simple form. It made the profiles simpler.

Katsaros: Yes, maybe profile calculations were made simpler. Then, of course we got computers; it became more of a routine thing to include those stability corrections and then we compare different experiments So, that was from the 60's. Is there anything else?

Businger: And that part of course was refined in the Kansas experiment, but in Australia there were the Hay experiments that already had several of those features, but they didn't have as good flux measurements as the Kansas experiment.

Katsaros: I'm very interested in what happened right after the 60s when you were invited to Russia. First, I think it was one time to just get acquainted and then you had a major experiment in Russia, when you were still really behind the iron curtain and very few Russians could come out and visit. Maybe it was a little time of opening there.

Businger: It wasn't too bad. Lots of people went to Russia at that time.

Katsaros: In the early 70s.

Businger: It was a detente. Khrushchev certainly made an opening to the West, made it more attractive to us. There were various groups that measured fluxes; the Russians were very early in making sonic anemometers; the Australians were strong in boundary layer or surface layer turbulence as we were at the University of Washington. UW and some other organizations, for instance in Canada, wanted to compare the various techniques to measure fluxes and profiles. It was an inter- comparison experiment. Canada was involved, because Mike Miyaki had become professor at the University of British Columbia; he was part of that group. The Japanese were also involved in flux measurements. So, we had several international flux comparison experiments. One was in Vancouver in 1969, one was in Tsimliansk in Russia in '71 and one was in Australia and I think that was '73 or '74. I didn't participate in that one, but one of my students, Will Shaw, did. So, the Russians came already to Canada. We picked Canada as being more neutral than the U.S.

Katsaros: That's probably wise in '71 and '69.

Businger: In '69, yes. I remember that Tsvang was complaining that we had problems with the weather in the Northwest and I said the gods were weeping because Russia had invaded Czechoslovakia and the peaceful movement was not there. He had no answer to that because he could not read the Russian papers here, so he didn't know what was going on.

Katsaros: It just happened during the time of your experiment? That was pretty daring of you to say. What did he say then?

Businger: He said he hadn't made up his mind about what was going on.

Katsaros: I see. Did you have a KGB person along at that time to come to Canada?

Businger: I don't know.

Katsaros: One of them might have been. Usually there was supposed to have been one.

Businger: Maybe Tsvang had that function at that time although it became clear towards the end that he was not a party member. Then a couple years later we had the experiment in Tsimliansk doing the same things; comparison of sonic anemometers.

Katsaros: The next probably a mach 2 or 3 or the next version. You had to do it again, because you had changed things.

Businger: Yes. That was a very interesting experience just from a social point of view to live for a few weeks totally dependent on the environment there that they had created, which was a small town built because they had a big damn in the Don River, half way between Rostov and Stalingrad.

Katsaros: Sounds like the boondocks.

Businger: The boondocks, it was like Kansas.

Katsaros: Farmland? Irrigation?

Businger: Farmlands, grasslands, not irrigation just mainly grasslands. It was not terribly flat, but it was flat enough for us. We could find a flat site. The living conditions were very interesting. We stayed in tents; they had tents, very nice tents with cots and little lights so you could read at night. But you also had the feeling that you had to be careful with conversations that were politically sensitive. You had to watch out. I remember Yaglom asked me who was in the tents next to mine, before he would answer because you had asked a sensitive question. Yaglom, I might say, was a dissident and he was suspect.

Katsaros: He was interested to talk openly with you

Businger: He was interested in talking openly. He would shut his mouth when Tsvang was around.

Katsaros: I see. What a difficult life.

Businger: It was a difficult life but at the same time we had the feeling being there that the borders was so far away that you had freedom of movement and freedom of doing what you liked. We didn't have cars but they gave us bicycles. I

remember Miyake and I once went to Zimliansk village on our bikes. It was a few miles and nobody stopped us, nobody interrogated us, nobody paid even attention and we would go in the shop and just look around at what they had and the people there were very friendly.

Katsaros: Nobody could speak Russian in your group and they couldn't speak English?

Businger: They couldn't. We spoke a few words of Russian.

Katsaros: I think you and I had had some lessons together in the early 70's.

Businger: We had some lessons.

Katsaros: Kak dela?

Businger: Kakoya pagoda sivogna? (*What is the weather today?)*

Katsaros: Did they have anything to sell in the stores?

Businger: Yes, there were some shoes and well it was nothing really that you wanted. That was interesting.

Katsaros: Nothing to take home.

Businger: Nothing. We were as curious about the shops as the shopkeepers were curious about us.

Katsaros: Yes. All these things helped open up the country, I think.

Businger: Very friendly groups and we could take pictures. The only place where we couldn't take pictures was in the airport, but we could take pictures of the experiment, the village etc.

Katsaros: That's the thing about science; much of science is fairly neutral territory. It was a long distance from what you were doing to where it would have an impact on military operations.
So, what did you do? How long was this experiment? How many weeks were you there?

Businger: I was there only two weeks. I think the whole experiment was two weeks.

Katsaros: And it was hot and dusty.

Businger: It was hot and dusty, yes. That was basically it. Every day we said, 'jarka'(hot) but we worked only a few hours in the morning and a few hours in the evening and in the middle of the day we were free to go swim in the Don River.

Katsaros: It sounds so exotic even today.

Businger: And go fishing.

Katsaros: Really, catch your own fish?

Businger: Fred Weller was active in fishing. The scientists occasionally had a work session on one of the islands that we had to swim to and discuss our results.

Katsaros: Sit there on the rocks and talk. That's how good science is done, I think. Maybe you didn't solve any really deep problem there, but you began to develop communication and trust .

Businger: It was a pleasant mood there.

Katsaros: Was there anything really interesting that you found? I don't suppose you found it sitting on the rocks on the island, but were there any interesting results from the inter-comparison experiments.

Businger: The inter-comparison experiment itself did not generate new insights in science, but we occasionally got an observation that was very interesting. There was one evening that we measured the profile of temperature and wind with height and above 4 meters the flow was laminar and below 4 meters the flow was turbulent. So, that gave me an insight---------(continued on tape 2----

END OF TAPE 1

Interview of Joost Businger

TAPE 2, SIDE 1

Katsaros: This is tape 2 of the interview with Dr. Joost Businger on August 29, 2000, on Whidbey Island, Kristina Katsaros interviewer.

We finished on tape 1, talking about a discovery of some special conditions in the evening data, maybe you want to continue about that.

Businger: Yes, so we measured the profile; at 4 meters, the flow was laminar and at 2 meters and below it was turbulent. The sheer production of turbulence near the surface is dominant and results in turbulent flow. You expect the transition from turbulent to laminar to be above the surface at a certain height. I had never seen that before so it was very interesting to actually try to calculate what the Richardson number was between those two levels. It all made sense, but it was not easy to get a good estimate of the critical Richardson number for the transition from turbulence to laminar flow.

Katsaros: You learned about the existence of these kinds of situations

Businger: Existence of the critical Richardson Number? Well, I knew that it exists, but it was so nice to see it actually in your measurements, right in front of you.

Katsaros: Did you have wind profiles too?

Businger: Yes.

Katsaros: Were they like jets?

Businger: No, the jet was much higher.

Katsaros: Much higher.

Businger: Yes, I have seen that also, but this was a special effect of the surface layer in the lowest 5-6 meters that we made the observations.

Katsaros: Maybe we could take a moment and talk about other experiences in your research where you really discovered something, the "ah-ha" experience.

Businger: Yes. There are several of those. Maybe I should start with the first one that I remember very well; that was a long time ago. That was actually in the late forties just after the war. I was working on my thesis. The literature that I had was still basically German literature. That dominated my research and the Meteorologische Zeitschrift, I believe, was more influential than the American Journal of Meteorology at that time. There was a German scientist by the name of Hans Ertel, who seemed to be a very prominent scientist, but he could not figure out how the heat flux should be formulated. He assumed that the mean vertical velocity must be zero because there cannot be any average mass transfer in the vertical.

Katsaros: You can't lift the whole atmosphere up.

Businger: Right. And that restriction if you go through turbulence calculations, doesn't allow you to propagate a heat flux except in third order moments. The thing, of course, is that w bar is not = zero, but rho w bar = 0 and the insight that happened to me—and I was sitting there on the balcony thinking about it—was such a joy. All of the sudden I realized it. If I had gone through the literature more carefully I would have seen that Prandtl had recognized this too, but I didn't have that literature. I was sort of stuck with Ertel's work. So, to me this simple insight, was a real "ah-ha" experience.

Katsaros: At least you had to bring up more warm air than you bring down cold air to keep the density the same. I just had that insight into this problem which is good. But

those things you have to work through as a graduate student. Every student has to work them through, and it's hard. But if the professor has done it he can convey it better, I think. That's why you have to struggle a little bit as you grow in your field.

Businger: That was to me the most memorable event of that sort. Later on I worked on my thesis and through similarity theory, which I studied in the physics department, I realized that the Richardson Number was not a good non-dimensional number for describing the wind profile as a function of height and stability. So, I looked for a replacement of the Richardson Number that would be proportional to the height rather than a complicated function of the height. I didn't know that the Richardson Number was actually proportional to the height in the unstable stratification. It certainly is not obvious from the formulation of the Richardson Number. So, then I formulated something that was totally parallel to the Monin - Obukhov similarity, which I didn't know at that time. This was not as big an event; it was just nice to see that it was not that difficult to get a z/L formulation. That is basically what I did. I called it $S_{N,}$ the stability number. It turned out to be the same thing as z/L.

Katsaros: Then you discovered later that Monin and Obukhov had thought of these things. When did you learn about them doing this?

Businger: Much later after I published my thesis.

Katsaros: Then you feel very proud you did your own independent development of it. I had a Russian do that to me, too. Sergei Kitaigorodskii had done the kind of parameterization I used for turbulence in the ocean surface layer. I had thought of all these options and then I wrote it up in my thesis and sure enough his paper was published earlier. But then I was rather proud that I could have done something similar to Sergei Kitaigorodskii.

Businger: You were in good company.

Katsaros: Yes, I was in good company even though it would have been nicer to be original.

Businger: I didn't think it was a big deal to use similarity to get a better non-dimensional quantity. However, it was an important chapter in my thesis.

Katsaros: It shows you physical relationships even if it doesn't explain reality. Similarities never explain anything.

Businger: But it is making the framework better.

Katsaros: Having worked it through yourself might have been your strength when you came later to some other things that you wrote on this subject.

Businger: So the turbulence kinetic energy equation became a central thing in all of this, because there is the buoyancy energy and there is the shear production energy. They determine this profile relationship. Then follows the insight that the buoyancy force is in the vertical and the shear production is in the horizontal, and therefore the buoyancy is much more efficient in creating w, than the shear production is. That insight was a nice one.

Katsaros: When did that happen?

Businger: That happened a few years later when I was in the states. In fact I think it happened when I was still in Wisconsin. I told Suomi about it and Suomi was totally uninterested in theoretical questions. He said: 'theories come and go, but good data stay forever.'

Katsaros: I think it is even more true today if you have a new datum collected. When I did my thesis with Konrad Buetner as my main advisor and you on the committee, I was working on the ocean turbulence and heat fluxes, it was you who made me go back and do the turbulent kinetic energy calculation. You helped me work through that for the kind of turbulence I was calculating. I'm glad you knew about them because it enriched my thesis a lot. Thank you!

That's a nice quote from Vern. I'll have to remember that one, too.

Businger: I was excited, but he sort of scoffed at it, and that is what I worked on. It was also the first thing to work on when I came to the University of Washington. It was the generalization of the mixing length concept and that idea of the buoyancy flux and the shear production determining the profiles. Then, during the sabbatical leave in Australia, it found its final form in the profile descriptions.

Katsaros: Would you consider that one of your major papers? The paper on the flux profile relations?

Businger: Yes.

Katsaros: That's really the sum total of many years of work.

Businger: The sum total of many years, the article that came out in '71 based on the Kansas experiment. In a way I feel a little more satisfied with what I wrote for the Workshop in Micrometeorology in 1973.

Katsaros: It's just more mature, the writing about it, but the ideas are the same.

Businger: Yes, and all the various aspects.

Katsaros: We read your chapter and several others chapters in that book. It became one reason I had stopped doing turbulence research after a while; because it had been done, it's all in there and not much has changed.
There is more happening now again. It's a third generation of scientists. I considered you the first…or part of that earlier antiquated work.

Businger: Taylor was one of the first.

Katsaros: Yes, a lot of it was in wind tunnels and yours were the fieldwork generation, the first. Mine was several years later. There was a whole group of your students among them Roland Stull, Jim Wilczak, Philip Kahn, Will Shaw etc.? And then there is a new generation of graduate students, mine and others who are right now doing wonderful things, in a very sophisticated way, building on all this. So, it will be nice to have these stories collected.

Businger: Another different topic came up when Bob Fleagle and I decided to write a book on an *Introduction to Atmospheric Physics*. I was working on the Clausius-Clapeyron equation for droplets. The Clausius Clapeyron equation is developed for the equilibrium saturation of vapor pressure of a flat surface. For droplets there was sort of a clever way of finding out how the surface saturation equilibrium pressure would change, but I found a way, a simple way, of deriving this directly with the Clausius-Clapeyron equation, introducing the radius of the droplets. I was very proud of that. It is in our book. It was not a separate paper.

Katsaros: That wasn't known before?

Businger: It was known but the way it was derived was done in an indirect way.

Katsaros: I see.

Businger: Another interesting set of measurements was carried out in the GATE experiment in the tropical Atlantic Ocean off the coast of Dakar. We were trying to measure the dissipation of turbulence kinetic energy and using this information to determine the stress over the ocean. We found that the dissipation appeared as a bi-model lognormal distribution. Many years later I was working on the conditional sampling technique to measure the fluxes of trace gases at NCAR and discovered a similar relationship. What you do is you collect samples of air when the vertical velocity up and when the vertical velocity is down in two different containers. The difference in concentration of the trace gas in the two containers multiplied by sigma w gives a measure of the flux. The distribution of the vertical velocity very similar to what we found in the distribution of the dissipation. So, there is a…

Katsaros: A link here, a thread.

Businger: There is a link here that I didn't realize until we had a 25 year commemorative meeting of the GATE experiment and then I realized that we already did similar things as the conditional sampling in '74…

Katsaros: Did you do it during the experiment or the analysis? You recorded every piece of data and then…

Businger: Yes, it was the analysis following the experiment, where we did conditional sampling.

Katsaros: You did some conditional sampling of the time series according to whether the *w* was up or down. I followed through with that in the SWADE experiment in the early 1990's. We haven't published it yet but we are talking about it still. There are some very interesting things that happen at multiple scales in the system.

Businger: Yes. Another interesting area is the entrainment at the top of the boundary layer. This is one thing that Steven Stage realized…I think he was the one who really brought it up.

Katsaros: In his thesis?

Businger: In his thesis on the convective boundary layer he showed that the downward motions reach all the way down to the surface. A consequence of this is that much more energy goes into entrainment at the top of the boundary layer than when you assume that all the mixing occurs near the top of the boundary layer, in fact about 5 times as much. This assumption we were able to test with the Boulder Atmospheric Observatory (BAO). Jim Wilsczak had the data from the tower and we looked at it and we tested the downward and upward fluxes at the various levels between the surface and 300 meters. The boundary layer was rather shallow, but high enough to test the model.

Katsaros: Are there sonic anemometers at many heights in the Boulder Tower?

Businger: Yes.

Katsaros: Kaimal was the one running that Tower was he not?

Businger: Yes, he was in charge of the tower. We were allowed to use the tower for that experiment. Jim Wilczak was the one who analyzed the data.

Katsaros: Was he still a student then?

Businger: He was still a student.

Katsaros: We are actually getting to mention many of your students in this discussion, which is good, Jim Wilczak.

Businger: Anyway, that was an idea that Steve Stage had worked on, which made good sense and here the actual physical proof was found in these observations, which was not simple. It was rather sophisticated.

Katsaros: Very sophisticated instrumentation.

Businger: The set of papers that came out of this was a significant contribution.

Katsaros: How does Roland Stull's work on the total boundary layer fluxes fit in with these two?

Businger: His study was earlier.

Katsaros: But he also had some idea of this, or maybe he picked them up after he was already a professor.

Businger: Well, Roland Stull was an interesting student. I remember that I gave a take-home test to the students on the atmospheric boundary layer, and I said, "If your paper is publishable, you get automatically get an A."

Katsaros: …makes sense.

Businger: And he took me literally and he wrote a whole paper with an introduction, but unfortunately, it was not publishable.

Katsaros: -- it was probably an "A" paper, anyway.

Businger: It was an A paper and it was a gallant effort.

Katsaros: Good for him!

Businger: Another topic is the radiative cooling at the cloud top. There has been a controversial approach to this by Doug Lilly, Jim Deardorff and my student, Philip Kahn. He was probably one of the brightest students I ever had. He came to me and said, "I think all that radiative cooling is in the boundary layer, not above the boundary layer," and we sat down and we looked at it and I agreed with him. And we worked it out and sure enough, that was, I think, a major step in understanding how the boundary layer cloud top interacts with the stable layer above. And we had a long discussion with Doug Lilly and Jim Deardorff about this issue.

Katsaros: Did you come to an agreement?

Businger: We finally came to an agreement with the help from Frans Nieuwstadt, my colleague from Holland, who was visiting the University of Washington. He and I worked out some details of the problems that Deardorff brought up that we had not resolved. That settled it, and that was a very nice effort from Frans.

Katsaros: It also shows the importance of some controversy, somebody asking difficult questions and the colleagues struggling with it. Nobody does this alone -- and the students, of course, are very important.

Businger: I agree.

Katsaros: You know ---- it may not have happened without the right people coming together at the right time and place in a congenial, free atmosphere. It probably wouldn't happen somewhere else. It's a sort of real serendipity in there in addition to a focused scientist….an arrow, like yourself, going through it all and gathering the results along the way.

Businger: Another topic that I think is very exciting, was very exciting at the time, was the surface renewal concept which your student, Tim Liu (or our student), brought up. It is amazingly accurate.

Katsaros: And it's held up well.

Businger: So I made a serious effort to incorporate it rather extensively in the book I wrote with Eric Kraus, "Atmosphere Ocean Interactions".

Katsaros: I think you gave us a lot of good press; some for myself and also my students; I appreciate it. Those are the two books that you have worked on.

Businger: Yes.

Katsaros: One with Bob Fleagle and one with Eric Kraus.

Businger: Right.

Katsaros: Both real valuable contributions.

Businger: I'd like to go back to an old problem that I was confronted with by the director of the Institute for Horticultural Engineering in Wageningen, where I had a job, while working on my thesis. He said, "There is a curious observation: when I am in a greenhouse, it's always warmest on the windward side of the greenhouse; why is that?" And that question stayed with me. I found the solution after studying greenhouses for a while. It's simply the pressure distribution across the greenhouse. Because the glass panels are not totally closed, there's a lot of ventilation between the greenhouse and the atmosphere around it. The pressure is lowest right around the corner of the windward face (on the side wall) and there is a higher pressure where the wind hits further downwind on that sidewall.

Katsaros: Where the wind impacts?

Businger: -- where the wind hits, but that is relatively less than the low pressure that is caused by the effect when the wind goes around the greenhouse. So it sucks the air out at the front end and it brings the cold air in at the downwind end of the sidewalls.

Katsaros: That is inside the greenhouse?

Businger: Inside there is a flow near the surface, that goes against the wind direction and warms up.

Katsaros: while it's in the greenhouse? So an important part of this problem is that the sun is shining?

Businger: And then I told the director the solution, about a year after I started working there. He was hardly interested. I was very disappointed.

Katsaros: I have a story like that, where someone asked me a good question and 20 years later, I finally had the solution and he wasn't interested. However, that's my story. You may want to say more about your science stories.

Businger: One more comment -- it's about frost protection. We were very lucky in Holland at that time that there was an April and May when we had frosty conditions, ground frost almost every night, and that was just luck. Often in the spring you don't have frost on the ground. We had these plants and the growers of the plants were very furious with us because we put them out of the greenhouse. We froze them! "Why do we grow those plants"? But that was the experiment. We got valuable data from that and later on when I was at the University of Washington, the question came up, "How do we protect grapes from freezing in California and Washington?" Phil Church was a wine connoisseur, a wine maker and he was very interested in this. So, I worked it out and gave him a formula: how much spray you should use for certain frost levels, for so many degrees. I had a formula for that. Then they could measure the temperature and see how much they should spray.

[Side A audio cuts abruptly; does not resume].

TAPE 2, SIDE 2

Businger: -- frost protection with sprinkling the crop that you want to protect. And this formula was used by a grower in California, a friend of Phil Church. Phil went down there and the man was enthusiastic. He said it saved his crop and he gave him a case of wine. Phil came and said, "Well, you did this." He was very proud of me. The formula worked.

Katsaros: Probably was used widely? To this day?

Businger: It is probably used.

Katsaros: So maybe we should move along to when you left the University of Washington and went to the National Center for Atmospheric Research (NCAR). The circumstances around that and how you enjoyed those years?

Businger: So by the end of 1982, the state government of Washington decided that they would make early retirement of faculty attractive and I fell precisely in the category that would benefit most from early retirement, but although I was chairman of the department at that time, the administration said, "Well, if you want to use this opportunity, we won't stop you from doing so." So, then, I called NCAR to see if they had a position and right away, they offered me a visiting scientist position for two and a half years. Without any further ado, I accepted this generous offer. Because I took early retirement, I was officially under retirement procedure by the beginning of 1983, but they certainly wanted me to finish that academic year of chairmanship. They paid 40% and NCAR paid half my salary already, before I was there. So it was all a very generous arrangement.

Katsaros: They knew you were going to work hard when you got there.

Businger: So, in the fall of 1983, I went to NCAR and enjoyed working for the Atmospheric Technology Division, ATD.

Katsaros: And then you developed some measurement devices for terrain measurements?

Businger: We concentrated on some field experiments that were an extension of the Kansas experiment. We did that in Wyoming as a continuation of the work that Steve Oncley had been doing with us at the University of Washington, after he went to NCAR. Tom Horst, who was also a former student was working in the same division. We carried out similar experiments with the facilities that NCAR had available.

Katsaros: I think you also developed new technologies.

Businger: Yes. That was what we named ASTER, (Atmospheric Surface Turbulent Exchange Research).

Katsaros: Makes a nice name. Was it a big change for you to go from a University, from a freer world, to this more engineering-oriented part of NCAR?

Businger: The freedom certainly was similar, because NCAR is so strongly related to the university community and there is so much exchange of university faculty. The only thing that's different is that you don't have graduate students to work with.

Katsaros: That's kind of a big loss.

Businger: Yes it is big, but you don't have to write proposals.

Katsaros: That's a big advantage.

Businger: A real big advantage.

Katsaros: It has pluses and minuses.

Businger: So, since I had had a very stimulating period with graduate students, it was OK to go totally into research and have that field program. I'm certainly glad that I was a faculty member before I took a research position.

Katsaros: I had a similar experience. You know, when I left the University I still had about ten years left to my career. Twelve, maybe. I also think that it was good, at that time -- you get tired of writing proposals.

Businger: I had my graduate students very well trained -- they wrote the proposals, usually, and I improved them. I was, of course, lucky riding the wave after SPUTNIK with good support for research.

Katsaros: And the field of meteorology was growing at that time.

Businger: It's much more difficult now.

Katsaros: But you were happy about your years at NCAR. Five years? How many were there?

Businger: Six years at NCAR. They were very pleasant years doing research, developing the ASTER facility, which is a good facility to use to make surface structure measurements. It is interesting; there was a decision between doing research and becoming an administrator or a manager. At that time, the position of director of the Micro and Mesoscale Meteorology (MMM) division at NCAR became available and I was asked if I were interested in taken that on. At the same time I had the opportunity to develop ASTER and I decided that was what I wanted to do.

Katsaros: It was kind of a climax of your career. You had spent all your years getting ready for something like that. The community needed it and all your training led to that; it could have led to the other one, as well, I suppose, but there might have been more adjustments to make.

Businger: Well, I had been chair for a year for the Department, so I had no difficulty with a managerial position.

Katsaros: Well, you still had to lead quite a few people in this effort.

Businger: Not that many, about four.

Businger: It was getting the funding for that and getting the engineering all worked out. Well, it was just about finished when I left and retired.

Katsaros: And retirement was in late 80s?

Businger: Yes,'89.

Katsaros: Eighty-nine. So you had 11 good years of doing many other good things: maybe we can talk about them a little bit. I know how much you have been active and supporting the American Association for the Advancement of Science, as their secretary, for many years now.

Businger: They asked me to be chair of the Section for Atmospheric and Hydrospheric Sciences. And I accepted that. Later on when I held the past- chair position, the secretary wanted to quit or quit, actually, before his term was finished. Then they asked me if I thought it would be fun to do that job.

Katsaros: That's the person who really works in that organization.

Businger: The secretary helps organize the symposia and the nomination of fellows.

Katsaros: I think it's a wonderful thing for someone who is retired and no longer were writing proposals, but you still working on the revisions of the book with Kraus, I think.

Businger: Well, it was a busy time of my life; I was to build a house and write a book with Eric Kraus (laughter).

Katsaros: But has it been rewarding to put in this kind of effort for an organization like AAAS, where Atmospheric Sciences is one of many sections?

Businger: It. A is just hard to say whether that's rewarding. It is... somewhat rewarding. The best thing is, actually that you get to know the new chair every year. Sometimes you know them from before; sometimes you don't.

Katsaros: Get your friend involved.

Businger: Get your friends, like you, for instance, and Dennis Hartman.

Katsaros: Yes, it was nice for me. One plans symposia and…

Businger: …make a good effort at promoting fellow-nominations.

Katsaros: And now they very often promote inter-disciplinary symposia. At the planning meetings you have to work with the representatives of other disciplines.

Businger: I organized the symposia for Frontiers of the Physical Sciences -- that was fun, for totally different people: a mathematician, an astronomer --

Katsaros: One of our University of Washington colleagues, Marcia Baker spoke in that one.

Businger: Marcia Baker, yes.

Katsaros: That was very nice. Not too long ago you invited your son, the meteorologist, Steve Businger to give a lecture. How does it feel to have your son being a well known scientist in the same field?

Businger: Well, feels good. It was interesting that when he was a teenager, he went to Holland, last year of high school... and he spent time with his uncle.

Katsaros: That's your brother.

Businger: No, his mother's brother. He was one of the directors of the Royal Dutch Company, so big wheel of a very big company. And Steven made the effort of comparing my job with his job. So, which direction should he choose..

Katsaros: He was at the crossroads, now.

Businger: When he came home, he said, "I'm going to choose your way because he looks so gloomy all the time."

Katsaros: …and my daddy's laughing all the time; he skis, he hikes, he has fun. That's my life (laughing). I think he did well learning from his daddy. He is having a good life and now you two work together some.

Businger: That brings me to two problems I think I would like too see solved -- and one is a very old one that I've been... kicking around and we mentioned it already in this interview: that is the critical Richardson Number when you go from turbulence to laminar flow. Whereas the critical Richardson Number for flow changing from laminar flow to turbulence, we know is 0.25 -- that is just a perturbation calculation. But, the other way around, we really don't know, and maybe there is not one single number because the turbulence kinetic energy equation has several terms that could affect this and it depends on what situation you are in. It still would be nice to have a clear cut experiment like the one in Tsimliansk, but that

was not accurate enough. We could say it is between 0.2 and 0.5. We need to know it much more accurately than that.

The other problem is how to get a handle on the measurement of stress that hurricane force brings. I've been contacting some colleagues about this, but the present technique is out. There is too much spray to make the sonic anemometer work.

Katsaros: And here's no platform.

Businger: Where's the platform? I would like to see that problem, solved. We are working with the smart balloon. I am dreaming that the smart balloon can maybe solve this problem.

Katsaros: Tell about the smart balloon.

Businger: Steven, my son has been involved with the smart balloon for several experiments already and works with Randy Johnson at the NOAA laboratory in Idaho. It has had success in getting measurements in boundary layers in some experiments? There was one in the Azores.

Katsaros: That may be ASTEX.

Businger: Yes, ASTEX--- that's correct. Now, he decided that the smart balloon might be an interesting tool to use in a hurricane, because it is so hard to get boundary layer measurements in a hurricane. He asked me if I would participate, since I know more about boundary layers than he does and if I could be a consultant. He wrote a proposal and it was funded, so now the smart balloon is being reinforced.

It can stand the forces and accelerations. It has a GPS positioning system and communicates with aircraft, land or satellites. You can program it to keep a certain altitude so that if it goes up, it will automatically correct itself so that it will become heavier. When it goes down, it will make itself lighter to go up again.

I was thinking that possibly if you have a motion sensing system in the smart balloon (the GPS is probably not fast enough to have careful measurement of all of the motions the balloon makes and the corrections the balloon makes). If you have all that information from the motion sensing package you could possibly extract an eddy correlation between u and w. Although you are in Lagrangian mode, your sampling may be slow, but once you are in an eddy and then go into another one, you'll have a very strong signal.

Katsaros: Maybe within the eddy, there is turbulence also.

Businger: Yes. Well, the balloon is about 3 meters in diameter so it is not that large, and you could fly it as low as 100 meters in a hurricane.

Katsaros: Risky if it hits a wave.

Businger: Yes, if you go any lower, that could be very risky.

Katsaros: Well, there's possibilities for joint work with the my Laboratory, i.e. with the Hurricane Research Division We have a similar project in airborne boundary layer measurements, but I'm saying to them:"Let's not go into hurricanes; let's just take measurements at 35 meters/second, 40 meters/second, even, 45 meters/second, where nobody has ever measured the stresses or any details of the flux processes before".

The maximum wind speed for stress measurements to date is 26 meters/second.

Businger: They're all sort of indirect measurements, which you can obtain from the geostrophic flow and the --

Katsaros: -turning of the wind --but even those things aren't measured very well at the scales of the highest winds in hurricanes.

So it looks to me like you have not stopped. You're still thinking and going strong. You know, planning the ultimate project. The one that stays on the horizon and beckons everybody.

So, is there a plan when this might happen?

Businger: Well, we hope to have one experiment, maybe this fall, and then next year have a serious expedition.

Katsaros: We have to get offline and discuss coordination with this other project. And there's a major hurricane field project next year. CLIMEX 4, it's called, with NASA aircraft. And the Hurricane Research Division's flight hours will be spent on some high-wind cases, I think, so that will be a good time to coordinate.

Businger: Right.

The other exciting aspect is that the dissipation of turbulence kinetic energy at those high winds produces a significant amount of heat. In fact, it can be larger than the latent heat of evaporation

Katsaros: That's interesting

Businger: And on that subject, Steven and I have just submitted a little note to the Journal of Atmospheric Sciences.

Katsaros: Nice! Is this your first publication together?

Businger: That's our first publication together, if it is accepted.

Katsaros: Let's hope. That would be a fun thing to have.

Businger: That is on the dissipation of turbulence kinetic energy in high winds.

It's, again, such a thing that you never think of, since usually at lower wind speeds dissipation is negligible for heating. But then, why not extrapolate it, because, you know, airplanes have trouble with frictional heating

Katsaros: Nobody has ever experienced it hardly. Even the people in the hurricane sit behind closed shutters when the wind blows like that.

Businger: That heat is used for evaporation of spray and that is also a reason why the temperature profile is not anything like what you might predict.

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Katsaros: So do you have any closing remarks for us? Tell about the joy of this career.

Katsaros: So what is the fun -- most enjoyable part, maybe some of the difficulties? Do talk about both, now, for a couple minutes until the tape runs out.

Businger: I can only say that I have been lucky and at such a young age I made a decision that I was interested in meteorology and that it stayed with me.

Katsaros: I think it has kept you young, also.

Businger: It kept me young.

The other, sort of, a second -- a distant second interest, was architecture. And now I build things and stand beside them (laughs).

Katsaros: You did that once before, in a way; your main family home was...

Businger: -- I had a real architect. I did this new house, really, myself. And I would like to encourage everybody to really think about conserving energy. Because we have to change our lifestyles, the use of water and the use of energy have to be reduced.

Katsaros: It is dangerous.

Businger: Is way over what we can -- what the Earth can handle.

Katsaros: In the United States, particularly.

Businger: And the rest of the world is going to follow.

Katsaros: Per capita, anyway.

Businger: So there's going to be a tremendous need for change in the United States.

Katsaros: The rest of them are already preparing because they don't even have those resources--- many of them.

Businger: They will. They will get it, and --

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Katsaros: --life style is the way to go.

Businger: Absolutely. We are always the example.

Katsaros: Well, that's a good note to end this interview on. I think we're very close to the end. Well, we might have a couple more minutes, if you have anything to add.

Businger: No.

Katsaros: Well then, thank you, very much. I'm so glad we did it.

Businger: It was a pleasure.

Katsaros: It's been enjoyable for me to go over the old stories and learn a few new things about how it really happened. Thank you, Joost.

Businger: Thank you very much.

END OF INTERVIEW