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TAPE RECORDED INTERVIEW PROJECT

Interview with Aksel Wiin-Nielsen
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Interviewers: Joseph Tribbia, Warren Washington, Akira Kasahara

Tribbia: This is an interview with Aksel Wiin-Nielsen. The date is June 29, 1987. We are interviewing at the National Center for Atmospheric Research in Boulder, Colorado (NCAR). My name is Joseph Tribbia, of NCAR. With me are Akira Kasahara and Warren Washington, also of NCAR.
Aksel, I will ask the first question regarding your early life and your childhood. Were there any particular interests in science that you displayed as a child?

Wiin-Nielsen: Let me say, first of all, that I was born into a family in which I was the only male child. I have three sisters, two older and one younger. My father was a schoolteacher, and he had been, as a matter of fact, my first teacher in both mathematics and physics at a level which you probably in this country would call the "junior high." I am very much aware later in life that the influence of my father definitely brought me into the sciences. I think also that he had a very great influence on my appreciation of physics and mathematics, and we also agreed, my father and I, very much, that the so-called "soft" sciences of biology, zoology, botany and so on were not for us. So I'm aware of that influence. Also, I think it became quite natural for me when I had to get into a new school to get into what you would call the "senior high"--that is what is called the State School, 1944, which you have in my curriculum vitae--there was no other choice for me than to enter the program where you majored in mathematics and physics. On the other hand, if your question leads in the direction that I should have had an early interest in meteorology, I must admit that that was certainly not the case.

I come from a family where we have a great abundance of teachers at various levels, and I think it was a foregone conclusion, certainly for my parents and also for myself, that I would go in the same direction and therefore after I finished high school and graduated into university which was the University of Copenhagen, which I entered in 1944, then I selected to take what we call the natural sciences. In those days, things were rather old-fashioned because, if you wanted to study any of the four subjects--mathematics, physics, chemistry or astronomy--you had to take all four. I always felt that that was really quite a large area to cover. I selected from the beginning that I would major in mathematics, but I spent at least four years studying also physics, inorganic/organic chemistry and astronomy parallel to mathematics and only after I had taken my first two exams at the University, then I concentrated on mathematics and graduated from the University of Copenhagen in 1950 with a degree which in this country would correspond to a Master's degree. We had a slightly different arrangement than here and even at that time, I was totally convinced that I most likely would spend the rest of my life as a high school teacher concentrating on mathematics and physics. So there was no meteorology at all.

Kasahara: So after graduation from the University of Copenhagen, you started working directly at the Danish Meteorological Institute?

Wiin-Nielsen: No, I didn't, Akira. I graduated, but already during the last couple of years, since there was a very great shortage of mathematics teachers, I had already started to take temporary jobs at various places all over the country, filling in for some teacher who was sick or on vacation or something like that, so I had quite an experience in teaching even when I graduated. Immediately after graduation, I spent four months at one of the state schools (senior high school) and taught mathematics and physics after which I entered national service in the military and spent a good bit more than a year serving in the Royal Danish Guard, a military outfit which is very old and which has the guard duty at the royal castles--wherever the royal family is. I have destroyed all pictures from that time, but there were some and you look rather ridiculous because you are dressed in light blue pants with a double-white stripe down both legs, a red jacket with white cross over and a hat which is about a foot-and-a-half high made of bearskin and with a tremendous leather strap to keep it in place. I never was very fond of the military and certainly what little there might have been left at that time was totally destroyed.

And then after that, after I came out of the military, it would have been 1951, I had already applied for and obtained a fixed position at the high school. I taught for the full year math and physics at all levels from, I suppose the youngest kids might have been 10-11 years old, up to the 17-18 year-olds who were in the senior class, and it was during that period of time--it was a school located on a small island in the middle of the Baltic--the island was no more than about 20 miles by 20 miles--that I realized that it might be better for my future students and I if we never met. I really realized at that time that there was not the satisfaction in teaching as I had expected there would be. The following happened:

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I remember one Sunday, towards the end of the school year, I was reading the Sunday newspaper, and in that newspaper, there was an interview with the then newly-appointed professor in theoretical meteorology at the University of Copenhagen. There had not been a professor in meteorology before, but one had realized at the university that this was becoming an important field, and they finally decided to appoint Ragnar Fjørtoft, who came, as far as I know, directly from the Institute of Advanced Studies in Princeton, where he had been part of the team around John von Neumann and Jule Charney. As a matter of fact, as you know, the first paper on numerical prediction was co-authored by von Neumann, Charney and Fjørtoft, and Fjørtoft was a visitor when he obtained the professorship at the University of Copenhagen. He came there, I think, sometime early in 1952. In the newspaper interview, which I was telling you about, he said more or less, "If there's anybody around who has a good background in mathematics and physics and has an interest in meteorology, I am in the market for hiring a couple of these people to work on a project which I am about to start."

I was sufficiently disenchanted with the teaching of these kids. I probably suffered at that time from what we in Denmark call the "island disease," the one where you live in a very confined space, where you are probably not very welcome because you come from the outside into an island community and the long-and-short of it was: I wanted out. So I had an Easter vacation shortly thereafter. I took the boat over to Copenhagen. I was lucky enough to have an interview not only with Ragnar Fjørtoft, but also with the Director of the Danish Met Office because naturally I couldn't just give up my job and not earn any money. So an agreement was made that they would employ me as part of the Danish Meteorological Office, but they would also permit me to start studies of meteorology under Ragnar Fjørtoft and to the extent it was possible for a novice like myself to spend about half-time working on his project. So that was my introduction to meteorology and I followed Fjørtoft lectures for about three years. Hans Buch, who is known in the general circulation community for having done a lot of wind statistics under Victor Starr at M.I.T., had at that time returned to Denmark. We shared an office and both of us were half-time assistants to Ragnar Fjørtoft.

I think I should also tell you what we were doing. It was at the time when Fjørtoft, of course, had been exposed to the ENIAC machine, which they were working on, but he was also an ingenious fellow, and he realized, of course, that when he returned to Denmark, there would be no electronic computer around at that time. Then he invented, I think, a rather ingenious method of integrating the barotropical vorticity equation using entirely graphical methods. It was

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time-consuming, it was cumbersome, but it was a good enough research tool that we, with reasonable accuracy, could cover quite a number of cases and thereby test the method. It did require that we use a heck of a lot of what you might call transparent paper, which was put on top, you copied it over twice, and then you displaced it 600 km. to the east and 600 km. to the west and you averaged those two fields, and then similar differences to the north and the south and you averaged those two fields, and then you averaged the two resultant fields, and lo and behold--you had the vorticity field. Then, at the same time, you had obtained the so-called mean field in which the vorticity was supposed to move and in which it was supposed to be conserved. So you also had methods of displacing things in the field, and an ingenious little instruments was found for the spacing of the isohypses; you could measure out exactly how far things would go in 12 hours and then you made another 12-hour step. Then you had the distribution of the vorticity after 24 hours and then of course you graphically had to solve the Poisson equation, and for this, Fjørtoft had developed a series expansion depending upon a number of terms, all obtained from the mean field and that then was carried out. Finally you had a prediction. We got the time down to about 2-1/2 hours, if I recall correctly, for the production of one barotropic forecast using these graphical methods over a region which covered, let's say--the final product covered the eastern Atlantic and most of Europe. Of course, you kept losing things all the time, because things went out one side of the region and came in the other, and the other peculiar aspects of Fjørtoft's method, was of course that he paid no attention whatsoever to boundary conditions. So mathematically, of course, it's an ill-defined problem, but in practice it worked reasonably well. And I would say that Fjørtoft himself was quite a master in doing these kinds of things.

Washington: In fact, I remember the elements of this were described in Petterssen's book, and I used it then as I was in graduate school. At what point did the methods get superseded by the use of computers in Europe?

Wiin-Nielsen: As a matter of fact, what happened was that, at the same time as we were sitting in Copenhagen making all these test forecasts to test the graphical method, Sweden--which has always been among the Scandinavian countries the one which is most technologically minded--they of course had realized that there was new development of computing in the United States. They had started a project, I think around 1951-2, of replacing an old relay machine which they had. As you recall, a relay machine was one you programmed by actually connecting wires between points until it did what it was supposed to do. They had one of those at the University of Stockholm, but then there was a national computer project made, and they turned out a computer called "BESK", which

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stands in Swedish for "Binary Electronic Sequential Computer." That was ready around, as I understand it, 1954-5.

In Sweden, as well as in Princeton, one of the first applications was indeed meteorology. The Swedes were rather well-prepared for that, because, first of all, Carl-Gustaf Rossby had been closely connected with the Institute in Princeton and Charney's arrival there, but then, of course, he had also in the late forties started to commute between Chicago and Stockholm. His so-called International Meteorological Institute had been formed, and he had furthermore seen to it that Bert Bolin had been at Princeton for a year, I think, he worked on mountain effects, but also learned in general about how to program these machines and make barotropic forecasts. So they were well-equipped, and I think in 1954 they started to make the very first barotropic experimental forecasts at the machine in Stockholm.

I would also say--I think it was obvious to everybody who got engaged in the graphical method that it was ingenious, it was probably just as good as the first integrations, but on the other hand, it was not the method of the future. The future was not in graphical things, it was in binary calculations, and the Swedes, as I said, had started on that. We followed this with very great interest from the University of Copenhagen and from the main office and then the following happened: In 1955, after Fjørtoft had been in Copenhagen for three years, the directorship of the Norwegian Meteorological Institute became vacant. He wanted very much, as a Norwegian, to return to Norway. He applied for and obtained the position of Director-General of the Norwegian Meteorological Institute and left Copenhagen in the summer of 1955. Now he was a man of honor, I would say, because when I went to him and said, "What's going to happen now?", he said: "Well, you are kind of caught in the middle, aren't you? You are halfway into meteorology, but I realize, of course, when I disappear, it will take quite some time to get me replaced and I know also that the university is seriously considering not filling the professorship right away." And then he said, "If you have enough money to pay for your own travel up to Stockholm and back again, I am invited up there to make a couple of lectures anyway. So if you can go along, you are welcome to do so and then let us see what happens."

So we made that trip, and that was the first time I ever met Carl-Gustav Rossby, Bert Bolin, Bo Döös--all these people who were already working with numerical weather prediction, and there must have been some conversation between Fjørtoft and Rossby about me because on the last day before we traveled back to Copenhagen again, Rossby made a little remark, "You will hear from me." So I went home and sure enough, within the next two weeks, I had a letter inviting me to spend at least half a year at what we called "MISU," the Meteorological Institute at the University of Stockholm, and typical of Rossby, he said, "You're welcome to come and work here. We hope that the Danish Met Office will give you leave, with salary, in which case we won't pay you anything." Typical.

I had to call him back and say, "Yes, I can get leave and I can also get paid my salary to the end of the first half-year, but I doubt very much that that would be enough. I have a family, a wife who would have to stay in Denmark." My wife, Bente, was teaching at a teacher's college at the time and our first child, the present meteorologist, Charlotte, had just been born, so I had to ask Rossby for some more money and he came up with an additional 500 Kroner per month, which amounted to about $75.

However, I went, under those conditions. It was poverty, no doubt, but then Rossby was of the opinion that if you don't have much money, you don't use much time spending it, so you can work all the time. This was indeed the case.

Now the first few months I simply spent my time learning to program. There were special courses for that. As you know, programming at that time was probably was what we call a simple language today. I learned it in about a month. Then I took Bo Döös' course in dynamic meteorology and numerical weather prediction, and I took Bolin's course in general circulation. So the first few months went by and I was coming close to the point where my six months were running out, and I was fully prepared to go back again. Then the following happened:

The chief of the military weather service in the Swedish Air Force, quite equivalent to the Air Weather Service in the United States, had come to Rossby, or I think Rossby had called on him to come, and asked, was the Air Weather Service interested in testing barotropic forecasting, in which case the Institute would be quite willing to take that on as a project. The reason that the time was ripe for that--we are now late in 1955--was that Döös and Bergthorsson had developed their first attempt at an objective analysis. They wanted to test the quality of the barotropic model and they wanted to see if one actually could carry this out day-by-day-by-day because, as you recall, Döös and Bergthorsson's analysis scheme was one where the forecast from yesterday was used as the first guess for today and then modified, so there was continuity in the whole thing. Well, there was an agreement between the Institute and the Royal Swedish Air Force and its Weather Service that they would have such a

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test. And then Rossby came and said, "Now we must put together a team." I always felt that the Swedish Royal Air Force must have been remarkably liberal, because I will now tell you who was on that team.

There was Bo Döös, as leader, a Swede. Then there was a Finnish meteorologist called Aimo Vaisanen. There was an Icelandic meteorologist called Hlynur Siggtryggsson, who is now the Director of the Icelandic Weather Service. There was an Iranian named Hessam Taba, who recently retired from WMO. He was in Sweden to study meteorology, but I think mainly because he had married a Swedish girl. And then, yours truly. So that means that, apart from the leader, not a single one of the team was a Swedish citizen. We came to the headquarters of the Swedish Air Weather Service about 7:00 at night, and then we started gathering all the data. We of course needed really only 500 millibar height and winds. Then there were some young assistants, nice Swedish girls, who punched all these data on a long paper tape. Then we also had a tape from yesterday which contained the predicted 24-hour field of 500 millibars within--this of course was a limited area/region. By about 10:00 at night, we then were driven down to the machine and then the whole struggle started. The first step was, of course, to get an analysis done, but as you probably also remember, those of you who have worked on these early machines, they were not very reliable.

The Swedish machine had Williams tubes, and they had a tendency to conk out all the time. Then the whole thing stopped. We had internal memory of 1,024 words. We had storage on two drums, which gave 2,048 words on each drum, so altogether 4,096 words storage on the drums, and they were used both for the storing of the program, but also for the storing of the data arranged on the drum. There was always an engineer present. It was not that you called an engineer when the machine broke down; he was there all the time because he knew it would break down. His major way of repairing the machine, just like a British officer during the war, he walked around carrying a little stick under his arm. It was actually more or less a foot long, made of steel. When the machine broke down, he would look in through the window at the tubes. And if there were a regular array of green points all lighted up, then the tube was functioning. He would find the one which had broken down, then he would take the stick, hit the machine a couple of times and normally it would shake into place again and he would say, "It's O.K.--go on." We continued, and of course the advantage was that the program storage on the drum--both the program itself and the data--they were not disturbed by the machine as such breaking down.

It was a kind of permanent memory as you have these days in little desk

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calculators. So we could always proceed. But of course we always had to go back at timestep because it was all fouled up. I think maybe we managed to do about 30-31 days continuously. Now that means we produced an analysis and a 24-hour forecast every day. But it was our purpose each night to get also a 72-hour forecast. If I recall correctly, it was only in about 2/3 of the cases that the machine was kind enough to function so long that we could get a 72-hour forecast.

Washington: Do you have any idea how long it would take to make a 24-hour forecast?

Wiin-Nielsen: Yes. For the region which we had at that time, which was about 30 by 40 points, a 24-hour forecast should actually run in about an hour and a half. But of course with all these interruptions, it took much longer. However, when we managed to get the forecast made up to 72 hours, then the girls, who stayed with us the whole night, they slept a little bit while we were fighting the machine. But as the results were coming out on the output medium, which was a paper tape again, they took their paper tape and then on an auxiliary machine, they could print it out in one long string of numbers and then they started plotting things in by hand on the map. When that was done, they brought it up to us and while we were waiting for the machine to produce a 72-hour forecast, you could already analyze by hand. We had no analyzed output, strangely enough, although that is really simple. It took quite some years until we got the technique of printing maps alternating with filled spaces and unfilled spaces. We didn't have that, but we simply drew them up from the plotted points and when that was done, she went back to the Air Weather Service with the products, which they then tested synoptically and from the point of view of prediction, and we went home, you might think. No, we didn't. We went back to the Institute and caught the morning lecture at 8:00. Rossby insisted that you never miss a lecture even if you had spent about twelve hours sitting with the damn machine. So, I think we managed, during that period, which was really the late fall of 1955 and the early winter, January, of 1956, to produce a series of admittedly experimental forecasts that were all made in an operational mode. I think it was about the same time that JNWP produced their first forecast, but I am not entirely up on the timing of the JNWP.

Tribbia: At this time, you were working formally for the Swedish Air Weather Service. Were you then formally enrolled as a student at the University of Stockholm and what happened to your student days as you progressed through the University of Stockholm?

Wiin-Nielsen: Well, yes, you're quite right. I had to make a living so that was the way I got

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my money. But already when I came in the summer of 1955 to Stockholm, I enrolled immediately into the University of Stockholm. That was possible because there always was an agreement between the three Scandinavian countries--Denmark, Norway and Sweden--that academic qualifications acquired in one country were good in the other two. There was really no problem getting into the university and I suppose after Rossby convinced me--and this he did in connection with the project I just told you about--that I better stay and get something out of it. Then, of course, I had to resign from the Danish Met Office, which I did. I remember, after half a year, going down and convincing Bente that she should give up her job and join me in Stockholm, which she graciously did, under the expectation, for the first time in her life, that this would only last a short time. I'll tell you later about the extensions--which took a little bit longer than anticipated, but she came. We lived outside Stockholm and, at that time, I really had to tell Rossby, "Now I have a family. There is child number one and child number two on the way."

"No problem. I will pay you twice what you got there." Eventually he did and living was not so bad. But, as I told you before, I took Döös' course and Bert Bolin's course during the first year. Second year, I took some applied mathematics because my own education in mathematics has really almost been totally in pure mathematics: function theory, number theory, that kind of stuff and, of course, the classical calculus and differential equations, but not really in the applied sense. So I took an applied math course in Stockholm, but again, typical of Carl-Gustav Rossby, when I had taken Bo Döös' course in the fall, when the next fall came, he said, "You teach it, because you must know this stuff, you have, after all, taken it." That was the general attitude. And of course for that you also got some extra money from the university. So you had to piece these things together. Certainly at that time, it was my goal--and Bo Döös had the same--to get our so-called licentiate, which more or less corresponds to a mixture of, I would say, a large American master's degree, where you have a dissertation and a Ph.D. It's somewhere in between. You had to take a comprehensive exam and you had to write a dissertation for the licentiate. The dissertation, however, need not have very much originality to it. I think typically the same as you find with a master's degree in the United States. There is an adviser holding your hand, you do the job, but you are not supposed to really have wild ideas. So it was my goal to get the licentiate; Bo Döös, who was then and still is, a good friend, was in advance of me, so he managed to get his degree and then he took off and went to Florida State for one year for further education. I was left with his job as assistant professor, doing the teaching, running two projects and still trying to earn some money. Then I asked Rossby if I now was well-prepared to take my licentiate. Typically, Rossby never gave

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an answer. He hated that people should graduate because it meant that he lost control over them and their salaries and so on. However, those of us who were then young graduate students, we had a remarkably good friend in the Finnish Erik Palmén. He came very often to Stockholm and spent a few weeks and then went back to Finland again. We always found a very good ally in him, and we explained to him that we had worked so long, we had written this dissertation, we had taken all these courses and it was about time that we got our degree. And he would then during his daily meal with Rossby--it was mostly dinner at one of the finer hotels in the city--and he would say, after a couple of drinks, to Carl-Gustav: "I think that Bo Döös really ought to get his degree now. You know, he is a man of the world, he has a family, he has worked for you hard and long. I think you should grant him a licentiate."

"Oh, I do not yet think so."

"Yes, as a matter of fact, if you do not grant him a licentiate, I am not coming back." So they had their little game going. I understand actually that Palmén played some of the same role in Chicago earlier, together with some others, but I found Erik Palmén a good ally and he said to Carl-Gustav, "Yes, I think that Aksel is about ready."

Washington: When was this, Aksel?

Wiin-Nielsen: This was in 1957. I had been there for two years. After all, I had three years of study under Fjørtoft, then two years in Stockholm. You might think that five years of graduate study would be enough to qualify for even an expanded master's degree. Then, you know, Swedish professors at that time were total dictators. They told you the day where the exam was going to be, you were the only one taking it, there were no people, you know, extra, who make the kind of quality control on these exams and Rossby had always conducted these exams in such a way that he would give three or four written problems and you had, let's say, five hours to do these problems and you handed them in and if that went well, then the next day, he would give you an oral examination that could be quite perfunctory if you'd been doing well on the written exam. If you were kind of shaky on the written, he would dig in further.

So I came there on a Friday, and I knocked on his door and he said, "Ah. Good morning. Yes, yes, yes, yes I remember why you're here. I have thought a lot about it and I have just changed the whole examination." There I had prepared myself solving problems and went through all the stuff and he said, "No, no. We are going to do it differently. I'm going to give you three problems and you

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will have till Monday morning to write whatever you want about these three problems and you can use the library, you can ask anybody at the Institute for assistance, the only thing is you cannot hire a consultant to write these." And I can remember, I think, what the three problems were. One was the question, very short--"Why is there a tropopause?" Problem two was much broader and said something like: "Give an account of the modern aspects, or the modern understanding of the hydrological cycle." The third one was: "Give a ten-year plan for the development of numerical weather prediction in the world."

Kasahara: No wonder he asked you only one--??

Wiin-Nielsen: It took the whole of Friday and I called Bente and said "I am going to work over the weekend because he has given me that much time. I really want to produce something." We had a very good friend living with us, a man about my age. He actually was a forester and I hired him when I came home. I said, "Carl, you can type. And I'm going to write and you will type and we will use Saturday and Sunday." So we worked like devils. Monday morning I went in with three packages of papers with answers to these questions. The tropopause paper was remarkably short. The hydrological cycle you could expand on, but the major one was on numerical weather prediction. There sat Rossby at his desk and he said, "Ah, yes, yes, yes, yes, yes, I wondered what happened to you. You must have worked at home, because I was in here yesterday looking for you." And I said, "No, I worked at home, I couldn't go in here."

"Do you have something?"

"Yes." And I handed it over to him. And he said, "That is really quite a lot." And he opened his left drawer, put the whole thing in there and closed it. Then he said, "I'll contact you and you better go home and get some rest today. You look pretty tired." And I was.

Then, three days later, he called me in. I thought he was going to say something about my licentiate. No. He was going to say that he had written rather a big article about the development of meteorology. It was actually written in Swedish and was going to be published in Sweden and would I please take care of the selection--

End of Tape 1, Side 1

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Interview with Aksel Wiin-Nielsen
Tape 1, Side 2

Wiin-Nielsen: As I said, he asked me to take care of selecting pictures and have them drawn for this article. I think you all know the article because it was the one which was later translated and appears as the first article in what is called the **Rossby Memorial Volume**. So that was what happened in the morning. In the afternoon, Bo Döös came to my office and said they had just called for an ambulance. Rossby had been in a meeting with the chemists, had collapsed and was taken to the hospital, but when he arrived there, he had already died. So I can't say that my licentiate is from Carl-Gustav Rossby, but I can say that my examination for the licentiate was certainly given by him. That of course created a very sad situation for the whole institute. He was certainly identical with the existence of the International Institute of Meteorology. It so happened that Bert Bolin was visiting in Woods Hole at the time. It was September, I believe, and we had to get lots of things arranged and of course came the funeral and all that. Bert came back from Woods Hole and was appointed immediately as acting professor and then life kind of started again, although missing the leadership which we had had before. I remember going to Bert, who was now acting professor and said, "Bert, you know I really took these exams and I think there is evidence to show for it."

He said, "Oh, did you? I was away."

"Yes, I know you were away, but the whole thing was one written exam and you will find it in the upper left drawer of Rossby's desk in there." So he went in and looked, and there it was and he took it out and said, "I'll look at it tomorrow and give you the results."

He is that kind of man who concentrates on one thing at a time and the next morning he came and said, "No doubt. Of course you have passed this. Where is your little book?"

This book is your student book that the professor is then supposed to write in, on a given date, "I have examined Mr. So and So and So in such and such a way." And then sign the whole thing and you will have your degree. Bert did just that the day after. Then you take it down to the administrative part of the university and they see to it that you get your diploma the next time there is a graduation ceremony.

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Then, of course after that, we had to find a way to continue our work. The International Meteorological Institute was of course running, essentially, on contracts. There was a small amount of money coming from the Swedish government, enough, I suppose to pay the professor's salary and I think his administrative assistant. But the rest of us were really paid out of contracts, and all of these contracts, I think, with the exception of those in atmospheric chemistry, came from this country. I worked at the time on a contract which was awarded by the then U.S. Weather Bureau. The idea was that we would--as a byproduct of numerical prediction and as a byproduct of objective analysis--they were interested in calculation of trajectories. This was really a problem proposed by Rossby because, as you know, in the last few years, he became tremendously interested in chemistry. He had a vast network created in Sweden; some sparse networks were created in the neighboring countries, but he was already at that time very much aware that the atmospheric chemistry, or for that matter, the surface chemistry on a given place depends upon the backwards trajectory of the air. So I worked together on that project with Dusan Djuric, who now for many years has been a professor at the University of Texas--

Washington: Texas A & M.

Wiin-Nielsen: Texas A & M. But we worked out a scheme together where we could make the Lagrangian trajectories and that resulted in the first paper I have ever written in meteorology. As a matter of fact, it was published in 1956, in Germany, in connection with a conference on numerical weather prediction, which was arranged by the German group. So that led to my first paper. Of course, I had my salary as an assistant professor, but I worked in the Swedish Meteorological and Hydrological Office as a teacher in training young meteorologists--those who do not have an academic degree, but are like Air Force forecasters--they existed in the old days in this country. It was, however, I think, quite obvious that it was very difficult to keep the institute together, having lost the leader which we had. A new mode had to be found. Bert, of course, had worked very much in dynamic meteorology. He had been one of the pioneers in both barotropic and baroclinic forecasting. But strangely enough, after he attained his doctoral degree, he switched completely. I don't think he has written a paper on dynamics since those days and changed entirely over into an interest in, first, chemistry, [and] later on, you might call it "atmospheric biology." That, of course, in turn was replaced to a certain extent, or coupled with, the CO_2 question, and the publications we have from Bert Bolin's hands during the later years are almost entirely in what you might call environmental matters closely connected to the atmosphere. He went that way.

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Kasahara: Do you think Rossby had suggested to Bolin that he undertake chemistry studies?

Wiin-Nielsen: That I don't know, Akira, but I do know that Rossby from time to time, although he was really quite formal, during an afternoon cup of coffee he would jokingly tell us something about his life. He never wrote any autobiography, but he told episodes. And I remember at one time when Bert also was present that he said, "The essential thing in any science is to find a green field where nobody has been before. Then you can make science." And I think Rossby felt already at that time that the field which we now call "dynamic meteorology" and "numerical weather prediction"--there were enough people around the world who were interested in that. Therefore, he himself shifted over and it might very well be that, although this is speculation on my part, Bert Bolin already at that time had become disenchanted with the essentially very hard and frustrating work it was to get the balance equation to convert with hyperbolic regions and all that stuff. He probably, as compared to me, Bert is much more of a naturalist. He is much more interested in what goes on in the real world and therefore I think he changed his interest.

So--Bo Döös was about to come back from Florida and then we got a visitor, as we did every year, Dr. Phillip Thompson. He was a very important man in our lives at that time because he was the man who was supervisor of the Air Force contract which we had. It was the habit, both of the Navy in this country and the Air Force in this country, that the supervisor would come about once a year. He had been giving himself rather a good time staying there, maybe a week, and really finding out what we were doing.

I was in charge of the dynamics and the numerical prediction problem at the time because Bert had wandered off to atmospheric chemistry, Bo Döös was at Florida State, Pierre Welander was off on oceanography, as he always was, Eriksen took care of the atmospheric chemistry lab together with his co-workers, and then in came Dr. Thompson and said to me, "I know that as an Air Force officer I am due for an overseas assignment as all Air Force officers are periodically, very soon. I have found out that it would be possible for me to be spend a year in Stockholm provided this can be made as an exchange." Well, at that time, Phil had just moved from the Air Force Cambridge Research Labs down to the newly-formed Joint Numerical Weather Prediction Unit where he was the chief of the research section.

"Would you be interested?"

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I had absolutely no doubt that I was 100% interested in that kind of an exchange. There was a question, again, of convincing Bente that this was the right thing to do. At that time, there were not only one, but two girls born, and the third one on the way. But of course, this was also sometime into the future. We had time to think about it, but, the long and the short, I promised: "For one year, Bente, you can take it."

She said, "If you promise me it's only going to be one year, then it's O.K."

Phil then went to Washington and worked there for quite some time and then he came to Stockholm: that must have been 1958. He was going to stay there for a year. I remember I left Sweden the day before Christmas, 1958, on the freighter *Mathilda Thordén of Gothenburg* hoping to make New York in eight days time. We actually left on the 23rd of December and arrived in New York on the 7th of January. It was a very long trip across the North Atlantic.

Returning to Stockholm for a moment, you know that was the time when Phil wrote his book, **Introduction to Numerical Weather Prediction.** That came out of that visit. I came to Suitland, not without friends because it had been the habit of the Air Weather Service to always have two officers in Stockholm for training and further education. There had been quite a number of these, I cannot repeat all of them because they were before my time, but I know that Art Bedient had been there early in the fifties. I know that Best, who later became the commanding general of the Air Weather Service, had also been there. In my time, there was Lloyd Vanderman, Scott Williams and Ed Jess who all took my course in dynamic meteorology, but before I arrived in Stockholm, some of them had returned and I think we first contacted the Vandermans and they in turn saw to it that we could stay temporarily at Andrews Air Force Base, which we did until we found a place to live. Scott Williams later came to the Washington area and so did Ed Jess, although he didn't work in the Joint Numerical Weather Prediction Unit but worked for awhile at Andrews Air Force Base.

I think later--well, I still had another student, Kindle, who went and worked for the Air Weather Service Headquarters at Scott Air Force base outside of St. Louis. I just met his son the other day, who is now also a Ph.D. in fluid dynamics and oceanography, but these people were a very great help because they actually created continuity. So that was my arrival in the United States.

Kasahara: So from the beginning you joined JNWP?

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Wiin-Nielsen: That's right. I came directly to JNWP.

Kasahara: The Air Force----?

Wiin-Nielsen: Yes, my salary came from the U.S. Air Force. You remember the JNWP was jointly sponsored by the U.S. Weather Bureau, the Air Weather Service, and the Navy. Each carried a certain number of persons on the staff. There was a mixture of officers and civilians, but my salary certainly came from the Air Weather Service. But I was all the time located in Suitland. At that time, George Cressman was chief of the project. Shuman had taken over as chief of the development section in which I worked, and there was an operational section which was mainly run by Art Bedient, when he was in uniform at that time and then there was a Navy officer called Ed Carlstedt, and he was particularly interested in creating output from the machine so it could nicely be sent further on. The arrangement with the JNWP at the time was that we had the third floor of Federal Office Building #4 with the development section at one end. Then a section where the computer was--at that time, a 701, to be replaced with a 704 shortly thereafter; and then, further down the hall, was actually a completely old-fashioned operating weather service where they drew maps every day and where Harlan Saylor and Ed Burnett were in charge of the daily forecasting. Fortunately these two bosses were rather sympathetic to this new field of numerical weather prediction and, on the other hand, some of the bench forecasters who were working for them, very clearly saw numerical weather prediction as a tremendous threat to their own existence and there were many discussions about the man/machine topic and so on.

Now the models which were being run at the JNWP at the time--the barotropic was the workhorse, done every day, out to 72 hours. At that time, already, and this is early 1958, one had become totally disenchanted with the two-level baroclinic model. It was quite obvious that the model was too lively, as compared to the barotropic. It produced, in the Charney-Phillips version, fields with much too low pressure areas and since of course it's really symmetrical with respect to lows and highs, the lows were too low and the highs were too high, which gives much too strong winds between them. Cressman, who was a very practical man, said, "Well, we can't use that. So let's decouple it." And then it was de-coupled in such a way that the forecast at 500 millibars was still carried out with the barotropic model, but what was normally known as the thermal vorticity equation was used to predict the thickness field between 850 and 500 millibars using the already-produced flow field at 500 millibars as the field in which things were affected. Now that didn't destroy the goodness of the 500 millibar forecast where the barotropic was obviously better than the

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baroclinic, but it did at the same time produce a forecast of a temperature field, and a height field at 850 millibars and some extrapolation down to the surface and these were the two models which existed at the time.

This was the era when Paul Wolff had been at JNWP. He had left before I came, but his contribution to numerical weather prediction was the simple one that since we can't forecast the motion of the very long waves--wave # 1, 2, and 3--with any accuracy in a pure barotropic model without topography and so on, the best thing to do is to analyze from the initial field the amplitude and phase of waves #1, 2, and 3 at the various latitudes, run the forecast as it normally was run, take away #1, 2 and 3 and put those we got initially in. So this was a model in which everything was flowing except waves #1, 2, and 3 which were kept constant. And as you very well know, if you don't do anything other than a pure barotropic model, then of course these wave numbers will back up towards the west, following essentially the Rossby speed which, of course, is totally unrealistic at those scales. Now that was a solution which was not much appreciated, except of course by Paul Wolff and perhaps some of the real practical people, and that I can understand. If you have an error in what you are doing, and if you have a simple device where you temporarily can remove it and still make a reasonable forecast, that's O.K. But of course it's never the solution. So at that time Norman Phillips was brought in as a consultant, and he pointed out to George Cressman that there is of course an effect which can be incorporated in the barotropic model, and which comes from the fact that if you consider the barotropic model as an equivalent barotropic model and integrate through the atmosphere, then there is a contribution from the boundary condition below, which actually is a pressure tendency which within the equivalent barotropic model then can be bent back into a term in the vorticity equation which gives a Helmholtz equation instead of a Poisson equation. If you analyze that model in the simple way of looking at what speed waves would move at 500 millibar, then of course you find a tremendous reduction in the westward propagation of these waves. There is an empirical co-efficient in front of that tendency term which Cressman proceeded to determine empirically. He seemed to have made a number of forecasts with various values of q^2 and then he fitted things together and he found what q^2 should be, and he got to the magic number of four.

Washington: Aksel, at that time when you must have thought about the possibility of using primitive equations, was the reason that the primitive equations weren't being explored at that time for operational work, was it the amount of time it took to solve on the computer?

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Wiin-Nielsen: Yes. I think that was the main consideration, that we were still in the 704 era, and we were still waiting for this fantastic machine, which was called the 7094. Of course, setting this in the somewhat broader context, as you recall, Jule Charney had actually shown that the barotropic equations could be integrated in their primitive form. And there was a paper on that in **Tellus**, I believe, in 1955. So the idea was certainly planted there. However, Cressman was totally of the opinion that that was not yet ripe for further development, and therefore he insisted on the models which I mentioned before to be replaced by his own three-level quasi-balanced model. I think from an operational point of view, this was a very good thing because it actually put a real baroclinic model into operation. One had learned enough about the balance equation in the initialization of quasi-balanced models that one could remove all these errors which came from a strict application of the geostrophic wind law and so on. And furthermore, it gave both forecasts at 300 millibar and 500 millibar and at the surface, and it did not have the full symmetry which of course exists in the two-level model. So I think that was a good stopgap model to use. It could run on the computers available at the time and then I remember that he said, "But of course it is not forbidden to think about the primitive equations." There is a photograph taken some Saturday at JNWP where there is Shuman and I up at the blackboard discussing the primitive equations.

Now I think that the fear of the gravity wave had been strongly implanted in both Cressman and Shuman. Of course, that fear of the gravity wave is quite understandable. If you read some of the early papers where gravity waves were explained as the "devil's action," you know, they are not in the atmosphere in the way in which we find them in the model. In the atmosphere they are of tiny amplitudes, and therefore it does no harm to remove them totally. That one did a lot of harm not on the gravity waves but while they are in the forecast by the constraints was perhaps not realized. But Shuman was of the opinion that it was probably totally impossible to make a realistic integration of the primitive equations unless one were going to make extensive use of his averaging technique. I was, on the other hand, very much opposed to this, partly because I couldn't understand his notation and partly because it would seem to me that if you averaged as severely as he was doing, then you would in any way only have very smooth large-scale fields left over. So I had my difficulties with the u bar xx, yy's, etc., etc., as he expressed it in his technique.

Anyway, I did not stay around at JNWP until the primitive equations were actually put into operation. The reason for that is again, Phillip Duncan Thompson, and perhaps also a fellow called Joe Smagorinsky.

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Kasahara: In fact, in 1959, I think I first met you, but I remember you were working in Joe Smagorinsky's operation at General Circulation Research Laboratory. What happened then?

Wiin-Nielsen: Well, it is about the first time in my life where I was asked to be the go-between. But we were on the third floor and Joe and his expanding group of Miyakoda, Manabe, Bryan, Douglas Lilly and so on were down below. I think it is quite well-known in the meteorology circles that Joe also was one of the pioneers in numerical weather prediction, having spent time at Princeton. There are pictures which exhibited that he certainly was there and worked with Charney and his other collaborators. It is also in evidence that both Cressman and Shuman were at Princeton at a later stage and learned something about modeling. Now I think that Shuman and Cressman must be characterized as the conservatives in the area of numerical weather prediction at the time. They were rather conservative in respect to development of models, they stuck to the proven. Joe, on the other hand, was going to integrate the primitive equations. He was going to follow up on Phillips' early numerical simulation of the general circulation, but he was going to do it with the primitive equation, he was going to have his own computer, and he was going to put physics into things. So I used to listen to these exchanges between Fred Shuman and Joe Smagorinsky. They were not friendly, they were really after each other. Cressman was perhaps more withdrawn, a little bit more of a gentleman than the two others, I don't hesitate to say, but nevertheless Aksel Wiin-Nielsen was used to be sent as the diplomat down to Joe and from Joe up to Fred Shuman so we could negotiate certain things. So I had very good relations with Joe and his people, being perhaps also myself a little bit "anti-Shuman." That put me immediately into their camp. Some very good relations existed at the time, and if it had not been for the creation of NCAR, I might very well have gone into Joe Smagorinsky's group. I was certainly very much attracted to do that; there were some very fine people in that group. I had a good relationship with Joe himself. But of course then came the "Blue Book" describing the program for the creation of this new national center for atmospheric research and when I considered that, I think the general aspects of concentrating on basic research at NCAR was what got me to go to NCAR instead of going down to Joe Smagorinsky.

Kasahara: How did you get that information? About NCAR?

Wiin-Nielsen: From the "Blue Book."

Kasahara: Who gave it to you?

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Wiin-Nielsen: Well, Phil Thompson gave it to me. It was around, I can't recall, but I think that's dated perhaps 1958 or something like that. And of course then things started rolling pretty fast. I knew after awhile that it was going to be in a city called Boulder, Colorado. I knew from my correspondence with Phil Thompson that he was going to be the director of what was called the Laboratory for Atmospheric Science. I knew it was going to have facilities for the benefit of the universities. I knew it was going to go into atmospheric chemistry. I knew that there was a fellow called Walter Orr Roberts who was going to be the Director, but I had never met him. Phil was still in Stockholm at that time. I remember those letters I received from him were marked "PDT/mj" and I always wondered who the heck is "MJ?" I knew the secretaries in Stockholm, who is M.J.? I asked once and he said, "Oh, this is an English girl I picked up called Margaret Johnstone, and she's working as a secretary for me."

I think it developed to be a fight between Philip Thompson and Joe Smagorinsky to get hold of me, and eventually the offer from NCAR came and the offer from Joe I had, and then I had to make this hard decision where to go. We were not entirely enchanted with the Washington atmospheric climate, either. Washington can be very hot during the summer, so we were happy to get out here. We came to NCAR, I suppose, in the summer of 1961.

I think before I start to tell you about my early experiences at NCAR...it might be useful for this interview to backtrack a little bit because I have pretty much stuck to my own career, but of course such a career--it happens to all of us--is influenced by a whole number of people. If I go back to my days at the University of Copenhagen, they were really very exciting years.

As you see, Denmark was occupied by the Germans, in World War II, already in 1940. At which time, I was still attending junior high. So all of my high school years were war years. As you undoubtedly know, these are situations where normal life is restricted very much. We had actually some run-ins with the German military forces while I went to high school because I used to travel in with a little old train to a nearby city where I went to high school. Every morning we had to show identification cards to pass through the German control at the railroad station, and we used to have our little fun by actually showing them the timetable for the week instead of the identification card. But they didn't pay much attention to it. I had a teacher in German, we had to have German, English and French in high school, we couldn't get around that. While he was a great admirer of German literature, Goethe and all the big authors, he was no lover of the German/Nazi system. We used to have the German troops

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march by the school always singing at that time because it was the early years of the war: *"Dann wir fahren, dann wir fahren gegen Engeland"*---"Ready for the invasion of England", which of course never took place. And when they marched by, we found it was quite fun to shut the windows with a big bang, just to indicate that we were not their kind. One day, in the middle of a German lesson, shortly after we had banged the windows, there was a very military knock on the door to the classroom and in steps a German captain, complaining about this demonstration of closing the windows. Well, he just went to the wrong guy because our teacher in German had spent many years in Germany in the twenties and thirties; he was absolutely fluent in German, in all the dialects and so on, and he started to speak to this officer, that he was here to teach these young people the German language, and all the culture of Germany before the Nazi machine, and if he was going to interfere with his education by marching his soldiers by, we were entitled to shut the windows because he was going to teach us about German culture. I've never seen a military officer back off. *"Jawohl, sir, jawohl."* And off he went, and we never had any difficulties with him thereafter.

I graduated from high school in 1944, and started at the University of Copenhagen in September of the same year. I think we studied in a normal fashion for only about three weeks. Then there was the occupation of the Niels Bohr Institute, where we actually were educated, where we took our math and physics and so on, and shortly thereafter, I think really the day after, the students of the university decided they were closing down the university. They would not continue with constant interference from the German forces in what went on, and of course the Germans occupied many of these institutes. So actually formal teaching vanished after three weeks. We'd only been at the university that short a time.

But we had some very good teachers and they agreed that they would continue the education underground. So we found rooms in public libraries, in shoe factories, in the back room of some of the unions which had meeting rooms, and we changed this every other week so they weren't able to find out and then we just came there and teachers came too and then they continued the normal course, but just in different surroundings that we shouldn't lose too much time. But of course what also happened shortly thereafter was that teachers started to disappear. I was not even aware at the time that Niels Bohr, who was our professor in theoretical physics, and his brother, Harald Bohr, who was equally famous in pure mathematics, that they were Jewish. We never thought about it, but there came a day in 1943 where the Germans had decided that they were now going to round up the Danish Jews and take them off to a concentration

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camp in Germany. Fortunately, there was in the German embassy, a man who warned the underground movement that this was going to happen and not all, but I think as many as 90-95% of the Danish Jews managed to escape to Sweden. If you know your geography, then, Denmark and Sweden are separated by water, but it is not terribly wide. Let's say a ferryboat ride today goes at the most narrow spot in twenty minutes and the widest spot in maybe an hour and a half. Now of course these people did not take valuables. They were sailed over in rowboats, fishing boats and so on, and it is also a mistake to think that these fishermen who had the boats were just national heroes. No, on the contrary, they took a very good amount of money to agree to sail the people across because it was dangerous. In Gilleleje, where I live at the moment (as a little aside) there are absolutely marvelous houses built during the 1940s by these fishermen for their own domestic use, but paid for by the Jewish people. It was remarkably open; I lived at that time not very far from the shore. You could see every afternoon when we came home that the Jewish people were on the trains going up to the shore to be sailed across. So we lost some of our Jewish teachers that were able to get across--we didn't know they were Jewish. They were, however, replaced by younger people who were quite willing to earn some money to continue the teaching which I described.

Also, after the war, it took some time before things were well-organized. I suppose that in my year all those entering these studies of math and physics in 1944 were really supposed to take our first university exam two years later. We deliberately all delayed it by half a year because we felt we had had such poor conditions. I also remember that we formed a--

END OF TAPE 1, SIDE 2

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Interview with Aksel Wiin-Nielsen

TAPE 2, SIDE 1

Wiin-Nielsen: We felt that we had a very good case to tell our professors that they ought to be rather lenient with respect to the stuff we were going to be examined in, because we had such poor conditions under which to study. I think, in retrospect and with hindsight, they probably did the right thing by saying, "No, you will be examined in exactly the same way as if it had been peacetime. You are not going to suffer in your later life from having a soft exam, so we are going to treat you exactly the same way as we treat everybody else."

I remember that first exam. We were 55 starting and there were nine people who got through. The rest of them flunked. Many of those did rather well half a year later, but there were nine of us who got through and I think, to a very large extent, this is a very significant nine because, apart from one very good friend who died very early of leukemia, the remaining eight are all professors in one field or another. I think five of them are professors in some kind of physics in Denmark. I escaped into meteorology so that makes six, and two are mathematics professors. So it was a good year, and afterwards I think it was all right.

Having taken the first exam, I also qualified to get a state stipend on which to study and in my particular case, it came out in the following way: they offered me a room at a so-called college. It was actually built by King Christian IV around the year 1600, actually 1622, and it was a very, very old building. But I moved in there; there were 110 students distributed over all the faculties and we each got one room. Then we got 150 Kroner, which in today's exchange rate and probably at that time, too, would amount to about, let's say, $30. That was the stipend for eating and on $30, you could live happily in those days. We were supposed to be the cream of the crop of the students and therefore we were treated in a very special way. You just had to put your shoes outside the door. In the morning you could take them in, some guy had polished them. There were people who came and cleaned our rooms and made the bed and so on every day, with the idea, I suppose, that we were supposed to be the gentlemen of the society as we grew older. We were also governed in this college by--I don't know what you would call him these days, but he had a clerical title (provost). This was a professor, and he turned out to be my own professor in mathematics, Harald Bohr, who was the superintendent of the college. He lived there, and had an apartment and took very good care of us. He was a very liberal man; we were not supposed to have anything to do with the other sex

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except on occasion when we could invite girls in and there would be a dinner dance or something like that. But Harald Bohr came very often down to a courtyard between four buildings, and he would come down on a summer evening with a bottle of something under one arm and a box of cigars on the other, and ask if there was anybody interested in playing poker. At which time he would then proceed to clean all of us there; he was a tremendously good poker player, and I remember asking where he had learned that.

"Oh," he said. "I have a friend in America called John von Neumann and he has developed a theory of games, slightly simpler games than poker, but it is applicable also to poker." I think I [had] a relationship with him which was much closer than other students of mathematics simply because he was a man I saw just about every day. I remember also when I finally had to take my major in mathematics, he was one of the examiners of the university and the evening before I had to take this exam, my habit has always been that I finish preparations the day before and then I have about half a day to relax completely. It's no good to study to the very last moment. So that evening there was a knock on my door. In comes Harald Bohr and says, "I just wanted to check that you were in good shape. I know what is going to go on tomorrow. I want you to know that there is no reason whatsoever to be nervous about this examination because I could actually take a man on the street, take him in there and I could examine him to a good 'B' without any difficulty. If I can do that, you are prepared, so it will go all right. " Sure enough, it did.

There were some other nastier professors who also examined me at that particular occasion. You have two examiners, and one that was "censor". The fields were number theory, analytical geometry and theory of functions. The other two, without mentioning their names, were probably the two most pedantic people I have ever met in my life, but I suppose mathematicians have a tendency to be that, because they always have to operate with their proofs, with all the pedantic epsilons and deltas and so on.

Of course, after the war the Bohr Institute was an exciting place to be because Niels Bohr came back from this country where he had participated in the Manhattan Project where he had spent a good bit of time at Los Alamos as a general consultant to the people working there, particularly Oppenheimer, but where he had also, I think, already at that time been very much concerned with what would actually happen after the war when things were--what with the bombs having eventually been thrown, but what was then going to happen to nuclear energy. Was it going to be something which was good for humanity? Or was it going to be essentially something which gave a very powerful war

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machine, and all these things continued to concern him a very great deal after the war. He wrote and spoke often about it also at the Institute, and eventually he wrote a very long open letter to the United Nations. The main point was that peaceful existence is really only possible if we make the atomic world an entirely open one. He was very much opposed to that the United States, the U.K. and U.S.S.R. were going to sit on all the secrets of atomic energy. He felt that a peaceful use of atomic energy, spreading this across the whole of the world, would be better when everybody knew than when only a few of them knew. And we know of course also with the advantage of hindsight, that all the spying that went on in those days would not have happened if we had been more open about the whole matter. As you also know, his open letter to the United Nations in 1951 was a total flop. I think it was politely read by a few people, but at the same time, of course, what was happening was the conflict in Korea and that took the whole of the attention of the United Nations at the time, leading to the military forces coming to Korea. We know, of course, that they were to a very large fraction, American forces, but formally speaking, it was a United Nations force with at least some contribution from Australia, New Zealand, U.K., and other places, but with the United States carrying the major part of the burden. I think Bohr got totally disillusioned at that time. I know that he spoke often about that he had talked to Roosevelt, he had talked to Churchill, he had talked to de Gaulle during the war, and I think Churchill went as far as saying, "I think we better put that man in prison because he is really over on the Communistic side." That had something to do with Bohr's way of talking. He talked in a very low voice. He built up his argumentation very, very slowly. And therefore, it took forever before he came to the point.

Now you can well imagine that a Winston Churchill who, at that time, was in the middle of the war effort, probably had to make a hundred decisions a day. To have him sit there quietly smoking his cigar for thirty minutes while Bohr was getting to the first part of the argument, he didn't have the patience for that. So I think he essentially threw Bohr out of the office. Bohr did slightly better with Roosevelt because Roosevelt had a way of opening him up and in addition to that of course Bohr had some of the Jewish community in and around New York and Washington, I think particularly a Supreme Court judge called Felix Frankfurter helped Bohr a very great deal in approaching Roosevelt. I think Roosevelt was quite positive to these ideas. The relationship between Niels Bohr and the U.S.S.R. came down to that a number of scientists from the U.S.S.R. had studied in Copenhagen in the thirties, including such a man as George Gamow, whom I met later here in Boulder. He had become a professor of physics at the University of Colorado and lived up in the hills here. I used to see him once in a while mainly because George Gamow wanted to practice the

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Danish he had learned the few years he was in Copenhagen, on me. And they were really delightful people as long as George Gamow was reasonably sober, which he wasn't very often, I might admit.

Washington: I must add right there, though, that George published a hundred books in his lifetime, so he must have been sober a few times.

Wiin-Nielsen: I think that during his earlier years this was indeed true, and that by the way also led to a little bit of a conflict between Bohr and Gamow. You see one of the books which George Gamow published was this series of "Mr. Tompkins" books. I think I recall the first one was called **Mr. Tompkins in Wonderland.** And the play which Gamow introduced there was that he wanted to illustrate the then modern physics. And remember now these are relatively early days in quantum theory, they are relatively early days in general relativity. Therefore, George Gamow illustrated this with Mr. Tompkins, who had dreams. And he dreamt for example one night that--he was actually a bank clerk, but banking didn't interest him very much, so he speculated about the world and therefore these dreams. So he dreamt one night that the speed of light was ten meters per second, or 10^3 cm./second so that set down by a factor of 10^7. Well, of course since the speed of light is the maximum speed attainable in any medium, according to the general relativity theory, then of course nothing in his world, where the speed of light is now ten meters per second, can move faster than that. And then of course he was a good craftsman, George Gamow was. He drafted all these figures which showed how the world would look, under these conditions.

Now in another chapter, the acceleration of gravity was reduced to some ridiculously small number, which of course meant you could jump over tall buildings and things like that. Now I read these books when I was maybe fifteen, sixteen years old and I found them tremendously imaginative and interesting. Bohr pointed a finger at Gamow and said, "George, you can't do things like that because you are not free to change one universal constant and keep another one as it is because these, my dear friend, are interconnected." And he went on and on.

Actually, I think their friendship was a little bit destroyed due to that, although I think both of them always admired the scientific work of the other. Now Gamow's contributions of course became much more in astrophysics, the sun and the stars, while Bohr stayed very much to the quantum mechanics and so on. But there are, I read up on it two years ago...there were celebrations of Bohr's one hundred year anniversary, and they brought back those who were

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still alive and there were books written and so on and Gamow was as much an artist as he was a scientist. Because there are marvelous poems that he has written about events in physics and just for fun. I know that he and Bohr and others, just for relaxation in the evening, they liked to go and see Western movies. There are two stories about that. One was, Bohr's rather satirical way of treating Westerns, because you remember the classical situation in the Western movie--the beautiful girl has just been caught by the bandits and they tie her to the railroad and the train is coming and she is going to die. As Bohr said, "I can understand that maybe they treat the girl like that. I can also understand the danger in which she is. But what I cannot understand is that there also was a photographer present in that situation who took the whole thing so I can now see it?"

And the other story is the question which of course must interest anybody like myself who has seen many Western movies because they are good relaxation. They are really like the adventures of my childhood. You have no--I read Hans Andersen at that time, or they were read to me. You never had any doubt [who] were the good and bad, and this is true in a Western as well. And then the question came up, how come that when they meet at high noon in the deserted street and they walk against each other and you have the good man in white, and the bad man is in black, and it is always the bad man who draws first. But it is also always the bad man who dies. How come? What is the theory behind this?

And Bohr developed a theory for this. Because, he says, the reason that the good man always wins is that the bad man is the one who has to make the decision. He actually decides, "Now I pull the gun, now I shoot." Now that's a conscious decision and therefore you see his facial expression, his body movements, he's now about to make a decision that he will shoot. Now you the good guy have only one thing to do, you want to live, you don't think about anything, you put it up and you shoot immediately. And therefore you are faster. Now that theory was challenged. They said, "No, no, no, that's just some nonsense." And Bohr was serious whenever he made theories so he went down to the toy store and he equipped everybody with a water pistol. For the next month, they had games going with each other, these guys, and I think Bohr showed each time that the theory was right. If somebody drew on him, he always could wet the other one.

And this of course, I think, shows quite obviously how the scientists at that time, and hopefully of today too, really consider the whole thing as a kind of play. They were not **really** all that serious. I mean when you read about them afterwards by their biographers, you think they were deep in thought all the

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time. They were not. They were playing just as much as we have been playing, at NCAR and at other places.

The Niels Bohr Institute, I think that was an exciting environment in which to study because you were surrounded by these Americans, Germans and Russians and other people who actually were just visiting staff members at the Institute, but you met them every day in the cafeteria and you listened to their discussions and you saw that although they might be very clever and something to look up to, they were also very human in their ways. I think they spent more time playing ping-pong than solving physical problems because in the library there was a rather large table--it was actually super-size as compared to what a ping-pong table really should be, but perhaps somewhat more narrow. It was a very good place to play ping-pong because people really smash, you had a long distance in which to do it. I played ping-pong in Denmark with the president of the Danish Academy because we are both from the same era, we both played in the library of the Institute of Theoretical Physics.

Kasahara: After you came to Boulder in 1961, in 1963 when we came we moved to Cockerell Hall and there was a ping-pong table there.

Wiin-Nielsen: Deliberately, we made sure that the ping-pong table stayed in Cockerell Hall, and I think that--either it was the last summer I was here or it was the first summer in 1964 when I returned for the summer period--I think I was in Cockerell Hall both these years. I used to play a lot of ping-pong. I played with you, Warren, and there were a number of other people who were quite good---we had Erik Eliasen from Copenhagen here as a visitor, he played occasionally. Oh yes, Fedor Mesinger, who was here as a visitor from Yugoslavia. We had difficulties beating him. He was quite good. But I have been a little bit dismayed to see that there isn't an obvious ping-pong table at NCAR today.

Kasahara: Well, a new director might change that.

Washington: Where could we put a ping-pong table?

Wiin-Nielsen: Well, I think down in the hall there in front of the big painting by Shapiro, couldn't we put it there?

Washington: I'm not sure I have any control over that.

Wiin-Nielsen: Well, maybe not. Anyway, I--of course then I had Fjørtoft as my professor and he was also a very original character who really didn't take very much very

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seriously. He was serious about his lectures. Now I think he might come close to the worst lecturer I have ever experienced. He is not very good in explaining what he wants, and he, [these were the] old days, he worked at the blackboard with chalk, and he always had the chalk in his right hand, and the eraser in his left. He thought the students really could see what was there; [when it] was already erased and replaced by something else.

But anyway, just because he had such great difficulties in explaining this, the more we actually studied. And I think that during the two years he didn't give a lecture which he didn't necessarily have to give. But we got through the the classical Norwegian stuff--Solberg, Bergeron, the Bjerknes and Fjørtoft's own highly original treatment, I think, of the baroclinic and barotropic instability problems which he really does without looking too much at the equations, but rather looking at isolines and their expansion and shrinking and so on. So that I think he did rather well.

Washington: He gave a series of lectures at Cockerell Hall in 1964, I believe, in '64?

Kasahara: Yes.

Washington: I think not many followed his lecture.

Kasahara: That one was highly--[interruption]

Wiin-Nielsen: Eliasen and Hans Buch and I who were those who participated regularly in his lectures, three students, helped each other and I think that most of the time we actually saw he was quite profound in what he was saying when we could understand it, and once in a while, we could also point out that he had been in the wrong, be that as it may. And I think that that experience of being a professor for those three years also indicated to Fjørtoft that that was perhaps not the way in which he should spend his life. And therefore the search for another position which he found as director of the Norwegian Weather Service--I think by the way at the same time very little has appeared from Fjørtoft's hand since then. He stopped rather early.

Tribbia: Can I take an aside here also? It seems to me in what we've discussed today that your career seemed to take a drastic change after your year in the Baltic on an island, and from that point on, it just sort of--everything followed, without too much thought as to what was going to happen next. Is that correct up to this point in your career?

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Wiin-Nielsen: Yes. I think this is a totally correct observation. I don't hesitate to say that the year in the Baltic was most traumatic. My nervous system was as bad as it has ever been, and towards the springtime, [it] was the only time in my life that I had a session with a psychiatrist. But I went to one psychiatrist and have never seen one ever since. But he gave me some very good advice. He said, "My young man, you have arrived on the wrong shelf. You better get out of here. You have been too much influenced by your father and your family and it's been a foregone conclusion that you should be a teacher. I'm not saying you cannot be a teacher but apparently you are very unhappy, there being one solution, you better get out if possible." I think it took fifteen minutes and I paid him a tremendous amount of money. But I got some very good advice.

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Tribbia: This is the second session with Aksel Wiin-Nielsen. The date is June 30. We are at the National Center for Atmospheric Research in Boulder, Colorado. My name is Joseph Tribbia, of NCAR, and with me again is Akira Kasahara and Warren Washington, both also of NCAR. Before beginning our second session with Aksel, I noted that I neglected to mention Aksel's birthdate in the first session: he was born on the 17th of December, 1924, in Juelsminde, Denmark, I believe.

Aksel, I thought that given the chronology of what we've discussed to date, we're now about the time when you were to receive your doctorate, and that involves something slightly different than what is done in the States currently. You might wish to explain that and then explain how you got your Ph.D out of that.

Wiin-Nielsen: Good. I'll start on that. Let me first of all explain the general requirement for the doctorate as it existed at the time that I took it at the University of Stockholm. There have been changes since then, but I took what is now called the "old-fashioned doctorate." The requirements were that you either wrote one single large piece of work and had that published as really the old, old system, but it had already been liberated somewhat when my turn came. The requirements were then that you had to present a number of already published papers; these papers should have a kind of a red thread going through them--that means they had to be connected somehow, but they could be separate publications and the final requirement was that you then at the end, before you submitted it to the faculty for judgement, you wrote an overview which actually described how these things were hanging together. And I had started already in Stockholm publishing some papers and since they were going to hang together, I couldn't use all of them. But I had published one paper in Stockholm which dealt with the consistency requirements for barotropic and baroclinic models over the globe or over limited-area regions. This was something which I actually was inspired to do by a paper by Hallmann in Germany, who had also looked at that problem. The real inspiration, however, came from the man who used to inspire me in those days and still does--Phil Thompson--because he had written a paper for the **Rossby Memorial Volume** where I actually could point out that he was not entirely consistent in the way in which he handled things. Now that, so to speak, put me on the line of work which I selected for the doctorate, and after I came to JNWP, there was plenty of opportunity to get into work of the same nature because this of course was the kind of model we worked with.

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A second paper which also became part of the dissertation had to do with the, let us call it, the average behavior of simple baroclinic models, simply because I took both a relatively long series of analyses and similarly of the forecasts to try to see how they behaved with respect to the very large-scale motion. Now we mentioned already earlier the problem of the retrograding long waves and I thought that the way in which Cressman had determined the so-called q^2 factor, that one ought to be able to derive that from the formulation of the model and not empirically, and indeed, if you consider what is really a truly equivalent barotropic atmosphere, then of course the requirement is that the wind doesn't change in direction with height. And that means in turn there is a temperature field in an equivalent barotropic model, but it is so constricted that the isotherms have to be parallel to the isohypses because otherwise you would get temperature advection, which is not permitted. And using that logically all the way through, I managed to find a value that was actually very close to Cressman's. That was not difficult, because it depended upon the vertical variation of the static stability, and there was some degree of freedom in deciding that. However, Larry Gates had already at that time carried out rather a large statistical investigation of these things. So that became the second paper, which really dealt with this quality, the behavior of the very long waves in the equivalent barotropic model.

In my paper, which also, depended upon these models, the inspiration for that came--well, in the end of course from the general circulation statistics, but particularly from Barry Saltzman, who had in '57 formulated the spectral equations, which he really didn't want to use for predictive purposes, but rather as a way of describing the statistics of the general circulation. I became interested in that already in Stockholm, and of course, we should admit that we didn't go all the way at that time. We were quite satisfied by having a Fourier decomposition along latitude circles, but we never really went to spherical harmonics, which we perhaps should have done, but we didn't. And that was the way in which I then used both analyses and forecasts to look for such things as the mean meridional circulation, and I remember being really rather surprised to find that even in the forecast of a simple, two-level model as was used at the time, that both the Hadley cell, the Ferrell cell and so on came out both from the data and also was maintained during the forecast period, so that became paper #3, and there was a fourth paper which dealt with the problems of the momentum transport in simple models, a problem we're actually facing today. We have great difficulties even with our fancy models today to keep the jet stream at the right location. So that was more of a diagnostic paper.

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I sent these over to Stockholm, and as I think I told you yesterday, Phil Thompson became the first opponent. This is rather, or it was rather, a formal affair, at that time. Always Saturday morning, both the man who defended his dissertation and the opponents came in white tie and tails, which they don't do anymore, but he was very formal. And you start yourself by governing the whole proceedings. You have no help with that. You welcome everybody, you describe your work in about fifteen minutes, and then you have to give the floor to the first opponent. Now the first opponent is the man who really seriously judges the scientific value. Then there is a second opponent, and that person you can actually select yourself. And I selected Bo Döös, and the requirement for the second opponent is to look more at the formal aspects. I mean, he is the one who tells you that you don't know how to spell, he's the one who tells you your figures should have been larger because then they were easier to read and so on. And then there's a third opponent, which you select yourself. And he is actually what you might call the court jester, the man who makes fun of everything. His job is, after this rather serious and in some cases perhaps slightly embarrassing situation, has to get everything to end up in a laugh, a happy occasion--which it ought to be. And I had George Veronis, who was a visitor in Stockholm at the time, to do that. All the proceedings were, of course, conducted in English, or Scandinavian English. Well, Phil, of course speaks perfect English, but the rest of us--George surprised everybody by speaking Swedish, which he did rather well. And of course there are all kinds of jokes which you can make that Wiin-Nielsen takes over the models from other people, but they are nicely shaped, etc. These kinds of remarks came. [Garbled]

That's the entertainment for Saturday morning for the academic world, and that's where they go and so on.

After that, you are left on your own. You can go out and have lunch with your wife and your family, but then in the evening, you entertained by inviting all the people involved, your professor, your colleagues, for dinner someplace. Rather a heavy financial burden, I would say, because at the time you take your doctorate, you are not a well-to-do person, normally-speaking. But it worked all right. Since I worked for the U.S. Air Force, I had a problem getting over there, but Dr. Fletcher, the head of the Air Weather Service, very nicely arranged that I could fly over military transport, as long as Frankfurt. So I think that's enough about that occasion which I remember as a very happy and joyous occasion.

Then I think we come to NCAR. I think I mentioned already yesterday that it

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was at that time, really something entirely new in the U.S. meteorological community, that these--I don't remember--12-14 universities who had a graduate program in meteorology had got together. I think it really must be considered as one of the first real events which took place after the United States was shocked by the early Russian satellite. There was great concern in this country at that time that the U.S. was not quite up to it, was falling behind, and that was not only in the rocket technology, or the satellite technology, it was a shot in the arm, as far as I could see, to the whole area of natural sciences. And of course to create NCAR was perhaps not motivated that way, but at least it was influenced by it. The real motivation was, of course, to create a place where more meteorological research could be carried out, and which at the same time would be a place where there will exist facilities which were too expensive, too cumbersome to handle for single universities.

In retrospect, this is exactly what NCAR has become and just the other day, when [this is an aside], when I visited the new Institute for Naval Oceanography, I noticed that in the middle of the director's table was the blue plan for NCAR, dated 1958. So apparently it's still a document from which you can find some inspiration.

I accepted Dr. Roberts' offer to come out here. We were housed at that time in the old Armory down on University Street, that is, the ground floor and the basement. I think the loft was still occupied by weapons which belonged to some National Guard unit or something like that. There were few people around when we came in the summer; it must have been August of 1961. Phil actually arrived shortly thereafter from Sweden, although he had already worked for NCAR through his Swedish office. Walter Orr Roberts was of course the director, Phil Thompson was the director for what was to be the Laboratory for Atmospheric Science...

END OF TAPE 2, SIDE 1

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INTERVIEW WITH AKSEL WIIN-NIELSEN

TAPE 2, SIDE 2

Wiin-Nielsen: As I was saying, Vince Lally was here, already at that time very much into various types of balloons, and of course his work later led to the Balloon Facility [NSBF] down in Palestine, Texas. James Lodge arrived and was to be the first head of the Atmospheric Chemistry Lab at NCAR. That was a long way still before the building in which we are today, actually existed.

The Armory--we stayed there for awhile. On the administrative staff, I think it is worthwhile to mention that in addition to Walt, I remember Ed Wolff and a lady called Mary Andrews, who later became Mrs. Wolff, and I believe still is. There was a girl called Harriet Hunter, who is now Harriet Crowe and those were the two prominent ladies. They were extremely helpful to those of us who didn't really know our way around in Boulder, and very good, I think, on the administrative staff. Shortly thereafter, arrived a young lady who was to work for Phil and myself by the name of Margaret Johnstone. I mentioned her already yesterday; she had worked for Phil in Stockholm. I think she was an adventuresome young lady who very much wanted to see the United States. I think the promise to her was from Phil: if you can make your own way to Boulder, you'll have a job. So she actually came by Greyhound bus from New York, after having made it to New York, I don't know how. I could say a little bit about Margaret because she actually turned out to work for me and not for Phil, and she taught me a heck of a lot of English, actually, being very sharp on these kinds of things. I think my letters and my articles became better after she typed. She was also a young lady with a lot of go--marvelous parties--and she went water skiing on the reservoir, she went riding, but then she also realized after awhile that with her seven years of elementary education--she came actually from Newcastle in England [where] her father was a miner. She came from rather a poor family and there was no money around for higher education. So she realized that this was still the land of opportunity, so she worked on an entrance exam to the University of Colorado, and we all, happily I might say, agreed to tutor her in various areas. Of course, she was headed for the sciences, but not the hard sciences, more biology. And she managed with some tutoring from various members of the NCAR staff, some she found herself outside, to pass the entrance exam, and then worked full-time and still attended the University for the first two years. Eventually I think she went down to half-time, got her Bachelor's degree, and went on to medical school in Denver, got her medical degree, and I think--well, I know--is now working in Long Island. That's one of these side stories of the early days of NCAR where you had

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people, quite strange ones.

I remember also in the circle around Margaret Johnstone was the librarian we had at the time. An old gentleman from Russia who actually ended up on the right or the wrong side of the Revolution, depending on your point of view, but he actually [was] on the White, not on the Red side, so he had to leave the U.S.S.R., worked for many years in Australia and then made it here, and was one of the librarians. He also, being a bachelor, was very interested in the social life, and taught us a lot about borscht and vodka and so on. But anyway, the work went on.

The next step was that we needed more facilities. Walt, I believe, worked together with the University of Colorado. They were quite willing to put up a building down on 30th Street, rent it to NCAR, and then take it over themselves later on. I don't know if they ever have got it back, but that was the plan. Anyway, to begin with, we certainly shared it with the University because the first computer facility which we had was a 709, I believe, installed in the 30th Street building. Walter McIntyre was the University's director of the Computing Center and we actually just bought time from them. But that meant that we could start to do some scientific work which required computing facilities and not just paper and pencil, which we were restricted to in the very beginning, and I think it was a happy cooperation with the University. It must have been, because I think that Walter McIntyre later on moved from the University, and was one of the Computing Division directors in NCAR before he got other thoughts, as you know.

Well, what about staff? One was suggested, one of the people I worked very closely with, was suggested out of the blue by Ralph Shapiro, who was at that time at the Air Force Cambridge Research, mostly interested in statistics, but he had a programmer called Margaret Drake. She apparently wanted to get away from the Boston area. She had served earlier, since she was and I think is a devoted Catholic--she had served as a schoolteacher for some Indian tribes down in Arizona and liked the West. But of course she now wanted to have a living out of it and on Ralph Shapiro's recommendation we accepted her as a programmer. It turned out to be a very good choice; she's still at NCAR, and I think has risen to the post of deputy of the Scientific Computing Division. So she came.

Then, of course, I didn't mention yesterday that a close collaborator of mine already in the JNWP days was a fellow called then Captain John Brown. We shared an office almost the total time I was at the JNWP. John was interested in

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following up on his academic education. He had at that time a degree from Louisiana State University, I think in chemical engineering or chemistry. Then he had in his Air Force days a Master's degree from M.I.T., and he was interested in a doctorate, so the arrangement was made that he be employed at NCAR and then gradually became a student at C.U. in the department of astrogeophysics and eventually got his doctorate. I think Phil Thompson had a lot to do with that as well, as an outside advisor.

But Margaret and John and I got started at that time on some general circulation work. As I mentioned earlier, I had already been impressed with the kind of thing Saltzman was engaged in. I think the general problem which we wanted to work on and the general question which we wanted to answer was not so much in which direction the momentum transport and the transport of sensible heat was going--that was already established rather well by the general circulation projects that had already existed at M.I.T. under Victor Starr and at U.C.L.A., essentially Yale Mintz' work. But our question was, O.K., so it's the waves that are doing most of the work, but how is this distributed? Is it mainly done by the unstable waves around wavelengths of say, 3-5,000 kilometers, wave number 6 or 7, or does the quasi-stationary long wave play a role in transporting the heat, the momentum, the moisture, and so on, and the answer to that was not really known at the time. So we got many data tapes from JNWP. We had good connections there. We were first referred to climatological data center in Asheville, but of course it didn't have the data nicely arranged for this type of work. We also took a big step in expediting the whole project, because I think we were the first to make the statistics directly from the analyses. Starr and his co-workers and Mintz, they had worked the other way around. They got time series of radiosonde observations in stations; they analyzed those as time series, and then when they had all that, then they plotted their maps and subjectively drew how things were and then they averaged to get the zonal mean and so on. We worked directly from the analyses as produced at JNWP, later at NMC, and started the data library which Roy Jenne later on has supplemented to a very excellent record.

That was a dangerous thing to do, and of course we had to test things quite a lot, because the analysis scheme which was used was designed by George Cressman, but was essentially the same idea as Döös and Bergthorsson had had with their first guess, and then an addition of data to that first guess. And of course in data-sparse regions, it could essentially turn out to be the forecast from yesterday, which became the analysis of today. And if the forecasts were biased, then of course that was a dangerous procedure to use. Nevertheless, we decided to go that way and of course we could test later on that the gross results

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at which we arrived for the total flux which was calculated, as long as that was in the ballpark of what otherwise was obtained by the other two parties I mentioned, and by further testing of all kinds of things. I think we felt that, although there might still be some uncertainty in this methodology so there were uncertainties in the others because you still had to put lines in at the places where there were no data. And whether you do that first or after probably doesn't matter too much.

Now, that gave of course the result that normally speaking, particularly in the three-quarters of the year that is not summer, then the quasi-geostrophic waves carry a good part of the load in transporting the necessary amounts of heat and momentum in the pattern in which this takes place. And we were really supposed to--well, we thought at that time that one year was a tremendously large amount of data to process, but we eventually got one whole year processed. And I clearly remember that we were in the process of doing the year of 1962 and we were ourselves here in 1963, so we took a recent year, and then Margaret came one day and said, "Darn it! These worked so well from January to November, and now I've just finished December and I'll take the means and they don't look like the other months, and I have thought, I have looked at the tape if there's anything wrong. I've looked at the whole box of IBM cards (which at that time was the program). I can't find any mistakes, but there must be something because it's totally different."

Well, we all checked. John, of course, was an excellent data man, very good programmer, and he and Margaret had written the program so they swore no mistakes, no fooling with the cards either. The data tape format was exactly the way it should be. So we finally agreed, all right, let's extend it into 1963, and see if it now holds. Let's furthermore repeat November and we repeated November, and we reproduced our results. So we finally got confident that something strange was happening and the general scheme followed the same pattern in January as in December, the same in February as in January, and finally in March it switched over and became what I would call the normal mode again.

Now what we used to look at was the--I mean, in addition to transport, we also calculated the amounts of energy, the energy transformations and so on, and of course the big results of Victor Starr earlier was what he later started to call negative viscosity. It really materializes in an energy conversion from the eddy-kinetic energy to the zonal-kinetic energy, and that of course is something which you might call negative viscosity. Because when the energy conversion goes that way, it actually means, if you look at the integral, that the momentum

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transport is up-gradient and not down-gradient and that could be expressed, I think, in a very bad way of talking about negative viscosity. I don't like the term, but Victor wrote a whole book with the title, including some galaxies and other things. There is no such thing as "negative viscosity." But I can understand that you can fall for two words which etched his mind very much.

But anyway, I think we managed to demonstrate more or less by accident, that the atmosphere can, for considerable periods of time, work in two essentially different modes. You might actually say that when it works in the normal mode, which it does most of the time, then it's really because the atmosphere is all the time baroclinic. But when it works in the opposite mode, where the energy conversion goes from the zonal mean to the eddies, that of course is also characteristic of barotropic instability and you might therefore call that a semi-barotropic mode.

Well, eventually we published the thing in a second paper which appeared in **Tellus**, and no sooner was that paper out before a letter arrived from Victor Starr: "You must have done something wrong. The atmosphere does not work that way." And John Brown knew Victor Starr quite well. I actually didn't know him at all. John wrote back to him, analyzed the situation, and said, "The only thing which we can see is that we, of course, are doing this over not even the whole Northern Hemisphere. We used the JNWP octagon and therefore, of course, we were missing a strip close to the Equator. And since it was not then a closed region, when you evaluate the integral, you can do it in two different ways. You can integrate by part and get a different formulation, so it all really depends upon the actual flux across the physical boundary which is being used." And John went to the effort of calculating the integral both ways--he calculated the boundary fluxes and so on, and I think he showed to his own and to my satisfaction that there was absolutely no doubt about the sign of the energy conversion. And Victor Starr stopped complaining after that.

So I think that Ed Lorenz, who was already then a visitor here in the summertime--we talked with him about it and he agreed and I think in his book on the theory and nature of the general circulation, he actually mentions that one cannot prove that the momentum transport always had to be up-gradient or for that matter down-gradient, and then he quoted our paper in evidence that the atmosphere can actually work both ways. There's no doubt that the original picture is certainly the mode in which the atmosphere works most of the time.

To see how we got into this by accident, it was only after we made the calculations that we then went back and looked at the maps, and of course when

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you look at--you can do it even now, the **Monthly Weather Review** has the mean maps for the month and it is quite obvious what really happened in 1962-63 was an extended period of blocking over the whole Northern Hemisphere in the preferred location, Atlantic and Pacific, and both places at the same time, so it really did go into reverse. That winter we learned later on was of course also a winter where all kinds of records were broken for highest and lowest temperatures, depending upon where you were, relative to the major troughs.

But the whole time I was at NCAR, that was really the main project which we were concerned with, spinning a lot of tapes and getting some results out. That we also worked on other things can be seen from the publication record, but that was really the main project.

In the meantime, of course, things were moving. I remember we were invited one evening back to 30th Street because Walter Orr Roberts brought the architect who had a tabletop model of this beautiful building which we are in now, and that was the first time we had some indications. Otherwise, at that time, the Saturday excursion was normally a walk up along the Mesa Trail until we came to the back side of the Mesa and could look down upon it from the Mesa Trail and imagine how the building was going to fit up here.

Generally speaking, I think that the staff was quite pleased with the design, but quite concerned about what was going to be inside. The only one who reacted strongly to the whole thing was Pat Squires, who had arrived as a cloud physicist from Australia. His general attitude was that as soon as an institution got a beautiful building, the institution went down the hill, scientifically speaking. And he used to quote people who worked at the New Mexico Institute for Mining and Technology way up in the mountains who did some important work on cloud physics in those days, and Pat always said, "Well, they live like monks without the burden of religion and that's the way science should be done." Pat took the consequences; he resigned, and became the director of the Desert Research Institute in Nevada, and actually only came back to NCAR at a much later stage.

In my case, what happened was that each time the Board--I think they were all the Board of Trustees at that time, there were not that many of them--they came to meetings out here, and each time I had a visit from Professor E. Wendell Hewson, from the University of Michigan, who always stopped in my office and said, "Well, I can report that in principle we are now going to have a department at the University of Michigan." And he kept hammering that I should go out there. Now I suppose and--well, we are two Americans and two

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people who are born elsewhere in this little group--I don't know how it is in Japan, Akira, but I think we were educated in Europe to consider a professorship as some of the highest there is, and we were also educated to feel that it was a responsibility, whatever else you did in life, to at least see to it that there was somebody coming after you who was more clever than yourself in your chosen field. I suppose furthermore as I told you yesterday, coming from a family of teachers, and I remember my father complaining bitterly that after I went to the Met. Office and then here that I was kind of the black sheep in the family--deviating from the standard path. And that I went into meteorology was not appreciated, either. I had an uncle who was in the Postal Service and actually a postmaster at one of the Danish Islands, and he shook his head and looked at me and said, "Aksel, what are you doing? You are getting into a dishonest field." And I said, "Well, I know that forecasts are not always perfect, but they are made honestly." And he said, "No, they are not, because I have to submit all your observations down from my island, and the lighthouse master out on the other side, he comes every summer and says, 'Nielsen, will you send these observations every three hours in the usual way? I'm taking four weeks vacation.' I think there is an indication of a certain amount of dishonesty there."

So I was rather unpopular. I remember this difficult choice, should one go to a university, or should one stay at NCAR? And the long and the short, after weeks and months of speculation, I actually turned them down once and said I had a perfectly good job--which I had.

Washington: What was on the time schedule of this, when you argued over in your own mind if you should stay here or not, was that early in 1963?

Wiin-Nielsen: No, it started even before that, Warren, because Wendell used to come into my office every time and talk about Michigan as this delightful place he thought it was, and that was already 1961. I was down in the Armory and he started hammering on me, you know, that's Wendell's style. And it was in kind of the back of my mind, but I had told Wendell, "No, I'm not going anyplace." Finally, they gave me a written offer and there was a dean who called me in Cockerell Hall and I remember saying, "No, I'm not interested, I have a good job." And put the phone down. But then they really started going seriously after me after that. And, well, let me be totally open about that.

I think down at 30th Street that then I had a little bit of a disenchantment with NCAR. I think that I was put into administration much too early. Phil was the director of the lab, and he made me at rather an early stage the assistant director. And with the clash which existed between Phil Thompson's lifestyle and

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ordinary office work, you can well imagine that the assistant director worked very hard early in the morning and well into the day, and I really felt that I perhaps carried an unfair large part of the administrative burden. It was not that I couldn't handle it, but it took time away from other things. And you had to recall too that this was a time where inside LAS we had to create a reasonable lab for the cloud physics people, we had Hans Dütsch from Switzerland as a long-term visitor here, and he of course was tremendously interested in ozone. We had the Knight people, who had to have a lab and so on. There were so many practical things of building and changing and getting equipment and installing it and so on, and we were all equally inexperienced in these things. We had no contacts in Boulder. We got that after awhile; we knew who the right people were to call when we had to have things done. They took time, all of these things. So I think finally that swung it over in the direction of Michigan, but I really was very sorry to leave NCAR at the time. I thought they were very, very happy years, and also of course, I never really worked in this building [the Mesa Laboratory]. I left already in September, 1963, when this building was not yet ready. I felt furthermore at the time that perhaps we had attained critical mass at that time--that NCAR was going to flourish, as indeed it did. And then of course there were such nice people as Warren Washington, who was already here...

Washington: Well, no, you had already left--

Wiin-Nielsen: Had I left before you came? There was not much difference though.

Tribbia: I think you came in June or July.

Washington: No, it was towards the end of August.

Kasahara: I was there. I came in July and Aksel was there.

Washington: I wonder if I could ask you just one question. You know, that summer, that spring/summer of 1963, it seems like a lot of the people such as Akira and Chester Newton and others had come.... Had you made some kind of conscientious effort to hire a lot of people in that one year, in 1962-63?

Wiin-Nielsen: Yes. I think, first of all, 1962-63 was the period when we had the 30th Street building, so we had some place, some physical space in which to put people. I mean, Charles Knight and Nancy Knight came at that time, and dropped their icebox in there. In the summer, of course, we didn't have room enough, but then we had Cockerell Hall, which could be used for the summer visitors and

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some of the permanent people actually moved to Cockerell Hall and some visitors were put in on 30th Street. But I used to have evening sessions with Phil, and I remember debating with him in all friendliness: "Should NCAR have synoptic meteorologists?" And we agreed at the end: "Yes, in limited amount, we should have some synopticians and some data people, and of course Phil said at that time that there were only three synoptic meteorologists who should be at NCAR. He used to mention Dick Reed, Chester Newton and Fred Sanders from MIT--they were the outstanding synopticians of the 1960s, and earlier, and eventually of course Chester was selected.

But it was also a period as I recalled it, where the initial difficulties with NSF, with the universities, were kind of over, and the budget was now starting to increase, so we could hire a lot of people, and in addition, to this particular question of synoptic work, it was quite obvious that what NCAR really wanted to have was people in the various aspects of the general dynamics of the atmosphere. We were not really at that time thinking too much about what we would be doing; it was quite obvious what dynamicists we wanted. And it was for that reason that Akira came here, Warren came here, John Brown was here, and so on. And so I think that when you are interviewed one day for these tapes, you can tell yourself why you decided to do what you did, which was excellent.

I don't know; should we go to Michigan?

Tribbia: Let me ask one more question. With regard to your staff, when, prior to the summer of 1963, then, if I understand you correctly, the only people actively working in dynamic meteorology at NCAR were yourself, John Brown and Phil Thompson. Is that correct?

Wiin-Nielsen: That's correct.

Tribbia: Then in the summer of 1963, Warren Washington, Akira Kasahara, Doug Lilly were all hired within that year, is that correct?

Wiin-Nielsen: That's right.

Tribbia: So you had already left prior to that buildup actually coming to fruition?

Wiin-Nielsen: Yes, you're right. It didn't really come to fruition in my time, although I had something to do with making the decisions. Because as you know, it takes some months before people actually arrive. Which was another aspect of why it

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was so difficult to leave NCAR. And I remember driving the car with my wife and the kids up to Michigan, and that was a kind of sad atmosphere. And of course the Midwest is not spectacular. Colorado is. I think the same thing happened at Michigan, as happened at so many places, that they make a decision in principle ("yes, we are going to do such-and-such"), and they hire the people necessary to get it started. That's the hunt which goes on. But when these people actually arrive, then they haven't brought their house in order, they haven't guaranteed the physical facilities. I used to live in a little bitty office up on the top of the roof of East Engineering until eventually we got some professors thrown out so we could get proper offices, but that took half a year.

Washington: I've been to that little office. It was terrible.

Wiin-Nielsen: Terrible. But I think that, of course, the Michigan years, that became ten or eleven years altogether, and the challenge, of course, was to create the department of meteorology, possibly including limnology and/or oceanography, but that was not decided at that time. But the meteorology department they wanted to have. Now of course, I was not the first meteorologist there. There were a group of people, four real well-known people who later became full professors in the department: Wendell Hewson, who had come there first; Jerry Gill, who was always a colleague of Hewson's, even when they were at Blue Hill and both of them, of course, were Canadian; A. Nelson Dingle or Bud Dingle; and Don Portman. Those four had been housed--administered through the Department of Engineering Mechanics, the fluid dynamics people and so on. They really lived on a shoestring because they were permitted to be there, because they brought in some research money. Those four people, of course, were all really very much in the air pollution field at the time, and also from a very early stage, they got into the pollution-allergy relationship, worked with the medical people. I suppose Ann Arbor might be just in the center of the ragweed, I don't know, but the Midwest is certainly known for having a lot of these allergies.

When I was brought in, it was because they felt they needed somebody from the outside to start it, and the man I worked with at the University of Michigan was a very old dean of the college called Stephen Attwood. He was just about to retire. I had also insisted on seeing the Vice President for Academic Affairs, a fellow called Roger Heyns, who later became the president of the university at Berkeley, in California. I had a very good friend in him. I think he understood when a decision was made, then it was not only a matter of arranging the academic facilities, but also the physical facilities and he gave me essentially a free hand to hire faculty. He only put one constraint on it: he said, "We have a

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general rule here that when I hire faculty, it should be in a reasonable proportion to the number of students." The decision, of course, was made quite early that we also wanted to have an undergraduate program. That was strongly criticized in some quarters of the meteorological community. There were many people who believed that one should not actually have a major in meteorology at the undergraduate level. One should rather try to find good graduate students who had an excellent background in mathematics and physics, chemistry, astronomy, any combination of those was considered good. I think I can see, I could see then and I can still see, both sides of that argument. I think that if the only purpose in meteorological education is to create the researchers of the next generation, then forget about the undergraduate program. Then it is really much better that the four years of getting a Bachelor's degree is spent in learning the natural philosophy, as the English would call it--mathematics, physics, chemistry and that kind of thing. Then you have, hopefully, a very good background.

It depends of course on what kind of university you go to. Because I understand that Michigan at the time, and many other places, you could certainly major in physics without having a single course in fluid dynamics, for example. Quite possible. But, OK, it's not so difficult to learn fluid dynamics once you know physics. But if the requirements, on the other hand, are to satisfy the needs of such things as NOAA, or the Air Force, then, of course, you need to produce other kinds of meteorologists than the Ph.D.'s. There is the justification for at least a few schools in the U.S. providing a Bachelor's degree with a major in meteorology, or very closely related fields. So we decided to go that way. There are not many other U.S. universities even today which maintains that, at least none of the major universities. But we went that way; I think it was a very wise decision because we got, very early, very good relations with what was then called the U.S. Weather Bureau and then ESSA and then NOAA. We were furthermore selected a few years later to give a summer course of about three months with the full summer term as continuing education for people within the U.S. Weather Bureau. Normally, the bench forecasters from the stations all across the country, thirty of them, were brought into Ann Arbor to live together in one of the dorms. They lived there and had a very tough course--the only thing we did for them was that we taught it in the physics department and not in the East Engineering building because the physics department was actually air-conditioned and summers in Ann Arbor are not terribly pleasant, if they are normal at least. So we went for the undergraduate.

The other unsolved problem in the beginning was whether or not we were going to have oceanography, or limnology. I asked around and I got as many opinions

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as I asked for, but I think the consensus was, "Why create a department of oceanography in Ann Arbor, midwest, far away from the ocean, must be a strange creature." But there was pressure and it came from the outfit within the University of Michigan called the Great Lakes Research Institute. That had been created quite a number of years before essentially by people who came out of the biology, zoology, botany departments because they felt that the Great Lakes were a kind of natural research--

END OF TAPE 2, SIDE 2

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Interview with Aksel Wiin-Nielsen

TAPE 3, SIDE 1

Wiin-Nielsen: As I said, there was pressure from the Great Lakes Research Institute because they had also brought in what you might call some physical limnologists. John Ayers, Jack Hough--perhaps more on the geological side, but also interested in the physical aspects. And they kept saying, you can just as well have a department of oceanography at Michigan as you can at Woods Hole, because we have the Great Lakes and the only difference in the physics is that there is no salinity. But that, as a matter of fact, might be an advantage because then you can make comparative studies of what is the major role of the salinity. Otherwise the Coriolis force works, the Great Lakes are large enough to show that effect, you have waves, currents, etc.

So the long and the short was that we did include oceanography in both the undergraduate and the graduate programs. I think, here again, it was a bit of a gamble because we had only one professor, John Ayers, who was an excellent physical limnologist, but really had no experience at all in teaching. He had been at Woods Hole and then some time at Cornell, and then some time at Michigan, but never the professorial type. Well, to remedy that situation, I found a young graduate from Harvard, Stan Jacobs. He was highly recommended and we took him in and he had a joint appointment with engineering mechanics, but really became our major teacher of dynamic oceanography, physical oceanography, the theoretical aspects. Later on, we got an M.I.T. man, Bert Green, whom we took from Norway where he was on a one-year stipend with Arnt Eliassen, so he was well-educated. He came in also. Eventually, the first five became I think twenty-two professors, also because we took aeronomy in as part of the program, also the undergraduate program. That was Fred Bartman, Leslie Jones who had grown up in what was called the aeronautical engineering department, later the aerospace, then brought over because they were essentially interested in the high atmosphere. So that became the department.

The undergraduate program in a couple of years had sizable numbers of students in it. I think it saved the department because the favored ratio of those days was faculty to student and, rather the reverse, the undergraduate program helped a heck of a lot in securing university funds to come into the department. Otherwise, I should say that this started in 1963, and for the first five years, I think we were really in an easy situation with respect to research funds from various sources. This was really the good old sixties, the happy sixties, there

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was still lots of money to get from the National Science Foundation. With the division we had, with some theoretical people who went to the National Science Foundation, the old group had very good contacts in what is now E.P.A. They worked with the Detroit Edison Company, they worked with the Atomic Energy Commission, they had their first Ph.D. student, Gene Bierly, anchored in the Atomic Energy Commission, so they had their person down there. Later on of course, Gene moved over to the National Science Foundation and I am sure he is absolutely neutral with some slight leaning towards Michigan, if you can put it that way.

Similarly, the power companies--all these were very good in providing money so that the general rule was that a graduate student in the first year, he was not only guaranteed admission, he was also guaranteed either a teaching fellowship to assist in the synoptic lab or in the general undergraduate courses, or a half-time research assistantship. At that time, we paid them $3600 for the academic year. On that, they both had to pay tuition and live. I don't think they had too much money. But they had cheap housing in the graduate student housing and tuition was not at that time what it is today, $600, I think it was, for the year, for in-state, and double that for out-of-state students, a not too difficult amount of money to set aside.

The graduate program in meteorology was of course already in existence. When I came, there were about thirty students in it. Some, five or six, Air Force people who came in for a year and a half to get a Masters degree, others working on their Ph.D. There was a lot of interest in those days in getting into graduate school in the fields of meteorology and oceanography, so I think we rose up to about seventy-five to eighty graduate students at one time. Now I recall these were certainly not all of them Ph.D's. A good fraction of them actually stayed only for three semesters, trimesters they were called. You could actually finish the formal course requirements for a Masters degree in about a year to a year-and-a-half, depending upon the speed with which you moved. So that worked, altogether it became rather a heavy department with its three branches of aeronomy, oceanography and meteorology. We started early with a student council which I negotiated with so they could have--so at least we could adhere just as much as possible to the requirements which they had.

The salaries at Michigan went up, followed the inflation rather well, so we were not badly off. My own work, in addition to being the department chairman, was a continuation in the first years of what I had done out here. Later on, although I was all the time amply supported by the National Science Foundation, we moved in other directions, and I was very lucky to get a whole number of very

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good graduate students with whom I still keep--well, I still keep in contact with some of them at least. The early, if I may talk with envy about the graduate students, the early ones I took over from others were Ananda Vernekar, who is now at the University of Maryland, did his Ph.D. on the mean meridional circulation and what maintains that. C. H. Yang, also called Ken Yang, who has been in his whole professional career at Air Force-Cambridge, worked on some data studies of non-linear cascade processes as we would call them today and did a theoretical study on the non-linear aspects of the baroclinic instability problem. It turned out that that was never published because Joe Pedlosky came first, and they were too similar. But that was an accident; actually, we didn't know that Pedlosky worked on it at the time. There was a fellow named Jim Bradley, who was an excellent intellectual property to have. But on the edge between what I might call very intelligent and slightly insane. He of course went out of meteorology later on, but that's too long a story to tell here and he's not related to our main topic.

Jacques Derome now sits as a professor at McGill University. Leonard Steinberg, Norman McFarlane--we had a good amount of Canadian students, not only because Michigan is close to Canada, but also because Wendell Hewson and Gerry Gill had very good contacts up in Canada. George Lawniczak, the only Navy officer who came to Michigan and got a Ph.D. Marvelous character. He used to have a big sign in his little office in the building where the graduate students were hanging out at the time: "Love thy neighbor, but do not get caught." He lived up to that. Lars Olsson from Sweden. Joe Sela, from Israel.

Then, the first time I left Michigan, for a short while, was in 1969 after I had been there for six years. I accepted during the summer term, a teaching position at the University of Santiago in Chile in South America. There I met a group of three young men who were essentially wanting to be a geophysics department. There was one named Humberto Fuenzalida, who I convinced to come to the University of Michigan. The other two got Ph.Ds in the United States as well, one at Wisconsin and one at Cornell. But all three went to the U.S. and got their degrees, and are now working together again, but on a much better footing than before.

The last of my personal graduate students was Mike Chen, who is now at Iowa State University, and as far as I can see, the only one who sticks to the latest studies. His niche is there, he's very good at it, and he has continued that.

There were other people who were not on the faculty, but who were visitors in

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Ann Arbor. I got a request from Erik Palmén from Finland if a young Ph.D of his could get a year as a research associate at Ann Arbor, Eero Holopainen, who is now the professor in Helsinki. He came and stayed for one year. Phil Merilees, who is shortly to join NCAR, was also a research associate at Michigan for a rather short time. He had itchy feet at the time, and went from Michigan to Florida, and worked out of Florida State for awhile, not too long, and then back to McGill where he came from and then of course, on to Toronto. We got Ferdinand Baer on the faculty from CSU, a very happy addition, I think, because he, Stan Jacobs and Epstein and myself really formed the dynamics group more than others. We got Warren Washington to be--what did we call you, an adjunct professor? That was the title which was used for the NCAR people who had affiliations with the universities. Also, a very interesting term you spent at Michigan. That, I think, was in the times of George Lawniczak and--

Washington: Lars Olsson.

Wiin-Nielsen: Lars Olsson. Then of course we had some eternal students like Alan Murphy, who took forever to get his Ph.D., but eventually got it and has been doing excellently, I think, in his little niche. He is of course known as the fellow who deals with statistics in all kinds of matters, but mostly in the prediction game. He took many, many years to get his Ph.D, but once he got it, he took off.

Otherwise, I think Michigan was a long, happy period. I liked the academic life. I had lots of contacts with other branches of the university. I know that I played 6,432 sets of tennis against Professor Jerome Jelinec, who was in the music school, and a very fine cellist. He told me so because he kept track to the day I eventually left Michigan, and he had a score for each set. But it took ten years to play all these sets of tennis. But it was a happy period.

Maybe things started to go slightly the other way towards the end. Not because Michigan in any way changed, not because the department changed, but really towards the end of the sixties, the student revolution, which is talked so much about in Europe and which really started in France, also spread to this country. At the same time of course, it was the Vietnam period, which created also within the universities a lot of uncertainty. There used to be many, many discussions among the graduate students over what the chances were of getting drafted, what you would do in that situation--would you go or wouldn't you go, and so on. And, of course, it also created what I think was called at the time, civil disobedience. We had our share of that at the University of Michigan. First, I think the movement by the black students to get more representation on

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the faculty, and not only in the athletic department, but across the university. Furthermore, of course the admission policies. Then, when the Vietnam period came, things became more violent. I remember on one occasion, after we had got a new president called Robert Fleming, who came from Wisconsin and had been a professor there but also a labor negotiator. They actually chained him at one time to the door of his own office and kept him there for awhile. He negotiated himself out of that situation, by the way, quite nicely. There were violent times. I remember sitting in the east engineering building when the Ann Arbor Bank blew up. It was just at the corner between east and west engineering, on a Saturday afternoon when there was a football game on. Nobody around.

It was difficult for us actually, really to understand what was going on. The college of engineering, where we belonged, has never I think, been considered a terribly liberal institution. Engineering colleges normally are not that--there were certainly a few liberals, but I remember being scolded by the dean because I had created a student council. One was not to talk with students. They were just to come to classes and get good grades and graduate and go away. There were some social aspects of this they didn't quite understand. As it turned out later on that our department actually did a very, very good thing because there was a mechanism by which one could avoid the confrontations because we could talk. The same dean, later on, actually gave me his apologies because he said, "Well, you were the only one who really saw what was happening. The rest of us were just wooden heads."

I went away from Michigan three times. I already told you about Chile. It was only three months. Then I had a full sabbatical year as a visiting professor at the University of Copenhagen, 1969-70. Then I went back to Michigan again. Then I had another visiting professorship at the University of Bergen in Norway, 1971-72. I could have stayed there, didn't like to, went back again to Michigan. And then we actually come now to the period called "ECMWF."

At times, Europe gets a shot in the arm. That's when they realize they have fallen behind those we Europeans compare ourselves with, normally the USA, and Japan. It comes every ten-fifteen years. We are just going through a period in Europe right now of that kind, but certainly the Space Age, computer technology, technologies in the airplane industry and so on, was quite obvious to Europe in the sixties, that really they were not following along. One had at that time the European Economic Community, but consisting only of the classical six countries, the Netherlands, Belgium and Luxembourg, France, Germany and Italy.

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It started as a coal and steel union, and later became the EEC, the European Economic Community, which was then in 1972 joined by Ireland, the U.K. and Denmark. They formed a commission for science and technology. They created projects of a European dimension. Project #73 was put in by the meteorologists, and the idea was to create a large, common meteorological computing center because they realized that machines were expensive and one could share. So it was known as the EMCC, the European Meteorological Computing Centre. That project got a lot of support, also from countries from outside the EEC which were invited to participate--Sweden, Finland, Austria, Switzerland, Spain, Portugal and Greece, Turkey--all these countries. It was the single project that made the fastest progress among all the others which existed. It's the only project I know of which has been converted to mortar and stone, has a building and so on. Some of the others have been quite successful, but it is probably the outstanding project which came out of that.

Plans for that were essentially finished by 1970-71. Then they got into a hassle: how should the convention be weighted? Well, they had lawyers to write a convention, they had finance regulations, they had personnel regulations, it took them ten months to decide what the working languages were going to be. But then there was a bright idea which, as far as I know, came out of Holland, actually. They had good connections in the United States, and one of the things which they had noted was that Kiku Miyakoda, from GFDL, in 1971-72 had made a series of experimental medium-range forecasts. I think they were made to twelve or fifteen days. But at least Kiku had been able to demonstrate that the chances were very good for extending the forecasts, which at that time were normally limited to 72 hours--that was a long forecast at that time. But that they could be extended, hopefully, a good bit longer provided it was done globally, provided the models were equipped, or putting it another way, provided physical processes were introduced in the models because the hope was to come so far out in time that one actually could cover both the birth, the lifetime, and the dissipation time of a cyclone, a typical cyclone. And that of course we knew in advance, that meant a week or more. And then you need both the creation mechanism and the dissipation mechanism.

Well, so around 1971, the Dutch proposed officially: "Let us change this, so it is not only a European meteorological computing centre, which it can also be. But the centre should have a purpose of a meteorological nature. It should be operational, it should make medium-range weather forecasts. Apparently the Dutch were supported very much by the French and the Germans and very little by the English. This project caught fire. They all agreed, "Yes, that's the way

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we'll do it." Then they went back and changed all the official documents so we now described this situation instead of the earlier one, and by 1973, they were ready. The final decision was going to be made, where is the darn thing going to be?

They invited all the countries participating to send in offers to be the host country. I think five were received. Italy, Germany and France all wanted it. So did Holland and so did Denmark. So they formed a committee, they traveled around and looked at all the sites, they wrote a report, the Interim Council, as it was called at the time, met. They decided unanimously that the European Centre for Medium-Range Weather Forecasts should be located just north of Copenhagen, Denmark. And that was the decision by the council, and then U.K. reacted. Sir John Mason got himself into gear and he issued an edict, "Either the centre is in England or we won't play ball." I think most of the people were of the attitude at that time, "Well, O.K., so if you don't want to play ball, don't play ball. You can save your own island, and that's fine with us." But then, Mason brought it even higher. He had a very good relationship with Prime Minister Heath, who was the Conservative Prime Minister at that time in England. Heath was, as you perhaps know, a devoted sailor. And consequently he depended very much upon good weather forecasts. Mason had a good relationship with him.

So he talked with Heath about this problem, and Heath said, "Well...we really must do this at the high diplomatic level, but it turns out that shortly there will be a visit by the Queen and myself to Germany. We will work it out with the Germans." And then there was, as is very often the case in Europe, a little bit of horsetrading outside the meeting hall. U.K. said to Germany, "You can have the European Patent Office; we want the European Weather Centre." They decided, and with the voting patterns as they are in Europe, nothing could be done about it. So England got ECMWF. When I first visited England, I stood with Mason and Knighting in the building which was the bar/restaurant for the college which they have there, and Knighting said to me, "Yes, over there, over there in the swamp--that's where ECMWF is going to be." And that's where it is.

Washington: It's a swamp?

Wiin-Nielsen: It's a swamp. We had to go through a few more investigations to see if we could build on it.

When the Centre moved to England, then of course, they had to give something

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to Denmark, so they selected a Dane to be the director. So that's the way in which it was offered to me to come to Shimfield Park or to Bracknell first to get this thing going. I thought that was one of these offers which you get only once in your lifetime. Not out of any disenchantment with Michigan, but simply because this was an opportunity for European meteorology to finally put its mark on the map. I accepted it in January, 1974. The appointment came through on the 23rd of December, and they asked me to start on the first of January. Which I didn't do, I actually went to Australia for some IUGG meeting I had promised to participate in, but then directly from there to Bracknell. Mason had to live up to his promises; he had given us forty offices at the top of the Social Security, and I was the only one employed. It was in the middle of the oil crisis. I worked on the east side in the morning, and the west side in the afternoon because we couldn't use electricity. There was some tall man in a uniform who came and shook his head whenever I put on the lights. This was January; it's dark in England at that time. I worked very much in Brussels, which was the headquarters of the EEC. We got the interim council to continue, we had a scientific advisory committee, and I could hire staff immediately--it just took some time to get them there.

The first two I hired were Lennart Bengtsson from Sweden (I don't think I could have kept him out of the Centre, even if I had tried--he was very, very keen on the idea). It took more to convince Jean Labrousse from France that he should come and head up the operational activities. I knew he was an excellent man, but the French are somewhat more complicated to deal with. Not Jean Labrousse himself, but his bosses in France until we finally got him released. They were the first two. Then, Joachim Kuettner. He was running a part of the WMO Programme--GATE. He had the GATE office, and that was going to close down shortly thereafter, so I could inherit his secretary, Jane Ryder. The fastest typist in the world, no doubt. And no errors. She was very, very good. Other things she couldn't do, but type she could. And thereafter, we got a German administrator, Baron Wolfgang Dieter von Noorden, a German nobleman, a terrible choice. He was really a pain in a certain part of the body while we were working with him, but he was simply enforced upon us. And then, we started to hire and I think in 1975 we were about twenty people.

Washington: Let me ask you a question here. I recall Lennart coming to NCAR sometimes to run our computers...was that after he joined the Centre?

Wiin-Nielsen: No, that was before. Lennart had itchy feet; he had good connections in Monterey, and I think he spent--long before he came to the Centre--maybe 1970, a year at Monterey. And I also know that he had very good contacts at

NCAR, and was here for awhile. I don't know how long. But he certainly was well-acquainted with the United States. I also remember during one of these visits he also came to Ann Arbor, and the kids thought he was such a funny person because his English left something to be desired at the time. But enthusiastic he has always been, and he continues to be.

The Irish very much wanted to be at the Centre. That was really an opportunity for Irish meteorologists to get out of Ireland for awhile. I think we have had many more Irishmen than they really deserve at the Centre, according to their contribution, which is very small. But there were good ones--I think perhaps the most well-known is Tony Hollingsworth.

I was told, at the time, by the people who hired me--and that was Süssenberger from Germany, Gosset from France--they were really the negotiating committee. They said, "We want you to be the director of the institution. We want you to do one more thing. Could you do everything possible to prevent the ECMWF from being influenced too much by the British Met Office? In other words, keep John Mason at a distance, and let him not consider and let him not treat the European Centre as if it was some subdivision under the British Met Office, which he would love to do." I had many, many fights with John Mason; he really wanted to take over everything. He had absolutely nothing to do at the European Centre except when there was a council meeting. I had thrown him out of the door several times by telling him: "You are on European property, you are not in England any longer. Will you please leave the property and stop?" I mean, he really wanted to take over the whole thing. I found out later on that it was probably some kind of a test which he put me to, because it turns out that the last thing he wants around him are "yes-men." If you just say, "Yes, sir, I agree, sir," then he considers you as the dirt. And then he will tramp all over you. But if you put up a reasonably good defense, if you can maintain your point-of-view, even in adverse situations, then he respects you. And when you finally get to the point where you really speak against him, then he respects you even more. We have become very good friends in spite of the fact that we really fought for three or four years at about an even level until he gave up and said, "O.K. He's good enough, the guy over there."

So he had become--I wouldn't say a friend, nobody can be a friend with John Mason--but a good acquaintance. And a good exchange of funny stories and as you know, Mason can entertain you from here to eternity with all kinds of more or less likely stories. But anyway, we got the British Met Office at a distance, we got some very good people out of it because when he realized that he couldn't fight us Europeans, then he decided to join us. And then he became

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very cooperative. We got Roger Newson out of the British Met Office--he's now with WMO. We got Dave Burridge, who is now the head of the research department, we got a whole string of people who were there for three years and then went back again. People such as Lorenc, who did some very good work; Templeton, who was equally good. Of course, it was not England and Ireland alone, it was from the whole of Europe that we had to get people in. So we got five years to put an operational product out, from 1974-1979. We started operations in November, 1979, at which time it had been already decided that I was going to go on to WMO, but the six years I had in England were probably the years where things were the most complicated. I had absolutely no experience being a kind of European politician or diplomat, which was necessary, because yes, they wanted the Centre, but they weren't really quite willing to cough up the money. We had terrible fights in the finance committee and the council before we decided on the budget for next year. I was assisted very, very much by both Lennart and Jean Labrousse. They were very good negotiators, they were much better than I was in defending the scientific and technical program because they knew every detail of every contract of every little bitty thing which was necessary. And occasionally, if you want a council to agree, you can either play the high road or the low road. Now, the low road is the one where you simply swamp them with facts, which tells them that you really know your business. That may convince them that you're worth giving money to. That was Jean and Lennart who traveled that road. I could take the higher one and speak in more prophetic terms about all the great things we were going to do--that convinced some of them. And eventually, we always got a budget. And I remember saying, at least the last time, "You know, Jean and Lennart, we really can't complain, we are always getting what we ask for, but it's the way in which we are getting the money that is so difficult." My secretaries, who of course also served to distribute documents for the council meetings and so on, we always had a reception the last evening after everything was done. We invited the ladies in and so on. But that was also the time of the November meeting that we had to agree on the budget for next year, and they said that they could read how things were going on the color of my head. If it was kind of normal, we were very close to a peaceful finish. If it was red, we were in difficulties, but not too much. If it turned violet, then we were really about to lose. And then when it changed to red again, then things were O.K. We sometimes had them wait, these ladies, two hours for their cocktails or their drinks because they couldn't settle on the darn budget. And it was never a matter of say, eight millions or ten millions--that was decided long before. It was little bitty things--were we going to be permitted to have a seminar, were we going to be permitted to have an account which actually paid page charges for publications? They could not understand it--they thought we were making

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money on our publications, and we told them, no it's entirely the opposite. We have to pay some money to have our work published in recognized journals. And they asked, "Why do you need recognized journals, you could get it published anywhere, you could publish it yourselves."

"But that's not the way it's done." On and on it went...but most of the time we got what we wanted, with a few defeats quite naturally on the way. And I think in a sense, that was perhaps the activity I was engaged in of which I'm mostly proud. It worked, it's still working, I think it's in there to stay. And of course, I must have been very lucky in selecting Jean Labrousse and Lennart as the first staff members because they have both been directors by now, and when Lennart retires, then an entirely different period will start. He really represents continuity back to day one.

Then, after ECMWF, which I think is the finest institution in the world for its particular purposes--I could probably talk for a long time about ECMWF and the various events which took place, but it was again one of these situations where a mixture between hard work and a lot of fun created the right kind of balance. We used to arrange excursions up and down the river for the staff in the summertime, we used to make cheap trips to Cornwall and Wales on a Sunday, because it was mostly foreigners who were over there. We arranged summer tennis tournaments and the English got us to play cricket and we got them to play softball. It was a fine period as long as the weather decided to be reasonable. It is not a delight weatherwise to live in Southern England, but it's mild at least.

Otherwise, I think both Lennart and Jean were probably considered by the junior staff as some kind of slavedrivers...

END OF TAPE 3, SIDE 1

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Interview of Aksel Wiin-Nielsen

TAPE 3, SIDE 2

Wiin-Nielsen: ...I should mention, since we are doing this for the American Meteorological Society, that in the early years we got tremendous help from the United States to get a flying start on ECMWF. I turned to Joe Smagorinsky and asked him for a copy of his GCM (General Circulation Model). I fully expected Joe, with his attitude, to say "no," but I thought well, I'll ask him anyway. And Joe thought for awhile, and he said, "Are they really going to have my model and make European forecasts out of that? It's probably not well-suited for that." But he finally said, "Yes, but under one condition. You send a man over here, qualified, has to be at the doctorate level, has to have some experience, and he stays at GFDL until he understands in every detail the way the model is designed and that it is not a black box--he really understands." I said, "That's fair enough." So we selected Tony Hollingsworth, and Tony, was, I think, for four or five months at Princeton. We paid for the whole thing, but he came home with the model. I turned to Yale Mintz and asked him the same, because he had this nice, two-level fast primitive equation model which had even had some of the best simulations I had ever seen of the general circulation, and there I expected to get a yes. I got a yes, and then I said, "Can we send somebody to really understand the model?"

"Well, he can read that in the report. It's all written up."

I said, "Yes, but it also has to be run, and we actually would like to send somebody."

"Who do you want to send?"

"There's a fellow called Robert Sadourny from France, who has earlier been..." I didn't have to say more.

"Yes! Him we will receive, nobody else." So I had to go and get Robert Sadourny convinced that he should spend half a year in Los Angeles, which he, by the way, I think, enjoyed. So he came home with the next model. In the meantime, we had got a 6600 installed in Kuettner's old space for the GATE program, and we got that up and running. And we actually ran some experimental medium-range forecasts on both the Mintz model and the Smagorinsky model, but I think mainly what they did for us, these two, was to show us the general structure of things as they were and from a detailed study of these two models came, actually, the working plan, where Lennart and I agreed upon, how we were going to approach designing the ECMWF model. I think you also know that Lennart is rather an impatient soul, and he wants results. He said, "What we will do is study the whole literature of a given parameterization. We will test them to the extent that it is possible, we will select the best, and that will go into the model." And that was the way he approached things, in a very un-philosophical, engineering-type of way, and out of that came an analysis scheme, out of that came a convection scheme, out of that came the prescription for when large-scale precipitation should fall and when it should not. Out of that came the treatment of land surfaces and ice. Out of that came the first assumption, of which we were actually wrong. We thought at the time that we could get away with using climatological sea-surface temperatures. That does not work. You actually have to have the most recent sea-surface temperature you can put your hand on, and I think now they replace them every third day or something like that.

Lennart steered the program as a dictator, he put two men on each job, had a staff of about fourteen, fifteen people. He required every single scientist to program himself, the whole thing. There were no programmers at the ECMWF and there never have been. I didn't quite agree with it, but if that was the way he wanted to work, O.K. He didn't drive people away, apparently, because he was also a very humorous guy. Jean in the meantime worked very hard with me on trying to get a suitable computer for us. Now, when I came in January, 1974, during the first few months, I really had my doubts because I had no other information than that IBM was not terribly interested, CDC was working on something, but it was at least several years out in the future before that could come. But then I heard that a certain Seymour Cray had left CDC because they didn't agree with his plans for the next generation of computers. I had met Seymour Cray already when I was at NCAR, because we bought CDC machines early in the game. I called Seymour Cray and asked if I could come and see him and he said yes, that I could. So I flew over there, to the woods where he lives up in Wisconsin, and he showed me a half-done Cray-1, which was still in the horizontal mode--he hadn't put it up yet. He described in general terms what that was. I've always had a lot of confidence in him as a computer designer, and I think I knew already at that time that that was probably the machine we were going to have, if I could convince the Europeans to buy it. So that's eventually what we got. It was not so difficult to convince the Europeans; they trusted us when we said something about speed and capacity and storage and so on, but what they were really afraid of was that here was this so-called Cray Company--who were they? How large was it? How much backing did they have of

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capital? How many machines had they sold?

I said, "They haven't sold any."

They said, "This is terrible--you can't do things like that." Finally, they ordered me back to Minneapolis to find a proper law firm, which could represent ECMWF in the contract negotiations with Cray. They were so afraid of being lured into dealing with a tiny little company that might go bankrupt in the next years. I think they would rather have waited. And I said, "Don't be afraid because we have an agreement with Cray that we only pay as we go for the first few years." The first machine (we really got the prototype after it had been in Texas) was on a lease arrangement, so the risk wasn't terribly large, but they sure were afraid of it. And only after the machine was installed, after they saw that things really were as they had been told, after they saw the "up" time being 98-99% on a 24-hour basis, then they were convinced. And when we actually bought the first machine, they had no hesitation at all.

Lennart continued his model development and by 1979, he was quite sure that he could get a decent five-day forecast. And on that basis, operational forecasting started in November, 1979, at which time I went to become the Secretary-General of the WMO in Geneva, and that was a job totally as an administrator, totally a gray person without any opinion on anything, that's the way the Secretary-General is supposed to work, being the servant of everybody. I think I didn't fit into that situation terribly well. I would say that I perhaps have nobody but myself to blame for actually running for office, which is the only time in my life I have been running for an office where you were elected by votes. That's perhaps my own fault. But I really regret having gone to WMO because I think I wasted four years of my time speaking from a professional point of view. Furthermore, my wife and I were very happy living in England, not terribly happy living in Geneva, which is a strange, artificial community. Yes, we saw the world. I don't think there was a single year of the four years I served in Geneva where I was not at least once on each continent, except Antarctica, every year. A horrible, tiring schedule. I suppose I have the normal Nordic Lutheran ethics, and when you have taken on a job, you carry it through, but it was without enjoyment. I really think I was very happy getting out of the WMO job. Another type of person than me fits into that situation perhaps more like Sir Arthur Davies who had the job for twenty-four years. He really knew all the tricks of the trade. I didn't. Furthermore, I became the Secretary-General at a period where the African continent, not only in WMO, but also in the other U.N. agencies were pushing very hard to get their people in on the high posts. They got it after four years in WMO, finally. They have other representatives

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which are not so fine, but be that as it may.

I don't think there's much to tell about my years at WMO except I did enjoy part of the traveling. I saw things I otherwise wouldn't have seen. I've been in China many, many times. I have been in both North and South Korea. I have visited plenty of the states of Africa and Latin America, and I think I covered two-thirds of the 157 member states in this traveling activity, which was mainly to prepare the ground for the technical assistance which all the developing countries get through WMO, and the United Nations Development Programme. I traveled to New York twice a year to sit on the administrative council chaired by the now not so famous, but rather infamous Secretary-General Waldheim, who was in a sense, my boss. We were an independent agency, but we belonged to the U.N. family and he was the Secretary-General of the whole U.N. I met many people in those years--diplomats, directors of meteorological services and so on. From the point of view of a tourist, yes, interesting. From a professional point of view, truthfully, drudgery. Paperwork, and quite empty in a sense. Because if you are not a developing country where there is a direct benefit from belonging to WMO, I think it could be said that a country such as the United States, a country such as Denmark clearly can exist quite happily without WMO. We get nothing from it; the United States only gives to WMO through its very large contribution, through the very nice attitude which exists in NOAA where they want to help building the global network, but quite frankly, I think most of the industrial nations could be without WMO. The developing world cannot. It's perhaps good to have a WMO, but for scientists or technicians such as myself, it's a very empty place, no challenges of any kind and to just be a polite boy hovering around all the time is not very nice, you know that.

That's not my style, I was happy to get out of it and I should say...they do pay very well. So there was not a financial problem there at all. It was very good. So I think that finished then, in 1983, at which time, after almost thirty years, I made full circle and returned to the country where I came from and they gave me a job which was vacant, as director of the Met Service. I have served in that job about three years, and that had many human and cultural aspects which I enjoyed, returning to a country where you at least know the language and can write it and so on. From the point of view of family and so on, it has been very nice to come back.

Itchy feet I have too, so that's why you find me back here again.

Washington: Thank you.

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END OF INTERVIEW

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