# American Meteorological Society
# Tape Recorded Interview Project

## INTERVIEW WITH NORMAN PHILLIPS

## 2 October 1989

## Interviewers:
## Tony Hollingsworth, Warren Washington, Joe Tribbia, Akira Kasahara

Hollingsworth: This is an interview with Norman Alton Phillips, the narrator. The interviewers for the time being are Tony Hollingsworth from ECMWF (European Centre for Medium-Range Weather Forecasts) and sitting next to Norm, Akira Kasahara. Norm, I guess you have read the documentation and you know roughly--

Phillips: Yes, Warren sent me the package.

Hollingsworth: --know these things. We'd like to begin with your early life. You were born in Chicago in 1923, grew up there. Was there anything in your family background or in your school background that inclined you toward science?

Phillips: Strictly speaking - not. Things which gave me a leaning in that direction I think were two. My parents took me to the home of a friend who was a microbiologist or some kind of biological scientist and he let me peek through his microscopes at fascinating colored slides of animals, plant material. Then the boy next door, who was about six years older than me, gave me his chemistry set, which he had gotten bored with, and I was fascinated by that. I went to the library and learned all about chemistry that way. So that's how I got interested in science.

Hollingsworth: What sort of family background did you have?

Phillips: All my grandparents are from Sweden--were immigrants from Sweden. My father's first job was as an accountant for the Illinois Railroad but he was laid off at the beginning of the Depression. He then eventually became a traveling salesman in spite of what had been a tremendous shyness on his part - he overcame that. And then he became a partner with and eventually ended up as the sole proprietor of a funeral business, funeral undertaker in that part of the south side of Chicago where we lived.

My mother--both my mother and father had only high school educations. My mother worked as a seamstress for a while before she got married, but otherwise was just a homemaker.

Hollingsworth: I see. Did you have brothers and sisters?

Phillips: A younger sister, two years younger than me, Alice. She started out in science also and went to the University of Iowa for several years. Got married during the war, became a homemaker, then went into ceramics, and has only recently backed out of that field.

Hollingsworth: I see. How about hobbies when you were a lad?

Phillips: Main hobby was for a long time chemistry. When I got to high school I knew whatever was going to be taught to me in chemistry and eventually got bored with chemistry in the first year or two of college. I took piano lessons. I got introduced to the French horn when I started high school and that's been an intermittent love and commitment of mine ever since. I think I dallied with stamps, but very, very briefly as a ten or twelve year old. Other than that it was sports. Not too much girls until I was about seven or eight. [Chuckle] I think I was about fifteen - no thirteen - when I had my first date.

Hollingsworth: I see. One of the things that has always struck me about you is that you're a very deeply conscientious and very hardworking guy. Does that come from your family background?

Phillips: I suppose so. I don't know what else to blame it on or to ascribe it to. The Swedes - Swedish community in which I grew up, I realize in retrospect, was an extremely conservative one and they've gotten more conservative as they've aged. But with that conservatism went a certain acceptance of the fact that you had to work hard and so forth.

Hollingsworth: What was it - a religious family?

Phillips: We all went to a Swedish Lutheran Church. My mother sang in the choir. But I would say basically not. I think my father was an agnostic as may have been my mother. I'm an atheist, I guess.

Hollingsworth: How about your teachers at school? Were there any of those who [interruption]

Phillips: They were <u>all</u> very good so I don't, I can't pick out any particular ones. I enjoyed the teachers; I enjoyed school. In retrospect, maybe at that time I didn't. I think I enjoyed it. It was easy - I didn't have to do any real studying until....I should have studied in my first year or two of college but I didn't. I didn't really study until I joined the Air Force Meteorology Program in World War II.

Hollingsworth: How did you come to join that?

Phillips: Well, the war had been declared in December and all I was doing at that time - December of '43 -Pearl Harbor.....

Hollingsworth: '41.

Phillips: '41, I'm sorry, '41. They had a weak ROTC at the university at that time. I joined that but I realized that wouldn't do very long and I heard of the Meteorology Program at the University. I didn't know anything about meteorology, I think I could have spelled the word. But, for example, I had no idea that I think of anything as simple as the fact that if there was a north wind blowing for several days in Chicago that meant that that air had come from central Canada. I never made that connection.

I was the only one in my fraternity who joined this program. I was very lucky because I was at the University of Chicago and that was one of the few places in the country that had that program.

Hollingsworth: Was it fortuitous that you joined the meteorology part of the ROTC or was there something that drew you in there?

Phillips: Well, it wasn't part of the ROTC but if you agreed to be drafted and....but your records were understood to be marked that you would be sent to the meteorology training program. That was the only interesting program I knew of. Otherwise, it was just be drafted and who knows what would happen.

Hollingsworth: So once you joined that program, what happened then?

Phillips: We had two rainy, cold months in the winter of 1942-43 in Keesler Field, Mississippi-Basic Training. Then we went to the University of Michigan where there were about 500 of us in what was called pre-meteorology. We were students who had not had a sensible dose yet of mathematics, calculus, vector analysis or physics. So the first three (3) months were spent giving us a very heavy dose of that. Then in that summer, which must have been the summer of '43, we were sent to meteorology school. I'm not sure all of the class ended up in meteorology school because by the time I graduated from meteorology school a year later, the Air Force didn't need very many meteorologists and there were very few of us who ended up as meteorologists.

Hollingsworth: So you graduated then in .....

Phillips: They sent me to Chanute Field which is in Rantoul, Illinois - close to where the University of Illinois is located in Champaign.

Hollingsworth: And then once you finished that basic stuff while training in math and physics and synoptics, I guess, you were assigned to....

Phillips: Well, we were given, we were assigned to the active Weather Service squadrons or stations. I went first to McCook Field in Nebraska for a month. This was a training base for B-29's which, if I remember right, until that time had not yet been used in combat. There is where I remember distinctly one of the pleasures of that month was that I first encountered a steak that was more than a quarter of an inch thick. I remember what that looked like more than I remember what the B-29s looked like.

Then I was sent to Grenier Field in New Hampshire which is just south of Manchester. Somewhat amusing because that's where I now live - across the Merrimack River from Grenier Field. This was the headquarters of the Eighth Weather Squadron and they serviced the airfields in the North Atlantic that were used primarily for ferrying planes, fighters and cargo planes, and generals -both U.S. and captured German generals -across the Atlantic.

I was fortunate that the head of that weather squadron was Arthur Merewether who had been head of meteorology for American Airlines and I believe he still was after the war. I was sent to a station in the Azores where we had to consider the weather over the entire Atlantic Region in order to service the flights that came to and left the Azores.

Hollingsworth: It must have been very difficult forecasting in the Atlantic at that time. Were there any weather ships?

Phillips: There were weather ships until VE Day - they were coded. But the codes were rather simple as I remember and I suspect that the weather ships were there because the Germans knew how to break the code, and they got the weather reports as well as the Allies did. Otherwise, we did get some weather reconnaissance reports. Mostly the ones we used were those from our own U.S. weather recon.

Hollingsworth: Did you ever go on any of these recon flights yourself?

Phillips: Yeah, I went several times. It was there that I met Joe Smagorinsky and I substituted for him once or twice, I believe. I don't know what he did when I substituted for him - I don't think he went to the weather station. But it was very impressive to ah, you know in school we'd seen these weather maps and you could see a front on the weather map. Cool air and warm air, wind shifts. But you were not that much....quite that much impressed by the usual fronts as they passed you on the ground. But when you're traveling through a frontal zone at, I suppose 150 miles an hour, things are compressed a lot more and you could really see what was a discontinuity. Showers, wind shift - you could see the winds by the foam on the waves. In fact, we took reports where the pilot would, I guess, fly a triangle and give us some kind of measurements of drift and we would use those to derive a wind - drift winds. It was very interesting. I think the entrancing thing about it was that you saw what was happening over what is a large area of the globe. And at that time, you know we didn't think of seeing the whole earth, like from a satellite. We were seeing it over an area that was larger even than the Weather Bureau forecasters were used to doing over North America. And occasionally you would see something go by you locally that you would have to try and unravel in terms of the models that you'd been exposed to in forecasting school. And we had some good teachers there.

Hollingsworth: This would have been the Norwegian model I guess.

Phillips: Yeah, the frontal model. I remember once how proud I was in the winter when we had a front pass and within 20 hours the wind backed around to the southeast, then to southwest and to northwest and although we were a single station out in the middle of the Atlantic, I felt I was entitled to send out that particular five figure code which said the front has passed - the cold front has passed. But it was very challenging. It was challenging in that you knew that you had to think a lot when you were on the job. There were some near misses. I remember once one of the planes for which I gave a wind forecast got blown over the Normandy peninsula at the time the Germans were still in charge of St. Lo and they were shot at - a passenger plane! Another time, I sent a flight to Newfoundland, and Gander zeroed in and I wasn't sure myself whether the plane had enough fuel to go on to Stevensville on the other side of the island. There were tense moments.

Hollingsworth: Yeah. Marking the PNR - point of no return - on a flight was always a hair-raising business. So you continued in that then up to some time after the war, I guess. Continued with the air weather service.

Phillips: Well, I was still in the Air Force at the time of VJ Day. Martha and I had gotten married just a few days before VJ Day. We didn't in fact realize it was VJ Day because our honeymoon overlapped with VJ Day and we weren't reading any newspapers at that time. We saw all these people dancing around and church bells ringing, kissing one another. Had to ask someone what was happening. They told us the Japanese had surrendered. And then I was transferred to an administrative position in the Eighth Weather Squadron, I guess, because the senior forecasters who had been in those positions were being discharged earlier than I was as a younger person. I was put in charge of communications. So I had to learn codes and communication circuits, things like that.

Hollingsworth: I see. So when finally did you leave the Air Force?

Phillips: I think in August of '46.

Hollingsworth: So you presumably had decisions to make at that point, about careers.....

Phillips: Oh, yeah. I think I had decided already in the Azores that I was going to go back to school. But instead of chemistry, which had already become very boring, I was going to take meteorology. At that time I remember thinking that the obvious way to improve forecasts was with statistics. And so one of my first, when I got back to the university, that was one of the courses I signed up for. It wasn't easy to sign up for courses because there were a lot of returning GIs, the classes were full, and a lot of the professors, I think especially the young ones, were very protective of the possibility of diluting the caliber of the students. So I had to argue, for example, with a professor that I would probably be able to pass the first course in mathematical physics at the university. This was taught by physicists at the University of Chicago. The mathematicians at the University of Chicago had nothing to do with applied math. I suspect they probably still don't. And I was fortunate I took statistics from Tjallings Koopmans, a Dutchman. I actually took class notes and got paid for it, ran them off on a little hectograph. I think he later on was - he got the Nobel Prize for Economics. But all I remember from him mostly is the 1/n factor in the standard expression for the standard deviation and things like that....sampling theory.

Hollingsworth: So when you went back to college you still had to finish your BSc, I guess.

Phillips: Yes. They gave me credit for about one additional year, so I had three years of college credit. So I got a BS after the first year back at the University.

Hollingsworth: Was there any meteorology in that?

Phillips: Yes, there was. I took several courses in meteorology. Dynamics, taught the first semester by Victor Starr who was....well, did you take courses from him at MIT?

Hollingsworth: Yeah.

Phillips: Then you remember he...then you have met him at least?

Hollingsworth: Uh hum.

Phillips: <u>Very</u> methodical, clear-cut lecturer and according to George Platzman's reminiscences that I've read, Victor was far ahead of his time in relating things like the finite difference Laplacian of the geopotential to relative vorticity, things like that.

Hollingsworth: Oh, so those ideas were novel at that time.

Phillips: Oh yes, definitely.

Hollingsworth: Who were your other instructors in meteorology at the undergraduate point?

Phillips: Horace Byers gave a course in physical meteorology and there was another course from Bellamy....I don't remember exactly what I got the first year, Tony. I'm mixing them all up.

Hollingsworth: Then in '47 you would have started the post-graduate....

Phillips: Yes, but there was no clear-cut line between the courses. No one really knew very far in advance which courses he would be taught in any given semester.

Hollingsworth: Did you get any courses from Rossby?

Phillips: Yes, except it was clear it was a .... this was when I had been there three or four years and he would, I think, tell us whatever he had been thinking about himself - either the previous night, previous day or in the previous several months. During the time I was there, for example, Palmén was one of the visitors. Rossby had a lot of visitors, especially from Europe. At this time the jet stream was being analyzed intensely by the Chicago school. Rossby talked about mixing of vorticity versus the mixing of angular momentum. He used and he also introduced us to some of the oceanographic things that he had done in the late thirties, two level models and we studied....towards the end of that period we would read Kahn's paper on adjustment - geostrophic adjustment. We didn't get exposed to all of Rossby's papers, I think partly because he was not that methodical a person, although I think we could have learned. We took dynamics, for example, also from Dave Fultz, who was also very methodical but he concentrated on going over about fifty percent of Horace Lamb's book. Much of which has turned out to be good to know in the interim, but probably was not as exciting as would have been having to read the series of papers Rossby wrote when he was at MIT and Woods Hole.

Hollingsworth: You would have studied the famous paper on the centers of action, then?

Phillips: Oh yeah, we did that one. But not the oceanographic ones or too much on adjustment.

Hollingsworth: There was a famous controversy between Palmén and Starr in the early fifties. When did that start up?

Phillips: Well it must have begun at the university in the time I was there. Starr - the controversy you are speaking of is the one where Starr thought the general circulation ... that almost all aspects of the general circulation ... and obviously simplifying things--could be explained by horizontally acting eddies. Palmén on the other hand preferred to emphasize the role of the classical meridional circulation, where zonally average(d) south to north velocity components that would be transporting heat and momentum. Much as had been described in the **Physikolische Hydrodynamik**, the Norwegian bible. Palmén eased off on this and eventually Starr wrote a short article in, it was published in, I think it was still **The Journal of Meteorology**, yes, in which he essentially outlined, as if no one else had ever done it, what happens in a cyclone when cold air masses move equatorward and warm air masses move northward and there is overturning in the east-west plane. Some of that might spill over into the north-south one. So I think they both realized that there was something to be seen in the other view.

Phillips: The other controversy that I knew about was between Rossby and Bjerknes. Bjerknes had come out with his isobaric channel theory of what made cyclones move and develop. And, while we were told that that theory existed we were never expected to understand it or try to understand it. Rossby put all his emphasis on the windfield and vorticity. As is also the case with Palmén and Starr, nowadays we would tend to blend those views.

Hollingsworth: You did your master's thesis with Palmén as a supervisor and then you started in on your PhD. work, I guess.

Phillips: Oh no - there was a long time in between. I did the master's thesis in '48. It was a synoptic paper describing the subsidence of a cold dome with Palmén.

Then I worked for a year and one-half to two years with Kuo on a Navy research project that Rossby had. Mostly under my own - I was doing my own things, as was Kuo in that time. I was doing relaxation and learning about relaxation and Southwell's technique. Kuo was writing his Doctor's thesis on barotropic instability. But towards the end of that period Rossby, I suppose, had to produce something for the Navy for paying us our salary and had me publish, pretty up and write a paper on some of the cross-sections that I had been drawing on the 80th meridian. I think I drew a whole winter's worth of cross-sections. Many of them in a rather sketchy fashion and I remember Rossby's shudder at the appearance of some of the cross-sections. So I learned quickly to draw them nicer. The jets were more complicated than the simple picture that had previously existed. Palmén together with his student, Newton, (and so forth), usually waited for the most clear-cut case to draw because they were after all proselytizing a new concept that there was this jet, in Palmén's view, connected and indistinguishable from the main polar front. The main polar front was still a mysterious concept whose explanation had not progressed beyond Bergeron's ideas. So, there was some usefulness to this more detailed picture of little jetlets.

Hollingsworth: There was a peculiar phenomenon in that paper that the jets seem to continually migrate southward if I remember - or maybe it was the other direction.

Phillips: Yeah, I often wondered about that, both then and since occasionally. I suspect it must have something to do with the .... either drawing them at various positions with respect to the trough. It must be that on the ridge they're harder to locate.

Hollingsworth: So when did you get going on your thesis then?

Phillips: I think it was about a year before I graduated. I remember thinking that, as I recorded elsewhere in this talk that I gave at NMC, that why not take Charney's paper -- you see, there was very little literature. You had the **Quarterly Journal**, the **Journal of Meteorology** came out - the **Quarterly Journal** was only four times a year - and there was the German journal which at that time, in the late forties, was really not accessible to us as Germany had not recovered that much from the war. Rossby was just starting up **Tellus**. So there were very few journals we had to read so we were easily able to keep up with many things. Even not necessarily in your own specialty. I don't know who drew my attention to Charney's paper in **Geofysiske Publikasjoner**. We knew of his thesis, although I don't think anyone perhaps except Kuo, either graduate or faculty, really read Charney's thesis. Perhaps the other Chinese - T.C. Yeh - probably read it and understood the mathematics of it. But Charney presented this recipe for how to predict large-scale motions. This is about the time that the first barotropic forecasts were made, so it seemed like it was going to work and so I had this idea, why not try a simple baroclinic model?

Hollingsworth: So before you started work you knew about the Charney work on the ENIAC and von Neumann?

Phillips: I'm not sure, and if I were to delve into it I would have to .... let's see, that came out in '49. Of course, yes, I must have known about it.

Hollingsworth: Now what motivated the two-layer approach?

Phillips: I had just seen Rossby use such kind of models in his lectures and then I remember Eady's paper came out while I was working on my thesis and I got the idea to convert from the finite .... from the two-layer aspect to a continuous model by matching up the different parameters in the instability formula for ...

Hollingsworth: But there was no idea at that point of interpreting the two-layers as vertical kind of differences.

Phillips: No. Jule suggested that. I think in the, must have been the summer of '52, I remembering getting a letter from him when Martha and I were in Chicago on vacation after I had been at the Institute for most of the year -- in which he suggested that that might be a more fruitful way to look at a two layer model, and it certainly is.

Hollingsworth: So in your thesis then, if I remember well, you formulated a two-layer model, you studied its instability characteristics, derived those, and they seemed to match rather well with Eady's.

Phillips: Yes.

Hollingsworth: There was [interrupted by Phillips]

Phillips: The beta effect was not in Eady's paper directly at that time, or otherwise.

Hollingsworth: No. So that thesis then must have got written rather fast.

Phillips: Well, I don't think it got written as fast as yours did.

Hollingsworth: No - nevertheless it was....you started to work and you finished it one year later.

Phillips: Yeah, but I suppose I had been thinking about such matters for the previous two years and reading about them and so forth. It really was not that much of a flight of imagination. And in fact, two-layer models, several two-layer models came out in the following year.

Hollingsworth: So you graduated from Chicago then in 1951 and you had no problem getting a job.

Phillips: No. Although I suppose a certain amount of inertia on my part or diffidence or shyness, I think I was - up until fairly close to graduation - I was expecting to apply to the weather service for a job. But George Platzman -well let's see. Oh yes. I had participated with George in the second ENIAC expedition sometime probably in the spring of '51. So Charney knew me and I had had an opportunity in early '51, before I heard the Eady paper, to present my ideas to members of a brief meeting with Rossby and Charney in attendance, and told them that I was having difficulty. I wanted to get the instability formula but the fact that the coefficients depended on y presented mathematical problems that I didn't think I could solve and they said "well, why do you have to allow for that." They more or less encouraged me to be very bold with that one. Then the instability formula came right out and then it was possible to compare that with Eady's. The things that I short-circuited in that way later on were the things that should have been short-circuited according to the formal quasi-geostrophic theory.

Hollingsworth: Were you astonished? Because I certainly was astonished that such a good match was possible.

Phillips: I remember being elated. I don't think I was astonished but I remember being elated. It did work nicely with the Eady stability formula. Perhaps the astonishment first came when I first read Eady's paper and I could see the stability diagram.

Hollingsworth: Because you'd already worked....

Phillips: I'd already gotten a similar diagram.

Hollingsworth: I see. This might be a good place to pause and flip the tape.

**END OF TAPE 1, SIDE 1**

## Interview with Norman Phillips

### TAPE 1, SIDE 2

Hollingsworth: We are continuing the interview with Norm Phillips on Monday, October 2nd. We've just been joined by our third interviewer, Joe Tribbia from NCAR. OK, Norm, we had voiced off the tape, we had got to the point where you had, you were telling us how you'd been involved in the second ENIAC expedition while you were in Chicago.

Phillips: Yes, that was where I began to learn more about how fortunate I was to have Platzman as a thesis advisor. He was able to explain to me what was happening with the Fourier method that we were now using in the second ENIAC expedition for inverting the Laplacian in barotropic flow. It wasn't too long after that that he found a numerical error in my thesis which gave Martha the shudders. But it was not difficult to correct and it was, in retrospect, not a very significant error. George, as you know, has a characteristic of being accurate as well as being right. And I think I've, I hope I've learned some of that from him. At least being accurate - that's easier to be accurate than be right.

Hollingsworth: Did he have much influence on your thesis work -Platzman?

Phillips: Not until he gave me suggestions on the final writing up. Rossby's philosophy at the university was very hardnosed. The faculty was not supposed to suggest thesis topics to the students. I think that was his position from Europe.

Hollingsworth: So anyway, you knew the people in Princeton. By the winter of '50-'51 you'd worked with them. And then you moved to Princeton. Who was there when you got there?

Phillips: Jule and his wife, Elinor. The Smagorinskys, Joe and Margaret, were there. Joe had participated in the ENIAC expedition as well. He was studying at NYU while working for the weather service and also spending time at Princeton, I presume, because Harry Wexler, who was head of research in the Weather Bureau, felt that numerical weather prediction was a sure bet and wanted the Weather Bureau to be involved in it and take advantage of it. Then Charney usually had a visitor. When we first came there it was Ragnar Fjørtoft. Fjørtoft had been there. Fjørtoft had been there for the second ENIAC expedition I think - although my memory is a little hazy at this point. We had Gambo, Gambo came shortly after from Japan. Davies from Kings College in London came. Eady had been there a year or two earlier. Platzman was occasionally there for several months at a time.

Hollingsworth: I remember in the conclusion of your thesis you were really bullish about the prospects for NWP already at that point. Even though you really in your thesis you'd set up the model and done the instability and done some tendency computation.

Phillips: Yes.

Hollingsworth: When you got to Princeton then it seemed there was a whole plethora of two-level models around at that time.

Phillips: Yeah. They didn't come out of the woodwork but they may have come out of people's desk drawers or something like this. The people working with Sutcliffe in England - working with Sutcliffe --finished his geostrophic reasoning and ended up with what Charney had done in '48. Phil Thompson I think did a PhD. thesis - I'm not sure if it was his thesis - at MIT with developing a two-layer model in a slightly different way. Eady, I think, did one by finite differencing his Boussinesq equation.

Hollingsworth: Did it?

Phillips: Undoubtedly, undoubtedly.

Hollingsworth: So did you start programing one of these?

Phillips: Sure.

Hollingsworth: Right away or....

Phillips: Not when we first came. When we first came, we programmed the barotropic model using Southwell's, or the extension of Southwell's relaxation called extrapolated Laplacian inversion. We did that in the barotropic model. And then we extended it to a two-level model. I don't remember whether it was extended to three-levels with the original Princeton computer or whether that had to wait for a larger machine. I don't remember that.

Hollingsworth: When you say you programmed the Liebmann - extrapolated Liebmann relaxation type thing. That wasn't for the ENIAC anymore. That was for the...

Phillips: No, this was for the computer that von Neumann was designing and having built at the Institute for Advanced Study. Quite contrary to all the philosophy of the Institute for Advanced Study. The fact of which ultimately led Charney to go to MIT and not stay at the Institute. But, the developments were being written up and were being passed around the country. I think there was a parallel engineering effort at the University of Illinois. Probably one in California. And I think it was used .... that was the pattern the Swedes followed when they developed the BESK in 1953-1954.

I don't think the English followed that. They had their own theoreticians with Turing and, the man at Cambridge, Wilkinson....

Hollingsworth: Wilkinson, J.H. Wilkinson was it?

Phillips: The guy who worked on upper atmosphere--Wilkes.

Hollingsworth: Wilkes.

Phillips: Wilkes.

Hollingsworth: Exactly. Was...did the invention of the stored program computer...was von Neumann's great achievement and was really revolutionary in the history of computation. Did you feel at the time or were you aware at the time that this really was a very revolutionary concept, or did it just seem like the natural next step after the ENIAC?

Phillips: My first exposure to computers was to von Neumann's except for the ENIAC. I certainly knew the difference between how the ENIAC worked and how von Neumann's worked - the logic of it in a crude external sense. For one thing, we were very intimate with Julian Bigelow and his wife who lived in the same housing project we did and he was the chief engineer as well as being mathematically inclined and participating in the mathematical, in the choices that had to be made between....the tradeoffs that had to be made in design. So there was considerable exposure to those kind of ideas.....

Hollingsworth: ....in the literature in England there is considerable emphasis put on the theoretical view that Turing developed of the potentialities of such a machine. Were these ideas current at all in Princeton?

Phillips: They may have been, may very well have been. I was not that deeply involved with the design of computers. I was involved in programming for them in meteorology.

Hollingsworth: One of the things I have never understood, perhaps because I haven't read enough about--

Phillips: I met Turing very briefly when I went with Eady....took a trip with Eady to Manchester. He seemed a very lone worker and not....Eady didn't make an attempt to approach him too closely, I mean too intimately. Perhaps there was a reason for that, I don't know. But I knew of Turing in that general time frame but I didn't know then and I don't know now the details and chronology of ideas that he had and ideas that von Neumann had.

Hollingsworth: One of the things that has struck me about the development in Princeton over the eight years or so that it was running there, or longer, was that meteorology was very fortunate that von Neumann saw it as a prime application.

Phillips: Yes. I think Jule has discussed this in his interview with George Platzman and has traced the background of that a little bit. Jule thinks that Zworykin at RCA in the Princeton area, who was a friend of von Neumann's, had been interested in weather and modeling weather with an electronic mechanism of some kind. Jule speculates that that might have been what introduced von Neumann to the possibility. There was this meeting you probably know and have heard of that Jule refers to and Platzman has referred to. In about 1947 (correction: August 1946; NAP), before Charney's paper on the scale theory, after he had done his instability theory, a meeting which took place in Princeton involving von Neumann, and various other people, including Charney, Haurwitz, Starr, Rossby, in which the idea of using a computer in meteorology for forecasting purposes was more or less broached officially to the meteorological establishment by von Neumann.

Hollingsworth: So the initiative came from him?

Phillips: I'm pretty sure it did.

Hollingsworth: Oh....(I thought?) the other way around. So you started then when you got sort of working with the extrapolated Liebmann procedure.

Phillips: Yes. It was very easy to do because it was just a mechanized version of what Southwell was writing about in his book on relaxation.

Hollingsworth: When did work on coding up the two-level model start?

Phillips: .....at that time in '51 we started programming I remember in the middle of the winter '51, '52. It must have been in the, I would guess probably....and then Charney wrote that note to me on vacation in the summer of '52. So it must have been September of '52. We had picked out this storm which was the one I had used for tendency calculation in my thesis. Bolin had independently picked it out to make studies on for what might have been the first baroclinic model if I hadn't come along it was an advective type of approximation in which there was much less relaxation involved. We used the same storm for both the barotropic model and the baroclinic model two-layer model. Then Charney, Schumann and Gilchrist (later the computer director at Columbia).....

Hollingsworth: This was Bruce....

Phillips: Bruce Gilchrist formulated a three-level model which Charney thought gave a better prediction of that storm. I still think it didn't, but that the faults were inherent in the geostrophic system and in the....geostrophic system and the questions of what geopotential or what windfield do you start with? We took the easy way out. We used maps of geopotential which define the windfield. So the approximation was in the wind with respect to the data. You can take the opposite tack - you can take the winds which are almost nondivergent, get the vorticity field from them, then use that to determine a balancing mass field. But now that's not going to agree too well with the temperature field. So then you make an error in the temperature field. I suspect, and perhaps Chuck Leith has alluded to this in his paper on the slow mode initialization that if you started with the slow modes with the winds and mass field, which of course are geostrophically related in the slow modes, if you started with those from an initial cyclone with the geostrophic model, and then when you're done with the geostrophic model, calculate the fast modes, modify the vorticity in the windfield and the mass field, you'll do better than if you start with either one or the other of those.

Hollingsworth: I remember your comments in that paper with Charney/Phillips where you said you were already certainly expressing doubts on the adequacy of the quasi-geostrophic thing - but I thought it had more to do with the sharpness or the fact that it couldn't handle a gradient wind in a tight circulation.

Phillips: Yeah. I no longer remember too much about which parts of that joint paper with Charney were his or which were mine.

Hollingsworth: Was there a clear separation of function or were you both...

Phillips: I just don't remember. Some of them I can probably still identify as his. The one about using geostrophic normal modes in the vertical to separate out the simplified equations into barotropic mode, two-layer mode, three-layer mode so to speak were things that Charney worked on, with the mathematician that he had with him. I had nothing to do with that.

Hollingsworth: So that paper was published....that paper with Charney....it was published in 1953 then. By the time you'd finished that work then you were ready to take off to Sweden?

Phillips: Yeah. Rossby sent an invitation - I don't know how I heard of it - to visit the institute there. Rossby had somehow, I believe, gotten support from the Office of Naval Research for his institute in Stockholm. 'Cause there was always a Naval officer present there, Dan Rex and others, and frequently an Air Force officer, Harold Bedient, was there. I remember I went there. I traveled on a Military Air Transport Service (MATS) plane. I went first to England and spent two months in Imperial College and then went to Sweden. I was going there to help the Swedes program up the barotropic model. Now they probably could have done it just as well without me but I enjoyed it. Probably enjoyed the feeling at that point that I was playing a key role, I don't know.

Hollingsworth: On the way through England you had two months at Imperial there.

Phillips: I arrived in London on the day that Queen Elizabeth was crowned and fortunately had made arrangements in advance for the room in which I was going to stay.

Hollingsworth: It was a very wet day if I remember.

Phillips: That's from an Irish viewpoint.

Hollingsworth: Those were the glory days for Imperial College.

Phillips: Yes. Shephard, Ludlum, Scorer, Mason, Eady.

Hollingsworth: The most mysterious of those people for me is Eady.

Phillips: Oh - that's nothing new!

Hollingsworth: How well did you get to know him?

Phillips: I think as well as anyone got to know him in two months because I spent a lot of time with him. I ate lunch every day with him. Went to a little restaurant down the street from the college and ate some horrible kind of spaghetti usually. Talked about meteorology most of the time. He was working with Fourier doing Fourier equations in the way - I'm not sure in the way they're used now where they did the transforms or whether he actually did the formal multiplication of the trigonometric series. I suspect it was the latter. But I remember when I went up to Manchester with him he had use of the computer up there at night. So we travelled up in the afternoon. I remember he insisted that as a matter of principle that we travel in a first class carriage. It may have been because then he could sleep on the way home on Sunday mornings. But we went up there on Saturday afternoon and worked late at night. In the middle of summer, of course, it was more or less light at night and he used, I remember, had the use of a whole bunch of crazy characters - pound signs and....ah....that was the Turing machine, the machine at Manchester was not Base 16 or maybe had a larger base so you needed more than the conventional alphabet to write mnemonic code, to write the code with paper and pencil. I didn't write any code, I just watched and talked with him a little bit while he was doing it and using the machine. Slept on a bench. He was very pleasant. He took me to Cambridge. Took me around Saint John’s College, that's where he went to school - at Cambridge. Introduced me to Wilkes.

Hollingsworth: Nice time.

Phillips: Oh yes, he was very nice.

Hollingsworth: In your own work at program [interrupted]

Phillips: I remember he was....the thing I remember most about Eady meteorologically was that he thought something was wrong that the geostrophic approximation was not leading to better forecasts than it seemed to be leading to at that time. Either the ones that Charney and I had done or the ones that the English group were beginning to turn out with the computer from Lyons - the Lyons computer.

Hollingsworth: You commented in one of your papers that was a clear difference in quality between the ocean and land forecasts. Was that....was.....you said that on forecasts in your papers over the North American continent were better in quality than the forecasts over the ocean. I guess in England if they were trying to forecast then they had a much worse data problem then.

Phillips: Oh, you mean one of my later papers.

Hollingsworth: No, I think it was in one of the early papers and in that series of forecasts with Charney.

Phillips: Oh, it doesn't come easily to mind. I don't remember what would have led to that.

Hollingsworth: Did you ever feel that you got to the bottom of the substance of Eady's comment.

Phillips: No. I think at the time he was thinking something more sophisticated was necessary or return to the primitive equation. But I don't think he ever said let's go back to the primitive equation. It remained for Hinkelmann to force that.

Hollingsworth: The Hinkelmann was really quite active by '53.

Phillips: Yes.

Hollingsworth: They'd set up a group....well he had a paper in '51 so that group had been active for several years already.

Phillips: They didn't have a computer they were able to come to the United States and borrow one.

Hollingsworth: Computer time...

Phillips: Yes.

Hollingsworth: Did you meet Hinkelmann at this phase....in the early fifties?

Phillips: Not in the very early fifties. I meet him later on in the mid-fifties.

Hollingsworth: The other thing that was going on in Europe at that time was, I'm not sure whether it was Europe or the States, was this remarkable theorem by Fjørtoft which was published in '53.

Phillips: Oh, about the energy going one way....

Hollingsworth: Energy going one way and then the other....

Phillips: Energy having to go in two directions, it couldn't go in only one direction.

Hollingsworth: Did you see the relevance of that to your work?

Phillips: No. You mean in terms of what it would say about a geostrophic spectrum and all that sophisticated stuff?

Hollingsworth: Yes.

Phillips: Absolutely not.

Hollingsworth: Absolutely not at that time. At what point did you [interrupted]

Phillips: And I don't think Charney did either. He certainly would have felt intuitively that it was a very fundamental thing, more deeply than I did. Perhaps more deeply than even Fjørtoft did.

Hollingsworth: Have you any idea what kicked off that idea in Fjørtoft's head? Why would he go look at.....

Phillips: No. The idea occurred to other people, I gather, from what I've read. Charney refers to some of it.

Hollingsworth: Some time around this period - early fifties - is the first reference that I come across to Ertel's work in the American literature. When did you first hear about this *wirbelsatz*?

Phillips: When I was a graduate student at the University of Chicago I knew about the three dimensional vorticity there. I don't remember the context in which I learned about it. Rossby and Ertle had published a joint paper, I think in the late forties. Kleinschmidt was using the concept of potential vorticity as a tracer and indicator of cyclogenesis but in a not....He would say there's a big blob of large potential vorticity up in the stratosphere and that explains a cyclone on the ground. And those ideas are now being exploited, re-exploited by McIntyre and his people and other stratosphere meteorologists. I remember in specific I had a paper with someone else....Newton and Bradbury. Dorothy Bradbury and Chester Newton and I had a paper about jetstreams near the tropopause in summer over North America. Rossby had pointed out to us the sudden appearance of a sharp cutoff and the sharp shear and the tremendous vorticity that this engendered. I remember taking on the job of computing the potential vorticity. I don't know whether it's in that paper or not, but I computed the potential vorticity, then showed that it could have come from pre-existing potential vorticity in the stratosphere if you were going to allow the air to be stretched vertically. Course I didn't say what made it stretch vertically. We were using the concept of potential vorticity quite familiarly in the late forties.

Hollingsworth: Already then?

Phillips: Yes. What is the date on that paper? Have you got that?

Washington?: '51.

Hollingsworth: Well certainly in the Charney/Phillips paper of '53 you were working with theta coordinates and you were at least suggesting that they be used.

Phillips: I think that's there mostly because Jule liked to put in a paper whatever the last words he had thought about the night before in that paper.

Because we never followed up on that potential vorticity. It got too difficult, I think.

Hollingsworth: One of the things that startled me when we got to England and were going on to Sweden still, but just going back to Princeton for a minute, you say in that sack lunch seminar you gave at NMC last year, that you managed at one point to make von Neumann sit up and take notice when you wrote a model in 50 words.

Phillips: Yeah. I think I could do that because I could write a little code which would go around the five points, the center and the four surrounding points, both to compute vorticities and to compute Jacobian(s). So that little box of codes, which consisted mostly of instructions to add numbers to addresses on the computer did almost all the work and you just had to have a few outside references that tell it what it does, now. It was like what now is called a little subroutine. That word I don't think existed at that time. But engineering-wise, it was a disaster because the tube would have burned out if you had run it without slowing up the operation by putting in fictitious multiplications.

Hollingsworth: And you actually did that?

Phillips: Yes, and that code was used for a long time as the diagnostic code on the computer to find out what was going wrong with the computer because that code, expanded beyond the fifty words because it was moving so slow, and to a lot of diagnostic tests as well. It was general purpose - not general purpose but it was used by the engineers and I wrote out a manual on what's probably wrong when the code stopped at this point.

Hollingsworth: One of the....I can almost tell when a guy learned computer programming by the number of comments and by the structure of the code. I've never actually seen one of yours but I've seen code written by some of the people who are a little junior to you and code at that time was just a....

Phillips: Yes.

Hollingsworth: It was just awful.

Phillips: It was thought to be an esoteric art which would be passed on in an apprentice system and kept a secret code society.

Hollingsworth: A lot of job security there!

Phillips: You know this cartoon where the family's still standing at the grave side, it's raining, and the deceased has just been buried obviously? There's a guy comes up obsequiously in a raincoat and says "Excuse me, did John ever mention anything about source code?"

Hollingsworth: So anyway, you had done your series of forecasts with the two-level model and you'd written it up, and then you took the high road to Sweden stopping off in England on the way. Then you got to Sweden for, sounds like six months or so from mid-'53 to....

Phillips: I was in Sweden from August through January and then in Norway (Oslo) two months.

Hollingsworth: Who paid for the trip?

Phillips: I got some salary from Sweden, from the University, and some still from Princeton. Transportation across the ocean was MATS for me. My wife's transportation and family's transportation was up to me. We sold our car and bought tickets for her on the "Stockholm."

Hollingsworth: About this time the BESK machine was being built, I guess. So you were building the computer or implementing a model on it at the same time.

Phillips: Yes. Fortunately, everything came to a head in early December '53. I remember Rossby had hoped to have it done before he went with his wife, Harriet, to a vacation in Capri. But, we made our first forecast and sent him a telegram. We felt very good about it and I'm sure it added to his enjoyment of Capri. We had paper tape as input.

Hollingsworth: That was a big advance was it?

Phillips: We had used paper cards at Princeton. But then for a while we did go into paper which was used, it may have been used to punch the cards which were then fed into the machine. Although von Neumann was an IBM consultant, I remember hearing that it took a lot of arm-twisting on his part before IBM would let the card reader be modified. Their principles....paternalistic principles of governing their rentals were very strong.

Hollingsworth: A few months after that you'd moved on to Oslo by this time, I guess. You perhaps were even back in the United States. The Swedes began to produce forecasts that could be used in real time.

Phillips: Yes. As I remember they did the first one in the spring of '54.

Hollingsworth: Did you feel bad about that because I'd have thought the U.S. should have been first to that particular milestone.

Phillips: No, I don't....I may not even have been aware. They may not have even notified me. I suspect they didn't, when they first did that. I only found out about that fact later.

Hollingsworth: In the time you spent in Stockholm and in Oslo, over this period of '53, '54, you were obviously working on the plan for the general circulation experiment. You certainly wrote up that plan in that paper in **Tellus** in '54.

Phillips: That was....see I'd been exposed to these experiments in Aberdeen- with the ENIAC - in which we were doing numerical experiments. The first one was with actual forecast data but the second one was hypothetical things with barotropic flow and with a jet to see what would happen. We got a lot of "noodling", so nothing was ever done with that. But that idea I guess I got at that time and I thought I told them, well, why not try something more beautiful, a baroclinic one which might imitate the general circulation. From graduate days in Chicago we had a pretty good idea of what the leading theoreticians and synopticians thought about how the general circulation worked. So it was not too difficult for me to first do this study in the '54 paper to see what baroclinic unstable waves might do- force an indirect circulation and then because of that particular model it would....the lateral north and south boundary conditions would require direct circulation further out towards the pole and the equator. And that this indirect circulation, in middle latitudes was the process, turbulent process that Rossby always referred to vaguely as giving rise to the surface westerlies. The explanation of surface westerlies had been the main challenge in the general circulation for centuries. They all knew that a direct circulation with the equator flow would not produce westerlies. So they had to put in little extra wheels, to end up creating polar flow in mid-latitudes. This seemed to all fit together so it encouraged me to go back to Princeton and convince Jule with that paper that yeah, that should be a logical thing to spend my time on. He was my boss.

Hollingsworth: One of the intriguing things....many interesting aspects of that paper, one of them was the idea of, if you like going to second order in a Rossby number, in that you had calculated the instability characteristics; from that you calculated the eddy transports and then you calculated the feedback on the mean flow. Now that looked like a rational order....sort of a rational expansion in Rossby number but you didn't actually refer to it in that....you didn't describe it.....

Phillips: I don't think it really is because it's.... when you do that, in that paper the unknown is the amplitude of the disturbance. All I was doing was bringing out the disturbance of an unknown size, any size you want to give it. But it's kinematics are those of an idealized unstable wave of infinitesimally small amplitude. We already knew, even from Charney's paper and some of Kuo's papers, that those were reasonably realistic in terms of patterns of descending motion in cold and warm air.

Hollingsworth: That idea seemed a novel idea to me but I'm not sure....where's the precursors for that idea of calculating the u-primes, d-primes, v-primes, t-primes and so on and then calculating the effect on the mean flow?

Phillips: I don't know of any. I didn't know of any at that time.

Hollingsworth: What prompted you to go at....to use that method to.....

Phillips: I don't know. I really don't know. The logical thing to do....I'm sure from the mathematical point of view and perhaps even physical point of view, there's a lot to criticize about that. I'm the one that's lucky that it works out that easily, that it works out to give a reasonable picture.

Hollingsworth: I'm unaware of any precursors to that type of an approach but I wondered if there had been.....

Phillips: I suppose some more sophisticated people, G. I. Taylor or Bachelor or the German hydrodynamicists would have posed a more complicated problem in which they're looking for more of a steady state in which the basic current has been changed and the perturbation has been changed because the basic current is different and everything is in a reasonable balance. I was not doing that. I was doing something very intuitive.

Hollingsworth: So that's how it was for you, intuitive.

Phillips: And in a way it turned out to be all right because in a way what that is, is somewhat of an initial value approach. Calculating what changes are going to take place in the zonally averaged flow. And that would be what happens in any general circulation experiment that I was capable of doing.

Tribbia: Norm, I was interested in your statement concerning the problem with the general circulation. I was wondering if von Neumann had influenced you at all by his sort of three problem approach in numerical prediction, stating that the short-term forecast problem was the first one to be done and then climate and then extended range forecasting.

Phillips: No, I don't think so. No. In fact I remember when I came back, after I'd done the general circulation experiment, I think von Neumann was traveling back and forth to Washington. I remember being asked to tell him what it was, in his office, and he was doing two or three other things at the same time. A desk full of papers. He was just absorbing it perhaps.

Hollingsworth: I don't think that comment came until a good deal later.

Kasahara: So it may well be that von Neumann had been very much interested in looking at....

Hollingsworth: I think that's probably the way it happened. Still going back to this '54 paper, the other really novel thing about it was the analysis of the energetics.

Phillips: Most of it came....yeah, that's true, did I draw a box diagram in there?

Hollingsworth: I don't think you had a box diagram but you had all the ingredients. It seemed to me that you'd worked out the concept of available potential energy and conversions between eddy and....

Phillips: But I do know definitely that I was thinking of that <u>only</u> in terms of a two-layer model. I had no urge or idea to extend it to multi-layer model let alone a continuous model like Lorenz or not recoverable. General flow is ________________. [Not corrected by NAP.]

Hollingsworth: Well, Lorenz's paper on energetics didn't appear until a year or two later in '55 or '56.

Phillips: It's much more fundamental than the one I did.

Hollingsworth: Were you aware of his work on it while you were doing yours?

Phillips: No. I remember when I was in Imperial College with Eady the main thing I was working on was trying to get some form of equations which would be well behaved. There were __________. [Not corrected by NAP.]

Hollingsworth: Could I just hold you here and stop this thing?

Phillips: Yes.

**END OF TAPE 1, SIDE 2**

**Interview with Norman Phillips**

**TAPE 2, SIDE 1**

Phillips: Playing with the ideas that later I worked up in this paper in '54 - I'd interrupted that while we were working on the barotropic model in Sweden - the idea was you can write an equation for how the zonally averaged quantities are changing; in principle you can write down all the terms. My question in my mind was which of those do you include with these equations that can handle geostrophics. It wasn't until I was in Oslo that....No, in Sweden, because I gave a talk on that paper in Stockholm before I left for Oslo, that it was clear to me that you can just let the quasi-geostrophic equations handle it themselves and be careful about the boundary conditions so that you would allow the dynamically correct circulations to develop.

Hollingsworth: Did you discuss these ideas with Eliassen when you got to Oslo?

Phillips: To some extent - I gave a seminar there. They were impressed. I remember Hoiland, as we left the lecture hall, saying to Eliassen, "See you can still do things with elementary functions."

Hollingsworth: Who said that?

Phillips: Hoiland.

Hollingsworth: Going back to the energetics for a moment, it seemed to me that once you'd got the formulas that you had there for the two and a half dimensional model that you could easily extend them to more levels with no problem.

Phillips: That possibility either didn't enter my mind or didn't seem interesting to me.

Hollingsworth: I have a few questions about the boundary conditions but we will come to those in a moment. When you got back to Princeton then sometime in the second quarter of '54, I guess.

Phillips: May of '54.

Hollingsworth: Was it hard to persuade Charney to spend a lot of computer time?

Phillips: Oh no. He saw this as a very logical extension of what he wanted that done with--the geostrophic equation. In fact he probably had; in my absence he would have eventually gotten to them some time.

Hollingsworth: When did you start work on the actual integrations?

Phillips: Oh, goodness. I suppose in the fall of '54. It took a while to work out the mathematics of what to do. Because as it turned out you could not solve for the zonal average changes in the stream function similar to the meridional velocity by relaxation. That was just unfeasible. Then I had to talk with Herman Goldstine who told me about this method which is now commonplace in Richtmyer and Morton of how you invert the tri-diagonal matrix going up and down a couple times. That worked beautifully. It took a while to recognize that problem and to humble myself to ask Herman Goldstine what ideas he would have, and to understand what he was telling me, and then program it. So it probably, I don't know, I would have to go back to the issues of **Quarterly Journal** and find out when they first announced the Napier Shaw Contest and then, because I remember I read that when I was almost done writing the paper, and work backwards from there.

Hollingsworth: A lot of thinking certainly went into the tuning of the diabatic forcing in that paper. At least in the write up you obviously went through the literature very, very carefully. Did you do that study of the literature before the work or after?

Phillips: When I needed it I did it, whatever literature was present at Princeton at the time.

Hollingsworth: Did they have a good library at that time?

Phillips: Jule had been very good and worked hard on the library, getting a library set up.

Hollingsworth: Did you try an adiabatic run at all?

Phillips: No. I suppose -- the code was in two parts, one for the zonal changes and another for the eddies. I may have tried the adiabatic part on the eddies. I remember that I was very careful that everything was, the code was reasonably well checked out. It was very likely that some checking into its vorticity diagnostics could be made to check out the code. I encountered problems that I hadn't anticipated, like the computational instabilities because of the way I first put in friction in the new scheme: friction evaluated at the central time step. I experienced that.

Hollingsworth: Did that take a long time to sort out?

Phillips: No. That was easy.

Hollingsworth: It was easy. You were still running it on the Princeton computer or there hadn't been an upgrade?

Phillips: I don't know that it was ever upgraded except with respect to input devices and output devices.

Hollingsworth: One of the things that strikes me -- well, before asking this question -- how long into the project had you got before you began to see the westerlies develop? What was your feeling when you saw them develop?

Phillips: I don't remember because, you see, the run didn't last very long before it blew up. I was undoubtedly completely absorbed in plotting these numbers out and drawing isolines - we had no graphics capabilities at all. You got a list of numbers and you had to plot them out on the map and then draw isolines. A very tedious task. I don't remember any flush of awareness that finally, yes, I had the westerlies. I undoubtedly had such a flush, I don't know.

Kasahara: Can I ask a question? How long does it take to calculate that result?

Phillips: I don't remember, Akira - unless we recorded it in the paper. I may have recorded it in the appendix in the paper.

Hollingsworth: One thing that struck me about the paper was that looking at those pictures now, the plots of the results, wave progressing every three days or so. Looking at it now it seems as if the boundary conditions - the north and the southern walls were very critical in that you had an energy generation region in the center of the channel and you zero amplitudes--

Phillips: --the eddies--

Hollingsworth: --the eddies north and south. So you have to have an energy export from the middle of the channel to the poles. And if the energy was going outwards then you had to have a momentum flux inwards.

Phillips: That's the case of.....Rossby waves and the real phase velocity - is that the argument you mean?

Hollingsworth: Yes. The same thing seems to work if the Rossby waves are unstable.

Phillips: I never thought of it that way.

Hollingsworth: Then once you got the convergence of the momentum flux you had to get the--

Phillips: --except that the momentum is not (can't understand the rest of his statement)--

Hollingsworth: --coming into the middle from both boundaries. So then from what we know now about--

Phillips: --except that the momentum flux was not being done by the waves which is what the feeling you are referring to. This momentum flux was taking place by the meridional circulation on coordinate by correlation of v and u bar.

Hollingsworth: It was a very definite u-prime v-prime in the...

Phillips: Oh, I see what you mean. Just the tilt of the wind.

Hollingsworth: Yes, the tilt of the winds.

Phillips: OK, I was thinking of something else.

Hollingsworth: So that tilt - the tilted waves were forced by the boundary conditions it seems.

Phillips: Yes.

Hollingsworth: Then once you got the convergence of the momentum flux, at least in "Palm-type" arguments, suggests that you're going to get a meridional circulation which will tend to counteract it.

Phillips: Yeah - although that stabilizing effect of geostrophic flow - yeah I think was more or less generally anticipated, even before I did that calculation, that geostrophic flow set up circulations which tried to undo what the horizontal flow was creating. Maybe even in my thesis I mentioned this (can't understand rest of statement). Fjørtoft may have mentioned that in his thesis also.

Hollingsworth: One striking aspect about that '56 paper[1] is the frequent reference to the dishpan experiments.

Phillips: And also the misspelling of Jeffreys's name. And he was one of the referees for the contest.

Hollingsworth: It wasn't just Jeffreys. Someone, either you or the editor, consistently misspelled Lorenz's name.

Phillips: Is that right?

Hollingsworth: Yes, there was a "t" in there.

Phillips: Oh dear, oh dear, oh dear.

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[1]The General Circulation of the Atmosphere: a Numerical Experiment (Quart. J. Royal Meterology Society, 1982, 123-164).

Hollingsworth: As regards the....were you conscious in the write-up of the analogies with the dishpan? Obviously you were but how....

Phillips: I think it was the fact that - probably the fact that the dishpan seemed to create a general circulation not too vastly different from the atmosphere in a lot of ways. It was part of the encouragement to go to Charney and say "let's do this." In other words, the atmosphere was not that unique. It didn't take those particular features, that only the atmosphere has, to explain the general circulation.

Hollingsworth: The other referee of the paper must have been Bushby, I think.

Phillips: That may be, I don't know.

Hollingsworth: I think it was Bushby. What was the reaction.... when did you have these results? When was it that they would have been gone out on the grapevine?

Phillips: Oh....not too much. I do not remember giving a seminar on it in America except at MIT.

Kasahara: I thought you gave a talk at New York in 1956 -January.

Phillips: That may be. OK.

Kasahara: That was the first time I saw you......

Phillips: OK.

Hollingsworth: What sort of reaction?

Phillips: I don't remember any more. I really don't remember.

Hollingsworth: What were your own feelings about it at the time?

Phillips: Oh, I was thrilled. I was disappointed because it didn't run on forever. There were some computational problems to solve but I was excited.

Hollingsworth: There was obviously a massive official reaction to it because by October of that year, '56, von Neumann - I guess - called a conference at Princeton.

Phillips: Yes. I gave a talk there, yes that's right. Oppenheimer was there. He nodded sagely when I talked about available potential energy. When I gave the talk at

MIT, C.C. Lin was in the audience, but it was in the afternoon and he was sleeping. That's the way it goes!

Hollingsworth: Presumably it was this experiment that led to the setting up of the general circulation lab within the weather bureau, Joe Smagorinsky's saying....

Phillips: It probably took Charney's encouragement with Harry Wexler to suggest that.

Hollingsworth: You weren't tempted yourself to join that lab?

Phillips: No, and I wasn't asked about it. I was still going to MIT. Shortly after I got to MIT, I still remember this with amazement, I got a letter or a telegram, maybe a telegram, from Rossby in Sweden asking me would I be the head of his institute? I'm amazed that I sent him back some kind of thing saying more or less, yes and I went to see Henry Houghton saying I can't turn this down. But somehow that got undone, and I'm glad both I and Rossby came to our senses. But anyhow, I had other things than Joe's group to consider for JNWP.

Hollingsworth: How much interaction did you have with JNWP during those years?

Phillips: I became a consultant for them - I don't remember how quickly - fairly soon, in '57 I suppose, '58. All the way through 1970 or so, but it was very occasional and one of the effects of that was that when the time came last fall for the Civil Service to figure out my retirement pay I had to wait and wait and wait and wait nine months for some poor clerk had figured my.....that I was working full time all that time, fifteen extra years and they were going to give me about twice as much money as I should. Some boss higher up the line knew that wasn't quite right and it took them nine months to unravel that. So that was something I had to pay for last year. Martha and I would anxiously wait for each mail.

Hollingsworth: Oh heck. So sometime in....when did the move to MIT begin to be discussed?

Phillips: In the spring of '56.

Hollingsworth: Was this because von Neumann was being pushed out of Princeton or because he was being hauled to Washington?

Phillips: Well, von Neumann, you probably know, had cancer, and as I recall, my impression was that at that time he was with the....Atomic Energy....he was a Commissioner on the Atomic Energy Commission. He had been spending a fair amount of time in Washington but he was attracted to the Boston area because of the medical facilities there. I'm sure MIT made every conceivable attempt to get him there even though he and Wiener were probably at opposite intellectual camps; maybe not, but he died and that left Jule without a high-level supporter at Princeton and at the Institute. As Jule has said in his interview with Platzman, he felt out of his milieu without some contact with data, the real world and he surely would not be able to get that at Princeton.

Hollingsworth: There seemed -- Jule in his discussion with George Platzman, describes some sort of controversy between Wiener and von Neumann. It seemed to have something to do with notions of predictability. Was that......

Phillips: Yeah. I don't know any more than what I read in that myself. I may have known more at the time but I certainly don't remember any more. I remember hearing at that time - second or third hand - something like that, that Wiener did not think highly of attempts at numerical weather prediction, but I don't think I ever heard any more than that.

Hollingsworth: But you didn't know why?

Phillips: No.

Hollingsworth: So anyway, you and Jule, I guess, had several offers and you..

Phillips: UCLA and at MIT. MIT reacted very much faster and I think Jule was recorded as (unintelligible) at a private university, not a state university, and was willing to upgrade the department in general by promoting Starr and Lorenz and let me come along. Jule got all he asked for.

Washington: Norm, I would like to ask a question. At that time was there any thought of having computing facilities wherever Jule went and you eventually went?

Phillips: MIT had a computer of sorts at that time or was getting one because I remember as soon as I got there I started prog..... At that time we had the luxury of hiring a programmer and I began working on filtering of the noise from the computation and that was with the MIT computer.

Washington: But that wasn't a factor probably in Jule's mind?

Phillips: I don't know, Warren. Maybe the promise that that would be there shortly was enough to keep Jule happy. He, I gather, was quickly becoming, and he wanted to get this burden of numerical weather prediction off his back once he'd established that cyclogenesis could be predicted. Then he got interested in hurricanes, general circulation, Gulf Stream - yes.

Hollingsworth: In that period then from '56 through 1960 or so, you have a whole series of papers where you were working on the development of the technology of primitive equation models. The prime and perhaps most enduring contribution there was the shortest of all those papers which was the--

Phillips: Sigma!

Hollingsworth: Sigma coordinates - which is still used by just about everybody except perhaps your team, Akira. It's used here too.

Kasahara: I decided to call - you said the sigma coordinate, Phillips' coordinate.

Phillips: ...at NMC I suspect their next short-range forecast model will have Feodor Messinger's coordinate.

Hollingsworth: Where did that idea come from?

Phillips: Goodness knows. I knew it was a problem, an awkward problem. I would not be surprised to hear that it had already been done in a slightly different form in other branches of hydraulics or fluid mechanics someplace.

Washington: Was there some work of Starr at one earlier time that hinted at some kind of coordinate transformation? In fact, if you look back at Richardson's work he actually does kind of a coordinate transformation in the first layer above the ground - not like the sigma idea but it's....

Phillips: I'll have to go back and look at that. Maybe I was influenced by that, I don't remember.

Hollingsworth: In this series of papers there was a sigma coordinate paper which seemed to have sprung out of nowhere. There was a series of papers on the mapping problem. You had one or two papers there. That problem didn't get solved for a long time -satisfactorily I think. You couldn't run a global model with--

Phillips: --polar singularities.

Hollingsworth: --with the polar singularities. That problem certainly took a while before it was effectively sorted out. Then there was a paper with the Eliassen grid.

Phillips: Oh yeah.

Hollingsworth: --which you presented at a session in Tokyo.

Phillips: That's right. That formed the basis for the Nested Grid Model I developed at

NMC.

Hollingsworth: It seemed to me that there's a link - there must be some relationship between the grid that Richardson proposed. The checkerboard pattern was using these moving from one set of grid points to another set of grid points at every half-time step.

Phillips: Really?

Hollingsworth: Oh yes.

Phillips: I don't remember that because I don't think he had a Lax-Wendrof scheme.

Hollingsworth: He was using.....he had the half-time steps he would have v's and u's in one arrangement relative to the geopotential point and they would have the opposite arrangement at the other set of time steps. [H. is wrong on this point--NAP]

Phillips: All I know is my impetus - the impetus for me came from Eliassen's project report at UCLA. He showed the advantage of a staggered grid -- the efficiency of it.

Hollingsworth: How much effect did this work have on the development of primitive equation modeling in general? We all know about the sigma coordinates but the mapping problem and the--

Phillips: I don't think the mapping problem....that those one or two papers on the mapping had any effect other than what I did. It prepared me for the familiarity with stereographic coordinate that I learned there. It made it easy for me to develop a practical hemispheric scheme....a robust kind of hemispheric scheme and then...

Hollingsworth: It was also at this time you said that you were beginning to act as a consultant for JNWP or was it with Smagorinsky's group, or both?

Phillips: No, it was with JNWP in Suitland. I think the computations that I reported on in Tokyo were actually done with the NMC computer - the JNWP computer. Cressman had some extra computer time at one moment and invited me to use it.

Hollingsworth: Who was funding your work at this time?

Phillips: Originally it was the Office of Naval Research, and then Air Force Cambridge supplemented that. Then the NSF was created and Jule found it much more compatible to work with NSF. And of course they were happy to support such an established giant. That worked out nice. I had to....I was on all of Jule's - not all of Jule's projects, but his major projects during all the time until I became Chairman at MIT in 1970. He did much of it - I did less of the dog work of renewing proposals and grants. He did most of that.

Hollingsworth: While you were doing this work in the second half of the sixties so to speak, Smagorinsky was working away like hell and I guess he probably had his integrations, his first integrations, with the two-level P model already completed, I think, by '58 or '59. Then he spent...he must have spent about four years analyzing and writing them up. Were you aware of the work that was going on in his lab at this time?

Phillips: Yes. He was working with the Kurihara grid at that time I think, or did that come later?

Washington: He's talking about the channel flow.

Hollingsworth: Channel flow, yeah.

Phillips: I knew about it but I don't know....because I took off from some aspects.

Hollingsworth: You mentioned Oppenheimer a little while ago, and if you could call Jule Charney the Oppenheimer of numerical modelling, the man with the brilliance and charisma, it seemed to me that Smagorinsky might be described as the Teller.

Phillips: Oh! Oh dear!

Hollingsworth: How would you react to that?

Phillips: I was afraid you were going to call me the Teller. No, too hard to draw analogs. I don't know Teller that well.

Hollingsworth: He was the man with the super, and Joe really went for it in a big way. OK. It seems to me that this coming to the end of the 1960's, sorry, the end of the of the 1950's and early part of the 1960's there seemed to be a sea change in your career. It was a very marked breakpoint from many different aspects. I wondered often what happened around that time that took you away from numerical modelling and into the work on convection, on dishpan and oceanography, and so on. It was a very marked change.

Phillips: I guess I had run out of ideas or something.... another area, and I was in a marvelous location to be exposed to new ideas. We had these seminars at Woods Hole, oceanographers, meteorologists and mathematicians and I thought I'd try my hand at slight theoretical works instead of NWP. But then there was a reverse, and in the early 70's I decided to go back to NWP.

Hollingsworth: Would it be fair to say that that's where your heart was all along?

Phillips: No, I enjoyed the theoretical work when I was doing it. But there were some fun things in that. The Rossby wave with Ibbetson, and with Walter Munk, the inertia wave spectrum in the ocean and .... That was a glorious place to be in the nation at that time.

Kasahara: Are you going to bring up discussion with ....

Hollingsworth: Oh, sorry, that's. OK.

Kasahara: How much time do we have?

Hollingsworth: We have plenty of time on this one, we have another fifteen minutes easily on this tape.

Tribbia: One other point to make. The non-linear computational instability and how....

Phillips: That was developed...that's a good example because the use of low-order systems by Saltzman and most profoundly by Lorenz, I was familiar with in the late fifties. I just played around until I found a system which seemed to have the right, what would you call it, chaos, I don't know.

Hollingsworth: Chaos in the literal sense.

Tribbia: Can I follow up on that? Were you searching for the answer to that question as why the general circulation model....

Phillips: That's a good question. Yes. Because I remember Rossby asked me, "Look, you started out the computations with small random perturbations and they developed into a nice eddy but yet you end up the computations which seem to be equally random noise. What's going on here?" I remember Rossby asked me that. I suppose that stuck in my mind from '56 or so, '57 it probably was when he said that to me. Until '59 or so, until I could get some simple example of it. Nowadays I don't think it's probably most difficult - I don't think it's that fundamental a thing. But that's the way it happens in that particular numerical system, but it happens in other numerical systems, too.

Hollingsworth: Lorenz was working with low-order systems certainly by the early sixties, possibly even earlier, and he seemed to be showing that there was a definite predictability horizon. Phil Thompson had already raised this issue in '57 and perhaps Wiener had raised it earlier even. When did this first strike you as a serious problem for numerical weather prediction?

Phillips: You mean the, what we now call "chaos"? I don't know how to answer that. That's not to say I haven't tried to think of that question in my own mind and wonder why. That's a little bit like the fellow in Moliere's play who was gratified to find out that he had been speaking prose all his life! I think meteorologists get so used to the idea that something bad is going to go wrong with their forecast that you're not surprised. It takes someone with the insight of Ed Lorenz to realize that there is something to explain there.

Tribbia: Let me just follow on that a little more. When was the concept of limits on day-to-day forecasting known?

Phillips: I don't think much attention was paid to it until Robinson raised the question in his Presidential address, I think it was, in England. I think it was in the early days of FGGE, when it wasn't called FGGE then, but in which the limits were still way out there. He made some assumptions, pessimistic assumptions, to say that people are hooting down an empty well here. So I would say that he woke people up.

Hollingsworth: This was after the studies by Charney and Smagorinsky and Leith.

Phillips: I'm not sure.

Hollingsworth: Robinson's paper was in '67 and the **National Academy Report** was published in the **Bulletin** in '66.

Washington: Yes, but Robinson was talking about it at meetings before this.

Hollingsworth: OK. So you're saying that this problem of the limits on predictive skill were something you just lived with all your life and....

Phillips: It seemed to me that we hadn't exhausted the possibilities of the data yet and that was my prime priority.

Hollingsworth: Let's talk about the data then for a little while because you had a paper in 1960 or thereabouts on the problem of the initial data for the primitive equations which was trying to explain some apparent anomalies in some of Jule's work and in some of your work and Hinkelmann's work. You used the idea of projecting the initial state onto Rossby modes and gravity modes. Then by looking at the linear aspects of the problem you could understand the ways in which the initial data could lead to noisy integrations and the sorts of things that you had to overcome that problem.

Phillips: To go beyond that it never occurred to me and one needed the Danish Eliasen and Machenhauer's insights and computations to show that the fast modes tagged along with the slow modes. That did not occur to anyone theoretically. That came from their computations, as I recall. One would have to ask Ferd Baer whether he thought of that independently of Machenhauer.

Hollingsworth: The work of Baer and Tribbia on one hand and Machenauer were rapidly established as being closely related to each other. They seem to come almost as a bolt from the blue in terms of the long development there had been an idea of geostrophic initialization or setting it up geostrophically, and then looking at the horrendous balance equation. In fact, Fjørtoft was still integrating the balance equation, I think. Well into the '80s even.

Phillips: Jule was still or would have liked to have solved that problem before he died.

Hollingsworth: But yet in that 1960 paper it was as if the penny was tittering on the edge of the drop but the penny didn't quite drop.

Phillips: You mean I did the simple thing. I just....all I did was....I thought all I was doing was showing that Hinkelmann and Charney were just looking at different sides of the elephant. That's all I thought I was doing. They wanted a slightly more general theory that would include both of them. Both of them recognized that fact immediately once I'd sent them a preprint.

Hollingsworth: With hindsight now do you feel that you....

Phillips: I missed the boat then.

Hollingsworth: How did people cope then through the sixties with the initialization problems?

Phillips: At NMC they used the....at NMC they did not make primitive equation forecasting until the late sixties. I don't know that anyone else was doing in the world seriously before then. They used geostrophic winds upgrading them with correction. They used that even in their primitive equation barotropic model and that model took a big jump in accuracy in a two-day forecast when they started using winds....put winds into the analysis in a direct way.

Kasahara: You took interest in working on objective analysis.

Phillips: That didn't start until 1974. Flattery had been working on it, of course he was..... They were just implementing that when I came to NMC in 1974. I remember some of the forecasters, some of the more conservative ones, were objecting to how that differed from the standard Cressman method.

Kasahara: In fact that scheme came from a somewhat different route, initialization, but more for analysis.

Phillips: Probably so, probably so.

Hollingsworth: Of course Dickinson and Williamson, here at NCAR in 1972, I think, had extended your work in the sense that they extended it to the sphere. Then Joe Tribbia in his work, he had an expansion in terms of a small parameter Rossby number. Why didn't he get the same result you got?

Phillips: He was doing something much more extensive than I was doing. I was not....I was ending up with only slow modes in the analysis. [I am wrong in this last sentence. NAP] He was putting in the fast modes that belong there.

Hollingsworth: His result came from a consistent expansion in Rossby number. And if there is one thing you're good at....it's expansions in Rossby number.

Phillips: That takes some insight.

Kasahara: I think you put it very correctly in saying you were interested in the description of slow modes.

Washington: Wasn't there a prescription in your paper to incorporate the quasi-geostrophic--

Hollingsworth: Sorry gentlemen, we're going to have to stop.

**END OF TAPE 2, SIDE 1**

## Interview with Norman Phillips

**TAPE 2, SIDE 2**

Hollingsworth: One second. Just say that again Norm. You were saying that your paper was a cheap version...

Phillips: My paper was a cheap version of what Chuck Leith did in his slow mode paper but without the happy choices of nomenclature that Chuck introduced.

Tribbia: I think we should backtrack because part of what was said was probably missed in the changing of the tape. I was mentioning that in the 1960 paper you had a prescription for specifying the divergent component of the wind in terms of the quasi-geostrophic expansion theory. In fact, I'd say ninety percent (90%) of what's done in normal mode initialization is encaptured by that approximation, and so to some extent, as Tony has mentioned, the penny was actually very close to dropping at that point.

Phillips: Maybe so. But I think the way the normal mode initialization had been carried on -- was carried on in practice in the first five to ten years of its existence, benefit -- made a big contribution by happening at the same time that people could apply it to spherical functions, global functions.

Hollingsworth: It was quite extraordinary in a way that Leith was able to demonstrate the connection between the two and then Akira Kasahara was able to extend it then to the beta-plane. It was a very remarkable business. OK. So going back then to the 1960's you'd taken on the horrendous job of editing JAS at that time. In your description of that, that you or Jastrow were referee for every single paper that was published.

Phillips: One of the referees.

Hollingsworth: One of the referees for every single paper. Was that standard practice for the editors of journals at that time?

Phillips: Maybe in smaller journals, I don't know. I doubt it. Werner Baum was the previous editor of **JAS**, the **Journal of Meteorology**, and I'm sure he didn't read every paper in that kind of detail. He was Dean and President of the University - all kinds of things. It may have been in the early days when Victor Starr and Ray Montgomery were editors in the late forties.

Hollingsworth: It must have eaten a lot into the home life.

Phillips: I expect it did, yes.

Hollingsworth: At that time too you published....

Phillips: It took up....I didn't play the French horn in that time so I had a lot of time to be editor.

Hollingsworth: I see, you lost your lip. You published a paper on geostrophic motion, a review paper. One part of that has lived and flourished in many ways. The standard quasi-geostrophic theory but the geostrophic motion of the second kind doesn't seem to have flourished as well in, at least in meteorology.

Phillips: No, it's primarily oceanic, it occurs in the ocean, it's a Sverdrup model. The only place you can find it in meteorology probably was theoretical, I don't know, Longuet-Higgins Type 6 perturbations. You and I may have even went over this. Remember you found all the errors in the Longuet-Higgins reprint.

Hollingsworth: Found one or two anyway. Does it have much relevance to the atmosphere, this?

Phillips: Yeah. That's what Lindzen used for the tide.

Hollingsworth: But none of that theory seems to be much good when people are working with numerical models or when they're....

Phillips: Well that's because it's associated with negative equivalent depths. They're not too relevant to the initial value problem.

Hollingsworth: But the same mode and structures occur in the modes that we use for initialization, it's just that they....analytically, they're continuous aren't they?

Phillips: It's a different set of them and it's complete without bringing those in. They can describe the total field, and you know .....

Hollingsworth: Without bringing those in?
Phillips: Without bringing those in.

Kasahara: Are you not referring to the geostrophic motion of the second kind?

Hollingsworth: Yes. It's Burger. OK. After that - what was going on at that time, this was '63.

Phillips: I came to NCAR for a year and was working on oceanographic problems.

Washington: '64 or '63?

Hollingsworth: You were working with Allan Ibbetson, too.

Phillips: Oh yes, on Rossby waves. Rotating, large rotating basin they had at Woods Hole.

Hollingsworth: That's almost the last experiment with the dishpan that I... I guess Hide's work on flow over orography with internal heating was still going on. It was still to be done. It is almost the last work in rotating tanks that I can recall happening in the United States.

Phillips: Oh, Dick Pfeffer at Florida State.

Washington: Still going on.

Phillips: Very elaborate sensors. I haven't followed that work. I don't know....

Hollingsworth: So then you got involved with Walter Munk.

Phillips: He came to MIT on a sabbatical. I knew that people making these measurements of currents in the ocean with moored buoys for the first time at Woods Hole. Ferris Webster and later Fofonoff or maybe in the opposite order. Fofonoff had pointed out quite specifically, I think in one of these seminars, that the motions in this peak which was not yet well defined by them, but around the inertia period at one site, which was I think around 30 degrees, actually constituted a major, a considerable fraction of the kinetic energy in the oceans, including all the currents, all the surface waves. Walter had had a student - what was his name, Merl Hendershott, did a study with a boat for a whole month, going in little rectangles around 30 degrees latitude off the California coast to tie down the tide. We thought this was explainable and I...I don't know where I had recently read about Airy functions and how they come into play at turning points and pointed out to Walter that this is probably what's happening. The student was finding this out at the same time. We just worked out the theory of theoretical description of a simple distribution of modes without the tide. So we showed that it would happen at all latitudes -no reason why it was bound to the 30 degrees or 60 degrees and that the frequency I think would be a little bit less than the inertia frequency. I've forgotten. When they refined the measurement they found that yes, that was the case. We predicted that little....

Hollingsworth: I see. I remember Peter Webster saying that the blood was flowing under the floor - that the work on that paper was very, very intense work.

Phillips: Yes. Actually, I went off finally to La Jolla to wrap up the paper.

Hollingsworth: My impression was that that's a fairly hard paper to read. Was it amongst the hardest papers to write that you've done?

Phillips: Yes, because he's used to - Walter's used to concepts that I struggled with at the time --spectra -- cross-spectra and covariances and a lot of other expressions. It was hard for me to read that paper too when I got the manuscript from him.

Hollingsworth: Around that time you....there was a lot of things happening in and around MIT with the, I guess the ideas of quasi-geostrophic turbulence were beginning to appear.

Phillips: Yes, Jule had done that.

Hollingsworth: Lilly's work on modeling that turbulence. How did this affect your thinking at that time?

Phillips: I tried to stay aware of it and abreast of it because you have to appear as an omniscient god to all your graduate students. It was a little bit too abstract for me to start spending thought on.

Hollingsworth: Who were your graduate students during this period? You were coming toward the end of the seventies now. You'd been at MIT...

Phillips: --sixties.
Hollingsworth: Sorry, to the end of the sixties.

Phillips: Webster came shortly after I was at NCAR so he was there. John Young, Mankin Mak and you. You came in...

Hollingsworth: I came in '67.

Phillips: Harold Solomon who ended up in oceanography and I think he spent most of his career in Japan. John Rhoads - not John Rhoads. Had several guys with their thesis, but didn't publish them.

Hollingsworth: Jim Sullivan was one.

Phillips: Sullivan was one. He went to work for one of the public affairs or environmental groups.

Hollingsworth: Who was your first one?

Phillips: Bill Blumen. First graduate student.....

Hollingsworth: Did you, by the end of the sixties you had become Chairman, I think in 1970, after Houghton retired. Did you continue to have graduate students?

Phillips: Yes. Not as many, but I had some. John Walsh...... my mind. Embarrassing.

Hollingsworth: Quite a few anyway.

Phillips: Not as many as a lot of the people there. Jule blossomed after I left MIT. Jule really blossomed - he'd not had too many students before then but his last ten years there he had a really large group.

Hollingsworth: Do you want to talk much about your role as Chairman?

Phillips: There's not much to say about it and maybe if you're going to close in five or ten minutes we can close with that.

Hollingsworth: No, we've got a good thirty minutes to go. Let's take it to the end of the period at MIT and then we can talk in the morning perhaps about NMC and thereafter.

Phillips: The Dean asked me to be Chairman because none of the other more senior professors I think would agree to be Chairman. I think I initially agreed to be Chairman for five years and that was becoming accepted at the Institute; until then, as was the case with Houghton, the Chairman essentially stayed there for life. Since then I think the typical tenure has gone down and is probably now more like three years. You only get a thousand dollars more for being Chairman. On the other hand, you don't have to pay any of your salary from contracts. I had the opportunity to do something but not very much because when I came in, I remember we came back from Brittany in August, I think of 1970, and they had a first meeting of the department chairmen at a retreat that MIT had, at a place out near Blue Hill. There we learned from the Provost the first thing we should do when we got back was to increase all the overhead that our faculty got for their salary. So I to go around to the faculty and convince them that they should not be getting 20-25% of their salary from NSF but they should put down 50%! Amazingly enough they did it and NSF and ONR, I guess, grudgingly acquiesced, probably because MIT had pulled the strings or something. Those four years were extremely tight financially. Jerry Wiesner had been put in as President because he was probably thought to have a good relationship with students because in the previous five or six years, those are the years of student unrest. A business man - manager, Howard Johnson, was President in those years. We went to Wiesner and he got all his financial problems to work out! But those were the main things I remember were the financial difficulties. They probably have not decreased any since.

Washington: Norm, at that time was it hard to get graduate students or did you turn down graduate students who wanted to come to MIT or were well qualified but didn't have financial support?

Phillips: Yes we did. If we didn't have financial support for them we had to tell them that. In fact I met several of those at NMC later on. They didn't hold it against me. I usually was conscientious and wrote thoughtful letters to them. I hired some faculty, Ron Prinn, an atmospheric chemist, was brought on board by me. He's a very strong part of the department now. Tried to get John, the man from Colorado here--

Hollingsworth: --John Hart--

Phillips: I think he came for one year, then came back. Hank Stommel went back completely to Woods Hole during that time.

Hollingsworth: That seems to be a critical power that the head of any institution has, is the hiring of people.

Phillips: Oh, and the other thing I did was I managed to get Charney treated properly as a name professor. He had been a name professor since he came to MIT, not since he came but shortly after he came, but he never got any perks for it.

Hollingsworth: He was a Sloan Professor.

Phillips: Yes, but he never got the perks that went with it. I found out that perks normally went with it.

Washington: What were the perks? I'm curious.

Phillips: He got something like twenty grand a year that he could do with as he wished. I understand that he used much of that to build up the computing facility of the department. I remember I had to write out something close to a treaty with the Dean as I was leaving -I knew I was going to leave and this was so everyone knew what had to be done at what time of year so that Charney would get his money because I knew Charney would not keep things straight. Like a treaty or memorandum of understanding.

Hollingsworth: Were there any hirings at that time that you could have made and that you regret now that you didn't?

Phillips: We tried to get.....well we didn't because people said no. One was Bretherton, another one was Hoskins.

Kasahara: In seventies. Early seventies?

Hollingsworth: Hoskins graduated in 1971, it seems, thereabouts. Around about this time NCAR, changing the subject now from MIT, NCAR had been going for, what, fifteen, sixteen years by 1974. How well do you think it had achieved its goals by that time, or was it on the way to achieving those goals?

Phillips: I think it was doing very well. The initial goals that different people had, differed from person to person, and could not possibly have been satisfied all of them by any conceivable institute. Some wanted it to be another institute of advanced study, some wanted it to focus completely on service to the universities. I think in those fifteen years a gradual combination was reached. There were compromises on all those demands. The universities - I don't think they were willing enough to see their best people go. In fact I remember writing a very strong letter to NCAR or UCAR, maybe it was, John--

Washington: John Firor.

Phillips: John Firor, about what I had heard were attempts to lure Ed Lorenz away. That must have happened many times for most universities and a lot of other people. I think NCAR did rather well.

Hollingsworth: Still doing well?

Phillips: Yes! They have lots of money problems.

Hollingsworth: So does everybody. One of the areas that didn't seem to have money problems in the late sixties, early seventies was oceanography. You were describing yourself in the late sixties at least as an oceanographer as much as a meteorologist.

Phillips: Not really, Tony. I picked off little bits of problems here and there as I recall. Had no thought of becoming--

Hollingsworth: Full-time oceanographer. But it seemed that was a tremendous surge in interest in oceanography at that time. Students crying out for courses.

Phillips: Yeah, as well at MIT, I don't know if this was before I became Chairman or not. Probably at the same time I became Chairman. Woods Hole and MIT got together and had a real working joint program. That must have been about the time you were there, that must have been after you were there, about the time you left. There was quite a demand to enter that program. There still is although I suspect the interest is a little bit more on the environmental aspect than it was at the beginning, twenty years ago.

Hollingsworth: So, 1970 - 1974. For me, one of the big landmarks in the period was Miyakoda's forecasts.

Phillips: Yes, yeah.
Hollingsworth: --because they--

Phillips: Seven forecasts?

Hollingsworth: He had a dozen forecasts eventually. They were very important for the development of the European Center which was happening at about this time. The political aspects of it were being discussed. But it's always seemed curious to me that GFDL at this time was somehow monastic in its mode of working. It didn't seem to have a whole lot of contact with your group at ...with you or Charney, for example. I just thought it would have been in the interests of both of you to have had a lot of contact. In fact, the modelling groups that Charney had most contact with was GLA, or GLAS at the time. Was there any reasons why there wasn't so much contact? Because the great....

Phillips: I think Jule was able to get computing done at GLAS that he could not have gotten done at Princeton because....well first of all, the computing time at Princeton was well spoken for and secondly, Joe Smagorinsky was strong enough a character that he wasn't about to submerge himself in anybody.

Washington: Even during that time Jule was actually computing here with our general circulation model. He asked us to compute geostrophic turbulence in the model.

Phillips: That may be.....

Washington: The turbulence work, we did that here. The students often came here to work with our models.

Phillips: Oh yes, yes...oceanography students.

Hollingsworth: I just wondered if there was anything - Joe's a very strong guy. I just wondered how it was that you've known these guys for, well, twenty-five years at this point. How come there hadn't been a closer working relationship?

Phillips: I don't know. In my own case I'm sure it was that I was busy enough with other things. I saw these people moderately often, at meetings or whenever we would visit one another. In Jule's case the reason may have been different. My computing at that time as done in the early seventies, was done - gee that was the stratosphere model - that was done....I did that using the MIT computer? No, the GLAS - used the GLAS computer. It was easier - just more available.

Hollingsworth: Presumably Smagorinsky must have tried, Smagorinsky and Miyakoda must have tried to sell the idea of medium-range or extended - extending the forecast range to Schuman. But as far as I can determine, that never happened. At NMC he did not....

Phillips: You mean he never bought it?

Hollingsworth: He never bought it.

Phillips: No, I believe....I remember it was Lloyd Vanderman at NMC who had been working for years developing a three, I believe it was a three-layer global model -- something done around the poles to make them stable. But it had no physics to speak of. I believe...and that started to be run. Taking some operational computer time in the very early, oh about 1975, after I got to NMC. I think it was done without much forethought due to the fact that without the physics it would be crazy to start running things off the -- beyond three days. It took a while for NMC to get physics into their models. Maybe that's what you want to talk about tomorrow.

Hollingsworth: Yeah, we can talk about that tomorrow. The other area that certainly was developing at this time in the late sixties, early seventies was GARP. There was a lot of activity, would you...cleverly, I think, managed to stay on the fringes of the committees in some sense. You never....or is that true? Did you actually get hooked into it?

Phillips: No, I was on the U.S. Academy Committee towards the end of the sixties. I remember Smagorinsky and Winn-Nielsen coming to us and saying we're not going to have a second satellite, we're actually going to have to start with only one satellite or something like that. I remember talking in detail to people about where the recon flights should go in the tropics.

Hollingsworth: This was in the run up to FGGE?

Phillips: --was not involved at all.

Kasahara: You were a member and then Chairman of the FGGE Advisory Panel, National Research Council from '76 to '80.

Hollingsworth: Yes, that was, that was...I was really wrong then but I'm thinking of the early stages between the '67 Conference and the end of GATE when they had got up the head of steam.

Phillips: I didn't go to Stockholm, and that was an important meeting.

Hollingsworth: It's in that first half if you like of the buildup of the FGGE Program that you...certainly you were involved in when the real FGGE thing got going.

Tribbia: One scientific area before we leave, the late sixties through the early seventies that I would like to ask you to comment on with sort of a brief exchange on the angular momentum conservation and the traditional approximation....

Phillips: Oh, yes! Thanks. That's still up for grabs. Comment on that?

Tribbbia: Yes.

Phillips: I think at the present time the most up-to-date interpretation is one which Veronis and I agreed to and I put in my Reply to him. I since found that the same arguments about the size of omega -- the rotation rate compared to the Brunt-Vaisala frequency were made by Queney as early as the late forties. Queney has a paper in **Tellus** also, in the second and third issue of **Tellus** in which he goes over in tremendous detail the proper equations for perturbations on a zonal flow. He ....considers this question.

Tribbia: I think, from the perspective of numerical weather prediction that to my mind, one of the classical problems that Norman Phillips would tackle because being as he mentioned, the stickler for accuracy, it's very typical of the kind of questions he would ask and answer.

Phillips: I don't think I've answered it satisfactorily, but more or less. I've answered it satisfactorily but not thoroughly. I still get papers to review on the subject, about every third or fourth........ [laughter]. There was one in **Tellus** just a couple of months ago.

Hollingsworth: Andy White has been working on that.

Phillips: That was a German. By a German author but I think it's in English.

Hollingsworth: One man we haven't mentioned at all and yet he figures very prominently in your writings. A lot of your linearized analysis is written in relation to his work. That's a man called Eckart. I don't know much about him.

Phillips: He was a classical physicist. Well, he was a physicist .....

Washington: Quantum, mechanics?

Phillips: Quantum theoretician. But I believe he, possibly through Woods Hole, became interested in fluid motion and wrote some papers on turbulence and mixing back in the late fifties. He came to MIT shortly after his book came out and gave a seminar, maybe two, maybe one in which basically what he had done was, he -- there was a lot in it -- but he examined the ray properties for wave gravity and sound waves in a simple atmosphere. He doesn't really go into Rossby waves - except in a very offhand way. Probably because he didn't see where in the equations they escaped his notice. They escaped his notice in the one solution in his context where omega is equal to zero. My copy of his book is very dirty and I puzzled through it many times in the early sixties trying to learn. It took me a long time to learn that rays were the paths that followed by particles moving with the group velocity. I never found such a statement in his book, **Hydrodynamics of Oceans and Atmospheres.**

Washington: It's a difficult book to read. He uses different symbols than we normally use. Actually I ran across a lot of his work when I was looking up some references on the early ideas on quantum mechanics. He was a contemporary of Einstein. Also he worked on the early theories of interactions of molecules.

Phillips: He eventually married von Neumann's widow.

Hollingsworth: Was his the first exposition that you had seen of the full spectrum of atmospheric motions, apart from the Rossby waves. Treatment of the.....

Phillips: No, because Eliassen had this paper about five years earlier in the German physical review series. I can't remember the title of the journal. Two articles back-to-back, one by Eliassen, one by Kleinschmidt and the one by Kleinschmidt is discussing potential vorticity. There are sound waves and gravity waves in **Physikalische Hydrodynamik...**the real....the first couple chapters in Eckart are the simplest cases. He is just talking mostly about frequency diagrams. That was the first clear exposition. The recent monograph I have written for WMO on dispersion of influences in large-scale meteorology uses his notation in the first part of the book. There are some advantages.

Hollingsworth: I wondered what it was about that book that caught your attention so much because time and again in your class notes and in your papers it's taken as a point of reference. Jumping off point in some way for a lot of the linear studies.

Phillips: I don't know. There are discussions of group velocity that are much more lucid, penetrating and easier to understand, thorough, than Eckart's. It's been many years I went back and read the relevant parts of his book when I was writing this monograph. Other than that I have not really looked at the book for the ten years or more I was at NMC. Did I assign it to you when you were a graduate student?

Hollingsworth: Well, I certainly was....

Phillips: Did you borrow it maybe or something?

Hollingsworth: I certainly am familiar with the work anyway, that's for sure. We talked about a normal mode initialization a little bit and we'll probably talk some more tomorrow because tomorrow morning we'll talk some about the last part at NMC. One of the points - I talked obviously with a number of people before coming out here. One of them I talked with was Clive Temperton.

Phillips: Oh, yes. He still in Canada?

Hollingsworth: No, he's visiting us for a year or two now. His question had to do with the fact that it's taken a long time to recognize closeness of the link between non-linear NMI and classical techniques in the general case. He wonders did you recognize this link all along? He does make the point in that Brad Ballisch in 1980 at NMC, tried a non-normal model initialization scheme in the spectrum model which he derived from the bounded derivative method and related it to quasi geostrophic balancing. Said he got exactly the same results as using the Machenhauer method but he didn't take the point any further. You were at NMC interacting with Brad at that time.

Phillips: What I remember was he was...he was verifying what Dean (?) perhaps even stated in your paper with a joint paper that they would be equivalent to some approximation.

Kasahara: I thought someone made a prediction in...

Phillips: In the meeting? You mean in the sixties?

Washington: 1965.

Phillips: Who was that?

Washington: Hinkelmann.

Kasahara: When you do succesively for example, first you put the divergence equals zero, its tendency equals zero - that's one pair, the next one's divergence tendency, second divergence tendency to be zero which in fact gives another pair.

Hollingsworth: Uh huh.

Kasahara: And so you can go on like that. And then for the linear case you could show convergence. Now whether you converge or not in the non-linear case...I think it's....

Hollingsworth: I think it's probably a good time to wrap it up.

Phillips: You have performed nobly.

**END OF TAPE 2, SIDE 2**

**Interview with Norman Phillips**

**TAPE 3, SIDE 1**

Hollingsworth: OK, Norm, we broke off last evening at the point where you were leaving MIT and going to work at NMC in Washington. I remember well when that news came and it was news with a capital N for people who were interested in the business. It was obviously great news for NMC, probably not such good news for MIT. How did they react?

Phillips: Very calmly. The administration has a policy they never try and outbid anybody, although in this case I was not leaving because of salary changes. In fact, I was willing to go with a drop in salary. The department had grown and I think mostly because they realized that one of them would now have to be Chairman, - it took a while before we got that straightened out. The students were curious about why I was leaving and so forth. I might as well tell you now the main reason I was leaving was I saw the students having all the fun doing the research and I thought I could still do some of that myself. I always like to be in touch with the data so that's why I went to NMC.

Washington: I wonder if you explain why you didn't go to some other place?

Phillips: I viewed NMC as the place where you had access to the data and where things should be happening even if now and then things were happening more intensely elsewhere. It could be a very good place to be.

Kasahara: Is that true that you had been a consultant before...

Phillips: Yes.

Kasahara: So you have been visiting NMC very often.

Phillips: Not too much in the later sixties. It was mostly in the late fifties and early sixties that I went on the average of maybe once a year. It was never for more than a week at a time.

Hollingsworth: I remember in 1981 attending a seminar you gave at GLA and Milt Halem in his introduction, which are famous for their tact and diplomacy, spoke about the astonishment he felt that you should have gone to NMC, which was a typically outrageous thing for Milt to say.

Phillips: Half of the audience probably was people from NMC!

Hollingsworth: For Milt that was probably a good reason to say. What - how was NMC at the time when you got there?

Phillips: How was it? It had just recently moved into a new building, they had had hideously cramped quarters before in Suitland. Meteorologically the LFM was slowly getting the bugs worked out of it; it still failed occasionally. The analysis programs were in a state of flux. Cressman analysis was still being done every place. Hemispheric analysis and forecast was the most ambitious model then in play. It was a development by Stackpole ... extension by Stackpole of the six-level model that Fred Schuman and John Hovermale had developed in the late sixties. It was still experimental when I went there. Only slowly did the length of forecast increase, I suppose that's in response to increased computer power and secondly, the realization that we would have to run hard to keep up with these upstarts in Reading.

Hollingsworth: Of course we didn't come on the scene until much, much later than that. What was your first work then?

Phillips: I wasted...I had been given a little hand computer as a going away gift from MIT. I was entranced by it and I found a problem that I could use it for which was looking at arithmetic aspects of the sigma system. Wrote them up in an NMC office note which was the most popular office note I ever wrote there, but one which I think had the least scientific content in it. And then I formulated, while I was doing that...I was becoming acquainted with the forecast system. I thought I saw that the LFM would be limited in its capabilities because it depended for its lateral boundaries on a forecast from a twelve hour old run of a hemispheric model. I saw that it was fairly straightforward to set up a stand-alone model which depended on other models <u>only</u> for its initial analysis. The first guess in its analysis. I was helped very much by Ken Campana who worked with me in the very early days. He understood or had been there long enough to feel comfortable with JCS or JCL or whatever it's called - the protocols that are used to latch into, to gain entry into that system. I still don't know which is the IBM notation and which is the CDC notation for that acronym. We managed to run very efficiently on the IBM 705?

Hollingsworth: That would be the 361, 190 or so.

Phillips: Something like that...361, 190 or 195 or something like that...because we had to pace the ... it was core limited, very strictly core limited and we had to pace the input and output so it would be read-out and read-in while computations were going on. Together with Ken we got that to be rather efficient. The first time we ran it with real data it made a fairly good forecast. We were very careful with the code and so forth. We didn't have any physics in it for a long time, nor did the LFM really, of any meaningful character. Then NMC began working hard on the principles of optimum interpolation in the mid-seventies and also under Joe Sella they got probably the first operational spectral system into operation. Although again, the global model had very little in the way of physics other than surface drag and condensation in it. It was a time of considerable effort and flux at NMC--a good time to have come there. It was known that FGGE was on the way, because it 1974 when I arrived at NMC. John Brown and I established very good relations and he was really working very hard to make sure that the forecasts during FGGE would be at least as good as those that were made before FGGE. That was his...that's what he worried - that the analysis system would fall apart when presented with the new data--satellite data and so forth. He managed to direct the division very efficiently along those lines. A lot remained to be done, especially the system that Andrew Lorenc and others developed at Reading was a notable improvement over what we were doing. They did an awful lot of good work.

Hollingsworth: When I visited you in 1975, I guess it was, after a short visit at GFDL, you had a code almost written at that point for the NGM but it didn't become operational for a long time after that.

Phillips: There were two reasons for that. First, we entered a competition for a new hemispheric model in 1977 at NMC. The NGM obviously failed in the outer regions where the grid was very coarse. The primary reason was that the run time was too long for the operational requirements--could not be satisfied with the 195 speeds. When the benchmarks were being designed for the succeeding computer I pointed out to Fred Schuman that the NGM was a logical candidate because we had written a code in a very efficient way for the 195 whereas the other forecast codes at NMC had more or less developed in an ad-hoc manner and they were easy marks for any computer company to improve upon in their own state-of-the-art codes. So the NGM was one of the principal, maybe the principal benchmark, when we ended up with the 205.

Hollingsworth: There are two major areas of development in the evolution of the NMC, NMC's models over the period from the late seventies - the time of FGGE - into the mid-eighties. That was in the area of analysis, initialization, assimilation generally - that was one area. And the other area was in the role of physics in models. NMC seemed to have a very considerable reluctance to implement or develop an extensive physics package for any of its models.

Phillips: I think that's certainly true. I think most of us there who should have been able to do it were unfamiliar with the theory of radiation or sufficiently unfamiliar with it so that we didn't think very much of going back to it. It took a while, I think, to realize that it really was important, even in a 48-hour forecast, to allow for the radiation effects. The other physics had more or less been in the model, or being put in the model, but it was the radiation which basically was not in the model. We all had surface heat flux, surface stresses, condensation, you did or didn't have internal turbulence, and that's more satisfactorily handled now today perhaps than it was then by anybody. Radiation was the main element that was missing. The LFM for example, had a simple six-tenths of a degree specified radiative cooling, modified only by some slight attention to the moistness of the lower layers of the troposphere.

Hollingsworth: Their treatment of convection, for example, was a little cavalier in that all convection had to stop at 300 millibars.

Phillips: Oh yes. There were those historical archaic remnants of probably originally machine limitations. The six-levels was definitely chosen as a machine specified bound. When Schuman developed that system he had his own version of how to treat the top of the model with a stratospheric layer--his potential temperature was constant. Once you've done that and you started out at the bottom of that layer at the tropopause, in some parts of the atmosphere that will occupy as much as thirty percent (30%) of the atmosphere, and other parts it'll be perhaps only ten percent (10%). You can run into considerable difficulty with the hydrostatic equation if you start trying to be too cute about temperature effects and in computing condensation pressures under those circumstances. So it, the Schuman model, and by that we should include not only the vertical structure that he assigned to it with the arbitrary 50-millibar bottom boundary layer and its top layer, but also the somewhat complicated horizontal finite differencing that Schuman worked out involving averages of various kinds, typically crossed. You'd average the DDX finite difference operator, you'd average that in Y, things like that--which he found with a lot of empirical work would give him the system of the "primitive" equations that would last for some days.

Hollingsworth: Did you ever, I should know but I don't know it, if and when you introduced a data assimilation system for the LFM itself or, sorry, for the NGM itself?

Phillips: It's still not been done. There was work being done in the last months I was at NMC, on using the NGM to recycle by itself with a view to being able to accommodate the forthcoming Profiler wind data. I don't know what the status of that system is now, it may be having to be phased back, but NMC had accepted responsibility to provide very quick analyses of that data for realtime use by aircraft so that you would have the latest analysis an hour or two after the observations were made.

Washington: Norm, I wonder if you could kind of give some rationale as to why you picked out the particular numerical scheme versus others that were possible, including the spectral.

Phillips: For the nested-grid model?

Washington: Right.

Phillips: It was very parochial. I knew I would want under operational conditions a very stable, robust system, especially with the multiple grids interacting both ways. I had found this system based on the Eliassen grid which Tony has pointed out to me as actually thought of by Richardson years earlier, but modified so that it was the single step procedure somewhat analogous to the Lax-Wendrof scheme. But that was a very robust scheme. It had no computation modes whatsoever and therefore it corresponded very closely to what you wanted to happen physically - what you wanted to imitate - that was...I could have made that decision probably before I arrived at NMC. I never regretted it. There were cases where one could see that influences coming across the lateral boundaries from the coarse grid to the fine grid out in the west were not handled correctly. There seemed to be kind of a refraction effect due to the computational phase speeds being faster on the fine grid than on the coarse grid. The disturbance's phase speed parallel to the grid interface was fixed, because it was, so to speak, an oncoming wave from the coarse grid, it looked like the fine grid obtained the faster phase speed by making the disturbance move too fast to the east, into the grid. But those are too subtle for me to consider in detail.

Hollingsworth: In the field of assimilation for the medium-range forecast model, at what point in the development of your interest in assimilation problems did the need for an accurate first-guess first really become an important issue for you?

Phillips: I think it was mostly by learning from the European Center's experience and realizing that the arithmetic being done to combine the first guess with the observations was not much different between the NMC system and the European Center system. The European Center's first-guess was better and that was at least an easy way to understand why one aspect of why the European Center's medium-range forecasts were better than the NMC ones. That was a contributing factor for me. We never saw any particular cases where the first-guess, or very many cases of these, where the first-guess was very bad in a systematic way. (It's hard for me to pick the right words here.) There are always cases where the first-guess makes an error so that you could throw out good data simply because the first-guess had a low, five degrees latitude off course. If you discount those cases, I don't think we ever saw any cases where we could clearly say that the model is always making heights ten feet too low in this region and we're not getting quite the impact from the data, therefore that we should correct it. There was this case that you and Lorenc and Steve Tracton worked on with Andrew Lorenc, comparing the FGGE analyses that showed - which had a considerable element of that in it - that the first-guesses were different. Enough so that in some cases the data was thrown out in one of the models but not in the other. That was about the time that it was sinking home that the usability of the data was often affected - but not often -but occasionally severely affected by quality of the first-guess.

Hollingsworth: A lot more questions about initialization and analysis where you had a great deal of activity and made many contributions in the early eighties but I would like to go back a little now to the area or to your involvement with the FGGE project. That sparked off many many developments in assimilation and in analysis and in forecasting. It looks like by being at NMC you got pitchforked into the middle of it around about 1976 or so when the U.S. National Committee....

Phillips: That was an enjoyable experience. I think I had to work harder on that Committee, did my homework more on that Committee than on any other of these national or international committees that I was on. It was very exciting and interesting and one was working at that level with people from an international collection of very committed and capable--and they were going to make this experiment work.

Hollingsworth: What were the biggest issues that you had to be involved with?

Phillips: The ones that....It was always a nagging worry that the initial design of the data requirements was being watered down and it was in effect--that the final decisions as you recall were that in view of the obvious financial and resource limitations that instead of observing the entire atmosphere intensely for an entire year that there would be certain special periods in which only....which were the only periods in which some of the more expensive instrumentation like aircraft recon flights would be implemented, tropical radiosondes. That was the nagging question all the time as I'm sure must have been in the minds of Smagorinsky and Wiin-Nielsen who were involved on the international FGGE panel, and there was a question that the second satellite was going to be delayed.

Hollingsworth: This was the polar orbiter?

Phillips: Polar orbiter - second polar orbiter was going to be delayed nine months. They had to make a decision to go ahead without it. This decision had to be made before the first one was even operational, I think. Then the distribution of resources for the tropical weather recon planes was critical. There just weren't quite enough -about fifty percent (50%) shortfall - in the planes that you might have been happy with. Paul Julian worked very hard on designing this from NCAR. I think that program worked out better than one could have had any reason to hope given the limitations in the aircraft. The data - we were never concerned very much with the data processing. That was handled almost completely by international committees. If I had been living in Finland or perhaps in the Soviet Union that would have been a serious matter, but somehow we didn't concern ourselves with that very much. Probably there was someone in the weather service or NCAR taking care of that.

Washington: As an experiment, do you have any feelings for the overall success and failures of FGGE?

Phillips: It has been an undoubted success but probably not quite in the way that Charney's original thoughts about it were constituted. He wanted it to find the limitations of deterministic forecasts - in the early sixties. A year is probably not enough to exhaust that question---to get a definitive answer to that because the predictability of the atmosphere varies from one month to another. But the basic idea that there would be a store of data carefully processed and archived that would be a resource for studies of questions similar to that has certainly been carried off very well even though several reruns of the data processing have been necessary. I think that was a marvelous success. Perhaps the only danger being that in retrospect it seems easier than it was at the time and it makes it too easy to think of other systems as being able to be spun up without an equivalent commitment. Other even smaller programs with smaller scope do require an intense commitment.

Hollingsworth: Who were the prime movers and shakers in getting the political commitments and financial commitments from the U.S. government?

Phillips: I wasn't too involved in that certainly but I would guess that it was primarily Bob White. I would defer completely to anything that you might learn from him on that score. Incidentally he might be a good one to interview on these matters.

Washington: He's scheduled.

Phillips: Is he?

Hollingsworth: Un hum. OK. OK.

Phillips: And another one would be Fleagle, Bob Fleagle.

Washington: And maybe Tom Malone--

Phillips: Tom Malone. Yeah.

Hollingsworth: Those guys seemed to have been holding the levers of financial power in U.S. meteorology for decades and those names keep coming up all the time. Right through the fifties, sixties and seventies.

Phillips: I don't think they would say they had the levers of power, they knew telephone numbers [laughter] addresses. And credibility so that people would, who could make decisions about budgets, paid attention to what they said.

Hollingsworth: Given NMC's involvement in production of the 3A data sets, and in fact of the (unknown?) assimilation system to receive this new data, it would have seemed like a likely place to have done the 3B production as well as the 3A production.

Phillips: I think the resources Schuman....did not see the resources available to do that. All we had was a 195 computer: GFDL had two 195s if I remember right, and they got a fifth generation computer before we did, as did GLAS. All that was post FGGE. There just weren't the resources and we were struggling to catch up with the European Center in the daily forecast.

Hollingsworth: We'd only started, nobody....

Phillips: Well, but we knew what we had and I think the (not intelligible?)

Hollingsworth: The other place that I had of thought might have taken on the job and had some capability up to '78 or '79, and indeed into 1980 I think, was NCAR. NCAR had worked on the....through Julian and Lally on the instrumentation but there had been a definite decision taken at NCAR that the 3B analysis was not something they wanted. Do you have any insight into why that decision.....

Phillips: I can imagine why although certainly you should, maybe you should ask Kasahara and Warren there. Had Miyakoda found out at Princeton when he did the 3B at Princeton, that's a major task you're undertaking when you're going to receive data, whether it's in realtime or archive data, and run an analysis system on it, and to set up that whole system from scratch---which is what they did at GFDL and what would have had to be done at NCAR. So I think that Miyakoda deserves some kudos for doing that and I'm sure half way along he wished he hadn't, but it's been useful to have his analysis scheme which is different. It was not an OI scheme, operated on the FGGE data. But I really don't know for sure why NCAR declined the honor.

Hollingsworth: It was striking for us setting where we were. We were greenhorns in this area as in every other area of NWP when we started. The main operational center didn't want to go to bat and try to get the resources that would have made it possible to do it at the most natural place in some sense. There had been, for me at least, this very paradoxical article by Chuck Leith in 1978 where he described numerical weather prediction as an exhausted technology or dying technology. He set out in great detail the enormous gap there was between potential skills and the actual skills which looked like a marvelous opportunity and then interpreted that as a signal that it was impossible to do any better. So it seems to have been a peculiar....

Phillips: I don't think that his article made any impact among the operational community. In fact, I don't remember where it was published even, it may have been published in a somewhat obscure....

Hollingsworth: **Annual Reviews Of Fluid Mechanics**.

Phillips: No one at NMC ever read those. I didn't after I went there. I read them frequently at MIT but ... so ... there had no dampening effect whatsoever. And administrators had not read it unless he sent them reprints.

Hollingsworth: It seemed to us at the time [interrupted]

Phillips: I don't know if that was a reason that .....

Hollingsworth: It was around about this time, '78 to 1980 that NCAR finally decided that it was going to disband its isolation booth and get out of that business altogether.

Phillips: I was an infrequent visitor to NCAR in the seventies and eighties so I am not familiar with their decisions.

Hollingsworth: I'm not sure which question to ask next. Let me talk some more about the FGGE data. One of the things that was very surprising for us in Reading to hear was that somehow or other in the middle of FGGE, Norm Phillips was working on retrievals. How did you get involved in that?

Phillips: In '79 or late '78 John Brown had a heart attack. He was not present to pummel his branch chiefs into responding aggressively towards developing a program to monitor and assimilate satellite data. So I stepped in and got authority from Fred Schuman to be the czar within the Development Division of what should be done in trying to meet this oncoming flood of data. Desmarais at NMC was the one person who had, up until that time, worked very hard. He'd developed codes so people could look at the data and to make it accessible to the existing NMC analysis schemes. He and I worked hard on that and I developed programs to monitor the receipt of the data which turned out to be very useful to be able to call up NESS people and tell them that something was going wrong in the system and the delivery. Eventually those problems disappeared or they developed their own realtime delivery monitoring system capabilities so that I no longer had to worry about it usually. Then it turned out that the forecasting people in NMC noticed occasionally some very bad data from the satellite in regions where there were enough aircraft reports that they could be confident that something was wrong with the satellite data out over the ocean. We early on found out that the satellite data over land was best left unused, especially over the western part of North America, and used it only over water. I decided to track down some of those most outlandish cases. I think it was mostly a feeling of irritation that the fact that Roy McCarter, who was senior - well he was the branch manager in the analysis section. A crusty old Air Force fellow - <u>very</u>, <u>very</u> conscientious. He worried more about NMC's analyses than anyone else at NMC, but nonetheless, a very crusty fellow. He was one of those who pointed these out to me. So I took it as a challenge and we found out that it was in cloudy regions and these were microwave retrievals and it turned out in this case that the NESS retrieval system, which was a statistical system that Bill Smith had devised, was ignoring the fact that the microwave radiance was not coming from the sea surface but was coming from raindrops. The result was a very cold sounding. I was convinced of this very quickly by looking at all the maps for December of '79 or '78 - I forget which it was - showing where these reports were bad. They immediately changed their retrieval system. But in the process of doing this I realized that their "third paths" [retrievals based only on microwave radiances—NAP] didn't seem as good as they could be. They were not using any control of the sea surface temperature. They used the sea surface temperature field while they were making retrievals but it was the analyzed field from NMC which depended on a variety of data. The NESS system did not use the sea surface information that was available on those channels that were used especially on the spacecraft, I've forgotten some of these details now, to make their sea surface analysis.

Washington: I'm sort of curious, Norm. How come that NESS didn't verify for the places where they had their fine data, the retrievals system?

Phillips: They did. Over land.

Washington: But they didn't do it over ocean?

Phillips: No,they had.....it was difficult for them. They unfortunately had budgetary policies so that they had almost no in-house programming capability. They had to contract out all of their programming which meant there was a long line between an idea and getting someone to write a program for it and test it on a computer. Plus, [you?] gradually get red tape involved in that request, in forms and so on. Whereas I could just sit down or come to the shop at night and lay out a program and do it that night. I had access to the people at NMC and those who had been there from the beginning who knew how the data was defined and located on the NMC data disk. NESS really depended on NMC to carry out this function. They never emphasized it too much that it was our responsibility but it became apparent that that was the only thing that made sense, and we took it over for a while. They have recently begun to get more clever on these things and more powerful at that. When they start getting telegrams from ECMWF that "what's wrong in the South Pacific at 200 millibars," and you get four or five of those, and their administrator begins to...

**END OF TAPE 3, SIDE 1**

**Interview with Norman Phillips**

**TAPE 3, SIDE 2**

Hollingsworth: I hope we didn't lose too much on that.

Phillips: At any rate, so that I could quickly develop my own statistical scheme, using whatever channels I wanted to as input data -- of course I talked with the NESS sounding people, Larry McMillin and others, Dave Wark, to get their advice. I worked with Desmarais on this also, and we came out with our first assessment of the FGGE errors over the ocean which we - of the satellite errors over the ocean - retrieval errors - including vertical correlations which were the only information on that for some years. Then I turned off the infrared part of that system and concentrated on the microwave retrievals over the Northern Hemisphere oceans. Those were the operational NESS retrievals between 30 and 60 north, I think until about two years ago.

Hollingsworth: Despite the fact that you were involved with the NESDIS on - NESS or NESDIS - on the microwave retrievals, that seemed to be the only link - it was a backdoor type link where you were doing some free programming for NESDIS. It seemed to be an in-built problem in the design of these organizations that they were set up as really an operational organization in some sense. That you had a problem, you designed a solution, the solution by definition worked and then you implemented it and you let it run. Whereas in our experience has been that the tighter the link between operations and research the faster you make progress both in operations and in research. In those institutional walls within NOAA didn't come down in 1980 even though you had demonstrated that it was....

Phillips: In the beginning in the FGGE years and for a few years after, there was a very powerful NESS committee that met, I think bi-weekly, headed by Harold Yates and the engineer who came out here to Boulder and was head of Boulder Lab for a while [C. Ludwig-NAP]....but the two of them, they were really operating the system - both the research and the operational system within NESS. Those within NESS were tightly related, I think. But Desmarais and I were on that committee for three or four years. Then about the time that Dave Johnson retired as head of NESS and these changes took place underneath and that committee was modified and even though I tried to get invited to it none of the administrators whom I spoke to about that ever invited me, the cooperation with NMC deteriorated to some extent. It took place mostly between their operational people, was a more direct thing between their operational people and Desmarais and me by telephone with us having no impact whatsoever on NESS planning. It was an unfortunate occurrence.

Hollingsworth: I think the situation has improved a lot in the last year or more because we have....

Phillips: Maybe because I retired.

Hollingsworth: Well, I think we have come up with some more evidence of the need for such a much tighter link in that area and it's certainly looking better. OK. So that's NESDIS and we've talked about FGGE. The other area now that I'd like to come back to is the area of analysis and assimilation again. I have some personal recollections of this. I'd like to ask you for your views of your own development in that around about 1980 or 1981 you wrote a paper which said that OI, in essence OI wasn't really working or the combination of OI and normal mode initialization really wasn't doing the business. So there was a need to have a final step in the assimilation which would be a Sasaki-type variational analysis to be run after the OI analysis. You published that and I was very surprised by the results in there because it seemed to me that they were wrong and that if you did the OI right and did the normal mode initialization right, then there was nothing more for a variational analysis to do. I talked with some of the people at NMC and they shared that view but nobody was willing to take you on and challenge you on that.

Phillips: At that time the OI I was familiar with was not a very good OI in operation (the NMC one). That may have colored my views. But the next paper I published contradicted that previous paper completely and said if you did the OI right there was nothing left for a geostrophically conditioned Sasaki analysis to do. I was engaged in the process of learning that time. I usually have always withheld publications, I have not published as much as many people but in this case I probably should not have published that one paper.

Hollingsworth: I thought it was in some sense an indication of the awe in which you were held within NMC. People really were in awe of you and weren't prepared to get in the ring and wrestle with you.

Phillips: Oh. That may be but I tried not to encourage that awe.

Hollingsworth: Were you aware or conscious of this yourself?

Phillips: Oh, yes, if I thought about it I could realize there was occasions on which I had the last word, occasions on which I had the last word but didn't mean anything also.

Hollingsworth: Then, after that....

Phillips: I think the situation is that the other people at NMC were not basically theoretically inclined in order to consider what was being done at a deep level or semi-deep level in analysis. They'd come from the background of the Cressman analysis - a very empirical process. They knew about geostrophic adjustment but basically were not theoreticians in any sense. Like we were talking in the car this morning, if you're not a theoretician you are inclined to accept the theorist as correct, or if you're a theoretician you're inclined to accept observations as correct.

Hollingsworth: But then after that you got involved in a lot of work on the statistics of short-range forecast error which are a basic building-block of the OI analysis and it seemed to be a fashionable subject amongst a small little coterie because of people at GLAS, Balgovind - those guys. They produced a statistical model for forecast errors, and you produced one, and there was a lot of activity about Kalman-Busse. You were advocating that. I remember that at the Stanford seminar in '82. Michael Gill was....

Phillips: We weren't advocating it, the....Kalman-Busse is, strictly speaking, is where you predict the evolution of the error covariance matrix and that is n-fold times more complicated than predicting the field itself. No one is about to do that. The OI is the shortcut in which you replace that synoptic distribution of error covariance matrix by a statistical climatological prescription. That's what the GLAS people were doing and what I was doing by offering various models to explain typical observed error covariances. They came out first with what is essentially the Rossby radius of deformation for the first internal mode as the horizontal co-variance distance. It seemed to me that that could equally well be explained by - that would also correspond to the synoptic instability scale. It would be fun, and I use that word advisedly, it would be fun and interesting to see what would happen with another assumption. I thought, well, let's assume random errors in Rossby modes and it explained a fair number of things. It didn't <u>explain</u> it but it imitated a fair number of features seen in real errors. But it was clear it would be a valid model only under certain idealized circumstances. So I could write the paper with these features in it and then I had an excuse for any situation in which the predictions were in error.

Hollingsworth: It was around this time too that you got involved in something that must have been quite new for you was the -- you were a member of a Working Group on Numerical Experimentation - or some such group - and you had to write a paper which obviously had political content on nuclear winter.

Phillips: Oh. This was the...Dick Hallgren talked me into being a member of the Joint Scientific Committee for the World Climate Research Program. I think the first meeting I went to was in China. George Golitsin and I got tasked with the idea of, I don't know that it's an idea, but with the job of writing a summary of studies that had been made on nuclear winter. This had some kind of a political history in that the Committee had been asked to do this. P. Morel had been asked to convey this message to the Committee by the WMO Executive Committee. I think was a response that had been engineered by ...I suspect this was a response that had been engineered by Hallgren and Mason to cool down some of the rhetoric that was coming at the WMO from the Soviets. I read many of the publications that had appeared in the past year -they were coming out hot and heavy, and George did the same in Russia. We met together in London the next year, a few days ahead of time, and put our thoughts together. I drafted something and sent it to him and he added and changed it and I polished it up. Then it was passed on to the WMO in the time limit that we were given. It was actually printed by the WMO as some kind of a temporary document. But it contains no new theory -it contains only our interpretations of what a rational person might reason from the viewpoints that had been presented. It was not a basic....input -- it added nothing new, I don't think, to the ongoing discussion. It's a summary so that administrators could read it and get a view of what two meteorologists thought about this noise that was happening about nuclear winter.

Hollingsworth: Were you lobbied in any way in writing this?

Phillips: No, I think not - I wasn't lobbied. I remember that there was one meeting at the Cosmos Club that Hallgren and Tom Malone, in this time frame, John Perry, in which the main topic of discussion was how could Hallgren counter this pressure from the Soviet Union at the WMO. I just sat quietly throughout much of that meeting. Didn't find a very cheerful view of meteorological politics.

Hollingsworth: Was that your only exposure to real high-level meteorological politics or had there been any earlier episodes of that kind in your career?

Phillips: No, I'd never been that close to it. I gave the Sixth WMO Lecture about three and one-half years ago in Geneva. Hallgren had a party that evening to which there were a surprisingly small amount of foreign representatives invited. One of them was a young lady from Brunei. I don't remember if before then or later that it came out that the Sultan of Brunei was one of those who had been tapped for contributions to the Contras in Nicaragua. Either that or she was there because maybe the Sultan would donate some money to the WMO. I don't know. But certainly in terms of meteorological "throw weight" she didn't deserve to be there.

Hollingsworth: During your time on the JSC were there any, apart from the nuclear winter issue, were there any issues that you thought were important and that you ought to express strong views of them?

Phillips: No. It was, it struck me it was different -- I quickly realized that this was different a program than FGGE. Before I went to the first meeting I had only the FGGE experience to draw on but it became quite clear that the World Climate Research Program was a long time affair. One thing that bothered me the most was the difficulty oceanographers were going to have in their modelling of the ocean, in modelling small-scale turbulent transport within the ocean. Computers would eventually enable them to handle both the Gulf Stream eddy sizes and the entire ocean basin eddies. But the small-scale-meter; and less-turbulence was something which I didn't think they were...knew, or anyone, not just them, anyone knew enough about to confidently parameterize in order to make preliminary assessments of things like where CO2 were going in the ocean, methane, and so forth. I didn't feel...outside of reminding John Wood and others of these features during the meetings. I didn't see there was anything useful to be obtained by emphasizing it more than that. He was, after all, devoting his life to attempts to measure and infer such processes. You can't tell a man that's not good enough - they haven't gone far enough in that. Especially since he is being very ingenious and successful in many ways.

Hollingsworth: What did you see as the key difficulties in that area?

Phillips: Well it would be the sporadic....probably the sporadic nature of the turbulence. I know of no insightful additions, comments to make. But I worry about it.

Hollingsworth: Still sticking with the political science aspects of your work over the last ten years or so, you've been a member of the Academy now for some long time. I don't know when...

Phillips: '76.

Hollingsworth: '76 you were elected. That is a very influential body in U.S. science and presumably in funding -the direction of funding that U.S. science.

Phillips: No, nothing like I gather is said or done in England. Nothing at all. It mostly is set up to respond to specific studies that are requested and funded by parts of the executive branch of the government - and for the Congress. Their most popular documents are three or five year updatings of diet suggestions for farm animals. They do a lot of very good work. They publish and do other studies - and lately Frank Press has been trying to get the Academy to have enough money of its own - endowment - so that it can sponsor studies that it thinks are due even though they have not yet been asked by the government itself. It's the National Research Council which is the so to speak executive arm of the Society, not the Society, the Academy that carries out these studies. That's where all the meteorological committees are organized.

Washington: Norm, you've chosen not to chair committees, I guess that are of the Academy. Is that correct?

Phillips: I was involved quite a bit in the sixties and off and on in the seventies. I just thought it time enough to get out of the way.

Hollingsworth: Have you used your position in the Academy to try to get certain studies done?

Phillips: No. I've been I must admit a somewhat passive member of the Academy. Most members are very passive. No, I've been involved since I've been a member. These different committees and panels of the Academy are, they like to have as high a representation of Academy members as possible. I've been on those since I've been an Academy member but it's tapered off.

Hollingsworth: Were there any of those studies that you were involved in that you enjoyed or felt were really worth while?

Phillips: No, I can't say that they were. They're kind of housekeeping duties that someone had to do. By being on the committee or panel you could see that they were done in a reasonable workmanlike manner. That was all you could expect. There have been some slightly more influential actions. For example, on ad-hoc panels, there was an ad-hoc panel that wrote up a statement for the **Science Advisor**, it's listed in one of these documents, of the things that the government should sponsor financially in meteorology, atmospheric sciences. We emphasized NEXRAD - I spent some time getting briefed on NEXRAD so that I could conscientiously push that as a legitimate push for the government -- and the Profiler. That was taken up again the next time the Atmospheric Sciences got a whack at it. They are still going forward. I remember the first time, Benton, as a representative of NCAR I think, came to us and "waved" a newly printed STORM - document, said that we should endorse this. I remember objecting strongly to that situation. We hadn't had a chance to read it. We were doing some things that would be beneficial to the STORM concept.

Hollingsworth: Change in tack again. I'm going to lead up I hope to finish this tape by talking a little bit about your monograph the WMO monograph. About where numerical weather prediction and modelling generally is going....

Phillips: Oh no, it has nothing to do with that.

Hollingsworth: Well, I think it has something to say about that.

Phillips: OK. Go ahead.

Hollingsworth: Before that, I guess as a denizen or a denizen of ECMWF for the last decade or more, do you think that organization has contributed much?

Phillips: Oh, absolutely.

Hollingsworth: In what ways?

Phillips: They have the talent and the money, computers, the power to do what should be done in the field of numerical prediction. And set an example for the other European counties themselves as well as several groups in the United States. Not just NMC and NCAR but also the Navy, perhaps the Air Force at Offut. And brought life, I think considerable life, back into large-scale weather prediction. A system that had not yet been exhausted.

Hollingsworth: How far from exhaustion is it? Or do you think there's a whole new world out there?

Phillips: I've made too many....I have not made <u>very</u> many -- in the early days I made statements of this nature that I think were wrong and I'm not about to make any statement. [laughter] Maybe too optimistic in the early days but now I'm not going to counter that by being too pessimistic.

Washington: There is I think a healthy change in NMC and I think Tony mentioned something about this earlier. In fact I'll be taking part in it later this week, as part of the UCAR Visiting Science Program. That change I think is how NMC is bringing lots of younger scientists in, stimulating the environment I think, bringing in some new ideas, different approaches. I think there was a period that NMC was kind of a dull place in the sixties.

Phillips: Maybe so.

Washington: I think having some senior people like you around probably helped a lot with some of these people.

Phillips: One of the things I was proud of starting at NMC was the weekly sack lunch seminar we had there which occasionally brought in outside speakers but they were frequently scheduled independently. But at least let us know within the building and within NESS occasionally, and GLAS, to some extent University of Maryland, what other people were doing on the local scene. That may have helped give a more academic tone to the place, the research aspects. I think the vision has been broadened in a large scale by the location in the building within NMC itself proper, of part of the NOAA climate people. It's less parochial than it was in the sixties.

Kasahara: I found that NMC's seminar activities was quite high. If you count also seminars given at University of Maryland and GLAS, the Washington area is one of the extremely high concentration of seminars. I'm sure that I found it very challenging. Is that what you have noticed?

Phillips: I think that yes, that's changed a lot. I mean GLAS coming down made a big difference. In fact, a noticeable fraction of the development division in NMC now consists of people who used to work for NASA.

Hollingsworth: It was probably a healthy development. Is there cross fertilization? Is there traffic the other way as well?

Phillips: There was one example where Bob Livesey went from NMC to NASA. I think he's still there at this time but I'm not sure. Civil servants are civil servants and they do prize their job security and relatively few of the people at Goddard have that security.

Washington: Norm, I just wondered if you had any philosophical statements or thoughts about, you know, the early periods of numerical weather prediction concentrated on trying to improve the data and the initialization. Now as we attempt to do more longer range forecasting, where the difference in between NWP and climate is kind of muddied. Is that sort of where the big payoffs are going to be, do you think? In the climate end and longer range or still on the shorter range?

Phillips: I think you can safely say that von Neumann's division of the problem is not turning out to be correct. You remember he said the easiest thing to make will be a short-period forecast, the next easiest thing will be to make the steady-state equilibrium, and the most difficult will be to make a medium-range forecast? I think what we seem to be finding out is that the questions we want to know about the final steady-state, the real climate, require information that we can only obtain confidently by trying to extend the medium-range forecast. That the test for general circulation models will rely very much on....will contain a very large element of testing its forecast skill in the intermediate medium-range. The Miyakoda approach supplementing so to speak Sasaki's approach--not Sasaki's but Manabe's approach, at Princeton: operational medium-range models being the standard against which the climate models have to justify themselves. Because some of the questions that you have to answer are really very difficult. Like the increase in CO2; what is the effect on the clouds? The clouds are a very important part of the thermo-regulatory mechanism of the atmosphere. Perhaps even more than CO2 itself. Definitely so because CO2 is important only where it sneaks in between the gaps in the water vapor spectrum and the clear gaps between the clouds. I sort of argue that there has to continue to be a healthy, a strong exchange of ideas and methods between operation predictions at the extended range and climate models. And you might extend that principle to say that to some extent the same thing must be true between the extended range model and the shorter-range model. When it comes to questions in modelling convective precipitation model on the short-range forecast, you can study processes over the region where you have good data with the short-range model. But where you try to make a longer-range forecast you are going to be handicapped by influences --- regions with less data and obscure the results of model effects or defects. So that you are like . . . to test the ability of a model to generate and forecast the mesoscale convective complex - not a hurricane (which is a completely different phenomenon) but a convective ?storm. You'd like to be able to do that and include that in the long-range forecast model because, for example, in the central United States most of the summer rain occurs in those systems and you'd like to be able to predict that more than a day ahead of time or more than twelve hours ahead of time. There's this exchange between the forecast time scales that I believe throws von Neumann's concept completely out of the picture.

Hollingsworth: Would you like to push that idea so far that you would say that the models have to be involved too in the interpretation and synthesis of the observation? Particularly the remotely-sensed data.

Phillips: Why, I suppose so, yes, yes. The technical questions of how you do that are a matter where you'd want to explore different approaches because it's probably not something that you can design ahead of time with theoretical certainty-- explore different possibilities. What may work in one system may not be the approach that works best with a different type of observing system or different location. But that's a rather platitudinous statement so I.....

Hollingsworth: There is certainly a push on nowadays to try to involve the simulating models very closely in the interpretation of radiances, for example, for sounding purposes.

Phillips: And there are dangers in that. The people doing it I think are aware of those dangers.

Hollingsworth: So let's talk about the monograph then which has two purposes as I understand it. One is to hammer home the message that there's a real problem still with the accuracy of our initial data for forecast purposes over the...

Phillips: Many portions of the earth.

Hollingsworth: Many portions of the earth. That I think is probably one of its primary purposes. The other aspect of it seems to be an attempt to explain why forecasting in mid-latitudes is possible at all.

Phillips: Yes. I was recognizing...I think I had had some idea of that way of looking at matters for many years. Of just gradually becoming more and more aware of it, that we're lucky. As it's spelled out in the monograph it's done in a rather didactic or socratic method I don't know what the... or no, dialectic manner. The main idea is that the atmosphere is nice enough to us that it prevents or minimizes the latitudinal spread of influences. This is what made Charney's, Fjørtoft's and von Neumann's first forecasts really possible. Ertel had tackled the question in 1940 or '41 or so, actually before Charney got on the scene, and concluded that you would have to consider the whole earth because of the Laplacian turn in the vorticity equation. Charney in his paper said well, no, the group velocity. But then he ignored really some of the literal consequences of following the group velocity arguments and implicitly considered only synoptic scale disturbances whose group velocity is not as large as the more planetary scale disturbances.

Washington: I wondered if you could mention if Kibel had any inkling of this?

Phillips: I'm not familiar with very much of Kibel's work. I don't recall seeing anything in his book on numerical weather prediction--that was new to me. He may have. It was only recently that I came across the papers by Ertel which - must we stop now?

Hollingsworth: I think so.

**END OF TAPE 3, SIDE 2**

**Interview with Norman Phillips**

**TAPE 4, SIDE 1**

Phillips: Ertel had visited Rossby at MIT sometime in the late thirties, but there is no record of this at MIT. The papers he published in 1940 and 1941, after the war had started in Europe, showed that he was aware of Rossby's formula which was published in late 1939. Reading between the lines, one can see, I think you can see, that what Ertel did was to derive a barotropic model by reasoning somewhat similar to what Charney later used, but not as clever by far and with the sole object, I think, of duplicating what Rossby did intuitively. There are many parts of the literature that none of us are familiar with and it is very difficult to get a true picture of the progression of meteorological thought and when something is forgotten, like Jeffreys's 1919 paper, it might as well not have been written.

Hollingsworth: The phrase Shaw used about Richardson's book **"The Soliloquy On The Scientific Stage."**

Phillips: I'd never heard that before.

Hollingsworth: How long have you been thinking about these ideas that you put forth in the monograph?

Phillips: Oh, off and on since the early sixties. We knew about group velocity from our student days in Chicago. Rossby wrote his own paper on group velocity in 1945 and we certainly read that as T. C. Yeh, a student, exploited in a one-dimensional barotropic model. Eckhart comes along in 1960 and points out how gravity and soundwaves can move. Of course Charney had used group velocity in his 1949 paper. Dickinson did a thesis on what he called propagators, rays, in the late sixties. All these impulses from outside tend to make one beware of these aspects of the atmosphere. The thesis that Mankin Mak did may be one of the earlier studies - quantitative studies - on latitudinal propagation. That was around 1968, 1969 or 1970.

Hollingsworth: Somewhere in there. What kicked it off afresh? Was it the work of Wallace and Kutzler?

Phillips: Wallace's ideas of the empirical nature of the centers of action - correlation centers, and then the work by Hoskins, and the man from Australia....

Hollingsworth: Karoly.

Phillips: Karoly.

Hollingsworth: They're modifying some of their thinking on these ideas now because of the fact that the effect of vorticity sources as they describe it, of a heating region, involves the advection of vorticity by the divergent component of the flow which changes the way in which the Indonesian heating can affect the main jet in the Pacific so that they're coming up with a much more complicated picture of how tropics and extratropics interact nowadays than in that paper by Karoly and Hoskins.

Phillips: This may be like you were talking about in the car with me yesterday on the way here, where your work is in?

Hollingsworth: Uh huh.

Phillips: That's an interesting question. It turns out, as you may remember from that monograph, that you can compute a two-dimensional barotropic influence function. But in addition to a point in middle latitude, in addition to the obvious upstream point, there is a favored location to the southwest where waves can originate that will reach their turning point at your location at a higher latitude with a magnified influence.

Hollingsworth: There are some very interesting aspects of that result of yours which is coming out from related work but in a rather different flavor where people are doing, if you like, sensitivity studies. They're looking at the evolution of the flow in the short-term and then looking at trying to estimate what perturbations to that flow will grow most rapidly. Harlow has been looking at that, and Talagrand has been looking at it, Joe Tribbia here has been looking at it, and Molteni and Palmer at our place. What they do is they run a model forward, linearize about the trajectory which it has taken and then they show that if you look for the eigen- values of the resultant operator which is the forward model R, multiplied with its adjoint or coming back the other direction, the eigen- values of that are the perturbations to which the initial state is most sensitive. In many of these calculations......

Phillips: Why should that be?

Hollingsworth: Well....

Phillips: All of the eigen functions....

Hollingsworth: The eigen functions are ordered and you pick, you can assign them.....

Phillips: It's easy to pick out the ones......

Hollingsworth: It's easy to pick out the ones which are the most likely to grow and which will excite the most rapid changes in the flow. In many of these calculations which are done so far on wavy flows, they find structures which are highly structured and have structures very similar to the structures you found in your sensitivity study. On the south side, on the southern flank, of the strongest jets.

Phillips: I'm going to be scooped here! I don't know why it has taken so long to print that thing!

Hollingsworth: I was hoping that I might have been able to bring with me some calculations that have been done on a zonal flow, but they didn't get the calculations done in time.

Phillips: This is an extension of the things that Hoskins and Palmer did with the hypothetical perturbation in a disturbance in a zonal flow that created amplification....

Hollingsworth: Simmons. If you'd like a follow-up to that, there is indeed strong similarities between results you're finding as to where the erogenous zones are and we're hoping to exploit that for studies of predictability. So that is indeed one interesting aspect of it which I think is going to be followed up quite actively over the next couple of years. So I would urge you to get it out quick. Just for your own interest, there was one small item.....

Phillips: Did I send you a copy of that? Yes.

Kasahara: I guess you have seen the draft.

Hollingsworth: Yes, I've got a copy of it.

Phillips: It's fearfully out-of-date, I think, on Rossby wave critical points. I'm prepared to take the heat for that since that subject has gotten very abstract and hard to follow.

Hollingsworth: O.K. I think that is about all the questions that I have. It brought us more or less up to date on your scientific work. So I think I shall bow out at this stage and hand it over to.....

Kasahara: What would you like to do in the future?

Phillips: What would I like to do in the future? Martha and I are looking forward to her recovery so we can travel about a bit. I think I can live without meteorology, but I'm not sure.

Kasahara: Do you have any other hobbies?

Phillips: Yes, I play the horn. It was a happy situation in Washington because there are high-quality professional orchestras there and so the amateur orchestras are definitely lower-level and there were plenty of them for me to get into. But where I live now in New Hampshire, the professional orchestras are somewhat lower-level and there are not large numbers of amateur orchestras that are there that were available in Washington. But that's possible to.....

Kasahara: So you'd like to pursue vigorously on that?

Phillips: Oh yes.

Kasahara: So what else is it that you'd like to....

Phillips: I don't know. I go into MIT occasionally. They've been very gracious in providing a desk that I can use - share with someone else. It's not too difficult to travel back and forth - there's bus service. How much I'll exploit that remains to be seen. NMC would like me to come and visit them occasionally, but I've been unable to. Last fall and winter I spent writing this--reading and doing literature studies--for this paper on the emergence of quasi-geostrophic theory that I'll be giving at a seminar this afternoon. That required a lot of time and I took advantage there of the library that Jule had set up and which is still being maintained. A lot of this old literature, it turned out, was available in his library. So I'm really quite happy.

Washington: Well, I guess you have an open invitation to come visit us some time.

Phillips: Why, thank you. That's one thing about meteorology. It's been very rewarding to be involved in meteorology but also it has been so much fun to meet the people in meteorology. Without exception, I think I can say that everyone I met was a delight.

Washington: Any other final words?

Phillips: No.

Washington: Well, I think....

Phillips: Keep up the good work!

Washington: I think all of us thank you very much for taking part in this interview.

Phillips: I'm flattered by it. Thank you for setting it up and I think you're doing a very good service to future generations, to record these things. I urge you especially to get hold of some of the older people like Horace Byers and maybe Tom Malone, so forth. Bob Fleagle, for example.

Washington: Thank you.

**END OF INTERVIEW**