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TAPE RECORDED INTERVIEW PROJECT

INTERVIEW OF PHILIP D. THOMPSON
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Interviewers: Joseph Tribbia, Akira Kasahara

Tribbia: My name is Joseph Tribbia. In the room with me is Phil Thompson, Akira Kasahara. And the purpose of our meeting today is to interview Phil Thompson concerning the history of meteorology as he has seen it within the past forty-some years. The date is December 15, 1987, and we're at the National Center for Atmospheric Research in Boulder, Colorado.

I'm going to begin by noting that Phil Thompson was born on April 6, 1922, in Rossville, Indiana, and Phil, could you tell us something about your earliest recollections and about your parents and growing up somewhere beyond the age of zero?

Thompson: Well, I think my first recollections begin when I was about three years old. My mother was also born--no, she was not born in Indiana, she was born in Lincoln, Nebraska, between trains, but her parents lived in Rossville, Indiana. Her father was the county assessor for Clinton County, Indiana. I think for a short time he was mayor of Rossville. His wife was a housewife. My father came from Dayton, Indiana, which is a little town about fifteen miles away. His father was a farmer, his mother was a schoolteacher who subsequently ran the farm. My father went to school at Purdue and studied zoology, and then moved to the University of Illinois, where he was an assistant professor. I was born shortly thereafter in Rossville, and then moved in a few weeks to Illinois.

As I say, I can't remember anything back beyond the age of about three. I do have some distinct recollections of when I was age four. One of them in fact had to do with science, or with the interpretation of the natural world. I remember my father sent me early one evening to post a letter in an old-fashioned slot mailbox on the street. It was about half a block away, it was dark and the streetlights were just turning on. And I tried to put the letter in the slot and it wouldn't go in. And I noticed simultaneously that there was a streetlight that was flickering in a very peculiar, rather scary way. So I ran back home with the letter, and told my father I couldn't get the letter in the mailbox because the streetlight was making funny lights. He came back with me, and gradually learned I had been trying to put the letter in the slot end-wise instead of edge-wise, and he pointed out to me in no uncertain terms that because two unusual events occurred at the same time and at the same place did not mean there was any real connection between them. That stuck in my mind ever since.

Tribbia: You mentioned your father was an assistant professor? Was your mother a housewife?

Thompson: Yes.

Kasahara: So from age three, what were the most memorable things that happened--

Thompson: Well, I think that among my most early experiences were summers that I spent with my father on a laboratory boat that he used for making fish-population census on the Illinois River and the Mississippi River, and the lower Ohio River. It was a large boat, about sixty feet long, sixteen feet in the beam, four foot draft. It was fitted like a regular laboratory with running water, compressed air, gas and so on, sinks. It had a crew of five and me. This was a marvelous existence for a small boy, sort of like Huck Finn. Our practice was to go from one town to another, put down anchor and take a small boat and go ashore. We would explore the town, find out where the best restaurant was--if there was a restaurant, and then they would take the small boats out and the seines and make a fish count. And I spent the day either with them seining, or exploring along the riverbank. I think I learned a great deal from observing what my father and the crew did. I was interested in what they were doing. In fact, I was interested in all kinds of things. One of the most memorable things was that I learned that I could attract a great deal of attention by falling off the boat. The crew got a little tired of fishing me out of the water because they knew that I couldn't swim, at least they thought I couldn't swim, so they manufactured a necklace of large corks which they put around my neck. And there was just enough buoyancy so that if I fell in the water, my nose was above water so they could take their time in fetching me out.

Kasahara: Were you interested in science already at the time of high school?

Thompson: Yes. There was nothing conscious about it. I don't think my father made any attempt to influence me at all, but I was raised in a scientific milieu, if you like, and I think it was a foregone conclusion in my mind and perhaps even in my father's that I would be a scientist of some kind.

Kasahara: So you were greatly influenced by your father?

Thompson: Oh yes, very strongly. Much of my education, I think, was received at home. It was rather spotty. When I was four years old--well, it was partly because childhood illnesses in those days were very long and you had to be quarantined so that you didn't come into contact with other people. I remember when I was four years old, I had scarlet fever and was quarantined for six weeks. My father was not able to enter the house and my mother was not able to leave it. I think my mother got rather tired of entertaining me, so she taught me to read, in desperation, so that I could amuse myself. Then later, I was occasionally withdrawn from school to go on trips with my father, and then he taught me, or my mother taught me. There were other things that contributed to this spottiness. At the end of the third grade, my teachers persuaded my parents to let them advance me to fifth grade. So I then went through the fifth grade and then they wanted to advance me to the seventh grade. And we started to do this, but my father realized that when one is so young, two years is a very big difference in ages in the same class. The result was that he withdrew me from school for a year. And I spent that year with my grandmother. And the agreement was that I would then go to the seventh grade after that year. But the result was that I have learned wherever I happened to be. (I think I strayed a little bit from your question.)

Tribbia: In your earliest recollections then, do you feel that your father or were there any other strong influences in other related areas in which you've become well-known, say, mathematics and the physical sciences, since your father was primarily a biological scientist--who might have been influential in interesting you in areas along those lines?

Thompson: Yes, there were two other very strong influences. At that time there was a very sharp division between faculty and non-faculty in a university town the size of Urbana, and the faculty families tended to stick together and their children tended to stick together in little groups or cliques, if you like. There was a group of five of us who were very close, particularly during high school days. These were Phillip Anderson, Pierre Noyes, Henry Swain, Warren Goodell, and myself. We played tennis together and went ice skating together and generally raised Cain together. But we all of us had a very strong scientific bent, particularly in mathematics and physics. Well, in fact, Anderson, Noyes and Goodell all had distinguished careers in physics. In fact, Anderson was a Nobel Prize-winner in Physics in 1978, I think. And I think we all learned a great deal from each other because we were constantly stimulating each other. Through our late high school days until even in our college days, we had a kind of informal mathematical competition in which we would pose problems to each other, trying to make a challenge that we ourselves could meet but we hoped that they could not. Anderson was particularly good at this. Noyes, I think, had the better formal training, but Anderson had a flair and I think perhaps I had a little of that also. We tended to approach problems in about the same way, that is, we would use any method that was available to solve the problem. So my association with those people through my school days had a very large influence.

Also, during my high school years, I learned a great deal of mathematics as a sort of a game. Two of my father's friends, I guess, recognized that I had some interest in mathematics and some ability, and contributed to my mathematical education by posing puzzles, or problems. And when I got stuck with the problem, they would give me some hint or a little help and see how much further I could push it. And I didn't realize I was learning mathematics--it was a parlor game as far as I was concerned. But they got me up through simple differential equations. I remember that the last problem that I solved before I left home was a problem in celestial mechanics. It was not a trivial one.

I didn't mention their names by the way: there were Wilfred Wilson, who was in the mathematics department; his field was topology. And the other was Harold Mott Smith, who was professor of theoretical mechanics.

Kasahara: So you went to the University of Illinois?

Thompson: For the first three years, I went to the University of Illinois.

Kasahara: Before you joined the U.S. Air Force?

Thompson: Yes.

Kasahara: Why did you join the Air Force?

Thompson: Well, herein hangs a tale. I could perhaps backtrack and explain something of my interest in meteorology. Actually, I did not develop an early interest in meteorology for the sake of meteorology, but for other reasons. And one of them was that I spent a lot of time and energy in designing and building model sailplanes--that is, not scale models of real sailplanes, but were specifically designed for high performance, either primarily for endurance. And I realized fairly early that the very long endurance flights that were made were not gliding flights, they were soaring flights--that is, the flights in which the sinking speed of the sailplane relative to the air was less than the vertical speed of the air, or upward speed of the air. And it therefore became a question of detecting where the updrafts were. And I discovered that by watching the flight of hawks and buzzards, that I could detect pretty well where the updrafts were, where they entered into the growing cumulus cloud. And observed that there was in fact a tilt to the updraft, depending upon the winds. I also read enough about thermal convection to know that one should look for the beginning of the updraft over a field that had been plowed, but not freshly plowed. And the reason for that, of course, is simply that you wanted to pick an area where the heat was going into raising the temperature of the surface and not evaporating the water. So by observation I soon became able to detect where the updrafts were and I knew where to launch the sailplane and was thus able to get some very long flights. So that was one reason.

The other reason I became interested in some aspects of meteorology was because I loved to play tennis. During the summer, when I had real summer vacations, I had nothing better to do than mow the lawn and play tennis. I usually played five sets every afternoon when the weather was good. But in Illinois where I grew up, there are frequent summer thunderstorms, if there's enough moisture to maintain them. So I spent a lot of time observing the growth and motion of the thunderstorms. The tennis courts were about a mile away and I wanted to be able to predict pretty well whether the thunderstorm would pass over the tennis courts or not. So the direction of the thunderstorm was important.

Well, that was my original interest in meteorology. Then in my third year at the University of Illinois, late winter or early spring, I went to a Sigma Xi lecture and the speaker was a distinguished meteorologist by the name of Carl-Gustaf Rossby. I had never read very much about meteorology, but I was curious to hear what Rossby had to say. He talked about the general circulation, he talked about the mean structure of the general circulation and the physical mechanisms that maintained it, and talked about the various physical processes that interacted to produce these very peculiar motions of the atmosphere.

Of course, Rossby was legendary as a spellbinder and I was completely taken in. I went away from that lecture thinking that meteorology was the most interesting and the most challenging field of science there was. Up to that time, my studies had been confined mainly to mathematics and chemistry and physics and various liberal arts courses. I had no contact with the science of meteorology as such, at all. But I then went to the library and I read a couple of books, one of which was Humphreys' **Physics of the Air**, which I didn't find terribly satisfying. Then I heard from Professor Page in the geography department that Rossby had an institute of meteorology at the University of Chicago, and that they were training cadets for the Air Corps to become weather officers, and they gave a course of one year at the Institute of Meteorology and upon graduation, you were automatically commissioned as a weather officer and you also got credit for the course, the courses that you took there. So I wrote to Rossby, and told him that I was interested and could I make an appointment to see him and then asked him to set a date. And I got back a letter, I don't know whether Rossby was the one who signed it--perhaps it was his secretary, but it gave me a date, a couple of weeks hence. So I took the train and went up to Chicago. And Rossby very kindly spent about half of the afternoon with me, telling me some things about meteorology, and he ended up by encouraging me to apply. So he gave me some application forms. I filled them out, and then the following August, in that summer, I was called to Chanute Field to take my physical examination and about a week later, was given orders to report to the University of Chicago, at the Institute of Meteorology. So about the second of September, I was commissioned as an aviation cadet and took up residence in International House at the University of Chicago. That was the beginning of my meteorological career.

Tribbia: Now cadets were trained for one year, I believe you just said, in meteorology and then was Rossby one of your instructors in training and who else might be instructors?

Thompson: There was a course in dynamical meteorology after one had a short course in hydrodynamics. The one in hydrodynamics was given by George Platzman. Then Rossby was initially the lecturer in the course in dynamical meteorology, but Rossby was a very busy man who had his fingers in many pies. They had already set up an Institute of Tropical Meteorology in Puerto Rico and as I recall, about one month into the course, he was called away to consult in North Africa, in preparation for the invasion of Italy, I expect. So he had to turn over the lectures to George Platzman.

Rossby was a very interesting lecturer. He had an interesting way of putting things, but he was rarely well-prepared because he was so busy. But Platzman was always extremely well-prepared. His lectures were just crystal-clear, and it was really a joy to listen to him. I found I didn't have to take a note, all I had to do was to listen to what George said.

Then later there was another course in dynamical meteorology in which Victor Starr was the lecturer. Victor was also extremely clear, but also very slow and sometimes you got a little impatient and wanted to say his next word for him. But my introduction to dynamical meteorology was through Rossby, Platzman and Victor Starr.

Kasahara: So you were in Chicago about two years?

Thompson: I was there as a student from September, 1942, until June of 1943.

Kasahara: You also taught there, as an instructor.

Thompson: Yes, after I graduated. I was kept on there as an instructor, at the Institute, mainly supervising synoptic labs.

Tribbia: How many cadets would be trained in a particular year?

Thompson: At Chicago, about 400.

Kasahara: Do you still know some of the people?

Thompson: I actually come into contact with very few of my classmates there. One is Len ?Snellman, who I think you know or know of; Ray Watta(?), who was a meteorologist at Brookhaven National Laboratories for some years; and Douglas Moore, who spent some months at my lab at GRD back in the early fifties. I have no idea what's become of him now. Most of the people in my class, I think, did not remain in meteorology. They went back to what they were doing before the war.

Kasahara: Now after Chicago, you went to UCLA. Did you get an Air Force assignment, or did you have a choice to do so--?

Thompson: I left Chicago in the summer of 1944 and I was then assigned to Air Traffic Control. They thought at that time that they had a surplus of meteorologists, and they cut down on the number of instructors in Chicago since they were going to discontinue giving the courses. So I spent fourteen months in the North Atlantic in Newfoundland, as an air traffic controller. Then I guess they changed their minds later because the Air Weather Service called me back. I went first to an overseas replacement depot at Seymour Johnson Field in North Carolina, where I spent two weeks, and I was supposed to go through a refresher course, but then they decided that I didn't need the refresher course. So they transferred me to Long Beach Air Force Base in California as a station weather officer.
Now here is one of the turning points in my life. While I was station weather officer at Long Beach, there was a captain from UCLA who dropped by one morning and said he would like to borrow some data. We had a long file of hourly reports and it was not available at UCLA. He was involved in some kind of research project and needed it. So I said, fine, he was quite welcome to it. I didn't catch his name when he'd introduced himself, but as I looked at him, he began to look more and more familiar. It suddenly occurred to me that he looked very much like one of my father's old friends, a man by the name of Aldo Leopold, who you may have heard of. In fact, Aldo Leopold and my father collaborated on several papers. Aldo has very distinctive features. They're very strong features and a rather aquiline nose, and I could see this in this chap's face. So I said to him: "Did you say your name was Leopold?" And he said: "Yes, Luna Leopold." So it gradually emerged that his father and mine were old friends, that my father knew his family quite well, including Luno. Then he began telling me about his situation at UCLA. He was head of a small research project there with one other officer attached to it, and they were both in the process of being discharged from the service. And they were looking around for

somebody to head up this project. Would I be interested? Well, I certainly was. So the upshot was that they nominated me as the new leader of this project at UCLA, and I was then assigned to UCLA, to the Divergence Project. That had been initiated by Bjerknes for the purpose of trying to calculate pressure tendencies by vertical integration of the mass continuity equation and the hydrostatic equation. Well, there was some question in my mind as to whether that could be done, but we could say more about that later perhaps.
In answer to your question, that was probably the decisive thing that determined that I would remain in the Air Force. Because that led to an interest in numerical weather prediction, and I saw one way of continuing work in that field and a career in the Air Weather Service.

Kasahara: So you met with Jule Charney during your time at UCLA?

Thompson: Yes, and as a matter of fact, his office was next door to mine when I first went there. This was in the fall of 1945. And Charney had just finished his doctor's thesis--that's the one on the stability and structure of baroclinic waves. It's a beautiful piece of work, certainly more sophisticated than anything that had been done up until that point. Jule was obviously a very bright guy, and we had a number of mutual interests including playing tennis and drinking beer and talking about meteorology and talking about science in general. We became very good, very close friends. Jule had considerable influence on me, too, at that point and later. I was much less influenced by Bjerknes and the work on the Divergence Project, because I began to realize that this was essentially a dead end and unless one dealt with the complete system of equations, formulated in such a way that they could be solved numerically, this was not a profitable venture.

Tribbia: I heard you mention that one of the reasons that you stayed in the Air Force was because of this association that grew out of your recognition of a friend of the family's. Would you have then in due course been just discharged from Long Beach eventually had you not gotten involved with the UCLA project, is that what you think might have happened to you?

Thompson: What I thought might happen, probably would happen, was simply that I would be discharged from the service, that I would return to graduate school, probably in mathematics. I failed to mention one other thing. There was another Air Force officer who was at UCLA at the same time. That was Joe Fletcher.

Joe was deliberately learning as much about geophysics as he could at that time because there were plans to establish an Air Force center for geophysical research. This was the brainchild of Colonel Marcellus Duffy and Joe Fletcher was his principal protege. Joe was certainly instrumental in establishing Air Force Cambridge Research Labs in a particular geophysical research directorate.

Joe told me what the plans were and he asked me if I were interested in joining forces with them whenever it got off the ground. And I was interested because I saw in that possibility of being able to do some things that I might find it difficult to do under other circumstances elsewhere. So I told Joe that I was interested and he said, "Very well. I'll call you when you should make a move." This is getting a little ahead of the story, but--

Kasahara: Before you went to AFCRL, you went to initiate the famous Princeton Project in Numerical Weather Prediction.

Thompson: Well, I didn't initiate it. I was involved in it in its early stages. Should we go on to that then?

Kasahara: You were at UCLA about one year?

Thompson: I can explain how I came to go to Princeton. I mentioned that I was a little disaffected by the Divergence Project, by the fact that it was too limited in scope. And I began to give some thought about how one should formulate the problem of weather prediction in mathematical terms. By that time I had begun to develop some feeling for what constituted a well-posed problem, complete with boundary conditions, initial conditions, a complete set of equations. And I started out virtually from scratch to try to figure out how to do this. I started off on the wrong foot by trying to make what was essentially a Taylor expansion around initial time. There are far easier ways to do it, as we know now, rather than trying to deal with a single equation. It's much easier if you deal with each variable separately in a step-wise fashion. In fact, I started some calculations to test out these methods. This involved an awful lot of calculation because the only thing I had was an old Monroe desk calculator, a mechanical calculator. And my recollection is about September of 1946, ?Holmboe called me in and he said that he understood that I had been trying to carry out integrations with higher dynamical equations as an initial value problem. He said that he had an article that I might be interested in. This was a clipping from the New York Times reporting on an interview of Professor John von Neumann. As a matter of fact, there was another person who was interviewed at the same time--that was __________________________________, at RCA. What they said was that they were designing, planning to build a very high-speed electronic computing machine with multiplication times on the order of 1,000 per second or perhaps 10,000 per second, that would be extremely flexible as a general purpose machine, self-programming, by which they meant stored programming, and their particular interest in doing this was to predict weather and to calculate the consequences of human intervention in the natural processes of the atmosphere.

When I read this, I knew where I had to go.

Tribbia: Can I interject a short thought? It seems to me that you on yourself, by yourself, were thinking in terms of numerical weather prediction. At that time, were you aware of Richardson's work and were any other people at UCLA interested in pursuing mathematical computational weather other than this Divergence Project which you've already mentioned?

Thompson: Oh, yes. No, I was not aware of Richardson's work at that time. I didn't become aware of that until over a year later in Princeton. Yes, there was one other person at UCLA who was interested and that was Jule Charney. In fact, Jule and I spent quite a bit of time discussing what the problems were and how to go about formulating the problem. At that time, we were operating pretty much in the dark. But unfortunately, at least from my point of view, Jule got a National Research Council fellowship and that took him to the University of Chicago for six months and then after that, for a year at the University of Oslo. So we were only in contact during that period once, personally, and then wrote letters back and forth, but that was a long, slow process.

Tribbia: One other point before we leave UCLA and go to Princeton, I know you've mentioned to me several times that another person who you met at that point, I believe, was Yale Mintz and did you interact with Yale much at that time?

Thompson: Not a great deal. Communication with Yale goes in one direction. Sorry, Yale. Well, at that time, Yale's interest was primarily in the general circulation and particularly, he became interested in the motions associated with the Hadley and Farrell cells, and he was particularly intrigued by the fact that the motions had to be spiral around those cells. At that time, I don't think it had really occurred to him to try to develop a full-scale general circulation model. He was more interested in semi-quantitative aspects of the problem, and was certainly not interested in general circulation models as we know them at the present time.

Kasahara: So after you arrived at Princeton, who was there at the Institute...Project, other than von Neumann?

Thompson: Well, I should back up a little bit and say that as soon as I heard about von Neumann's interest in this problem, I called up my commander, who was then Colonel Holzman, later General Holzman, and told him that I wanted very much like to meet with von Neumann and could I please travel to Princeton to meet him? And also, if that were the case, would he arrange the meeting? So Holzman told me that yes, he would arrange the meeting and I could travel by military aircraft as extra crew. So I flew by B-29 to the East Coast. I met with von Neumann for about an hour and I suppose my interest must have been apparent, because he asked me if I would like to join his project and I said, certainly. He called Holzman and Holzman said to go back and get my clothes and other gear in

California and come back. So I did.

Well, at the time when I arrived there, which was I think late in October of 1946, the people who were there were Paul ?Canet, who was from the Sorbonne in Paris, who was noted mainly for his work on the linear theory of lee waves and other types of transient waves. Chaim Pekeris, who had been one of Rossby's students at M.I.T. and whose primary interest at that time was in tidal theory, but had also been working on blast effects. Gilbert Hunt, who was at that time out of the Air Force and was working for his doctor's degree in mathematics at Princeton University. There had been a fourth member of the group there, Albert Kahn, who was known mainly for his work with Rossby on the adjustment problem. But by that time, Kahn had left. Kahn, incidentally, was more or less responsible for looking at ways of approaching the initial value problem. And I somehow inherited that function when I came.

Well, I don't think von Neumann had an appreciation for how difficult a problem this was. And I think he thought that after about a six months' survey of various aspects of this problem, that we would have a program laid out and we would know more or less how to proceed. But then I think he began to see that it wasn't going to be as simple as that, and was secretly relieved that the machine would not be ready as soon as he thought--this gave him some leeway. But the upshot was things went so slowly that Pecorus went to Israel, Canet went back to France, Gil Hunt got his degree and went to Cornell. I was left there alone as the sole member of the Meteorology Project for a long time.

Well, I was aware that the machine was not going to be available for a long time, but I was a little distressed about the slowness of progress, but was aware that one person who was as untutored in this as I wasn't going to make much headway with it. I had built up some associations with the people at N.Y.U., namely Bernhard Haurwitz, Hans Panofsky and through visits to N.Y.U., with Jerry Spar, Al Blackadar and others. So I did have some contact through N.Y.U. But in Princeton I was pretty much alone.

Kasahara: I am interested in knowing a little bit more about how von Neumann conceived the Project, and with whom he discussed his problems and how initially he collected those scientists. Do you know anything about it?

Thompson: Yes. Late in the summer of 1946, I think in August, he called together a group of about a dozen and a half people. He did this with the help of Carl Rossby. These were mainly dynamical meteorologists who at least had some peripheral or incipient interest in the problem of numerical weather prediction. In fact, most of the well-known dynamical meteorologists in the country were there. I was not at that time a well-known dynamical meteorologist so I wasn't there, didn't know anything about it. But these people were picked as potential participants in the Meteorology Project at the Institute for Advanced Study, even if only on a temporary or part-time basis. Actually, Charney reports that there was some disappointment in the outcome of that meeting, there were some that evinced an interest, there were others who were very skeptical about being able to make rapid progress after having made such slow progress for so many decades. They were reluctant to become associated with the project.

It is apparent that von Neumann did underestimate the effort and the time it would be necessary in just preliminary studies to determine what should be done, and how to go about using a computer effectively. Von Neumann himself did not have any very deep knowledge of meteorology. He read enough that he knew about what the state of the science was, what was missing, what hadn't been done, what should be done and in general knew what kind of people they needed to fill in these gaps. I think that he was depending upon people who had some knowledge of meteorology, of physics, of mathematics, and of computing in general to act as, if you like, as go-betweens or translators between him and the wider world of meteorology. There were very few people like that. In those days, there were almost none, so we had to wait until people were trained or until they learned things themselves and taught it to others.

Kasahara: Well, this is somewhat ahead of--I remember that von Neumann was very much interested in turbulence, among other things, of course.

Thompson: Yes--

Kasahara: You were interested very much in turbulence. I was curious to know, did you get some ?inference from--

Thompson: No, actually I didn't. I didn't learn until later that one of von Neumann's motivations in establishing something like the Meteorology Project was as a kind of example of, if you like, experimental mathematics, which he thought was necessary as a sort of preliminary to any progress in the--

END OF TAPE 1, SIDE 1

INTERVIEW OF PHILLIP D. THOMPSON

TAPE 2, SIDE 1

Tribbia: Then von Neumann was using the Meteorology Project as an example of the study of non-linear mathematical problems?

Thompson: Yes. But at that time I was not aware of his interest in problems of turbulence. My interest in turbulence didn't arise until about 1951. In fact, I did not actively pursue the problems of that kind until 1956. I think your original question concerned turbulence as an example of the kinds of problems that von Neumann felt would require the use of a large computing machine.

Tribbia: I'd like to switch slightly to the kind of social life a rather young twenty-four year old, as you were at that time, would have at a very esteemed scientific institution with people like von Neumann and Albert Einstein.

Thompson: Well, I was a little overpowered. I mean, I was a very young man. My education was certainly not complete. I had heard of many of these people, von Neumann for example. Hermann ?Weil was there. I had come into contact with Weil before because his son lived with us for some years in Urbana, ________________________________. So I had at least one friend at the Institute.

Einstein, you mentioned. He was still coming into the Institute in the afternoons for about three days a week and held court at tea time in the Commons Room at the Institute. There were many other interesting and distinguished people there at the time. Two of the visitors while I was there--one was T.S. Eliot, the poet and stockbroker. Another was George Kennan, who was of course a famous expert on Soviet affairs and at one time our ambassador to the Soviet Union. There were a multitude of young, very bright mathematicians and some of these I was in contact with every day. Abe Taub lived in the other half of a duplex house next door to us. P.A.M. Dirac lived two doors away. Ukawa, the physicist, lived almost next door. Herman Goldstein was across the compound from us. All of these were young, they had children. We had children and were mixed up with the same kind of domestic problems. There was a vigorous social life there. I must say it was certainly the most stimulating and interesting time I've ever had.

I learned a lot, probably irrelevant to what I was supposed to be doing. I learned a lot of mathematics, either by listening or by going to lectures or by reading. But if I may, I'd like to go back to another point, that is feeling a little inadequate to pursue the path that I was supposed to be engaged in. I thought that von

Neumann would do well to bring up reinforcements. I'd been corresponding with Jule Charney, and I thought that Charney would be ideally suited for this. There was another development, and that was that Joe Fletcher called me and told me that plans were now far advanced to establish a geophysical research directorate as part of the Air Force Cambridge Research Labs, and he asked me if I would establish and direct one of the new laboratories, the Atmospheric Analysis Laboratory. I said yes. He said, "Do you have a regular commission?" I said no. He said, "Well, go and get one." So the upshot was that I went down to Fort Dix and took the examinations for the regular commission in both the Army and the Air Force, and I passed both and had a choice and I of course chose the Air Force. So I was very soon given a regular commission in the Air Force. And then Joe Fletcher advised me that they would assign me to the geophysical research directorate late in the fall of 1948.

So my days at Princeton were numbered. I knew I was going to leave and if the project was going to retain any kind of momentum at all, von Neumann had to bring someone in. Well, it turned out that von Neumann had already been thinking about this himself. He was busy with a lot of other things, for one thing with the Atomic Energy Commission, and he was busy as a consultant for Los Alamos. But between all of these things, he had been thinking about trying to bring up reinforcements. I mentioned the name of Charney to him earlier and he asked me what I thought of him. I said, "Well, you couldn't find any better combination of mathematical savvy and physical insight than you would find in Jule Charney." Jule had written to me as a matter of fact, indicating his strong interest in joining the group and had also told me that Arnt Eliassen was up for his sabbatical leave, and that he also was interested in coming. So I mentioned this to von Neumann and the upshot was that both Jule Charney and Arnt Eliassen were invited to come, Eliassen for his sabbatical year, and Charney, indefinitely. And of course Jule was really the natural leader of the group, as he was in fact.

Kasahara: So you left Princeton before Jule Charney came?

Thompson: Actually, Jule came in the summer of 1948, so there was an overlap of about four months before I left. And of course we did a lot of talking during those months.

Jule, of course, immediately began to build up a strong group, working first with Eliassen, then with Fjortoft and then with a number of other visitors--George Platzman, Joe Smagorinsky, John Freeman. They developed plans to carry out integration of the barotropic vorticity equation. The Princeton machine, of course, was nowhere near completion by that time, although the design was fixed and the calculations were done on ENIAC. In the meanwhile, I had gone to GRD and being very much interested in the problem of numerical weather prediction, I began to assemble a very small group there to follow up some ideas that I had.

Berkofsky and ?Amos Gallaghan carried out integrations of the two-dimensional linearized vorticity equation on a card machine, a punch-card machine. Actually, we got very good results, better than the ones they got on the ENIAC.

Tribbia: When you left your involvement in the Princeton project, at what stage was the Princeton project, was there at that point in time a plan to integrate the barotropic equations at that point or was the state of the project still wrestling to some extent with the problem of integrating the primitive system as opposed to a filtered system?

Thompson: Well, remember that Jule had just finished his paper on the scale of atmospheric motions while he was still at the University of Oslo. That's the paper that appears in **Geophysiche _________________________**. So Jule at that time was very much interested in the use of the filtered equations and what we call quasi-geostrophic equations now. So his idea was to proceed by gradual stages, to start first with the barotropic equations, which is a very simplified form of the filtered equations, and gain some experience with the techniques of integration and overcoming various problems that certainly arise when you're dealing with the equations in more general form. So, having done the first integration of the barotropic vorticity equation, the next step would be to go on to simple baroclinic models with low resolution in the vertical.

It happened about that time Norman Phillips had just finished some studies on a two-layer model, two homogenous layers of fluid, and that gave them a clue as to how to proceed from the barotropic model to the next, more complicated model. And then they realized there was a very easy way to extend the number of levels to three or four, although I don't think they ever went beyond three.

Actually, I think this strategy was evolved during the first couple of years that Jule was there, that is during 1949 and early in 1950.

Kasahara: So after you went to GRD, you still had contact with _______________...

Thompson: Oh, yes. In fact, up to that time the meteorology project at the Institute for Advanced Study had been financed entirely by the Navy through the Office of Naval Research. And then we developed an interest in the project as well and we volunteered to provide half of the financial support. So the upshot was that the Office of Naval Research and GRD jointly supported the Princeton project, and we had a kind of informal steering committee consisting of Dan Rex and myself--Dan from the Office of Naval Research and me from GRD--who sort of kept an eye on the project and followed with great interest what was going on. The result was that I had an excuse to go to Princeton fairly frequently, so I maintained my contacts with Charney and Phillips and other people there in this way. Yes, we were in frequent contact clear up to the establishment of the Joint Numerical Weather Prediction Unit and even after that.

Kasahara: I know you took your degree, Doctor of Science, from M.I.T. in 1956, so you must have been thinking already, at the time of GRD, about some of your thesis work--

Thompson: Well, I went to GRD late in the fall of 1948 and after a couple of years I began to think I should get some formal education in meteorology. Up to that time, my formal education consisted solely of the elementary courses that we were given in the cadet program at the University of Chicago, but beyond that I had no formal work in mathematics or in dynamical meteorology. I thought it was time to learn something. So I applied to go to graduate school through a program the Air Force had, and they said, yes, that I could take a year to go to graduate school. So my idea was to try to get a Master's degree at M.I.T., and I had had some previous dealings with Henry Houghton, who was chairman of the department at M.I.T., mainly concerning contracts between M.I.T. and GRD. And Henry said, why bother with a Master's degree? He said that there were a lot of formal course requirements for a Master's degree that would probably take you more than one year. He said, why don't you go for a Doctor's degree because there are no formal course requirements, except for a foreign language and for the minor, but there weren't any for your major subject. So he suggested that I simply take what I thought I needed in meteorology, take a minor in whatever I wanted--which was mathematics--and pass off the language requirements.
So that's what I did. I took Houghton's course in physical meteorology, which covered cloud physics and radiation. I took Willett's course on essentially climate and long period changes; and Lettau's course on turbulence. Then I took some mathematics courses and Phillip Morris' courses in theoretical physics, and passed off the language requirements in six weeks, wrote my thesis and then I was done.

But you're right. Although I hadn't been thinking about using what work I had done previously as a thesis, I had already had enough ideas that I had a thesis subject, I knew how to tackle the problem--it was just a question of doing it and writing it out.

Kasahara: You also taught at M.I.T.--

Thompson: When I graduated at M.I.T., Henry Houghton offered me a job and I thought hard about that. It wasn't the only job offer I had. I also had offers from Courant, the Institute of Mathematical Sciences at NYU, and another that was financially more attractive, but I decided in the long run that I would find it easier to get access to computers and to get support in terms of people and money if I stayed in the Air Force, so I told Henry no. But he said, well, if you're going to be in the Boston area anyway (which he presumed I would be), will you please give a course? So I said yes. So I gave the first course in numerical weather prediction at M.I.T. I must say I had a very distinguished class. Salzman, Pfeffer, Hans Buch, Sig Fritz, Lou Berkofsky, Kirk Bryan--I think those are the ones you would recognize most immediately.

Kasahara: So after that you became the head of the Numerical Weather Prediction Project at the Air Force--

Thompson: Yes, well, I was pretty sure that they would establish an operational numerical weather prediction unit soon. In fact, I had been involved in some discussions about the planning of such a unit, and I thought that there were two things that needed to be done. One was to carry out extensive enough tests starting with real initial conditions, that we could make some sensible choice between different models for operational purposes, And the other was that were very few people who were trained in numerical weather prediction, both from the standpoint of meteorology and from the standpoint of computing technology. So the idea was that we should assemble a group of people in GRD from the Air Weather Service, from the Weather Bureau, from GRD, to carry out a project whose aim was partly to carry out these very long tests of procedure, and the other to provide the nucleus of a staff for an operating group. And of course the group at Princeton were doing the same kind of thing. They were also training some people there, who would later be transferred to the JNWP unit.

So the upshot was that by July of 1954, when the JNWP unit was formally established, then there were people from Princeton and from GRD who were simply transferred...

Kasahara: So JNWP was organized between the Weather Bureau and the Air Force and the Naval Weather Service.

Thompson: Yes.

Kasahara: But it was located at the Federal Office Building--

Thompson: Federal Office Building #4, Suitland, Maryland. So this group was assembled in July of 1954, and then we began looking at various alternative computers that were available commercially. I was not deeply involved in that.

Kasahara: What kind of computer--

Thompson: IBM 701 was what we finally ordered. That was ordered for delivery in the spring of 1955, and the first operational forecast began on May 15, 1955.

Tribbia: I'd like to backtrack a little bit and go back to your M.I.T. days. You related a story to me at least and I'm sure all of us would enjoy hearing about one of your office mates at M.I.T., who you managed to have some rather interesting conversations with.

Thompson: Yes, well, the people who were most influential in my education at M.I.T. were not my professors. My thesis advisor was nominally Victor Starr, but Victor steadfastly refused to talk to me about what I was doing. I don't know whether this was intentional or not, but although he and I and Ed Lorenz and Gilles Lancoul went to lunch together, he just wouldn't talk about that subject, he'd talk about anything else. I expected that the people who were most influential were Ed Lorenz and Gilles Lancoul. There were other people with whom I was involved there. One was P. V. Davies who was visiting there at the time, and Larry Gates who assisted me with some of the calculations that I had to do for my thesis, but I would say that Ed Lorenz among all of those, had the most influence on my thinking. And he has subsequently admitted that I had some influence on his. Ed's a very deceptive person, as you may know. He's very quiet and unassuming, then he opens his mouth and all of these marvelous things come out.

Kasahara: I'd like to follow up a little more on Joe's question, but I'd like to ask you more about the scientific aspects, namely, were you writing the thesis physically up at M.I.T.--it was published in 1953, but obviously--

Thompson: I began writing the thesis, I believe, early in October, 1952. It was finished on December 10, 1952, and I turned it in, but I didn't graduate until the end of the semester, which was in February, 1953, I think.

Kasahara: I think you used that model later on for pressure forecasting.

Thompson: One rather similar in structure. This was essentially what they called in those days a "two-parameter model," which one can think of loosely as being either a two-level model or a model whose state is characterized by the geopotential and temperature fields at one level. The version that we used later was the so-called thermotropic level, which is really a vertically integrated model, in which the variables are essentially some kind of weighted function of the geopotential and the temperature. It was, I thought, a rather elegant way to formulate the equations, but it was clear that the structure of the equations was very much the same as it would be if you used the finite difference approximation in the vertical.

Tribbia: I'd like to address with you the issue of, at that point in time, what people perceived could be done with the barotropic integration, certainly with some approximations associated with the quasi-geostrophic approximations. Was it the sentiment at the time that the greatest efficiency in such a model was the lack of a baroclinic structure, or was it perceived that a similar __________ might be the lack of ageostrophic motion?

Thompson: Certainly, in the beginning, that is the period from 1950 up until 1953 or 1954, it was perceived that the next biggest advance would be made by introducing baroclinicism into the models, rather than to worry too much about the approximation of the equated geostrophic approximation. It was not until 1955 that they began to appreciate that there might be considerable differences in the filtered form and the primitive form of the equations for models that were otherwise equivalent from a physical standpoint. I believe it was in 1955 that Charney published his first paper on the solution to primitive equations. Hinkelmann, incidentally, in 1951, had suggested that the primitive equations might be a better point of departure, according to numerical weather prediction. But at that time, he was not fully aware of the difficulties of the initialization problem.

Kasahara: So you were in charge of JNWP from 1954-58?

Thompson: No, I was not in charge of JNWP. Cressman was in charge of the entire unit, I was the head of the development division. So the group that I had was responsible for introducing new models or improvement on models that were already in operation and for working out some details of methods of integration.

Kasahara: So what was the most difficult aspect of developing the prediction models in those days? You touched on many numerical problems, so clearly--

Thompson: The biggest difficulty is one they're still struggling with, and that is to get the models introduced to practice after you've developed them. There were in fact no major changes in the operational models during the first five years or so, partly because I guess they thought they had invested a great deal in the models that were developed initially, so the tendency was to make some rather small changes to correct errors that you could see were developing systematically. Examples of this were the fact that you got very rapid retrogression of the large-scale components of the motion, and this was at first corrected by a very simple procedure, which was suggested by Paul Wolf, and that is simply carrying out a rather crude Fourier analysis and removing the largest-scale components and making a forecast with the shorter-scale components, then putting the stationary long-wave components back in. This, I thought, was cheating. It may be right, but for the wrong reasons. Then we gradually began to understand what the problem was, although I don't think we understand it completely yet, but how the physics of the model could be changed to correct this. Another defect that showed up in systematic errors in forecasts was that the energy contained in the fluctuating part of the motion decreased, and the kinetic energy associated with the mean motion increased with time, so within 48 hours, the forecasts were seriously in error. We eventually tracked this down to the fact that although the models were quite successful in predicting the north-south transportable momentum correctly, we had no way of putting it back in at the surface because we were not accounting for the exchange of momentum between the earth's surface and the atmosphere. But with the inclusion of some friction in the boundary layer, then that error was reduced considerably.

But it was changes of that magnitude that we made as a result of the work we were doing in the development section. Of course, I left then, in 1958. After that, Shuman, who was very much interested in the solution of primitive equations, introduced a completely new model. By that time, I guess, the Joint Numerical Weather Prediction Unit had been absorbed by the National Meteorological Center.

Kasahara: In 1958?

Thompson: Not in 1958. It was absorbed by ESSA in 1962, and ESSA later became NOAA, of course.

Kasahara: Who was there at the time it was JNWP?

Thompson: When it was established, Cressman was the head of the unit. Joe Smagorinsky was head of another unit, which was concerned mainly with the operation. Then, I was head of the development division and then there was another division which was responsible for the day-to-day operations, that is the analysis of the initial data, the mechanical details of computing--maintaining schedules and so on--post-processsing the output, and disseminating the forecasts. And that was headed by Ed Fawcett. I don't think Smagorinsky was satisfied with the position there, or didn't get much satisfaction from it. He then left to establish the forerunner of the Geophysical Fluid Dynamics Laboratory...

Kasahara: That was separate from JNWP.

Thompson: Yes. For awhile, it was in the Federal Office Building #4, then it moved downtown to Pennsylvania Avenue. And then of course later, it moved up to Princeton. The people in my group were originally Fred Shuman, Art Bedient, Bill Hubert from the Navy, Paul Wolf from the Navy and a civilian, an assistant, whose name I don't remember now.

Kasahara: You were using the computer 704?

Thompson: When I left, we had the IBM 704. The first machine was the IBM 701. In those days, of course, we programmed in machine language.

Tribbia: In going through your publications during that time, a lot of them, the practical bent associated with some of the problems that were growing up or coming up at JNWP, but at this point, also, we begin to see some hints of working statistical fluid dynamics. Perhaps we can try to get into this area a bit and find out how you gradually became interested in the statistical approach to the problems of weather prediction.

Thompson: There were two things that came up while I was at the Joint Numerical Weather Prediction Unit that led me to a statistical treatment of dynamical problems. One of these, I think, was not very important. I was trying to understand the mechanism of blocking, and I was struck by the paper by Cressman who described blocking as associated with splitting of the jet. What I wanted to do was to cook up a relatively simple model to see if I could account for this phenomenon. The simplest model one could think of, of course, was the barotropic model. So I thought that one of the easiest ways to separate out the effects of the average zonal motion and the fluctuating motions by splitting the problem into two pieces. One is the statistical effect of the eddies on the average motion, and conversely, the effect of the average motion on the eddies. And what came out of this, of course, was an enclosed system of equations with suitable assumptions. What I suppose you would describe now as second moment closure of the problem. It was, in effect, a kind of a theory of very large-scale turbulence in the atmosphere, which the eddies are characteristically thousands or ten thousands of kilometers in scale. It turned out that the barotropic model was successful in predicting you would get splitting of the jet, and the presumption was that that would account for some aspects of blocking. It would not account for the initiation or breakdown, but would predict the propagation of the zones of maximum windspeed.

The other problem, I think, had more far-reaching results. That concerns your question about predictability. At that time, about the only information we had from over the Pacific Ocean, at least near the continental United States, was from weather ship "Papa" in the Gulf of Alaska. Weather ship "Papa" was not terribly reliable. There were times when the reports did not come in at all. There was a big hole there, and no data at all, and one simply had to guess at what was there. One had to make a forecast anyway, so you would make a forecast, but on some occasions, the data came in, but late. We were able to go back and put in the data, then re-run the forecast with something like correct initial conditions. Then we compared the forecast with and without the data from weather ship "Papa." What we discovered that we might do was characteristically get errors of perhaps 150 meters at 500 millibar height as a result of not having the data, but a couple of days later these would show up as errors of perhaps 240 meters over southern

Canada. Now that's a pretty big error. Then, I began to realize that this was a particularly flagrant example of what happens when the initial stage is not correct, but that everyday, the initial stage is in some degree incorrect. That is to say that the initial analysis is based on a rather small sampling of data which is rather widely scattered in space that's transmitted digitally, so there's some roundoff error. But even worse, there are large errors of interpolation, depending on how far apart the stations are. So even if the analysis we made were the most probable, or the best analysis you could possibly make under the circumstances, there were many neighboring initial states that were almost equally probable. So then the question was, well, suppose that you consider not one deterministic prediction, but a whole ensemble of deterministic predictions, each starting with a slightly incorrect initial state, which is more or less randomly distributed around the most probable initial state. And the question is how this spread of the variance of this ensemble of forecasts becomes, how does it vary with time. What else does it depend on? Does it depend upon the spacing between the stations? To what extent does it depend on the error of the observations, and to what extent does it depend upon the synoptic situation you're trying to deal with?
So one of my first excursions into statistical hydrodynamics was to solve that problem. Well, I didn't really solve it, but I solved part of it and was able to make some estimate of how rapidly errors grow and about how long you could make forecasts before it became no better than guessing. At that time, my estimate was that the RMS error might double in about two days and the practical predictive scale would be lost after about a week.

Kasahara: Of course, __________________________ is very popular right now because of all the computing power, so____. It's very easy to see the ____________________________________. But in those days, the computing power was so small, and here you were talking about the uncertainty of initial conditions. Was your thesis accepted cordially in those days, or what was the reception--?

Thompson: It was a very funny reception. I think probably people just hadn't quite gotten used to the idea yet. They didn't really want to introduce any element of uncertainty into what was pleasingly deterministic. That was partly an emotional resistance, if you like. Oddly enough, the Air Force immediately realized that this had some potential because it enabled them not only to know what the most probable situation would be, but also the probability that your estimate would lie within certain limits around that. Questions like the latter are important to people who are involved in transport by air, not just once, but day after day after day. Or who were involved in strategic missions, for example, in strategic bombing where you're not really concerned with the outcome in a single raid, for example, but with your average success over many raids. So questions like that are interesting to people like that, and they gave me a Legion of Merit for it.

Tribbia: I'm interested in what your sources of inspiration were for the mathematical approach to the problem that you took. In looking through those papers, they are extremely ahead of their time in terms of being typical meteorological literature in the following sense: you are looking at two-point correlation functions of meteorological fields, and probably one of the only people who is looking at such things in large-scale meteorology at that time. What books, papers, conversations can you recall that led you to formulate the problem in such a manner?

Thompson: Well, I can answer that easily: none. I sucked them all out of my thumb. No, I don't recall having seen anything in the literature before then that dealt with problems of that kind.

Kasahara: During the time [you were at] JNWP, you published a __________________ memorandum. The title is, **Statistical Dynamical Theory of Turbulence ____________________________________.**

Now that seems to me the first of your idealized treatment of more pure form of turbulence theory. Obviously, you started from your more practical objective of predictability, application to the predictability, but now you went into more the nature of turbulence--

Thompson: Yes. I had been becoming increasingly interested in turbulence, because it seemed that no matter which way you turned, turbulence is a very important element in practically any problem you can think of. I had read enough to realize that there was essentially no fundamental theory of turbulence except for extremely special cases such as turbulence produced by a grid in a wind tunnel. Homogeneous isotropic turbulence of the kind that Batchelor discusses in his book. But in the atmosphere, one does not encounter homogeneous isotropic turbulence. It's almost invariably non-homogeneous non-isotropic. So what I was interested in doing was, in using the same kinds of methods I had thought of to deal with this blocking problem, to a problem which is physically more clean-cut, in a way, as flow contained between smooth, parallel walls. I wanted to see if I could extract some kind of closure, the kind of statistics that might apply to an-isotropic flows.

END OF TAPE 2, SIDE 1

Interview of Philip D. Thompson

TAPE 2, SIDE 2

Tribbia: Phil, before we pick up the narrative again, I'd like to ask you a couple of questions that were somewhat neglected in the first half of our interview. These questions are just of a personal note concerning your family and your relationships, and the first thing I'd like to ask you is, when we asked you about your early childhood, we neglected to ask you whether or not you had any brothers or sisters, and whether they were at all influential in your development.

Thompson: Yes, in one way or another, they were influential. I have two brothers, both considerably younger than I, a brother, John, who's seven years younger and a brother, Peter, who's nine years younger. My brother, Peter, oddly enough followed in his father's footsteps. He studied molecular biology, and is now head of the department of zoology at the University of Georgia in Athens. I say "surprising" because it's not very easy to be the son of a scientist. I'm sure my children appreciate that.

My brother John was a little longer in deciding what he wanted to do. He declared at first that he had no intention of going to college. Not having any clear goals, he joined the Air Force and was the crew chief of a B-29 during the Korean War. As crew chief, he had to become familiar with a number of aspects of aircraft maintenance and in particular, engines. So he went to a number of engine schools. Then he decided he was interested in airplanes, not merely engines, but in aeronautical engineering generally. So he returned to Purdue, finished in three years, got his degree in aeronautical engineering, and just recently retired as head of the advanced design division of McDonnell Douglas. In fact, I think he designed the engines for the first supersonic commercial transport.

Does that answer your question about my brothers?

Tribbia: I think so. Being that you were the eldest, and you were primarily more influential on their development, I would guess, than they on yours.

Thompson: Yes, I think so. In fact, I think probably I changed the course of my youngest brother's life in that he thought he wanted to become a high school teacher, teaching French and science. And I was convinced that he was capable of far more than that, and persuaded him to go to graduate school.

Tribbia: Once again, before we get back to the narrative, I would like to note at least twice now you have mentioned that you have children and this makes one naturally assume that you were married somewhere along the course of your life, so could you tell us approximately what time that occurred and how many children--

Thompson: Well, I should start by saying that I met my wife while I was an instructor at the University of Chicago, and she was a Navy ensign taking the meteorology course in parallel with the cadet courses. A hideous example of teacher-student relations. We were married the following year; our oldest child was born in February, 1945--that's Jennifer. She is rather belatedly taking her doctor's degree in clinical psychology at the University of Illinois. The second child is two years younger--that's David. He has had a rather checkered career; he took his first degree in chemistry, his second degree in law. So now he is involved in supplying chemicals for the elimination of toxic wastes. The third child is Daniel, who's three years younger than David. He initially started out in mathematics, switched to engineering and physics, and he is now a metrologist, not a meteorologist, at Livermore National Laboratories. The next child, Sally, is three years younger than Daniel. She is a housewife and wife of a farmer of a sort. They run a family business in growing seeds, particularly melon seeds, which are imported all over the world. The youngest child, Wealthy, has a scientific bent, but she is probably stronger in other subjects, in particular literature and history. She hasn't decided yet what she wants to be. So that pretty well covers that.

Tribbia: Then we'll continue on from where we were at the end of the taping session yesterday, and for anybody who might be interested, the word that may be left over at the end of the tape as an answer to your question--I believe, you were responding to Akira's question on what made you decide to tackle a real turbulent fluid problem as opposed to a practical meteorological problem at this specific time.

I think the next stage in your career takes you now from JNWP to the University of Stockholm, and how did you get to arrive in Stockholm, and under what circumstances were you brought there?

Thompson: I will have to backtrack a little bit. I didn't explain that research was not the sole function of the laboratories at GRD. Another function was the support of university research, and in fact the laboratory that I headed had an annual budget of 1.2 to 1.6 million dollars per year specifically for support of outside research, primarily university research.
I mentioned earlier that ONR also supported university research, and between the two of us, I imagine we were able to supply between four and five million dollars a year to the university community.

But it became apparent by about 1950 that virtually all of the university research in the United States that was worth supporting was being supported, and if we supported any more than that, we would really be scraping the bottom of the barrel. I was aware of the difficulties that various European institutes were

having in regaining their former strength. So it occurred to me that it might be worth finding out what the situation was in these institutes that were trying to get started again: find out what they were doing, who was there, what they were working on, what they wanted to do, what their capabilities were and so on. With a view to seeing whether or not there was something that was worth supporting and could be supported. So in the winter of 1950-51, I made a month-long trip through Western Europe, visiting some half a dozen institutes. I stopped off to see Canet at the Sorbonne to find out what the situation was in the French universities. It turned out that they weren't very much interested, didn't have any specific plans. It was too fragmented. I then went to Bad Kissingen in West Germany, which was then the headquarters of the Deutsche ________________. There, there was a small group that was working on numerical weather prediction. They were Hinkelmann, Deppermann, Hallmann, and Reimann--generally known as "Die Fier Manner." It was really appalling; they had virtually no facilities, no support. Calculations were done by girls, either doing the calculations by hand or with a few old calculators. I was very favorably impressed with what they were doing. They clearly needed help and could use it well.
I also made visits to the International Institute of Meteorology in Stockholm--I'll return to this later--where Rossby had assembled a group of people who had been contributing and were contributing a great deal in the development of numerical weather prediction, particularly in its more fundamental aspects. Bolin, for example, was there, Doos and others. There were many visitors coming through Stockholm at that time. I also stayed for some days at the Institute of Theoretical Astrophysics at the University of Oslo, which at that time was directed by Svein Rosalund. He himself was a physicist, but most of the other people there were essentially meteorologists. Solberg, Bjerknes was still coming in a couple of days a week, Ilias and Fjortoft. That was obviously a very strong team. Incidentally, I met Vilhelm Bjerknes there. I gave a seminar there, and he attended it.

Then I stopped off at Imperial College on the way back and met Peter Shepherd and Eady. Eady, of course, had done some of the early work on numerical weather prediction.

As it later turned out, we were able to give support to three of these groups, namely the group in Bad Kissingen, in Stockholm and in Oslo. These were contracts between the Air Force and those institutions, originally administered through my laboratory but later through the Brussels office of the Air Research and Development Command.

I'll say a little bit about our contacts with Rossby. I had, of course, been in occasional contact with him ever since I had been in Chicago. When I visited him in 1951, I suggested to him that a good deal of the work that they were interested in doing at the Institute in Stockholm was directly applicable to the objectives of our laboratory and indirectly to the Air Force objectives. And I suggested to him sort of a general program consisting of what they were actually doing that could be shown to be directly applicable to our interests. And I encouraged him then to submit a proposal. And he said, "You seem to have this well in mind, why don't you write the proposal?" The upshot was that I wrote the proposal. And then Rossby took it, and, as he said, "Scandinavianized" it. Then he submitted the proposal. There was a little difficulty about the transfer of funds because the Swedish government is religiously neutral, or was at that time, and it would have been awkward for the Institute in Stockholm to accept funds directly from the United States Air Force. So that was a problem solved by laundering the money, that is, we wrote the contract with Rossby as principal investigator, but the institution was the Woods Hole Oceanographic Institution. Then, through Rossby's dual role, the funds were transferred from Woods Hole Oceanographic Institution to Stockholm. So that's the way that was done. At any rate, I had close contacts with the people in Stockholm and was very much interested in what they were doing.

By about 1957, I was becoming a little dissatisfied with solving the day-to-day problems of the Joint Numerical Weather Prediction Unit, and I wanted to get a little bit closer to more fundamental and more interesting problems. Also, by that time I had been in the Air Force for fifteen years, and I realized that I had the possibility of retiring with a pension that had my military pay at the age of forty. So I began to think of eventually settling in to an academic life, which was very attractive and familiar to me. In thinking where I would like to go, I thought of Stockholm. There were people who I knew and respected there and liked. So I broached this to Cressman, and we jointly talked it over with my nominal commander at Andrews Air Force Base, and I simply told him that there were research problems that I wanted to work on and that seemed to be the ideal place to do it. There were the kinds of facilities and people there that would be needed. They said that they were very sympathetic to this, but they felt this would be leaving a considerable gap in the Joint Numerical Weather Prediction Unit. They said, "If you can persuade Rossby to send someone to replace you for the time you're gone, fine." So negotiations were opened with Rossby, but before these were concluded, Rossby died. But we continued the discussions with Bolin, and he found that Aksel Wiin-Nielsen would be interested in coming to the Joint Numerical Weather Prediction Unit for awhile. So the arrangment was that I would go to Stockholm for two years and Wiin-Nielsen would join the Numerical Weather Prediction Unit for two years. Ultimately, this was extended by another year...that's a little bit further along in the story.

At any rate, having concluded these arrangements, I went to Stockholm in September, 1958. I got started working on some problems that I had been thinking about.

Kasahara: Did you have some special assignment at the Institute or--

Thompson: Formally, I was attached to the Air Attache's office of the United States Embassy, although I had no duties at all there. It was just for administrative purposes. What I was doing actually was studying and teaching and writing at the Institute. I did give some short courses, and I wrote a book while I was there.

Kasahara: So that's how your book got started.

Thompson: Yes, while I was still at the Numerical Weather Prediction Unit, I had been approached by a representative of Macmillan Company, and he was looking for people who, as he put it, "had books in them." I had visions of being pregnant with something like a large book. He asked me if I were interested in writing a book on numerical weather prediction, and I said yes, that I thought I would be. It happened that I had just finished giving a course on numerical weather prediction at George Washington University, and therefore, this material was fairly fresh in my mind. So I agreed to write the book, but I didn't immediately do anything about it. In fact, I didn't do anything about it for, I guess, six or seven months after I arrived in Stockholm. Then they wrote to me and reminded me that I had agreed to write this book and they asked me if I would like a modest advance. I think this was a way to obligate me in some way, or make it more urgent. I didn't do anything about that for awhile, but the following spring, this came to a head when my sons decided they needed a sailboat. At that point, I accepted the advance and we bought a sailboat.

I started writing the book about the first of April, 1959. I wrote steadily all day and all evening for two months. At the end of May, it was done.

Tribbia: I actually have in front of me a copy of the preface of the book, and just think that you had some trepidation and misgivings about writing a book on a topic that seemed so infant at the time. On the other hand, I think that book was the standard in numerical weather prediction for at least ten years and it is still widely read today, which is some twenty-five years after it was written. One of the amazing things to myself in reading that book is that it's a very thin volume of 160-some pages, I believe, but within it, there's a course in dynamical meteorology, a course in numerical methods and also some topics of practical concern towards doing actual forecasting. How did you come upon that mix of topics and also, how did you clarify things in your own mind so well as to be able to write as concise a book as you were able to?

Thompson: I think that the mixture of subjects arose from, first, I was trying to write a book that could be read by just about anybody who had some sort of background in mathematics and physics, and not specifically for somebody who had specialized in meteorology. But I was also aware that there were a number of people who were trained in meteorology who were not familiar with some of the technical details of numerical weather prediction. There was a notion of solving differential equations by finite difference methods; as an example, user relaxation methods. That was not widespread at that time. Such considerations as these more or less determined what the subject matter of the book was. But for the rest, the level at which things were pitched in practice was determined by figuring out which steps to leave out, which to put in. That I just had to decide practically on a sentence-to-sentence basis. That's a difficult question to answer, Joe. All I can say is that just having given the course the year before, I guess I still had some feeling for where people had difficulties and where they didn't.

Kasahara: Did the publisher ask your advice--

Thompson: To change one word. It was wrong.

Kasahara: Of course, we know that you write very good English, precise and so forth. In fact, this has been one of the most successful ones--

Thompson: I'm a little surprised that Macmillan didn't put out a second printing. I think they only printed 10,000 copies. Whereas the Russian translation, I think, sold something like 25,000.

Tribbia: One of the areas in the book that seems to be very well-developed by you, almost refined to an art form at this point, is the notion of filter approximations, and what one was actually doing when one made a filter approximation. I'm not certain, but this may be one of the only places or the only place where that work was in fact was written down and published. Was that refinement sort of a gradual continuation of the notions since those beginning talks and correspondences you had with Jule Charney in the late forties?

Thompson: Certainly the general idea of filtering was contained in Jule's letter to me in February, 1947, and I don't think even he realized at that time exactly what it meant. Otherwise, I think he would have been led more quickly to the more general form of the quasi-geostrophic equations. Speaking for my part, this became clear through a matter of notation, as much as anything else, of simply putting markers on operators that appeared in certain equations but not on others, and he simply retained that tag so that he could trace through the derivation where that term came from. Then you could see clearly when you get the final form of the frequency equation exactly what it is you have to leave out in order to exclude certain classes of wave solutions. Is that what you were talking about?

It was just the clearest way I could see what was actually happening.

Kasahara: So during the Stockholm time, other than writing, what do you remember most? [Some of this is hard to hear.] ____ ____ ____________________________________--the technique of harmonizing ______________________________________surrounded by...

Thompson: Oh, yes, the whole problem.

Kasahara: Yes, developed during your Stockholm time.

Thompson: Yes. I remember what prompted that. Back in 1956, I served as a consultant to the WMO working group on networks, which met in ________________, the Netherlands. At that time, I was not a member of the working group myself, but became interested in the problems of analysis and of the analysis error. Then, in 1959, they asked me to serve as president of the working group and we met in Stockholm. At that time, I was much struck by the fact that the distribution of stations was certainly far from uniform and the question was what is the analysis error in regions where the data are very sparse. This led me to wonder if there wasn't some way that couldn't capitalize on the dynamical properties of the system in order to re-construct what was in regions where one had no data, which were surrounded by a band of stations from which we had frequent observations. So that's what led me to study this problem of trying to re-construct the situation inside a region where you have no observations by making use of good information in a sequence of times outside. I still don't think that problem has been pushed as far as it can be, and I've given up on it.

Kasahara: So during your Stockholm time, you were associated with the Embassy, but were you continuing monitoring the contract of the Institute?

Thompson: No, not at all.

Tribbia: In teaching courses, who would have been some of the students that you recall going through the Institute at that time?

Thompson: Most of them were actually members of the staff. Lennart Bentsen was one; Breuetssen; Holmstrom; van de Boogaard attended the lectures; there were a group of Swedish Air Force officers who sat in on the lectures. Virtually all the staff of the Institute. I gave one course of lectures on statistical hydrodynamics, actually, because I was just then becoming interested in more general aspects of the use of statistical methods in dealing with dynamical systems.

Kasahara: So you stayed at Stockholm three years.

Thompson: Three years.

Kasahara: And then you moved to Boulder. How did that move happen?

Thompson: How did that happen? Well, as I'm sure you know, people had been talking about some kind of national institute for atmospheric institute, or meteorological research or something, for several years.

Kasahara: You were in Boulder--

Thompson: Not very much. I was just aware of what was going on. At that time, the so-called Berkner Committee of the National Academy of Sciences was meeting fairly regularly, about once a month, I think, in various locations, and were discussing the general state of meteorology all the way from the standpoint of development of the science to the standpoint of manpower, access to large facilities and so on. and looking to see whether it would be profitable to establish such an institute and if so, what form it should take and how it should be administered. I was not really privy to many of these discussions; I was invited to two of them, simply to talk about certain problems that I thought were important, problems I thought might profitably be undertaken by such an institute, but was not deeply involved in planning. So it came as a great surprise to me when one day I got a telegram from Walter Roberts. I had heard that in June, 1960--reading this in the local English-language paper--he had been appointed as director. I had no idea what his plans were. I got a telegram from him and he said he would like to discuss with me the possibility of my being associate director, and could I come to Boulder to meet with him to discuss this? So I cabled back that yes, I would be willing to discuss that. Actually, I thought about it quite a bit before I answered, but on balance, I thought I would be interested in doing this.

So I flew to Boulder and was met at the airport by Bernhard Haurwitz and Walter, because Walter had never seen me before and wouldn't recognize me.

Tribbia: ____ _____________, you were acquainted with Walter Orr Roberts.

Thompson: I had never met him before. I don't really know what discussions led him to ask me to consider the job. I suspect that he had had long discussions with some of the original members of the Board of Trustees, in particular Henry Houghton and Tom Malone, who, I think, were probably most responsible for suggesting that I be associate director.

Kasahara: Also Henry Houghton was really involved in the--

Thompson: At least the way the legend has it, Walter accepted the position as director only if it were located in Boulder. I don't know whether he had it in mind then that HAO should eventually become part of NCAR, but I wouldn't be surprised if it was in his mind.

Kasahara: So after you visited Boulder and talked to Walter Orr Roberts, you accepted the position. But then, you went back and in fact, I think you started already there.

Thompson: Yes, I still had commitments in Stockholm and I told Walter that I could not come immediately, but I also recognized the necessity for beginning to recruit immediately. Although I never heard Walter say so, I think it likely that he felt as I did, that it was necessary to make a quick start--it could not be started by very gradual stages. It had to make a considerable splash and gain momentum rapidly, so recruiting had to begin immediately.

So when I returned to Stockholm I set up the Stockholm office of NCAR in the Institute, which consisted of one desk, one filing cabinet, one typewriter, one girl--that was Margaret Johnston.

Kasahara: Your Stockholm operation--how long did you operate that way before you came to Boulder?

Thompson: I was appointed in October, 1960, and I actually arrived here in July, 1961.

Tribbia: So to your mind the first order of business was recruiting and then did you immediately begin recruiting people to join NCAR at that time?

Thompson: Well, yes. While I was on my way to Boulder, I simply wrote down, first, a definition of the fields that I thought were important and should be strongly represented at NCAR. With each of these fields, I associated names of people I knew who were very strong and who were capable of deciding for themselves what ought to be done, and then tried to get those people. The first list consisted of the people at the very top, no matter who they were and with no regard as to availability. So in many cases we struck out, but these preliminary explorations invariably led to other people, and other people found out that we were interested and so by process of percolation, we were able to find the people that we wanted to start with. I guess in general my notion of how we should get started was to decide first of all upon where our strengths ought to lie, get the best possible people we can and let them decide for themselves how they could most effectively proceed toward these objectives and then leave them alone.

Kasahara: Of course, Walt Roberts himself was the chair of the astrophysics and astronomy department at the University, and HAO.

Thompson: Yes.

Kasahara: So HAO became part of NCAR.

Thompson: Not immediately. Several years after the formation of NCAR, HAO became a part of it.

Kasahara: Your responsibilities covered the rest of the scientific facilities...scientific computing aspects...?

Thompson: The facilities division at that time did not have responsibility for the computing division. That was separate. That initially was concerned mainly with scientific ballooning and for the development of field facilities. What is now the Scientific Computing Division started as a section within the Laboratory of Atmospheric Sciences of which I was director. I think that may have been a good way to begin because the first director, Glenn Lewis, and his staff were fully aware of the fact that they were providing a service to the scientific staff, and were in daily contact with them all the time. Subsequently, it was transferred to the facilities division for reasons that I will not elaborate here.

Tribbia: Can I ask if you can recall in 1960 what you perceived to be the areas in which NCAR ought to go into at that time, in other words, you related to us that you wrote a list of topics that you thought were ripe for development. Can you recall some of those topics, if not all of them?

Thompson: Oh, yes. I guess first and foremost is atmospheric dynamics, particularly large-scale dynamics. I was aware of course that numerical weather prediction was not the only place where you could apply numerical methods. There were equally difficult problems that did not lie in numerical weather prediction. There were some problems that were so complicated and so non-linear the only way you possibly could solve them is through numerical methods, coupled with a very high-speed machine. It was also clear that most universities were not going to be able to afford the very large computing facilities, and it would be logical therefore to build up strengths within NCAR in this area. So there were two things that were linked together: one was the computer and the people who would use it for a wide variety of problems in dynamical meteorology. I also believed Rossby when he said that atmospheric chemistry was going to become increasingly important, and it has. So we started out by building up a strong atmospheric chemistry division. If the only problems involved were problems of pollution, I think we would have been in pretty good shape on that score. But it rapidly became apparent that the problems were not so much scientific as political, and in a way, our atmospheric chemistry group became obsolete because they had been concentrating so much on pollution problems. So the character of that program has changed immensely, as it should.

Another area was in cloud physics, partly because it's a large and complicated area in itself, but because potentially it should be an integral part of any model which deals with things on larger scales. We also needed some strength in radiation. At first, the emphasis was on instrumentation and interpretations of the measurements. Now, of course, the emphasis has shifted so that it's tied in more closely with the modeling, the design of general circulation models, climate models and so on. Those were the main areas where I thought we had to have strength and would be providing a service.

Kasahara: So in the area of atmospheric dynamics, you contacted Aksel Wiin-Nielsen to join NCAR.

Thompson: Yes, so Stockholm lost. Aksel was instrumental, of course, in getting it off to a good start in atmospheric dynamics. I very much regretted seeing him go, but our loss was Michigan's gain.

Kasahara: I remember when I joined NCAR that you mentioned that when you start a new organization, obviously the key persons are something you're unsure about and it's difficult to attract good people. So your strategy was, if I remember correctly, to invite distinguished scientists and then also get younger PhDs for a year to work and mix them, and hope something develops. Is that what your philosophy was?

Thompson: Yes. I wouldn't say it's substantially changed. That's one reason I like the Advanced Study Program so much.

Tribbia: At the same time, or actually prior to this, there's the development of the Geophysical Fluid Dynamics Laboratory. Joe Smagorinsky had first of all pulled the general circulation modeling section out of _________________ and moved into a different location as far as the numerical prediction unit and then subsequently moved to Princeton. In your ideas as to atmospheric dynamics problems at this time--1960-1961--were you envisioning a general circulation modelling project occurring at NCAR in subsequent years? One of the topics that you felt was a ripe scientific problem...

END OF TAPE 2, SIDE 2

Interview of Phillip D. Thompson

TAPE 3, SIDE 1

Tribbia: ...I was asking Phil whether he envisioned general circulation modelling as being a ripe scientific problem in 1961?

Thompson: Yes, I certainly did, but perhaps not for the same reasons that Joe Smagorinsky did. Joe, I think, was striving for a definitive answer to the general circulation problem--that may in fact account for the rather long delay in the publication of their results. They wanted to publish the final answer.

I was looking at it from a somewhat different point of view. That is, I didn't think the problem was going to be solved in one stroke. The problem was really much larger than that. It shouldn't be regarded as a process of evolution of models which we included in greater detail, the processes that would ultimately be important. Furthermore, I regarded NCAR's general circulation model as being more of a tool, rather than as a final result, in the study of other problems. In fact, I thought that the range of problems in dynamical meteorology that could be profitably be studied at NCAR was much wider than the general circulation problem.

By the way, I left out one other field in which I thought we should have strength--that's in the general area of boundary layer turbulence, and that is the reason for example that we tried so hard to get Doug Lilly to come here, and also Jim Deardorff. Because it seemed to me that this was going to be an important element not only in the development of general circulation models, but also in the development of methods of extended range forecasting.

Kasahara: Would you like to elaborate a little bit on your plans at that time on how to proceed to attract scientists in the areas of radiation, atmospheric chemistry, cloud physics? Those were somewhat outside your area, so being in Stockholm, perhaps you had much exposure to and got familiar with those topics.

Thompson: Yes. For example, in the general area of boundary layer turbulence, that was a natural--Doug Lilly had probably done as much as anybody in establishing a connection between the structure of the turbulent boundary layer and what happens on the larger scale. And Deardorff was certainly the logical candidate in pursuing this from an experimental point of view.

There were no lack of volunteers in atmospheric chemistry. Jim Lodge volunteered himself; he had already had considerable experience with this in

Ohio, and very rapidly built up an atmospheric chemistry group, which was good for its purpose. Particularly in dealing with questions of pollution. In cloud physics, we persuaded Pat Squires to head up a group and Guy Agoyer, another group. We were not so much concerned with field experiments but with laboratory experiments. Doyne Sartor more or less voluntarily joined. So, at that time, within a couple of years after the formation of NCAR, there was a strong group in cloud physics. In radiation, the person who was most prominent at that time was ______________________, who was primarily involved in calculations of radiative transfer but who also was interested in developing instruments for measuring radiation. And dynamics, of course, we already had some strength through Aksel, through you and the people you assembled around you.

Does that answer your question? Hans Deutsch was here, also.

Kasahara: Would you like to say something about how you started the computing facility?

Thompson: Oh, yes. Well, at first, of course, we didn't have any computing facilities of our own, but the University had an IBM 7090. So we arrived at an agreement with the University to use the 7090. That was not entirely satisfactory, so we made a contract with the National Bureau of Standards, who had a small computing center with different equipment. That wasn't very satisfactory either. As a matter of fact, at that time, we didn't really have a director of the computing facility ourselves, but I persuaded Glenn Lewis to come even before we had a machine, simply because there had to be someone who was familiar with the specifications, with the alternative configurations, and who knew all aspects of computing. Glenn was unique in that he not only understood the physics of the problems that we were going to solve, but he was a very good mathematician himself. He was an extremely good programmer and he understood the hardware. I think probably now that things have advanced so far that no such person now exists. Glenn knew exactly what it was we wanted, how we wanted to operate. And after some study, he decided we should get a CDC 3600. Herein lies story: at that time, Thornton Fry was a more or less full-time consultant to Walt Roberts. Thornton Fry was a mathematician who had spent most of his life at Bell Labs, and in other commercial concerns, mainly concerned with the construction and sale of computers. Thornton took me aside when we made this decision, and he said he thought I was making a terrible mistake: that we would never be able to make full-time effective use of a machine that powerful. It would be lying idle. I said, "Well, Thornton, I don't agree with you, I don't think that's the way the wind is blowing." In fact, it wasn't long after we got the 3600 and heard that the CDC 6600 had become available, that we began to lay plans to acquire the CDC 6600. Actually, budgeting required about a two-year lead time, so we had to get started almost immediately. Needless to say, the 3600 was completely saturated in a matter of half a year, and we got the CDC 6600 and it was rapidly saturated. In the meantime, we had begun plans for acquisition of the CDC 7600, and this is just the way the pattern has gone. As soon as we get one machine, we start laying plans for the acquisition of the next generation machine. Invariably, they're used full-time almost immediately.

I never said, "I told you so" to Thornton.

Glenn Lewis was very effective as a leader of that group, and I must say that I was very sorry to see him leave, but I understood and sympathized with his reasons for leaving. I think he felt it was becoming a little too big; although it was inevitable, I think it probably is too big.

Kasahara: So do you know what he is doing now? Have you had any contact with him...?

Thompson: Several years ago, he was actively seeking a job as a director of a computer center, and I think that he was head of the computer center at NASA Ames, if I'm not mistaken. I know when he applied for a job there, they called and asked me for a recommendation. He was interested in animal behavior...and he spent several years studying animal behavior.

Tribbia: I noticed, Phil, that you served as director of the Laboratory of Atmospheric Science for the first four years. After the spinup of the staff, accumulating the number of good people--as many as you could in specific areas ______________________________, what was your next task, or what did you then decide to do...?

Thompson: After about three years as director of LAS, I had the feeling that we were sort of over the hump, that I'd inflicted my own prejudices on the Laboratory of Atmospheric Science long enough and it was time for some new blood. At about that time, Walter and I had begun some discussions about a new enterprise, which came to be known as the Advanced Study Program. One thing that was quite striking is that many of the people who contributed most, well, certainly to theoretical meteorology, during and immediately after World War II, were people who were not originally in meteorology. They were originally in mathematics or fluid dynamics or physics. Another thing that struck me was most people, after completing their doctor's degrees in meteorology and other fields, seek jobs in universities, teaching jobs, and their first years are spent not in following up on their thesis work, or in seeing what directions they ought to moving in, but in giving courses. Now these are invariably courses that are completely new to them. They're starting out from scratch, and they spend most of their time in preparation of teaching material and laying out their lectures. This is a hard job when you go through it for the first time. It probably is for a few years, so just at the time when they should have this flexibility to explore a little bit, they're taken up with the burden of preparing and giving courses. Also, it struck me that there are people are sort of on the periphery of meteorology, who could contribute a great deal, but don't have any mechanism for doing this, for finding out which problems are most important, how their work might be related to other branches of meteorology, but don't have any mechanism for doing this, for finding out which problems are most important, how their work might be related to other branches of meteorology and so on. And we began to think of some kind of mechanism that would capitalize on some of the positive aspects and alleviate some of the negative aspects. We hit on the idea of establishing the Advanced Study Program. The general idea was that we should invite some very senior people, as senior fellows, to the Advanced Study Program--only a few at any given time--and on a temporary basis. And something on the order of a dozen younger people should be invited at the time they receive their degrees or soon thereafter. They should initially spend one year here with the possibility of extension for another year. We saw in this another advantage, too: that is, by picking the cream of the crop from a large number of applicants, you had access to a number of possible future scientific staff for NCAR on indefinite appointments. But that was not really the primary reason in the beginning. Historically, it worked out that way. So we decided that we should seek a new director for the Laboratory of Atmospheric Science and I would start the Advanced Study Program.

I had met Will Kellogg many years before, and I thought he might provide a somewhat different viewpoint. His strengths are not the same as mine; he might exert some influence for the good that I couldn't. So Will was approached and he accepted and he came and I moved over and started the Advanced Study Program. I may say that [this was] about the most satisfying position I have had at NCAR because it's given me contact with a lot of very, very young people from a variety of different backgrounds and interests, and I must say it's fun to see how they develop and what they accomplish.

Tribbia: In the early days of the Advanced Study Program, who were some of the young people who had come through the program and who were some of the people you wanted to bring in as senior fellows with the study program at that point?

Thompson: Let me see. There were, in fact, some permanent members of the Advanced Study Program, two of which were an embarrassment to me. One was Bernhard Haurwitz and the other was Sydney Chapman. I'll have to tell you a Chapman story.

Dr. Fry, whom I mentioned earlier, was a gentleman of the old school and he believed that people could not maintain proper self-respect if they did not dress properly. He approached me one day along in the spring and he said that he noticed that there were a number of people who were coming to work without jackets and ties. Some were even wearing shorts, and that I should really do something about this--it didn't speak well for the organization. I pondered on this for awhile, and that afternoon it happened that I went over to my other office in Cockerell Hall--it was during the Thermal Convection Colloquium--and I was looking out the window across the quadrangle and there striding across the quadrangle was Sydney Chapman wearing an undershirt, shorts, and sandals with no socks. I thought, oh, my God, how am I going to tell Sydney that he has to wear a jacket and tie? That was the end of that.

Later, there were some other permanent members. Chuck Leith and Jack Herring were also members of the Advanced Study Program. This was done mainly for administrative convenience because they just didn't fit very well and their styles didn't fit very well within the other divisions. It was recognized that this was a rather awkward arrangement, and it was later changed, of course. Then we brought visitors like Ed Lorenz, George Platzman, Sverre Arnessen spent some time here, Paul Roberts, Phillip Drasin--people of that caliber who all interacted very well with the younger people, George Platzman in particular. Extremely conscientious about hearing their troubles.

Kasahara: So your finding and selection of staff and visitors was successful, and I can't imagine how you could have done it otherwise. By looking back, do you have anything you wanted to do but couldn't do it, or any difficulty you remember, your particular joys, anything remarkable one way or the other?

Thompson: I'm sure if I had it all to do over again, I would do some things differently.

Kasahara: How differently?

Thompson: Well, a lot of these decisions are made in your bones, so to speak. I trust my bones pretty well, but sometimes they betray me and I've made some errors in judgement of people. Now, in general, though I must say that NCAR has been remarkably successful despite the fact that it has become large. I think this is partly attributable to the fact that the average age of the scientists at NCAR has not increased one year each year. Another is that we have pressures on us from outside, from UCAR, from the National Science Foundation and from other agencies of the government. We're accountable. When things don't go well, then we have to find ways of changing. And we have changed, but it hasn't destroyed the character of NCAR. What I hope is that we never feel impelled to go only for big science, that there should be a suitable mixture of small science and big science. Both are necessary. One aspect of NCAR that I personally like very much is the fact that is possible to move rather freely around within the organization, and I've certainly done so and have not felt awkward in doing it.

Kasahara: Now one scientific area in which you continue to have interest in is the problem of turbulence...

Thompson: As I mentioned earlier, I had become convinced that the turbulence problem was important and really pervasive--it was everywhere, and we should not attempt to compete with people who are approaching this from another point of view--let's say from the engineering point of view or from the standpoint of field observations and so on. But that we should rather pursue this from the standpoint of the development of the fundamental theory of turbulence, which is why I was so anxious to bring Chuck here, and why we later invited Jack Herring to join. At one time, of course, we had considerable strength in this so-called Turbulence Club, which was sort of a loose coalition of some six or eight people at any given time, and which has now been born again in the geophysical turbulence program. Since then, of course, my interests have become a little bit broader to include not only turbulence, but the general area of statistical hydrodynamics because there is a great range of problems in which there is a strong deterministic element, but also a strong stochastic element. And the question is how do you introduce the elements of uncertainty into a deterministic system, and how you can treat this analytically or by a mixture of analysis and numerical calculation.

Tribbia: After serving as head of ASP, you then, I think, became a project leader and senior scientist within NCAR, and pursued not only statistical hydrodynamical problems but a number of other topics, I think, that continued with you through the years. Apart from these statistical problems, what areas are you currently working on, or is statistical hydrodynamics your primary problem at this point?

Thompson: I would say that's still the primary problem, but I have begun to be drawn back into more nearly the mainstream of dynamical meteorology. This work that I did on the large-scale response to differential heating is a step in this direction, and I intend to pursue that direction a little bit further. And also, in seeing some of the results of Rol Madden and Bill Randall and Harry van Loon and so on, I've begun thinking a little bit more about the problem of propagation of effects on large scales. The kinds of things that Grant Branstator has been doing. I have some definite ideas about how to proceed. I mentioned yesterday in Randall's seminar, for example, that these WKB methods are not very applicable to the atmosphere, and I know of methods that are much more powerful than WKB methods for dealing with problems of propagation effects and I intend to make a stab in that direction within the next year.

Kasahara: Why don't you go back ____.

One interesting thing you have done is to publish an essay in a book called **Weather** through Time-Life. It seems to me to be very appropriate because you have done all your time and studying at NCAR, and you have all the knowledge of that time. How did you--

Thompson: Let me see. All of this came up in the first year I was president of AMS. I was installed as president in January in Los Angeles, that was in 1963 or 1964, one of those two. There I was approached by some fellow by the name of Robert O'Brien. He explained that he had had some discussions with Time-Life books about the possibility of writing a book on weather. And he needed a collaborator because he himself was not a meteorologist. He had read quite a bit in popular books, couldn't really judge whether what he read was correct. He asked me if I could tell him what I thought such a book should cover. So we spent perhaps an hour and a half talking together and I left him with a sort of an outline, and when he left, he said that he was actively engaged in trying to find some collaborator in such a book and after our discussion, he thought that I would be a good person to do it with him. The upshot of that was that he talked to the people at Time-Life and they wrote to me and we met with them and we agreed to write the book. Well, actually Bob O'Brien did almost all the writing. Some pieces I wrote simply because it would take him much longer to do the research and to write it. So this went on for a period of about eight or nine months, I guess, and Bob came to Boulder several times, stayed for several days at a time. We would clear up any questions that had arisen, or I would give him references or the names of experts whom he could consult. The hard part actually came when this reached the editorial staff of Time-Life, and this was an extremely painful process because their practice was to form a team of editors for a particular project, to edit a particular book. And evidently their operating style was a little chaotic because they periodically changed the composition of the team or even the leader of the team, so you weren't dealing with the same team all the time. They all had different ideas about what should be in the book and what the layout should be. So there were a number of painful sessions in the editorial offices at Time-Life in which we argued about what should be included, what should not. Of course, they were trying to make the book as jazzy as possible, profitable. Well, it turned out to be immensely profitable for them, but not particularly to me. I don't remember, I think it went through about twelve printings and in several languages.

Tribbia: Having been at NCAR since its inception, aside from your own personal scientific problems that you wished to address, which problems in the future do you think NCAR is not only capable of addressing, but ought to be addressed?

Thompson: That's harder than any scientific problem. I think that NCAR should take a much stronger lead in figuring out how to make use of the results that we get. Some years back, people spoke of technology transfer. In many instances, this is only a question of getting the guy who did the work together with the guy who needs the results and working closely with him so that you're not just handing it to him on a silver platter ready to use, but so that he himself actually has a stake in it, who understands what you can do, what you can't do, what his limitations are, and who really understands all the details of the application. Now, at least in my mind, the way to do this is fairly clear, but it takes more effort than most people are willing to put into it.

There's another class of problems, though, that you cannot characterize as simply as technology transfer and this concerns questions of policy, public policy. These are much more difficult problems, as I think we have all come to realize. Largely the political system is involved. As long as people are elected for more than one term, I think their primary objective is to be re-elected and to be popular. Some of the solutions we will clearly have to adopt are wondrously unpopular. Part of the problem is us, where we don't spend as much time communicating with the public and with our elected officials as we should. There ought to be ways of doing this short of lobbying. We, of course, have made steps in this direction. The formation of ESIG, for example, is a small step in that direction. Mickey Glantz' work is in that direction. But I don't think we have put as much effort into even simpler problems. We have been trying in our own way to do this in our relations with NMC, but there are even problems there. One can only try, and if that doesn't work, try something else.

Kasahara: Coming back to your scientific work, I notice that you haven't quite provided essentially a figure that by now ____ you have been thinking for a long time, and now that you realize how to do that, and start earning--in one year, I noticed that you made it sort of a ________________ effort, tried to apply statistical hydrodynamical approaches to weather and climate. But clearly, if you're talking about fundamental theory of turbulence, it's a long, difficult way. I get the impression that you feel now that it's about time to do drastic surgery, or introduction of some ideas to make it turbulence theory useful and practical. Is that a ________________ observation, or...?

Thompson: Well, the turbulence problem is never very far from my mind. You know the von Karman story. Somebody was interviewing Theodore von Karman soon after World War II, and this chap asked von Karman what he considered to be the major problems in modern physics. Von Karman said, well, he thought quantum mechanics and turbulence theory were the two biggest mysteries in modern physics. And when he died and went to heaven, maybe God could tell him about quantum mechanics.

Lots of people have tried to develop a fundamental theory of turbulence. Some very well-known people have given up on it. I don't think I would spend a whole lifetime, a whole career, and concentrate and study this problem. I would regard it as more or less of a hobby, an avocation. But I just can't get away from it--it's like a beautiful mistress. You know that she treats you badly, she's being ornery, but you just can't stay away from her. So periodically, this question comes up again in my mind, and I keep casting about for some different and simple and natural way of representing the motion of a fluid, and some way of treating the analytical difficulties. And I seem to get a little bit closer sometimes, or at least it's a little clearer in my mind as to what direction one should go in. But you're correct in assuming this is in the back of my mind, yes.

Tribbia: Phil, about your most recent answer to Akira's most recent question, it seems that what you've been doing in the course of recent time has been to attack the problem of turbulence, but to spin off useful methodologies from those attacks. Useful in the sense that they can be used in weather prediction per se. Was that a premeditated move on your account or did you just in the course of attacking the problem come up with things that you thought might be applicable towards the related problems?

Thompson: Well, I'd long been interested in some aspects of stochastic prediction, or stochastic dynamic prediction. The early study in predictability in 1957 was an example. But there I was concerned not so much with the growth in probable error in specific regions or locales and in specific situations, but what would happen on the average taken over the entire flow. Now, what had happened later was that Epstein and his students, Fleming and Pitcher, had tried to apply stochastic dynamic methods to the growth of error in individual modes in spectral representation of the model. So in this way, you could construct the spatial distribution of error with time. The problem was that these methods were very expensive in terms of computing time. Generally speaking, the ratio of computing time required for stochastic dynamic prediction and for deterministic prediction was on the order of n^2, where n is the number of modes in spectral representation. And this of course just becomes exorbitant as n becomes large. So one thing I was looking for was some way in which you could somehow simplify the closure, still using spectral representation. But to bring the computing time for stochastic dynamic prediction down to a point where it was comparable with that required for deterministic prediction, and did in fact find a closure that made use of two things. One was a kind of local-ness of non-linear interactions between different triads. And the other is by making use of conservation principles by which you could express co-variances as linear combinations of variances, and thus reduce the number of equations and variables.

But I was still a little unhappy with that, despite the fact that this was fairly successful. It occurred to me the non-linear interactions associated with vorticity invection are much more highly localized in physical space than they are in wave-number space, across the whole spectral domain. I'd considered this originally, but I thought it would be too difficult. But then when I looked at it again, I noticed something. By making use of an analytic conversion in the case of two-dimensional non-divergence flow that Laplacian operator, and in baroclinic flows and generally in the inversion of another type of several order operator, you could derive a set of second-moment closure equations. And they're extremely simple. They can be solved in only marginally more computing time than is needed for deterministic prediction. So it is an essentially an economic problem, or that of arriving at a compromise between accuracy and computing efficiency. At least that's the way I did see the problem and I still see it. These, of course, did involve ideas that arose out of the study of turbulence. I certainly wouldn't have thought of approaching it that way if I hadn't.

Kasahara: Of course, no one expected really stochastic and dynamic calculations to be the same as a deterministic one because we certainly need estimated rate of error, initial error. To do so, we alternated with the ________________________ calculation. So if it turns out you only need ten realizations to estimate that, and the _____________________ standout is sufficient, then of course stochastic dynamic relation has to be, to gain over what they call the ____. So, it seems to me one crucial question is, if you do use the ______________ approach ___________________________, what is the meaning of the number you need to get a reasonable accurate estimate that compares with the computing effort required for stochastic dynamics?

Thompson: In the first of the two studies that I made in stochastic dynamic prediction, I dealt with a very simple system consisting of two triads which were coupled through one mode, and I could find no a priori reason for choosing one set of initial conditions rather than another. That is, you could choose them essentially at random, but there is no particular advantage in not choosing them at random. And in that case, I found that thirty realizations was not a large enough sample, and that the statistics became stable only when we increased the sample size to about 100 realizations, so that in that very simple case, it was clearly not economical to determine the probable error by Monte Carlo calculation.

Now it's conceivable there may be something peculiar about mini-mode systems, that is, there may be certain directions in phase space where the probability-density distribution tends to...

END OF TAPE 3, SIDE 1

Interview of Philip D. Thompson

TAPE 3, SIDE 2

Tribbia: You had ruled out a many-mode system in which--I'll let you finish the thought.

Thompson: Well, it may mean that in a many-mode system, the probability of distribution in phase space may move or spread in certain preferred directions. In that case, one might introduce some bias in the choice of initial conditions represented in the sample, and thereby reduce the number of realizations that have to be carried out in a Monte Carlo calculation. But I wouldn't bet the store on it.

Tribbia: I'd like to ask you about one aspect of research which you've done which we haven't touched on to this point, which involves some numerical work. In fact, its use of numerical methods, the Lagrangian method of integrating the equations, and I was wondering if the first report, I believe, that I've seen of your work along those lines was in the volume on **Numerical Methods for Weather Prediction**, and that was published, I think, in the late sixties. When did you begin this work and what led you along those lines?

Thompson: Well, Lagrangian methods have always held a certain attraction. If you're dealing with a physical system which has local conservation properties, you would like to be able to capitalize on those properties exactly. An example, of course, is the case of non-divergent two-dimensional flow in which the component of vorticity normal to the plane of motion is considered following the motion of individual fluid elements. And the idea is attractive, because it can presumably start by dividing the fluid up into small area elements. Associated with each one of those is a vorticity, or potential vorticity, your absolute vorticity, which is conserved for all time as the fluid elements move around. The remaining problem, of course, is to determine from the spatial distribution of vorticity, the stream functions and associated velocity field, so that you then know how to move the fluid element over the next time step. Now, the way this has been done previously is to interpolate the vorticity distribution from an irregular array of fluid elements onto a regular grid and then invert the Laplace operator to construct the stream function. Then, generally speaking, you have to re-interpolate back to the fluid elements to find out how they should be moved. Each time that one carries out this interpolation, it involves a certain amount of smoothing, so the fluid elements don't go around sharp bends, for example. It occurred to me that there is an exact analytic method for doing this because not only is the vorticity conserved, but the area of the elements is also conserved. You can write a stream function which relates to the distribution of vorticity and the area of the fluid elements to the stream function and velocity at the position of the fluid elements. It's an exact analytic integral, which has to be evaluated numerically. But this is a straightforward method of doing this by finite sums.

Well, this was very appealing. So Arthur Mitzi and I carried out a number of experiments using different densities of the fluid elements or different sizes and compared to the exact analytic solutions. It's extremely accurate, but it's also extremely expensive, because in order to determine one velocity component at one fluid element, you have to carry out as many multiplications as there are fluid elements in the whole assemblage. So it's just not economical; it's pretty but it's costly.

So that is in abeyance at the moment. It's conceivable with renewed interest in Lagrangian methods that this might eventually be of some use.

Tribbia: I think for myself I've exhausted the questions I can conceived of asking you, Phil. I would like to thank you very much for a very interesting interview and it's been marvelous discussing your career with you.

Kasahara: Thanks so much. I certainly enjoyed hearing your very interesting scientific ______________________ career.

Thompson: Thank you.

END OF INTERVIEW