An Interview with

PHILIP THOMPSON

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Conducted by William Aspray

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Philip Thompson Interview
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Abstract

Thompson describes his career in numerical meteorology. He discusses attitudes of the early 1940s, including those of Victor Starr and Jules Charney, towards the work of L. F. Richardson and the possibilities of predicting the weather numerically. He describes the Numerical Meteorology Project at the Institute for Advanced Study and the roles of Charney and John von Neumann in that project, partly from his first-hand experience there in 1946-47. Next he recounts the activities of the meteorology research group he organized at the Cambridge Air Force Research Center and the calculations they did in the early 1950s on electromechanical calculators and on an IBM 701. He describes the establishment of the Joint Numerical Weather Prediction Unit in Washington, and his work there from its founding in 1954 until 1958. Thompson discusses the U.S. Air Force research center he established in Sweden in association with the work being conducted at the Institute of Meteorology at the University of Stockholm. He then recalls how he left Stockholm in 1960 to become associate director of the National Center for Atmospheric Research. His interview concludes with some general comments about recent research in numerical meteorology and the revolutionary impact of the comp uter on meteorology.

PHILIP THOMPSON INTERVIEW

DATE: 5 December 1986 INTERVIEWER: William Aspray

LOCATION: Boulder, CO

ASPRAY: This is an interview on the 5th of December, 1986, with Philip Thompson in his office at the National Center for Atmospheric Research in Boulder, Colorado. I'd like to begin by asking you if you could briefly describe your career in numerical meteorology. In a minute or two.

THOMPSON: Well, I first became interested in dynamical meteorology and the possibility of applying it to weather forecasting as a student at the University of Chicago at the Institute of Meteorology. At that time, there was virtually no connection between the theory and the practice of meteorology. At that time, I didn't really have the opportunity to do very much about it. I didn't have the opportunity until about 1945 when I was assigned to UCLA. At that time, I learned about Richardson's work and had the good fortune of meeting Jules Charney, who was there at the same time. He had just completed his Ph.D. and did a thesis on baroclinic instability and was beginning to turn to new problems. He and I had a number of discussions of the problems involved in numerical weather prediction and, in particular, how we could avoid the pitfalls that Richardson had fallen into.

ASPRAY: I see.

THOMPSON: Unfortunately, Jules left soon thereafter to go to the University of Chicago for six months and then he had a one year NRC fellowship in Oslo. So our paths didn't cross for some time thereafter. Then working alone, I managed to formulate a complete set of equations that seemed to have the possibility of providing the basis for a system for numerical weather prediction. I could see the computational load would be staggering. Well, I think it was about in September or October of 1946 that Professor Holmboe gave me a copy of the *New York Times Magazine*. And in it was a published interview with John von Neumann and Vladimir Zworykin. In that interview von Neumann announced the intention of designing and building a large stored program machine for the purpose of predicting weather and of calculating the consequences of human intervention in the atmosphere.

ASPRAY: I see.

THOMPSON: As soon as I learned about that, I called my commander and asked him if I could pay von Neumann a visit in Princeton to talk about the problem and, if so, if he would agree to make the arrangements. Well, he did and several days later I traveled to Princeton and I talked about an hour with von Neumann, and in the course of that he asked me if I would be interested in joining his project.

ASPRAY: This would have been when again?

THOMPSON: This was in, I think, October 1946.

ASPRAY: Okay.

THOMPSON: So I returned to California and picked up my gear and went back. At that time, there was a rather mixed bag of people there. I shared an office with Paul Queney, from the Sorbonne, a little office under the eaves of Fuld Hall. We had a hard time communicating because his English was no better than my French. Also there at the same time was Chaim Pekeris, whose interests were primarily in hydrodynamics of blast effects, although he had worked in meteorology at MIT, when Rossby was there. Also there at the same time was Captain Gilbert Hunt, who was about to be demobilized, and he was working on a Ph.D. in mathematics. Well, his interest was primarily in mathematics but he only had wartime training in meteorology. Those were the only ones at that time that one could think of as being meteorologists. Well, eventually Queney returned to France and Pekeris returned to Israel. So for a long time, I was there alone. Then... This correspondence will indicate Charney had mentioned he was interested in coming. And he asked if I would simply drop a bug in von Neumann's ear and find out what the possibilities were. Well, this had already, in fact, occurred to von Neumann between his various other activities. He had given some thought as to getting a long term leader for the meteorology project and he wanted to know if I thought that Charney would be a good choice; and I told him yes, you'd have a hard time finding a better combination of physical insight and mathematical savvy. It also turned out that Charney had been working very well with Arnt Eliasson at the University of Olso and had also done some work with Ragnar Fjortoft. Fjortoft was tied up at the moment, but Eliasson was scheduled for a leave of absence, so Eliasson decided to spend his sabbatical at the Institute with the meteorology project. So he arrived there with Jules Charney in the middle of the summer of 1948. Unfortunately, I was scheduled to help start and organize and direct a new laboratory at the Air Force Cambridge Research Labs.

ASPRAY: Do you want to take at this time a moment to tell me about what you did at Princeton during that period?

THOMPSON: Mainly it was a question of educating myself. It became apparent that the way to start was not to use the full equations, as Richardson had tried to do, but to start with very much simpler models of the atmosphere. What I was doing during a good deal of this period was simply learning a lot more about fluid dynamics. I plowed my way through Lamb, read a number of papers on dynamical meteorology. I was particularly interested in Rossby's papers, since he had proposed very simple models. In fact, one of those eventually did prove to be the basis of the first numerical weather predictions that were made. I didn't make them; they were made by Fjortoft, Charney, and von Neumann in 1950.

ASPRAY: On the ENIAC.

THOMPSON: Right. In fact, I felt pretty isolated there being all alone.

ASPRAY: What was the nature of your contact with von Neumann during this period?

THOMPSON: Well, I saw him, I imagine, every two or three weeks, something like that. We'd chat for a half an hour or an hour about questions that had come up in my mind. Sometimes I asked him for advice or he just wanted a report on what I was doing. He was a very extraordinary man. I've known quite a few mathematicians from the great to near great, and he was like none that I'd ever met before. He was not a narrow person, he wasn't the sort of person who's a bit of a freak and knows only one subject extremely well. He knew many things well. He thought like anyone else, but much faster. He could penetrate much more deeply than most. A little unnerving.

ASPRAY: I've heard that from a number of other people, too. What was his knowledge of this material?

THOMPSON: Well, of course, he had a very good feeling for fluid dynamics. He had done some work, in fact, was working at that time on some problems in fluid dynamics, particular with regard to blast effects. He was working with Bob Richtmyer at Los Alamos. (Incidentally, Bob Richtmyer lives here in town.) Von Neumann did not have any very specific knowledge of meteorology. He'd read enough to get some sort of feel as to what the state of the science was, what it was all about, what was missing, what ought to be done; but was not very definite about how it should be done in detail. He was relying, I think, more on people who had a background in physics and mathematics, numerical analysis and computing, and had the physical insight to see what you must retain in a fairly simple model in order to make it simulate reality in some specific respects.

ASPRAY: Do you think that his knowledge of meteorology ever came up to a fairly high level or was it something he left to the rest of the group that was involved?

THOMPSON: Well, I think he developed a good feeling for what the problem was. I remember a meeting in 1956 that they held in Princeton involving, oh, perhaps a dozen and a half people, and von Neumann made some remarks about the prospects for the future. He seemed to feel that the problem of short range forecasting was pretty well in hand. Well, I think he had a somewhat naive view of how far we had come and how far we still had to go, but he was looking ahead. He said that he thought that the next problem that they should look at is not really the extension of the range of prediction, say something on the order or a week or two weeks, but what was happening on a much longer time scale. The reason that he gave was the following: that was that what happens in the short range depends very much on the initial conditions and does not depend too much on the way in which the system is forced. That is, the way in which it depends upon the energy inputs and dissipation. At the other extreme, on the very long time scale, the atmosphere very quickly forgets what it looked like in the beginning, and its behavior is dominated almost entirely by the integrated day-to-day effects of the energy inputs and by the dissipation. So one might expect that at either of these two extremes there would be a kind of idealization that would work pretty well. In between, where the initial conditions had about the same influence as the energy inputs and dissipation, would be the hard problem. And, in retrospect, one could see that this is indeed the case.

ASPRAY: What time frame is this mid-range?

THOMPSON: On the order of several weeks. We have no trouble in saying that the seasons are going to change, but we do have trouble in predicting what is going to happen in the time span of two to three weeks.

ASPRAY: What did you learn from von Neumann? What kinds of things did you talk about with him and what kind of knowledge passed?

THOMPSON: Well, remember, I was a very young man and rather untutored. There were many things in mathematics that I did not know, which I had to learn very rapidly. There were no formal classes at the Institute, so I was forced to pick up as much as I could by reading or by inventing it myself, or directing questions to von Neumann. If he couldn't give me the answers very quickly, he immediately gave me a good reference to get a start.

ASPRAY: One more question before we go on with your career. Did you have substantial interaction with Herman Goldstine or any of the other mathematicians that were associated with the Institute?

THOMPSON: I learned some rather astounding things about numerical analysis from Herman, yes. Mostly our contacts were social, however. I have great respect for Herman. Not only is he a good mathematician, but he understood the art of numerical computation. He understood the design of the machines. He's a mine of information.

ASPRAY: Let's go on with your own career. You said you moved on to the Cambridge Air Force Research Center.

THOMPSON: Yes. I organized the Atmospheric Analysis Laboratory of the Geophysical Research Directorate at

AFCRL, and I remained as chief of that laboratory for three years.

ASPRAY: What was the purpose for organizing this? What was the interest of the Air Force in setting up this particular...

THOMPSON: Well, the general interest of the Air Force was simply to gain a certain amount of competence in geophysics. Meteorology was part of that. One of the first things that I did when I went there was to start assembling a small group of people who were working on the same kinds of problems that we had started out on at Princeton.

ASPRAY: Who were some of those people?

THOMPSON: Louis Berkofsky was one, Richard Craig another... Those two, I think, were the ones that had the greatest interest. So we continued work in parallel with the Princeton group. In fact, we were in very close contact with them.

ASPRAY: When you say you were in close contact, how was this manifested? Letters? Were there visitors regularly?

THOMPSON: Well, I went down to Princeton every couple of weeks or so.

ASPRAY: I see. And did they send visitors up to see you also?

THOMPSON: I don't think they ever came up specifically to see us, no.

ASPRAY: What was your research program? What was the nature of the meteorological work that was going on?

THOMPSON: Well, it covered a fairly wide front. It was involved not only with numerical weather prediction, but also with some aspects of micro-meteorology, that is, the dispersion of gases and smokes, things of that kind. There was a little bit of work on cloud physics. We were also involved in some field programs, the so-called Sierra Wave Project. We were exploring the structure of lenticular clouds, lee waves in the lee of the Sierra Nevada Mountains in California. We were also involved in one of the bomb tests at Bikini Atoll. Those were the main things that we were involved in at that time.

ASPRAY: Okay. Could you describe in some more detail the numerical meteorological aspects of this?

THOMPSON: Yes, we worked out a simple program for calculating predictions from a very simple barotropic model, for which, in simple terms, the equation has the same form as that for a fluid that is in purely horizontal two-dimensional motion and incompressible. Under those circumstances, the absolute vorticity, the vertical component of the absolute vorticity is conserved. This involves only one equation and one unknown, namely the streamfunction. Well, it's a very simple equation apart from the fact that it's non-linear. We wrote a program for solving that equation to produce forecasts.

ASPRAY: It was a numerical solution, not an analytical one?

THOMPSON: It was half analytic and half numerical in that I actually got the solution in the form of an integral and then the machine was used to evaluate the integrals.

ASPRAY: I see. Well, there was a certain amount of debate about what the most appropriate models were at the time, whether a two-dimensional or a three-dimensional model was most appropriate, in this period. Could you comment on that?

THOMPSON: Well, all of this is, of course, connected with the economy of computing.

ASPRAY: Yes.

THOMPSON: Well, there were a number of attempts to try to reduce the dimensionality of the problem to two or "two and a half". The latter term actually appeared in the literature.

ASPRAY: I've seen that but I didn't quite understand what that meant.

THOMPSON: Well, these were attempts to try to use some technique of integrating vertically to find a single level that was more or less representative of what was happening to an extremely deep layer of the atmosphere. Well, I think everyone recognized that this was only a stop gap measure, and we were driven to this only by economic necessity. But I think we all realized that eventually we'd have to consider multi-level models in which variables could vary continuously in the vertical.

ASPRAY: What was the success with the two-dimensional model that you were working with?

THOMPSON: Well, I would say that the quality of the forecast was on a par with what a skilled forecaster could achieve.

ASPRAY: By synoptic means?

THOMPSON: Yes, by empirical means.

ASPRAY: And how long did this approach with the two-dimensional model go on at Cambridge?

THOMPSON: Well, I think that probably died very soon after I left. I left in 1951 and went to MIT. I began working on somewhat more complicated models while I was there, in particular, the two parameter model which you can think of as being characterized by conditions of two different levels of the atmosphere.

ASPRAY: I see.

THOMPSON: So I really lost interest in the one-dimensional or barotropic models at that time. I think that ended with the publication of the results, which I have somewhere.

ASPRAY: Okay. At MIT were you working in collaboration or close contact with other members of the MIT staff?

THOMPSON: Well, yes. I should say that I was a graduate student at MIT.

ASPRAY: I see.

THOMPSON: Up to that point, I just hadn't bothered [to get my graduate degree].

ASPRAY: You'd had a pretty impressive course of work experience before you went back to graduate school.

THOMPSON: Yes. Well, actually I wasn't there long. I was only there sixteen months until I got my degree. My thesis advisor was Victor Starr. Victor had a... Well, I wouldn't say a deep interest in numerical weather prediction. He was curious about what I was doing, but he just refused to talk about it. I saw him every day. We always had lunch together with Hsiao-Lan Kuo and Ed Lorenz, but he just didn't want to talk about it.

ASPRAY: I see. So you eventually did this within a vacuum, in a sense.

THOMPSON: Yes. I think the only person that I talked to very much was Ed Lorenz. For a while, he and Hsiao-Lan Kuo, T.B. Davies and I shared an office together.

ASPRAY: What was the result of your doctoral research?

THOMPSON: A paper. I developed a baroclinic model and calculated a prediction as an example of the application. The computed 24 hour changes in pressure, temperature, and vertical component of velocity were then compared with what actually happened. The computed vertical motion was compared with the observed distribution of cloudiness and rainfall. This pretty well convinced me that relatively simple baroclinic models would be a vast improvement over the barotropic models. In fact, after I left MIT and went back to AFCRL for a year, we carried out extensive tests with a model very similar to that. And that, in fact, was one of the first operating models of the Joint Numerical Weather Prediction Unit in Suitland, Maryland, after it was set up.

ASPRAY: What kind of computing equipment were you using in Cambridge?

THOMPSON: At that time, it was an electromechanical machine. We ran the machine with punched cards.

ASPRAY: Yes. It was sufficient to get operational work of some sort done? Or at least to discuss...

THOMPSON: Hold on, no. When we carried out these tests for the AFCRL in 1953 and 1954, we didn't use our own machine. We used an IBM 701 in New York City at IBM headquarters.

ASPRAY: I see.

THOMPSON: Yes, we made a sample of 120 forecasts to evaluate the accuracy.

ASPRAY: After a year back at the lab in Cambridge what happened then?

THOMPSON: Well, the decision had been made in the summer of 1953 that they would establish a Joint Numerical Weather Prediction Unit, that the best thing to do is to pool the resources of the Air Force, the Navy, and the Weather Bureau. In fact, the year back at AFCRL was really preparation for that. So, we all assembled in

Washington in the summer of 1954. I came from AFCRL, there were several from the Weather Bureau: Joseph Smagorinsky, Fred Schuman and George Cressman -- Bill Hubert from the Navy, Paul Wolff from the Navy, Lieutenant Colonel Zartner who was at Cambridge with me. The following year, we were operational.

ASPRAY: Was the purpose to do research or to do operational forecasting?

THOMPSON: Well, the primary purpose of the Joint Numerical Weather Prediction Unit was to produce forecasts. But we also realized that models would be in more or less a continuous state of development for a number of years, so we had to attach to that a development section, of which I was head. So I was expected to continue research and to develop and improve models.

ASPRAY: You told me about the background of the Air Force moving into this joint project. Could you tell me something about the Navy and the Weather Bureau's activities prior to this?

THOMPSON: Yes. The Navy didn't have any program of its own, so it assigned, let's see, how many people... I think there was only one Navy man who was assigned to the project at Princeton, Lieutenant Commander Stickles. But there were several Weather Bureau people who spent time at Princeton in preparation for the JNWPU. George Cressman and Fred Shumman were there for fairly short periods of time, but Smagorinsky was there for quite a long time. I think that's the main cast of characters.

ASPRAY: Okay, and what about the Weather Bureau? Had they had an activity before this, other than sending people to the Institute?

THOMPSON: No.

ASPRAY: They hadn't. Can you describe some of the activities that you undertook, some of the research you undertook at JNWPU?

THOMPSON: Well, one of the first things we did was to concoct a three-level model, to give us more vertical resolution. That, in fact, went into operation in May of 1955. For seven years the forecasts were based on that model. We also ran a barotropic model in parallel with it, partly just for purposes of comparison to diagnose the differences. That didn't cost very much. A number of things emerged. We found there were systematic errors in the forecasts. One was that there was a very large scale component of motion that moved to the west whereas it should remain more or less stationary. We spent quite a while figuring out why it was doing that and how it could be cured. We ultimately figured out why that occurred and remedied that. We also discovered that there was too much momentum that was being transported systematically into the westerly winds in the middle latitudes and it gradually emerged that the problem was that the model was correctly transporting momentum toward the core of the jet stream; however, it was not removing momentum at the surface. So as soon as we realized that, it was no great mystery as to how this came about. So we had to introduce some kind of boundary layer dissipation in order to get the momentum balance correctly.

TAPE 1/SIDE 2

THOMPSON: Well, this would come under the heading of nuts and bolts. But I was interested in a number of other questions at that time. One was that I was looking forward to the time when we could try to extend the range of predictions. It became pretty clear from the performance of the short range prediction schemes that one probably could not predict the phase of individual disturbances very accurately over long periods of time. So you therefore either looked to see how the very large scale components behaved or how some kind of average of the state of flow behaved. I guess what I was looking for was some kind of statistical-dynamical formulation. And, in fact, I had a crack at it in 1956. There is a paper in Tellus that describes that. I also began to get a little worried about the question of predictability, that is, to what extent was forecasting really deterministic? That is to say that the correct initial state is never known. The data are incomplete for one thing and you'd just get a sampling of points that are spaced rather far apart. The individual measurements are also in error. There are round-off errors in the transmission of the data and things of that kind. So the question is, how is the error propagated through the forecast? One thing that I looked at is the following question: imagine that you start with a whole ensemble of initial states which consist of the most probable one, but with random error fields being superposed. Now if you consider the whole ensemble, how does the behavior of this ensemble evolve with time? What I concluded was that the root mean square error would about double in a couple of days and destroy practically all predictive value in about a week. Nobody paid much attention at the time, that was about mid-1957. It wasn't until about the mid-60s, I think, that people began to get interested in this problem and many now work on it. Lorenz did and Leith and Kraickman. Well, I was there for three years, four years I guess, 1954-1958, and then I left and went to Stockholm.

ASPRAY: Do you want to tell me about that period?

THOMPSON: Well, I decided that I wanted to go to Stockholm. For one thing, it's a little bit difficult, or I found it difficult, to keep from being distracted in an environment which has primary aims which are different from mine. So I requested assignment to Rossby's Institute of Meteorology at the University of Stockholm. Unfortunately, between the time when I requested this and when I actually went there, Rossby died. But they were still a very strong group in dynamical meteorology.

ASPRAY: Who were some of the people?

THOMPSON: Aksel Wiin-Nielsen was one. Ingemar Holmstrom, Hilding Sundquist, Bo Doos, those are the others that come to mind immediately. There were also many visitors that were passing through the Institute at that time. It was much stronger in dynamical meteorology then than it is now. In any case, the Air Force did assign me there, nominally to the American Embassy, as sort of a supernumerary, but in fact I stayed at the University. And I wrote, did a little bit of teaching. That little book on numerical prediction was written there. Then I decided I wanted to stay another year and asked if they would extend my assignment and they did. I must say it was very pleasant. I enjoyed being in Sweden. Then in 1960, I had a telegram from Walter Roberts, who had been newly appointed director at the National Center for Atmospheric Research. He asked me if I could come and discuss the possibility of my being associate director. Could I come to Boulder and talk it over? So I did and the upshot was that I agreed to come if they could find some way of getting the Air Force to assign me here. I don't know all the details of how that was done, but I would guess it was not that straightforward a task. But anyway, they did manage that. I was officially appointed associate director in October of 1960. But I had to return to Stockholm, so I set up a Stockholm office of NCAR consisting of one desk, one filing cabinet, and one young lady and a typewriter. We actually began recruiting while I was still in Stockholm. Then I came physically on the spot here in Boulder in July of 1961.

ASPRAY: And you've been here...

THOMPSON: Ever since.

ASPRAY: Continuing with the work in dynamic meteorology?

THOMPSON: You must understand that when I came here for a number of years I was pretty well loaded down with administration. I wasn't very active for a period of about 15 years. There have been many changes since then and I was relieved of administrative responsibility and I'm perfectly happy to do what I am now doing.

ASPRAY: Let's go back now and talk about the earlier period that we went over. Could you tell me something about the attitudes towards numerical meteorology and particularly towards Richardson, say before and during the second world war?

THOMPSON: Well, at the time Richardson's book was published, I would say that the general reaction to it was one of disappointment. Some people may have been unsurprised, but not for the right reasons.

ASPRAY: Not for the right reasons?

THOMPSON: Not for the right reasons. I think people got the feeling that maybe this mathematical approach to weather forecasting was simply hopeless.

ASPRAY: Because of the complexity of the numerical process or the intractability of the...

THOMPSON: It was clearly intractable from the analytical point of view. The other alternative, of course, was to use some kind of graphical scheme which Vilhelm Bjerknes had proposed, or discretization of the kind that Richardson proposed. But Richardson himself mentioned that it takes 64,000 human calculators to keep up with the weather as fast as it happens in nature. In his book, he paints this picture of the weather factory, as you've probably seen.

ASPRAY: Yes, I have seen that.

THOMPSON: Well, I guess he thought that he would just be hopeful. He sounds a little wistful at the end of that description. He says, "Perhaps at some distant point in the future this might happen." But I think people pretty much discarded it; there was very little interest in it. There were some people who emotionally were against it, not for any objective reasons but because they really wanted to believe that forecasting should be an art.

ASPRAY: Could you tell me the kinds of people who you have in mind?

THOMPSON: Well, among practicing forecasters, certainly, there was a degree of emotionalism. I couldn't name anybody very specifically.

ASPRAY: Okay, but it was an attitude that ran through the practicing forecasters?

THOMPSON: Yes. I think there must have been many scientists who had this in the backs of their minds all the time, and the only reason that they didn't proceed further with it is because of Richardson's experience. They did not see any immediate prospect of being able to carry out the calculations as rapidly as we had to do them.

ASPRAY: Was Richardson's book well known, say, when you were coming through school?

THOMPSON: No.

ASPRAY: It was not.

THOMPSON: No, it was not.

ASPRAY: It was rediscovered later on when things started to have promise or...

THOMPSON: Yes.

ASPRAY: More as a curiosity, I suppose, in some ways; though it was something that people wanted to explore why he failed?

THOMPSON: Well, of course, we, Jules and I, were interested in trying to figure out why he had failed. I think, as I explained in that article that I gave you there that I was pretty sure I knew at least one reason why he failed, and that is in his calculations of changes of pressure he has to compute the divergence of the velocity field. The point is that the terms which contribute to the divergence are of opposite sign and almost exactly compensate. The divergence is very small, but it consists of two large terms. So what that means is that each of them individually has to be observed very accurately in order to compute the divergence accurately and, in fact, they're not observed that accurately. So, one of the main problems that Jules deals with in his early papers on numerical weather prediction is exactly how you get around the problem of computing the divergence. Some people also pointed out that Richardson was using a primitive form of the equations, which presumably have solutions which correspond to sound and gravity waves. And sound waves, in particular, travel very rapidly, which means that in order to maintain computational stability you have to use very short time intervals. They thought that Richardson hadn't fulfilled the CFL condition and, in effect, he hadn't. He didn't fulfill this condition, but the point was that he didn't forecast long enough for this effect to take hold.

ASPRAY: I see. So CFL doesn't come into effect.

THOMPSON: No, that had nothing to do with his failure.

ASPRAY: Were there other things that you and Charney mused over when you were discussing Richardson's work?

THOMPSON: Well, that was certainly one of the major questions. In fact, as you'll see in the letter that I gave you, the first letter there, he clearly recognized that the problem was somehow tinkering with the equations so that they don't have certain kinds of solutions of an awkward type, but retain the ones that you want almost intact. He outlines a way in which you can do this and in a second letter he tells me that he's found it. We also were aware that he [Richardson] really couldn't calculate the time rate of change of velocities very accurately. For exactly the same reasons you can't compute divergence very accurately. That is simply that the atmosphere in middle and high latitudes is almost exactly in a state of mechanical balance. That is, looking at the forces that act in the vertical, gravitational force is almost exactly balanced by the buoyancy force. That's okay, that's very close; but the horizontal forces, or primarily the horizontal pressure gradient force and the Coriolis force, due to the earth's rotation, these are almost exactly in balance. That is, they compensate. So you have to measure each of those individually very accurately if you want to compute the difference accurately. We were aware of that also. That was another reason that these [Richardson's] calculations were not very good.

ASPRAY: At, say, the end of the second world war where were the centers of research that might contribute to numerical meteorology? You mentioned the Institute's getting started at the time, Chicago, where else?

THOMPSON: Chicago, MIT, NYU, UCLA. That was about it.

ASPRAY: Outside of this country?

THOMPSON: Well, yes there were groups abroad who were very quick to pick up on this as soon as they saw it as doable. The Napier Shaw Laboratory in the British meteorological office established a group within three years after the first calculations were done at Princeton. That would make it about 1953. The International Institute of Meteorology at the University of Stockholm was quite active and Rossby was very much interested in all this. There was a regular parade of people coming through Stockholm and talking about it. There was a group in Germany at the Deutscher Wetterdienst, working with Hinkelmann. Hinkelmann, Holmann, Reymann, Wippermann. Die vier Manner.

ASPRAY: There was a Japanese group, wasn't there?

THOMPSON: Yes there was. The person who was most involved there was Gambo, at the Japanese Meteorological Agency. And the Soviet Union. Actually, Blinova carried out some early experiments, about 1945, using a method which was proposed by Haurwitz in this country, by decomposing the flow into spherical harmonics, each of which was then propagated at its own frequency. Actually, it didn't work too well. Then Kibel, Blinova's husband, devised a scheme which I think is more or less equivalent to what Charney had proposed in 1948. They were active by about 1954.

ASPRAY: Independently proposed as far as you know? Proposed this independently from Charney?

THOMPSON: As far as I know.

ASPRAY: Were these groups primarily set up for operational forecasting or for research or a bit of both?

THOMPSON: In Great Britain, Germany, and Japan, yes.

ASPRAY: A bit of both in those three cases.

THOMPSON: No. They were primarily for operational forecasting. At the University of Stockholm, no. They were mainly interested in numerical weather prediction as a branch of dynamical meteorology. The same was true at Oslo.

ASPRAY: You mentioned to me how you heard about von Neumann and his project. Did you learn anything about how von Neumann got interested in meteorology? I've heard that he had contacted Rossby at the suggestion of Reichelderfer, but I don't know. I can't verify that.

THOMPSON: I don't know how he got in touch with Rossby. I know that he did and that, well, I think Rossby was a sort of an entrepreneur in all of this. I think he was behind the meeting that was convened in the summer of 1946 at which there were something like a dozen and a half people, perhaps a dozen of whom were the most prominent dynamical meteorologists in the U.S., and some of whom were knowledgeable representatives of potential funding agencies.

ASPRAY: This was held where?

THOMPSON: In Princeton.

ASPRAY: In Princeton.

THOMPSON: Right. I think it was August of 1946. That would have been about two months before this interview with von Neumann in *The New York Times Magazine*.

ASPRAY: Did you see anything of Zworykin while you were at the Institute?

THOMPSON: I met him once, I think, maybe twice. But no, he wasn't a frequent visitor. In fact, Zworykin wasn't very active or very involved in this whole enterprise. One of his engineers was involved for a while in the study of a memory device.

ASPRAY: Oh, Rajchman. Are you thinking of Rajchman and the work on the...

THOMPSON: Jan Rajchman?

ASPRAY: Yes.

THOMPSON: He had a kind of an electronic gun that could scan over and renew the bits. It could sense whether there was a bit there or not. If there wasn't, he'd erase it and if there was, he'd restore it.

ASPRAY: But nobody from over there was...

THOMPSON: I think he was the only one that was actually involved. But eventually, of course, they didn't use that device anyway.

ASPRAY: Can you tell me something about the interaction of the Institute, if any, in the activities of JNWPU after it got started. Was there continuing close contact?

THOMPSON: You mean with the main body of the Institute or with the faculty or...?

ASPRAY: Well, say, once the group has moved to Suitland. What's the connection between Suitland and what's going on in Princeton?

THOMPSON: Well, there was no formal connection, but there were certainly informal connections simply because they'd been colleagues who had worked together in the past. I was very much interested in what was going on in Princeton. And Jules was interested to know how things were going down in Suitland, but I don't think he ever showed his face down there.

ASPRAY: I see. I guess by that time, von Neumann's already in Washington anyway doing work with the AEC. Did you see him at all at Suitland?

THOMPSON: No. Well, he was, of course, quite ill during part of that period. Let's see. I think it was about the time... yes... he was in Washington at the time the Joint Numerical Weather Prediction Unit was started.

ASPRAY: I heard some comments about the fact that he tried to set up some sort of connection with the University of Maryland with JNWPU and that fell through somehow, or another. I don't know anything more than that.

THOMPSON: First I ever heard of it. You would think I would have heard something of it. There is, in fact, now a connection between the successor to the Joint Numerical Weather Prediction Unit and the University of Maryland. But that is a fairly recent development.

ASPRAY: In the process of developing practical methods for prediction, what kinds of contributions were made to numerical methods?

THOMPSON: Well, at first, most of the methods were standard finite-difference schemes. The grids were centered difference, staggered grids of one kind or another. Then we started using semi-implicit methods, which are much more stable and faster than explicit or purely implicit methods. Those, I think, were sort of developed outside of the mainstream of numerical mathematics by the meteorologists. Then we shifted to spectral methods and those I think have been used by meteorologists as much or more than by anybody else. So, I would say that probably something like a third of the methods in use were invented, concocted, for very specific purposes and the rest were more or less standard methods.

ASPRAY: Did these methods developed for meteorology get used in other fields as far as you know?

THOMPSON: Yes.

ASPRAY: Can you give me some examples?

THOMPSON: Well, spectral methods are widely used in turbulence calculations now.

ASPRAY: Can you give me a clue to the literature on this or people who were particularly involved in it, or any way of hooking onto it.

One of the things that I'd like to explore in more detail and I'm not quite sure how to get into it, is the relationship, the role of the computer, in all of this. Maybe one way to get started is to talk about your experiences with the computers you used during this time. For example, at Suitland, what equipment did you use?

THOMPSON: We started out with an IBM 701. That was, I would say, just barely adequate for our purposes. For a three level model to make a twenty-four hour forecast took about three hours. That was after the data cut-off time. That is, we would allow a certain amount of time after the observations were made for them to be coded, and transmitted, and collected. That is several hours after the observation time. We had an objective analysis scheme by which the machine went through the data and made certain internal checks for errors. It could stop and flag the suspected error and then we could look at the list and go back and check to see if, in fact, they were errors, and drop those reports that are judged to be in error. It then went through a routine of automatic interpolation to map the data, from irregularly spaced points onto the points of a regular mesh, for computational purposes. This took about an hour and then it took another couple of hours to make the forecast. Now you can see that that's already eaten up one quarter of the forecast -- *period*.

ASPRAY: Right.

THOMPSON: So that, in effect, you're only making an eighteen hour forecast.

ASPRAY: Can you give me an idea of the order of magnitude, the number of computations or logical operations that might be involved in doing this?

THOMPSON: Well, in those days, I think it was on the order of a billion for a twenty-four hour forecast. Well, what I'm saying is the 701, as fast as it was, was really barely adequate for our limited purpose at that time. I might add we did not make forecasts for the entire globe or for the whole northern hemisphere to begin with.

ASPRAY: The range was continental U.S.? Is that right?

THOMPSON: It extended somewhat beyond it in all directions. Then later we did make forecasts on an octagonal grid that extended from roughly twelve degrees latitude on up to the pole. Well, feeling a little cramped we ordered an IBM 704.

TAPE 2/SIDE 1

THOMPSON: We ordered an IBM 704 which was considerably faster than the 701, and were able then to go on to somewhat more complicated models -- in particular, to the primitive equation model. By that time, I'd left and I don't know very much about the history of the machines that JNWPU had since then except that they now have a Cyber 205.

ASPRAY: Were you in on the decision to acquire the IBM 701 for the JNWPU? I understand they also considered buying Engineering Research Associates machines?

THOMPSON: Well, there were, I think, two or three, maybe three machines, that they considered as possible candidates. I don't exactly remember how the decision was made. It happened that the tests that we had run at AFCRL were done on the IBM 701, but I'm sure it had nothing to do with the decision. The codes were written for the 701. I imagine mainly that IBM had a reputation for putting out pretty reliable equipment.

ASPRAY: In your 1961 book, you talked about the role of the computer as an experimental device. Could you elaborate on that? Could you give some examples that expand on the comments that you made there?

THOMPSON: Well, yes. I mentioned earlier that one doesn't usually like to proceed immediately to the most general possible model simply because there are a multiplicity of things that could go wrong and when something does go wrong, you don't know why. The place you have to begin is clearly with the simplest model, I think. You try to understand that and see in what respects it works and in which respects it doesn't work. Then you gradually work your way through a whole hierarchy of models which you keep changing little by little trying to understand the effect of each change as you go along. Now, actually, no one has ever been quite as systematic as all that; but, in general, this is the kind of strategy that people have followed or should have followed. Well, inevitably, there's a certain amount of tinkering that you do. There are a lot of what-if questions. I mean, suppose you change the viscosity a little bit, what is the effect? And sometimes you find some amazing things. You find that the effect of increasing the drag is not to reduce the wind speed, but that it will remain about constant. All kinds of questions occur to you that you could answer very easily, that are hypothetical conditions, things that you would normally do in a physics laboratory, for example. But what we deal with is just too big and uncontrollable, so you simulate this in the machine. It is very effective. So that is certainly one aspect of experimentation, but in others it's really much more primitive than that; you're just groping around for some kind of crude formulation of the problem because it hasn't really gelled yet. You don't really know quite how to do it. You just tried something, tinkered with it and see what works and what doesn't. I don't know of any handbook that tells you how to do this, but I think most people do this at one time or another.

ASPRAY: So it's likely, for example, not only did you do it as part of your work at various times, but maybe Charney was trying this kind of approach...

THOMPSON: Well, I know he did a certain amount of tinkering around, too. At MIT he had a little PDP machine. He wanted to get a feeling for the instability of a whole aggregate of point vortices. So he had his programmer code up a little program to follow around each vortex in a large ensemble of vortices. He would sit there and watch the cathode ray tube for hours on end, watching these things evolve and trying to get a feeling for what they did. Whether or not they tended to cluster, or spread apart, and so on. It was as if it was a kind of play, but it was still experimentation.

ASPRAY: So is it fair then to say that the computer gets used not only as the instrument of doing the computation once you've reduced your simplified equations to numerical algorithms, but also in terms of the determination of that set of simplified equations in the first place?

THOMPSON: That and even discovery. There have already been some examples of this. Jim McWilliams, who has an office just over here [he points], discovered more or less by accident while he was doing numerical tinkering, that there is in fact a tendency toward vortex coagulation. In the meantime another group of people looked at the theory to see why this happens. That result was very interesting and an unsuspected phenomenon. If you go back and read von Neumann's little essay... I guess it was given as a talk called "The Mathematician." Have you read that?

ASPRAY: Yes.

THOMPSON: He suggests the machine would become an instrument of discovery in mathematics.

ASPRAY: One of the things that historians of technology are interested in is the fact that an advance in one technology might stimulate advances in other technologies or other parts of the science, and here's how it seems to me that it might apply in this case. With the development of the computer, you had the possibility of doing numerical calculation. Yet you need better data and communication of that data. Was there a whole series of advances stimulated in bringing in more data, systematically taking it in, communicating it, checking to see if it was reliable because of this?

THOMPSON: To some extent, but not to the extent that one would like. International governments moving in international concert don't move very fast. One can think of ways in which the ability to compute rapidly has contributed to the development of instrumentation, observation, and certainly the transmission of information and the way it's processed after it comes out. Mostly it's been applied to recently developed instrumentation, whereas most of the standard observations are made with instruments that were designed fifty years ago.

ASPRAY: Did satellites start to play a role in say the late 1950s?

THOMPSON: No, it was later than that. Of course, it was hoped that satellites would eventually replace conventional weather observing systems. It's a very attractive idea. It might be economical if it worked. What we tried to do is to infer the vertical distribution of temperature from measurements of radiances at a number of different wavelengths, of the order of nine or a dozen, something like that. When you have the temperature, it's in principle possible to integrate the hydrostatic equation numerically to find out what the vertical distribution of pressure is. If you do this at many points, then you have a more or less complete picture of the pressure pattern and at least a rough idea of the flow patterns as well. So it was hoped that eventually by using such devices you could dispense with the conventional radiosonde system. However, there are some built-in problems. For one thing, in order to determine the temperature distribution you have to solve an integral equation. It is an essentially ill-posed problem. There are various statistical ways that they've tried to use to do this, but they're imperfect, and apparently not good enough to supplant the conventional methods, at least not yet. People will keep working trying to improve it, but meanwhile they're not going to give up the conventional system.

ASPRAY: Once you had the Joint Numerical Weather Prediction Unit set up, with the cooperation of a number of agencies, did that give you the power to get more systematic data collection? Could you recommend and get, say, the Weather Bureau and the Navy to secure funds for that kind of thing?

THOMPSON: Well, we could speak our piece. We were only one small voice out of many. The business of weather observation is pretty expensive. I don't know what the NOAA spends annually for maintaining radiosonde stations or expendable equipment, but it must be of the order of several hundreds of millions of dollars. Presumably, the United States could, itself, increase the density of observations over the United States to make us happy, but that wouldn't be much use unless every other country did too simply because the interactions are very long-range. In order to forecast weather for the United States forty-eight hours hence, you need the data over the Pacific Ocean and Atlantic Ocean. So in order for this to be very effective, you would have to influence not only the policy of your own government, but also you'd have to work out some international agreements through the World Meteorological Organization and that's essentially a political organization. So I don't think what we recommended or would like to recommend had much to do with what happened.

ASPRAY: Did the success in numerical prediction lead to a training problem, after all you need people who had some knowledge of meteorology, some knowledge of mathematics? Probably there weren't very many people at first who had that combination.

THOMPSON: No, there weren't. In fact, some of the Weather Bureau people who were in the Joint Numerical Weather Prediction Unit had never been exposed to these until they went to the Institute for a short crash course. Well, the results of research generally find their way into the curriculum of graduate schools. I gave the first course ever in numerical weather prediction in 1953 at MIT. So there were immediately twelve people who were trained to the point that they could actually write a program. By 1963 or 1964, something like that, there were perhaps a dozen departments of meteorology that had courses in numerical weather prediction, generally graduate level courses.

ASPRAY: That supplied an adequate group of people for the need?

THOMPSON: Oh, I think so. By now I think every graduate student has been exposed to the kind of machinery that you need to do this sort of thing. In fact, I find it a little appalling in some respects. Analysis is a lost art now.

ASPRAY: I see.

THOMPSON: The advent of the use of very high speed electronic computing machines has changed completely the way in which meteorologists view their problems and the ways in which they approach them. This is true not only in numerical weather prediction, but all of dynamical meteorology. It's becoming true in other branches of meteorology; in cloud physics, for example, people had begun using numerical methods to study the growth of droplets by diffusion or coalescence of droplets over a broad size spectrum, things of that kind. They are extremely difficult to do analytically. People have been looking in detail at the way in which convective clouds evolve by using very complicated two-dimensional models with all the thermodynamics in them. Radiation calculations now are done on the machine. Before they had to integrate over broad bands of the spectrum; now they can virtually do it line by line. Turbulence calculations are being done on the machine in two and three dimensions. There's practically no branch of meteorology that has not been completely revolutionized by the machine. It's a way of life!

ASPRAY: It's now a standard working tool.

THOMPSON: It's a better hammer. But you have to remember, it's only a hammer.

ASPRAY: I suppose that a meteorologist would commonly argue that no machine is big enough or fast enough for his purposes, but can you say something about when the machines became powerful enough that the field really started to open up?

THOMPSON: Yes. I think about the time when the CDC 6600 came out there was a more or less abrupt increase in the order of complexity that could be handled on the machine. By this I mean that it wasn't the complexity brought about by merely increasing the resolution or something like that, but I mean qualitative changes in the physics, and the range of the interactive processes that could be included.

ASPRAY: Can you give me just one example as an illustration of the qualitative change in a particular narrow area of meteorology that happened with this kind of new computing power?

THOMPSON: Yes. Putting in, for example, detailed treatment of the turbulent transfer of momentum in the boundary layer in a very complicated general circulation model which describes what is happening throughout the entire atmosphere. That made a qualitative difference.

ASPRAY: And prior to that, this was just ignored or...

THOMPSON: Well, it was what we called parameterized. It was a way of providing a kind of a simple statistical description for what went on. It didn't describe in any way the detailed workings of what was going on in the outer layer.

ASPRAY: Okay.

END OF INTERVIEW