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TAPE RECORDED INTERVIEW PROJECT

Interview with George Cressman
August 24, 1992

Interviewers: Warren Washington, Norman Phillips, Ron McPherson, Jim Howcroft

Washington: This is an interview with George P. Cressman, August 24, 1992. We are interviewing at the National Meteorological Center in Camp Springs, Maryland. I am Warren Washington. The lead interviewer will be Norman Phillips, and we have in the room Ron McPherson, Jim Howcroft and myself. Norm Phillips will be conducting most of the interview.

Phillips: George, thank you for sending your curriculum vitae. It helped me organize questions. The logical place to start, I think, is your youth, who your parents were, where you grew up and how you first got interested in science since then...you got a B.Sc. from Penn State, your first degree.

Cressman: I was born in Southeast Pennsylvania, in West Chester. My father was an educator, and my mother had also taught, but she devoted her full-time energies to running the family and house, probably about the time I arrived. I was living not very far from Harlan Saylor. My dad was assistant county school superintendent and Harlan's was the county school superintendent.

Phillips: Harlan Saylor was one of the chief weather forecasters in the Weather Service, who died about a year ago, and was about George's age.

Cressman: Life in a small town in those days is probably a lot different from the way it is now. We kids had to amuse ourselves without the benefit of automobiles, and we did this mostly outdoors, I think. Our absence from the house was welcome a good bit of the time, and we used to go out to the countryside on our bikes to swim in the summer. We were constantly doing hazardous sled riding in the winter and all in all, we kept ourselves very occupied outdoors. Everything we did depended on the weather. Harlan and I were good friends, and the first thing we did when deciding what we were going to do the next day or even that day was to think about what the weather was likely to be. So we became sort of self-taught weather amateurs. There were hardly any books in those days in which one could read about the weather, and it was just as well.
An interesting result of this was that we both went to Penn State to major in

physics with the idea of learning meteorology and becoming meteorologists.

Phillips: Had you ever met any meteorologists at that time?

Cressman: Only one. One time in high school Harlan and I went down to Philadelphia, to visit the Weather Bureau station. It's very interesting that Mort Rubin was employed in the Philadelphia Weather Bureau office at that time, but we didn't see Mort...we saw another fellow who I believe was a sub-professional and he was plotting on a map and drawing lines on it, so that the meteorologist in charge, Mr. George Bliss, would then come in and approve everything he did or make suggestions and then George Bliss would dictate the forecast. No fronts were on the map--it was not Weather Bureau policy at the time--and the rationale they went through at the Philadelphia office was probably about the same as occurred in most Weather Bureau offices at that time. The forecasting was a combination of extrapolation and experience. If you try to think what was meant by "experience", experience was remembering similar situations and how they behaved. Petterssen once described experience as "forecasting developments similar to those of comparable situations."

Let me continue, get Harlan and me to Penn State at least. We both decided we wanted to be meteorologists, but I should mention that, at that time, Harlan became the cooperative observer for our town, West Chester, upon the death of the former cooperative observer. Somehow he let George Bliss know that here was a willing volunteer, and the thermometer shelter and instruments showed up in Harlan's backyard. So he was the official cooperative observer for West Chester, and when he left to go to college, his dad took over the job.

Phillips: I suppose that system still operates this way, the cooperative observers.

Cressman: I hope it does. I don't know what it is today, but this is a very good program because you get a lot of data from little places in tremendous detail. How much analysis takes place of these data is another question.

Phillips: Was there some particular reason why you both went to Penn State?

Cressman: I would say largely financial. But Penn State had a pretty good reputation, as it still does. And it was rather inexpensive for a state resident. They had no meteorology department, so Harlan and I both enrolled in the physics department. Now about the time we got there, Helmut Landsberg showed up at Penn State. He was, as you probably know, a Jewish refugee from Nazi Germany. He employed Hans Neuburger.

Phillips: What year might this have been?

Cressman: Harlan and I arrived in 1937, in the fall, and I think Landsberg as well. I wasn't aware of Landsberg when I was a freshman. Hans Neuburger must have shown up around the same time. And so there was a department in which they had two people; each came from Germany and was in meteorology. They were the nucleus of the meteorology group at Penn State. Landsberg established the normal courses you would have at that time; you would study a little bit of theoretical meteorology largely based on the German books and writings. I had to read one of the books. I think the author was Koschmieder. Was he an instrumentalist?

Phillips: The name is familiar, but I think it was a dynamics book with his name on it.

Cressman: Then maybe that was it. It was Koschmieder's dynamics book. Then there were a couple more dynamicists. However, Central Europe at that time was big on climatology. They had what was to my mind a very comprehensive and thorough study of climatology and probably done in the most boring way anybody could imagine. You remember hearing about the Köppen-Geiger climate classification system. I knew that from A to Z by the time I left Penn State.

Phillips: And you've made use of it every day since.

Cressman: It was an odd thing, not so long ago on that time scale--I would say ten years ago--I was invited to a symposium in Vienna, and one evening we went to a meeting and a dinner--and some of these typical Austrian climatologists showed up. I felt as if I were transported back to 1937 or 1938. You had to have been exposed to the Köppen-Geiger system because they talked in this kind of shorthand. But they were still doing all kinds of studies which were probably excellent from an economic point of view because I had the impression that practically every serious valley and hilltop in Austria had a very well-known climate, which is of course is what you need if you're going to have agriculture in a country with that kind of relief. But they were still doing the old climate studies, which reminded me so much of Köppen-Geiger. You had to know Köppen-Geiger to know what they were talking about. So in a sense I felt transported back to the days of the late 1930s when I visited Austria.

Phillips: Before you got your B.S., were there any professors at Penn State who particularly inspired you?

Cressman: I had some very good physics professors. There were rotten math instructors. For partial differential equations, I had a gentleman who taught mathematics to my father when he went to Penn State, and he didn't have a good mastery of the

subject any longer; whether he ever did, I don't know. That was a handicap I had to overcome in the future.

Phillips: Would I be right in assuming you entered the Air Force Weather Program?

Cressman: Yes, of course. The way that happened was, as you certainly know, the Germans were gobbling up Europe at the time, the Nazi bestiality was headlines everywhere, and it was becoming reasonably clear to people who read the newspapers at all that we were going to be in the war sooner or later. About that time, say late 1940, I think, the Air Force completed the establishment of their Weather Officers' Training Program at five universities. They were MIT, UCLA, University of Chicago, Caltech, and New York University.

Phillips: It was also at two air bases, one at Grand Rapids, Michigan, and one at--

Cressman: That was later at Grand Rapids, and it's worth saying something about the Grand Rapids operation. Anyhow, I had applied to MIT for a graduate scholarship in meteorology, and I received what was probably by most standards a rather generous offer. I had to be absolutely self-supporting because when I finished college, my brother had to start and my father was not very highly paid. So when the Air Force program was announced, Landsberg informed us of it, and Harlan and I both applied. As it turned out, Harlan was sent to MIT and I was sent to NYU. At MIT when Harlan arrived, there were Sverre Petterssen and others, and when I arrived at NYU, there was Athel Spilhaus. Gardner Evans was there, also. He was one of these old-time synopticians who was so clever it was hard to believe. With a very limited amount of data, he could, by and large, produce a rather good forecast. One time during the war, in 1942, we were giving a lot of weather information to the enemy by simply printing yesterday's weather in a lot of stations around the country. One could draw up a weather map from this. It was rather limited information but Gardner Evans took a series of days and with the information available in today's newspaper, drew up a weather map for last night. They turned out to be quite good. He sent them to the Air Force and suddenly the information disappeared from the newspapers.

Well, at NYU, Spilhaus was of course the leading figure. Jim Miller and Bob Culnan were both there. So we went through our nine months of training. We were sent to Mitchell Field, commissioned as second lieutenants and made available to the Air Force. Next thing you know, I'm back at New York University as an instructor, so I instructed the next class. At that time, there was some kind of a deal cooked up between Rossby and Spilhaus. Rossby was at Chicago, and the deal was that two of us instructors from NYU were sent to the University of Chicago, and two from the University of Chicago were sent to NYU. This was, on its face, to help homogenize the instruction at the

universities. I had no idea what they did at Caltech where Krick was...I don't think I even want to find out. At the University of Chicago I met Rossby, Victor Starr and a number of other people who you know, and was kept there to instruct for awhile--I don't remember if it was one or two courses.

Phillips: Were you there when the war ended, then?

Cressman: Oh, no. This was still pretty early on. A rather curious thing happened when I was there. I've never really quite figured this out. I was called in by Byers to his office. Horace Byers managed things for Carl Rossby. Byers had some visitors. Joe Kaplan was one of them. Byers introduced me to them. I was informed that there was going to be a massive invasion of China by the United States. Our Army would move northward along the eastern part of China, as close to the coast as possible, and set up a really big base for an invasion and assault on the Japanese islands from northeast China. I was supposed to go along on this. I was supposed to meet with another fellow by the name of Bishop, from UCLA. He was going to come to Chicago, and we would put together a course to teach the first group of forecasters that would be sent. The teaching would be done in North Carolina. We would then meet a second group of forecasters one or two months later, and then we would go with them. So I thought that this sounded fairly interesting. We hastily put together a short course, went to Goldsboro and taught the first group of forecasters who were going to China.

Phillips: You were still single?

Cressman: No, my wife was with me. You know, I knew I had to go sooner or later, and I thought this looked like a better deal than a lot of others I could think of. This was extremely classified at the time. We had to teach behind locked doors and drawn window shades. We taught the first group of weather officers who were going. The last thing I said to them in the last class I taught was: "I'm not sure how this is all going to work out. If you fellows would do me a favor, I would ask you, wherever you get where you're going, send me a note and tell me where you went." Bishop went back to UCLA and I went back to Chicago. Two months later I got a card: The whole group went to England.

I learned more on this one when I interviewed Spilhaus. A little later Spilhaus was sent to northeastern Siberia to help establish a weather service for an assault on Japan through Siberia. And with further reading of the history of the times, I discovered what probably was the explanation for all this. The Japanese captured the East China air bases so fast that the U.S. had to call this off. Although the idea of the U.S. was to move in and take over these Chinese air bases, Japan got there first.

In the meantime, there was another thing happening. When did you go to Chicago, Norm?

Phillips: After the war. I first went there in 1940. I spent two years there, in chemistry.

Cressman: I think it was in 1943 that the first self-sustaining nuclear reaction was accomplished in the former west stands of Stagg Field, which is just across from the old meteorology department. That was in Chicago. We didn't know it at the time. We instructors used to go out there and play touch football on Stagg Field. We had not the faintest notion, of course, what was happening back in the subterranean areas.

Phillips: Squash court.

Cressman: This is all well-documented in a book you can buy in bookstores, The Making of the Atomic Bomb, by Richard Rhodes, page 428. But that was big news to us much later.

Phillips: I remember reading about it in a translation from the Russian, a Russian article that had been translated from the English. It came back that the atomic bomb was developed in a pumpkin patch, instead of a squash court.

Cressman: So what happened was that the mobilization was off, and I was sent off to the West to do forecasting. Mostly it was training for heavy bombers.

Phillips: Now what do you mean to the West?

Cressman: To the Western states. I was sent first to Great Falls, then I was sent to Mountain Home, Idaho, and back to Great Falls. I had some difficulties out there. It seemed that the graduates from Caltech were thoroughly indoctrinated in the Krick school of thought. This penetrated the Air Force Weather Central at Great Falls before I arrived. I arrived there very innocently and immediately banged my head against this Krick business. Without going into the details, what happened first was that I was called into the office of the Commander of the Weather Central, who outranked me of course, and informed that it was time for me to shape up about the Krick system. And I said, "Well, you can't be serious--that's a bunch of crap." Well, guess how long I lasted at Great Falls! I went out faster than the wind blew across the prairies.

McPherson: George, do you remember the names of any of the people who were the apostles of the Krick system?

Cressman: I remember very well, but I'm not sure it would be a good idea to record them at

this time.

Phillips: You can if you want to--you have a form which you can sign at the end of the meeting which states how long this will be kept--

Cressman: No, I think it would be preferable to omit the name of the fellow who was running the Weather Central. May I add, however, that our regional director of the Air Force Weather Service, who was responsible for that area, came to interview me as a recalcitrant and obviously ignorant meteorologist. And I told him in quite unvarnished terms what I thought of Krick's system and what Great Falls was doing with it. Shortly after that, I got my transfer out. Not long after that this poor fellow had a nervous breakdown. So their revenge finally caught up with me. I was taken out of forecasting, and sent down to Homestead, Florida, to teach meteorology to pilots, along with forecasting.

Phillips: One interjection here. I vaguely remember that in the series of books or pamphlets--reports--that were published by the University of Chicago, one of the earliest ones is devoted to the climatology of Southeast Asia. And the rest of these reports are on more dynamic subjects, and it's always stuck in my mind that this was an oddity. Would this have been related to the first study you mentioned when you were studying Southeast Asia?

Cressman: Not to my recollection, but my recollection these days is not what it used to be. I did write some really silly things at New York University. In fact, I ran across a title of one that I had completely forgotten. Apparently I was a co-author of a report on the meteorology of the Black Sea that I did at New York University.

Washington: George, I wonder if you could say something about the differences in general between the Krick method and the more conventional method.

Cressman: I think it was a bit worse at Great Falls because we had to issue somewhat longer range forecasts, up to ten days into the future. But the Krick method was essentially an analog method, and I have come to the conclusion that somehow there must have been considerable influence on Irving Krick by Franz Bauer. You remember reading Franz Bauer?

Phillips: Yes.

Cressman: Franz Bauer was a person who, back in the early days, tried to classify weather into different types, and then, once he had established the types, would make a projection, a forecast based on what this type usually did. So the trick was to get the type right. Supposedly if you got the type right, you had a pretty decent analog forecast. Well, the influence of Franz Bauer on Krick is something I

haven't been able to trace; I haven't tried too hard because I had such a difficulty with Krick's disciples I thought that this could lead to nothing but problems. We used to laugh at Krick's system as "forecasting developments comparable to those of analogous situations." Much to my surprise, in doing a piece on the history of forecasting for the AMS committee on history of forecasting, I ran across a reference by Franz Bauer which had almost that exact title.

Phillips: In German, of course.

Cressman: Yes. Well, my German is a little slow these days, but with time I can figure it out. And to prove it, I may say that recently I translated an obituary of Ertel. I thought, "I am going to see if I can translate this." And I did. But it was very flowery in nature, in typical old-time German prose. It took me a long time to translate it. That's the most difficult kind of German you can work with.

Phillips: It borders on poetry.

You ended up in Puerto Rico in 1945, according to your vitae.

Cressman: What happened in respect to that was that I was still in Homestead, Florida, when it was obvious that the war wasn't going to last too much longer, and I got a telegram from Rossby. People didn't use telephones in those days the way they do now, and he asked me to come and see him. I got a couple of days off, and I went by train to Chicago and back again.

When I got to Chicago, Rossby said that he thought I ought to continue my graduate studies. I had an MS by then, and he thought I ought to come to Chicago to continue. I said, "That's all very well, but I've got a family to support and I've got to do what I can do and not what I want to do." He said, "Well, don't worry about that, we'll find something for you to do and you'll get paid a little bit." And I immediately agreed.

Phillips: Had you known Rossby before?

Cressman: Oh, yes. You see, I taught a class in Chicago for them, about nine or ten months. And I guess the gestation period for meteorologists is the same as for everybody else--nine months--they gave us nine-month courses.

You mentioned Grand Rapids. This is worth mentioning. At the close of the third wartime class at the universities, the Air Force decided this wasn't producing what they needed. They had to have a much bigger training of meteorologists because all these plans were envisioning fantastic numbers of aircraft, and tremendous air power to conclude the war against both Germany

and Japan. And they could see that they were not getting enough input of meteorologists through the universities. At least, I surmised this was the reason. So they established, in addition to the universities, a very large weather officer training program at Grand Rapids, Michigan.

Phillips: And at Chanute Field.

Cressman: Well, Chanute Field is another matter. That was more for the enlisted people.

McPherson: Grand Rapids' school later moved down to Chanute Field--

Cressman: I'm not surprised at that.

McPherson: --and that's the class that Norm and I were in.

Cressman: Because up to then, all that was done at Chanute Field (under Colonel Don McNeal) was training for observers and enlisted forecasters. The idea of working at a big Air Force shop at Grand Rapids had a very bad odor to it for me, and I was considering ways to avoid such an assignment. Many years later, somebody sent me a copy of a newspaper clipping about that school at Grand Rapids. At that school they had as many, in a synoptic laboratory, as 1,000 students drawing weather maps in one enormous room. There were over 100 instructors walking back and forth in the aisles. I was suspicious that something like this would happen and I wanted by all means to keep out of it. I would much rather go to East Asia and deal with what happened there.

Phillips: Returning to the tropics, you did spend some time in Puerto Rico, I gather.

Cressman: That's right. At the end of the war, I got a call from Rossby and went to Chicago to see him. He said that he wanted me to come to Chicago and study, but first--there was a "but first"--he needed me in Puerto Rico for awhile. So I checked with my wife and she approved, saying that the end of the war was coming and there would be thousands of people looking for jobs. She agreed that I should go to Puerto Rico. So I signed on and soon I was en route to Puerto Rico. Herb Riehl, Clarence Palmer and Bernhard Haurwitz were there, as was Elmer Schacht. I wonder if you knew Elmer.

Phillips: I believe I may have at one time.

Cressman: He had been at Chicago. The senior staff at San Juan was Clary Palmer, Herb Riehl and Bernhard Haurwitz. So it was interesting. They had managed to get a fair amount of data put together and out of that group, came, I think, fairly definitive work on such things as waves in the easterlies, extended polar troughs,

etc.

Washington: George, was that group primarily there to teach, or was it to do research?

Cressman: No, there were no students that I knew of. Somebody might have had a class someplace, but I can't remember anything about it if that were the case. It was almost entirely research, and when I was there, I was on the contract with the Office of Naval Research through Chicago.

Phillips: Were there any Puerto Ricans involved?

Cressman: Yes. One of the early junior members there, he must have been a student at the time.

Phillips: The name José Colon comes to mind.

Cressman: That's it, José Colon. You're right. He stayed there after nearly everybody else left.

McPherson: He was in charge of the Weather Bureau office in San Juan actually until perhaps five or six years ago.

Cressman: He eventually did move into the San Juan office as an employee of the Weather Bureau. I had the pleasure of appointing him as Meteorologist-in-Charge at San Juan.

Phillips: Then you did this as earning your keep for Rossby in Puerto Rico; you came back to Chicago.

Cressman: This was part of the deal. I would go back to Chicago. He would pay me a salary enough to keep body and soul together. In the meantime, I would work toward my doctorate. If I was not interested in my doctorate, forget the whole thing. He was quite right and I knew it. So he had no trouble with me on that. That's how I got to Chicago again. When I got there, he said that he wanted me to run a synoptic laboratory on a daily basis. That sounded to me very attractive and a lot of fun. So when I asked for a teletype circuit to get this data or that data, there was never any question of it. They came. We established nearly as much as you could in those days a worldwide database, on a daily basis, as you remember. Rossby was always after me to expand the geographic scope of the activity. We got the circuit that brought in the Russian data and we had just about all the data we could get. It was remarkable. Rossby wanted to have this laboratory and as you know, he made terrific use of it.

Phillips: Were other professors at the university at that time heavily involved in the--

Cressman: I think the other professor who was most interested--I think you'll probably agree with this--would be Palmén.

Phillips: Of course, he wasn't there all the time.

Cressman: But it seemed to me as if he were. He had tremendous impact and he came and went, and whether Palmén was there or not there, you couldn't avoid thinking of Palmén. If you wanted to make a change in things, you'd think, "When Palmén gets back, what is he going to say about this?" So Palmén was ever-present in my mind.

I loved Palmén. He was a terrific fellow to have around. And you know how Rossby would bring in other people--he'd bring in Bergeron, Jacob Bjerknes, anybody who was worthwhile he could think of. Rossby loved to have a clash of ideas. And he would deliberately destroy his own ideas in order to get people upset, and develop a good argument. And you know how many discussions and arguments on a completely informal basis took place around those map tables. We had a current display up on the wall like that and the map tables down there, and I had to do my work on the map tables, so these guys were arguing all around me. It was fun.

Phillips: That was before the days of numerical weather prediction, of course. Nonetheless, it was after Rossby had developed his wave formula in 1939, and after he had developed the concept of a constant absolute vorticity--and I recollect that those must have formed the basis of much of your thesis.

Cressman: One should say that Rossby was so far ahead of people that nobody remembers the number of papers he wrote that turned out later on to be very significant in dynamic meteorology and in numerical weather prediction. I still now and then run across a paper of Rossby's that, it seems to me, people ought to know better.

Phillips: I agree with you.

Cressman: Anyhow, Rossby had already developed these ideas. It must be said in fairness to Jerry Namias that he (Namias) was the first to make use of constant absolute vorticity trajectories and the long wave formula in forecasting. Jerry had the long-range forecast section in Washington, and he worked almost entirely on the five-day mean charts. In those days, the 700 millibar chart was as far up as they thought they could go and be reasonably correct. So all his work was done with the 700 millibar charts, plus surface charts, of course. Jerry was the first one to make an effort to use constant vorticity trajectories in the Rossby long wave

formula in actual forecasting, as far as I know. I never did figure out how he did it because using data at that level will give too much retrograde motion for the long mid-tropospheric waves. Jerry must have had some empirical factors in what he did that I haven't found in the literature.

If I could briefly summarize the forecasting approach developed at Chicago, we were to some extent trying to apply barotropic ideas for forecasting long wave motion, including downstream energy propagation at the speed of the group velocity as well as keeping track of the jet stream and its complications and secular shifts. I think it is fair to say that we clarified the distinction between barotropic and baroclinic processes. We understood at least a simplified idea of an equivalent barotropic atmosphere. We shifted the focus of forecasting to include the whole troposphere, from the fronts at the surface to the jet stream at the upper levels. The next step would have to be a more comprehensive barotropic forecast, which had to wait until Fjørtoft developed his graphical integration method. Certainly, the work Palmén and Newton did on the jet stream in those years deserves special mention.

Phillips: I remember at that time, when I was a graduate student (I was several years behind you), when I was reading, doing my Master's thesis and reading, trying to find out what I should do for a thesis, that there was this confusion or uncertainty about what was a long wave that would be governed by Rossby's formula as opposed to a shorter wave. Rossby--there was a European name for them--"*schnell-laüfer*"("fast runners")--and no one, I think at that time or since, has had a clear separation--definition. Was that a problem at all?
When you take a five-day average, those shorter fast moving waves will disappear.

Cressman: The criteria that I used had to be cold troughs and warm ridges, that is, increasing amplitude with height. Now when you see a trough with temperature and geopotential strongly out of phase, that's likely to be a *schnell-laüfer*.

Phillips: Yes, there was another pamphlet on that, a report from the University of Chicago, called **Forecasting in the Middle Latitudes**, I think, in which there were formulas derived on the basis of temperature advection. . .

Cressman: There was a general circulation report. It was in the June, 1947, **Bulletin**, and I think it was signed "Staff Members, University of Chicago." I don't even know if Rossby put his name in there explicitly. I still have a copy of that at home.

Phillips: Was this the one which first talked about the jetstream?

Cressman: I think it was, because Rossby was very anxious to get this out, to start people

thinking and talking. You know Rossby liked nothing better than a good argument. If he could get somebody to argue against him, he was in heaven for the day.

END OF TAPE 1, SIDE 1

INTERVIEW OF GEORGE CRESSMAN

TAPE 1, SIDE 2

Phillips: This is the second half of tape number one. The original participants are still here. . .we've gotten to the point where George is about to select his thesis topic at the University of Chicago.

Cressman: Well, actually, that wasn't too difficult because Rossby had encouraged me to write things up as we went along. I wrote three papers, more than that, but three of the ones I wrote, I was able to bundle together and use as my thesis. Two were on the forecasting of the long waves in the westerlies; another was on the variation in the structure of the upper westerlies.

Phillips: I remember very much in the catalog at the University for the department it stated specifically that the student was expected to select his own thesis topic. And that was the case as I remember it, and I gather it was the case for you too. You and he happened to be working on the same thing, but you had to decide which of those things--

Cressman: Well, actually, I was overcome with delight when I learned that he was perfectly willing to accept the papers I did anyhow as a thesis.

Phillips: In many parts of Europe, that is what constitutes a thesis. Of course, several papers are published.

Now, you were married at this time. How was it living in Chicago at this time? Did you live close to the University?

Cressman: I lived in the 9600 block of South Merrill Street, between the city garbage dump and the steel mills. You couldn't see either, but you could certainly tell which way the wind was blowing.

Phillips: I lived about two or three miles west of there.

That must have encouraged you to finish your thesis rapidly and move to a more suitable location.

Cressman: No, I was quite happy in Chicago. But there was a little problem in that we already had two children and our third child was on the way. The house in which we lived was completely inadequate for a third child. I decided to do something to live in a better place. Rossby would have kept me on at Chicago

and in fact, I had a free choice to stay in Chicago or to leave. Well, I had gone off on a consultant trip for Sverre Petterssen.

Phillips: This was for the Air Force?

Cressman: Yes. Sverre Petterssen was Director of Scientific Services for the Air Weather Service at the time, and he asked me to come to Washington for two or three weeks to consult at the Global Weather Central at Andrews Field. When I got back, I had an offer from Petterssen to be a full-time consultant in the Scientific Services directorate, to work mostly with the Weather Central but also at other places around the world. He offered me a very generous salary. So in view of the fact that I couldn't see how to continue our personal life in Chicago because of the pending childbirth, complete inadequacy of the housing and other difficulties of life in Chicago, I talked to Rossby and he didn't try to argue with me one way or another. He just said, "Do what you like." So I left Chicago at that point. For me, it was a matter of how to house my family.

Phillips: Yes, housing was very scarce at that time in Chicago.

Cressman: I left Chicago with a great deal of regret because I loved to work there. I thought it was an absolutely unparalleled opportunity to work with the people who were there. I don't think you'll ever find a place where before or since they assembled so much talent under such fantastic leadership, as in those years in Chicago.

Washington: George, was this from 1949 to 1954, the consulting?

Cressman: Yes. They took me on as an official consultant and I worked mostly with the Andrews Weather Central, which was their Global Central, but also was sent pretty much around the world to work with other Weather Centrals and also on individual projects. As part of the individual projects, I must mention one in particular. I believe it was the first atomic bomb test series on U.S. soil since the war. It took place in Nevada, outside of Las Vegas, and I was asked to run the weather support for the first two series. This must have been somewhere around 1951 or '52. They were both conducted on the Nevada Proving Grounds, which you can now find in any map you buy at the drugstore. We had a weather station south of Las Vegas, approximately somewhere between seventy and ninety miles away.
That was pretty exciting work, I must say.

Phillips: What kind of questions were involved? Did you do global wind observations?

Cressman: The first thing was to be able to conduct the test, because the bomb explosions were mostly from air drops, so you had to have a clear view of the ground. None

of this bombing by instruments, not that sort of thing over the continental U.S. There were certain conditions on clouds also because of the monitoring instruments used. And then you had to avoid a fallout mishap. Well, it turns out there are two kinds of fallout mishap possibilities. One is massive fallout from the main explosion. But the other is after the massive fallout, little local currents and mountainous terrain can carry radioactive debris into little hamlets. There were some little tiny hamlets in the desert out there and there were a few mishaps which occurred from just drifting of surface winds around the mountains one way or another. The advice of the meteorologists was not always the controlling factor. There was a certain, what you might call a "desperation factor" that builds up. I'm sure this takes place in the space program, too. And that is, if you have an enormous expensive operation you want to bring off, and it's very critical in many respects, and if you have repeated postponements because of weather, intermixed with postponements for other reasons, after a while the test director is willing to accept weather conditions for the test which would have been unthinkable at the beginning. That's what you call weighing in the desperation factor. This can't be ignored, it's a fact of life. So there were a few mishaps, but nothing really calamitous as far as I know.

Phillips: Did you have local, special observations--

Cressman: Yes, we had quite a few local, special observations and I think in retrospect I made a mistake in not requiring more. There is a little place called St. George, Nevada, I think, which got a bit of a dusting from some radioactivity which was surface drifting sometime after a shot. This reached the newspapers some years ago, but I don't really know much about it. But the force of the bomb made a lasting impression on me. They were exploding bombs, I think, of probably twenty to thirty kilotons yield. A few of them were much smaller. I think there was one of about one quarter kiloton.

The experience of seeing these bombs go off was to me a shattering experience. For example, we ran the weather service for the first series from Nellis Field, at Las Vegas. They were shooting these things off very late at night, in the early morning hours. When the moment came for one of the shots I left the weather station and climbed the airport control tower. Of course, you get spectacular, fantastic visual effects when the bomb goes off, starting with all kinds of illumination of the ionosphere. I wanted to be there when the shock wave arrived. It came, and I thought the glass in the control tower would break. Everything was rattling uncontrollably. The tower itself was rolling and shaking. All this was at a distance of something over 70 miles.

Phillips: These were "smallish" bombs.

Cressman: Yes, quite small. I fortunately didn't have to go on the hydrogen bomb tests in the Pacific. One of the fellows I had worked with for some years, a forecaster, died of cancer after an exposure to radiation in the Pacific from a hydrogen bomb test.

Phillips: What happened when you had your family--did you have your family with you?

Cressman: No. Not in Nevada I didn't.

Phillips: You were on temporary duty from Washington.

Cressman: That's right. I was amazed at the second series in Nevada to run across Athel Spilhaus. He was the test director for at least one shot. It was to be the last one. He might have been the test director for other shots, I couldn't quite get this clarified when I interviewed him. But maybe he, like other people, doesn't remember everything. (His memory is a lot sharper than mine.)

Phillips: (You're doing very well, George.)

Cressman: Anyhow, I used to see Spilly once in a while at the second test series. His shot was an underground shot of comparatively small yield, less than one kiloton. On top of the buried bomb was an enormous rock. It was the size of a house, and I used to admire that rock from time to time as I went past it. Not long before the shot I went in quite late at night to what passed for a mess hall there, and there was nobody there except Spilly, so I went over and ate with him. Before I left, I said, "Spilly, this shot of yours; where is that rock supposed to go?" And he said, "It's supposed to land on that mountain in that direction." Well, I thought, that was very interesting and I'll watch. But of course, when the shot went off, you couldn't see anything because of dust, debris and junk flying in all directions in that huge cloud. Even from a little bomb like that. I saw Spilly after the shot and I said, "Spilly, where did that rock land?" He said, "It vaporized."

Phillips: In 1954, George, the weather services--the Weather Bureau, the Air Force Weather Service, and the Navy Meteorological Service--got together and set up the Joint Numerical Weather Prediction Unit. I think it was about a year in gestation, and I think they had already selected the computer by that time. They very quickly, as I remember, fastened on you as the director which seemed to me, then and now, to have been a wise choice. Can you recollect many of the details of the starting of this venture?

Cressman: Yes. You may recall that Sverre Petterssen was Director of Scientific Services for the Air Force. Dan Rex had a sort of comparable position for the naval weather establishment, and Harry Wexler did for the Weather Bureau. These

were three pretty knowledgeable men, to say the least. Everybody knew what was going on in Princeton. You may recall I was sent up there for six months; Fred Shuman was there. I was an Air Force employee at the time. I think Petterssen had left the Air Weather Service at that time and Bob Fletcher took his place as Director of Scientific Services.

Whoever it was thought it would be a good idea if I would join the Princeton group for awhile. This was all arranged without difficulty. You may recall that Ragnar Fjørtoft published a paper in which he showed how to integrate a quasi-geostrophic two-level model graphically.

Phillips: I kind of remember that.

Cressman: Well, I'll never forget that. Karl Johannessen was working with me at the time, and we thought this would be worthwhile to try out in forecasting. So we worked out the method at the Andrews Weather Central of the Air Force--there was a global weather central there. Bob Fletcher, who was Director of Scientific Services at the time, thought it was interesting enough that it had to be carried to some of the main Weather Centrals overseas. I took it to Weather Centrals in Frankfurt, Germany, and in Tokyo.

This integration by graphical methods was a very interesting business. The graphical arithmetic was done by drawing fields of variables on acetate. Then by overlayering one layer of acetate on another, you could perform addition or subtraction by the way you traced through intersections of lines. Of course, in the end everything came down to arithmetic. Multiplication of a field by a constant was done simply by re-labeling the lines, and you could reduce the operation to these steps. We would draw these lines on acetate, and by shifting and adding or subtracting do a Laplacian. We would do advection simply by carrying a variable along with the space-mean flow. All these operations were done on acetate. You can imagine what kind of a mess it got to be, with a stack of acetates and grease crayons all over. In the end, you got your forecast. But still, it was a geostrophic forecast.

Phillips: Over how large an area were you doing this?

Cressman: Hemispheric.

Cressman: We couldn't get any worthwhile results out of the surface chart from a graphical two-level forecast, the useful results coming at 500 millibars from a barotropic forecast. I found that the forecasters didn't like to do this kind of work, partly because of the mess involved with keeping the various acetates straight and the cleaning job of the acetates, which had to be done with carbon tetrachloride, now

thought to be carcinogenic. So it's a good thing that computers came along because otherwise we would have had difficulty with people not wanting to be forecasters anymore. It was interesting, and I do think one can get a reasonable short-range quasi-geostrophic forecast that way.

Phillips: There was a matter of a time extrapolation which was, as I remember in that system, a little bit more of a guesswork kind of thing.

Cressman: What you do is produce a space-smoothed flow for a basic flow and do your advection in the space-smoothed flow, which had the short wavelengths eliminated. You're dealing with the longer wavelengths, which by their nature are going to be much less mobile. Of course, it was no good for baroclinic developments.

The method itself was such a messy operation that I think as soon as we walked out the door, they quit it.

Phillips: When you became director of JNWP, I can imagine this might not have been a straightforward job because after all there were three services which were supporting you and which you had to support.

Cressman: At the beginning, there was not the slightest problem of that type. We had, you might recall, a steering committee, and on that were Harry Wexler, Dan Rex and I think Bob Fletcher (I think Petterssen was gone by then).

McPherson: George, were you still an Air Force employee at this time?

Cressman: Yes.

McPherson: So you were sort of the Air Force--

Cressman: I was the Air Force person on this, but I was also made Director. At the beginning, I would say that this multi-agency activity was remarkably free from stress from our employers. Some did develop later on, but I think it was a particular personality that was involved.

Phillips: Did you decide on the staffing both initially and later on yourself or in concert with--

Cressman: The decisions were made by what was called the Joint Meteorological Committee. This was composed of the heads of the three weather services, two military and Reichelderfer. And everything as far as I know was quite harmonious at that time with these people. Reichelderfer was much more widely

respected than some people would have you believe. He got along beautifully with these people, and it worked extremely well in those years.

Washington: George, can you say something about the attitude of the Weather Service forecasters at that time?

Cressman: My main familiarity with field weather forecasters at that time was with the Air Force people. There was a lot of interest in the new ideas, but at the same time some forecasters were of two minds. Everybody was in favor of better forecasts, because no forecaster wants to have his name on a poor forecast. There was an undercurrent of regret, as many feared the days of the meteorological maestro were drawing to a close, along with a sort of subliminal fear that the fun was over and forecaster jobs would never be the same again.

McPherson: George, who were at the JNWP unit, which was almost legendary? Who were some of the people that you were working with, the scientists on the staff?

Cressman: Let's see, there were Fred Shuman, Phil Thompson, Bill Hubert and Art Bedient, all making substantial contributions.

McPherson: Art Bedient?

Cressman: Art Bedient was immediately recognized as a real genius in working with computers, so there seemed to be no competition and he was put in charge of the computer operation. Art was one of the cleverest men I've ever worked with. He loved working with computers and he made a lot of innovations to bring performance out of those computers that nobody thought would be in them.

McPherson: He was in the Air Force at that time, is that correct?

Cressman: Right. I think he made Lieutenant Colonel before he left us.

Washington: George, I wonder if you could explain a little bit more of the transition from the graphical techniques to the computer techniques?

Cressman: It wasn't much of a problem, actually. The graphical techniques died rather quickly. I don't think anything serious was done along these lines in the Weather Bureau, and I am almost sure that when Karl Johannessen and I left the Air Force Weather Center rigor mortis set in very quickly on the graphical operations.

Washington: But there was a meeting or something of that sort which led to going to the computer--

Cressman: Yes, there was the Joint Meteorological Committee. I believe I mentioned who was on it at the time--Reichelderfer was the chairman--and they jointly decided overall what should be done and when and how the costs would be divided. They worked together rather harmoniously. General Moorman was a really extraordinary man who would have been much wider known in the profession if he hadn't been in the Air Force. He was a man of temperament and ability comparable to that of Reichelderfer. The Navy changed people often but the Navy nevertheless through those years worked harmoniously with us. There was, as far as I was aware, no significant bickering, and cooperation in every direction.

McPherson: How was it that Reichelderfer and Moorman--how were they persuaded that numerical weather prediction was a thing to be investigated?

Cressman: That's not so difficult to explain. Reichelderfer relied on Wexler and Wexler in turn relied on Smagorinsky, and Charney had a pipeline to anybody he wanted. In the Air Force, my recommendations carried a lot of weight. And the same in the Navy for Dan Rex; I think he was a lieutenant commander at the time. Dan Rex was a rather extraordinary fellow, a very knowledgeable and a very smooth operator. He could get along with the devil himself. He was very, very good. But he also had very good judgement. So he was, as far as I could see, the chief advisor to the Navy at the time.

So it was a winner to have these people advised by Wexler and Dan Rex and to some extent by the Scientific Services people in the Weather Service, which included me.

Phillips: Maybe what Ron is referring to is the fact that the first paper reporting success with the barotropic prediction was the Charney, Fjørtoft, von Neumann paper in 1949, and the JNWP unit was set up in 1954. One can understand why nothing was done immediately in 1950, say...

Cressman: You know as well as I do, Norm--and I know you're asking this question rhetorically--but the first experiment which was done on the proving ground computer was really a feasibility test. They used a mesh length of 763 kilometers. What can you do with a mesh length like that? You can go through the arithmetic and show that nothing blows up in the course of the forecast, and that's almost a miracle, with that kind of a mesh length and the dimensions of the grid. When you get that result, "Look, it **ran**," it still has a recognizable pattern. That was a stunning success.

Phillips: It was probably good that the computer was limited because if it had been much more powerful, you would have had hemisphere and you would have had the

waves moving westward.

Cressman: Then you're in deep trouble already! It's lucky that we had the time to get used to that. You know the first model was really called the Charney-Phillips three-level quasi-geostrophic model. Fortunately, that also had rather limited areal extent.

Phillips: Now was there much pressure on the unit in its first days to get operational immediately or almost immediately?

Cressman: I think we all felt the pressure although we were not harassed by the respective services. [They] showed absolutely remarkable tolerance for getting this underway, and nobody thought this was going to happen in a month. One of the things we tried to do was keep the whole profession involved by a series of papers. We wanted to use the **Monthly Weather Review** as a vehicle for this. I'm sure you remember that...There were pieces in the **Monthly Weather Review** which were mostly exposing and diagnosing problems that were unsolved. So in a way I think we kept as much of the profession as possible involved. It was an attitudinal problem that we had to cope with immediately, and I think that was the way to do it.

Phillips: I know that you've been emphasizing the serenity and excellence of the working arrangement between the three services. Nonetheless, in only a year or two, we find it being separated.

Cressman: It was longer than a year or two. The Navy wanted to establish its own operation later on. This was pretty much at the urging of Paul Wolff, who was a member of our staff. The Navy had a lot of things that they wanted to do that didn't have a very high priority in a jointly-operated unit. We were concentrating on the solution of the dynamics in a reasonable and useful way, whereas the Navy was quite urgent in getting some applications, and this was also I think partly due to the personality of Paul Wolff, who was on our staff at the time. When the Navy peeled off in about 1958, Reichelderfer was able to get full funding from the Weather Bureau budget for the whole operation and the Weather Bureau took it over. That was something that should have been done in the first place if it were possible; it just wasn't in the cards then. They needed the support from the other services. So the Navy did establish eventually--I can't remember the year--a facility which they wanted to use for a lot of naval operation purposes. In my opinion, it was premature, but on the other hand, if they wanted to do it, they wanted to do it. In the meantime, life would continue reasonably peacefully and we didn't suffer any lack of progress because of it.

McPherson: You had mentioned earlier that Dan Rex, a remarkable person, was the advisor.

Was he supportive of this separation?

Cressman: Dan Rex had disappeared from the scene by that time, and I don't know what happened to him. I would have known at the time, but I don't remember now. Dan Rex was, among his other attributes, one extraordinarily good politician, a very smooth operator and very intelligent person besides.

Phillips: We had NMC itself established as part of the Weather Bureau at that time, in 1955, or thereabouts, and you were the director of that until 1964. Can you discuss the main highlights of that period for you, from a _____ point of view?

Cressman: First of all, taking over by the Weather Bureau I viewed as a coup d'etat by Reichelderfer. Somehow, he worked a miracle and under John Rooney as his Congressional budget supervisor, he got the funds to take it over.

Phillips: Rooney was the Congressman.

Cressman: He was the Congressman from Brooklyn who oversaw the Department of Commerce budget for many years. Without mentioning any names, I can say that I was at a hearing at which Rooney presided, a hearing on the Weather Bureau budget. And in watching the defender of our budget, I would say that Rooney was like a snake casting its eyes on a mouse--this was supposed to make the mouse paralyzed. He had a really extraordinary personality. Thank God he was one of a kind.

Phillips: There were eight or nine years in which you oversaw a lot of technical and personnel developments...In addition to solving the barotropic equations on a computer sometime in this period, NMC took over the responsibility for data exchange.

Cressman: We absorbed the old NAWAC--National Weather Analysis Center. This was done by Reichelderfer probably with the advice of Harry Wexler. That was done in 1958 or so, as I recall. But Reichelderfer did it, in any case. I think it might not have been too difficult. He had to go to the Congress with agreement from the other agencies. That must have been how it worked; it wouldn't work any other way. The other agencies probably took some cuts or at least assigned some of the cuts they were going to get anyhow to this transfer of responsibility. I say this without knowing anything about it, but that's what I would surmise. It seemed to happen very peacefully, and I thought it was the thing to do; it should have been done earlier, but that's O.K. It was fine when it was done.

Phillips: What you seem to be telling me and my learning is that this was a gradual

process rather than a sudden process...

Cressman: There was a date at which the Weather Bureau took it over, and I'm not aware whether or not there was any serious infighting on this...

Phillips: From the point of view of technical problems, was there any?

Cressman: No. I wouldn't say so. We still kept people from military services with us, and I think some of them were detailed and some of them retired. Art Bedient retired from the Air Force and he stayed with us. Phil Thompson moved on, but he might have anyhow. Dan Rex was not the kind of a person to stay in--he didn't work for us, he was on a committee that oversaw us and eventually the Navy people were all moved out to Monterrey to establish the Navy center that we discussed briefly. No serious problems.

I don't believe Art left us for long. Do you remember, Jim?

Howcroft: He had a tour in Hawaii, then he came back.

Cressman: Now I would say that was a serious temporary loss. But we survived it and Art came back.

Howcroft: And immediately started innovating...

Cressman: That's right.

Phillips: On the opposite end, how about the distribution of forecasts? There was no mechanism set up to distribute numerical forecasts originally; how was this first approached?

Cressman: When we all agreed the time was ready, we began to put the barotropic forecasts on the facsimile circuits. When we thought we had something ready, it was my responsibility to take it to a committee that oversaw such things and explain what we wanted to do and why we wanted to do it and give the background for it. I can't remember any stress at all with that. In effect, we decided what was ready when it was ready.

McPherson: What year, roughly, did you have available to facsimile service?

Cressman: We had it available when we started in 1955. Facsimile was already going.

McPherson: I recall though when I entered the Weather Bureau in 1959, that facsimile in small Weather Bureau stations was a new thing.

Cressman: That's right. As a matter of fact, it was still not at all small Weather Bureau stations when I was Director. I had to find money to put it in the last survivors of the smaller stations.

Howcroft: The ones in the Air Force had facsimile first.

Washington: At the time you started putting the numerical forecast on the facsimile, it was generally regarded to be better than what the experienced forecaster could do.

Cressman: We didn't make that claim...as far as I recall. What we said to them in effect was here's some pretty good stuff and we recommend that you pay attention to it.

McPherson: And the forecasters were part of your organization at that time?

Cressman: No. At that time, we were still an independent unit, and one of the things we wanted to do was to get some reasonably realistic product on facsimile at a very early date so people would feel we were paying our way. If the heads of services went forward on our budget requests, they wouldn't have to say, "Well, this is coming along fine but we don't have anything to show you yet." We wanted to have products in the field. So you might say there were two reasons for it: one was educational, and the other was political.

McPherson: So the unit was actually transmitting numerical forecasts while the unit was still an entity before it split?

Cressman: Yes.

Phillips: Let's go on then to another question. The first model put out was a barotropic model. Am I correct in remembering that before you left your stint as director of NMC, that the center had progressed beyond that?

Cressman: Yes. We had issued two-level forecasts for a while, but they were a disappointment. You know they had a term involving the thermal wind and its vorticity. It was thought of as a baroclinic term, but it didn't do us any good. When a baroclinic event occurs in nature, the simple idea of a horizontal non-divergent surface in mid-troposphere doesn't apply. In a real baroclinic development there is a substantial re-arrangement of the mid-tropospheric (and vertical mean) flow. One needs, as a minimum, three levels in the vertical or you can't change the barotropic behavior of the model's forecast in mid-troposphere. I published a critique of this matter in 1961. The best results we got for some years came from a stream-function barotropic model. It had two advantages: first of all, it was relatively simple and didn't misbehave, and secondly, it was cheap so you could include a very large area in your forecast

and extend the time range of the forecast. The advantages of fast running together with large area and a reasonably decent performance of the product were all in its favor.

END OF TAPE 1, SIDE 2

Interview of George Cressman

TAPE 2, SIDE 1

Phillips: How long were the forecasts made, what period are you speaking about?

Cressman: I don't know when we went to 72 hours; we did, first, 36 hours, then 48, then 72. We just turned it out as we were able to get data from a larger and larger area, and I think when we were finally able to cope with hemispheric data, at the time we were doing Northern Hemisphere charts--that was probably around 1958.

McPherson: Your famous paper on the analysis was in fact a Northern Hemisphere paper.

Cressman: There was a problem of the retrogression of very long atmospheric waves over a large area and Bert Bolin first had a clue as to what to do about this. I followed it up with a little bit more, say, at least calibration. I published a paper on that in 1959. So in effect that stabilized the long waves--it kept them from running amuck as they would have done without this term. We were able to process the data automatically in 1958. We had objective analysis all the time so that by 1958, we were able with automatic data processing of the data from the circuits together with a reasonably fast objective analysis and a hemispheric-barotropic to produce something which was clearly a step forward and of considerable value.

Phillips: One of the things that has always impressed me as an extremely important step made by NMC--that people tend to forget about--was the gathering and the inclusion of aircraft when producing analysis. Am I correct in that matter?

Cressman: Yes, we did that...

Phillips: After all, you have to get the guys who are out in the planes to want to get their reports back into Washington.

Cressman: First of all, there always were some aircraft reports available on one circuit or another. We went to considerable trouble to try to make sure we got the circuits to carry the majority of what was available.
Secondly, there was a question of quality control and just the general processing--how you cope with data at a single level in the forecast. Well, none of them were too difficult. In making these forecasts available, the aeronautical industry got hold of them--I forget exactly how this was done--but they were tremendously enthusiastic, particularly because our best products in the late fifties were the wind and temperature distributions between 500 and 200

millibars. And that's where all the jet traffic was flying. You remember that jet aviation didn't appear all of a sudden in 1946; it came in gradually. So the aeronautical industry proved tremendously cooperative in improving their own pilot reports and in getting better distribution of them. We had some other help, but I think it was mainly the aeronautical industry that really shaped up fast because they saw in this a real advantage. So this expanded to an enormous extent and today I'm sure you could look around NMC and see a vast number of aircraft reports. All along they worked very, very well with us in arranging for better collection and better communications.

Phillips: Do you remember the name of any group that was particularly helpful?

Cressman: We had to work through the U.S. Air Transport Association. There were international conferences on this too. I remember going to Paris for one because I felt that part of my role had to be as a propagandist, just for self-interest. We were the most successful in terms of benefit to cost for the aviation people. That may still be the case...

Eventually, we got to pretty much complete automation of the whole operation with the aviation industry.

Washington: George, since we're talking about technology, could you say just a little bit about when computers were introduced, even going back to JNWP through the early years?

Cressman: I may need some help on this, but let's try. When we established the Joint Numerical Weather Prediction Unit we ordered an IBM 701, which had an electrostatic memory among other things. It had a pretty large drum; the drum was where the data were really held...the data were slipped into the electrostatic memory for awhile and we would hope nothing went wrong and we would get it back on the drum. Initially, we had to do everything twice; all the calculations were repeated. And then we just compared the check sums. If they were equal, fine, go ahead. If not, go back and do something over.

You were with us in those days, Jim.

McPherson: I remember the check sums very well.

Cressman: The check sum field was a standard feature, particularly dealing with electrostatic memories, which were rather volatile.

McPherson: This lasted for a long time, the particular checking of calculations with the check sum.

Washington: I just wanted to know where you started using baroclinic models.

Cressman: We immediately issued three-level forecasts, I believe, and then we soon learned that this was not a winner. We came back with barotropic forecasts. Even with the barotropic forecasts, we had difficulties because of the quasi-geostrophic approximation; we quickly got out of that into a non-divergent barotropic forecast. There was only one difficulty left with that, and that was the very large-scale retrogression. So we stabilized that and then we had a real winner. That was a good forecast for that time, very useful--because a lot of forecasters were keying on the 500-millibar forecasts anyhow. Even with the very old-fashioned forecast schemes, most people were aware of say, steering, where certain things in the low levels were steered, so to speak, by high-level flow, and this wasn't too useful when the high-level flow kept changing. So you were steering with an uncertain wheel. But once we got the high-level flow, it simply was a really good large-scale barotropic forecast: we had a winner. That gave us a little time to work harder on the baroclinic forecast.

Phillips: Did you want to--were you hoping to hear some information about this progression to more mass computers with time?

Washington: Well, I just wanted to see if the critical thing that slowed down progress was not having a large enough computer, or maximum computer.

Cressman: We always thought so. But I'm not sure that was right. We had to learn some lessons first. One was to get out of the quasi-geostrophic models as fast as possible. Secondly, to enlarge the area of forecast domain as large as possible. We worked very hard getting into a hemispheric domain as fast as we could. At that point, I would say we were at the proper starting point, but there was no way, I think, we could have realized that at the beginning. Nobody was telling us to do this and nobody was even thinking of it. As a matter of fact, I think, by and large, the barotropic forecast had a bad name everyplace before it was available. People said, "My God--you can't forget the thermal field in the atmosphere." But it wasn't a question of forgetting the thermal field, it was a question of forgetting the out-of-phase between the thermal field and the wind field.

Then we had a lot of other things we had to work out. We had to work out automatic data processing, we had to go to hemispheric extent minimum coverage, and all these things had to be learned.

Phillips: Was there ever anytime when you thought you had a very good case for buying the new state-of-the-art computer, but were frustrated for more than a year, say

by financial difficulties?

Cressman: Not that I can recall. I believe it's accurate to say that whomever we were working for was fully supportive and we also realized that we couldn't ask for the moon. On the other hand, there was always some limitation imposed on us by means of computer limitations. But I think I wouldn't have done it much differently if I had to do it over again because there were a lot of things that had to be worked out. For example, a simple problem of automatic processing of the teletype data without people being in between the data circuits and the computer. You've got to get the man out of the process here. For example, there was no indication at the beginning or end of a report. What you do is to establish a stencil, so to speak. When you find a fit you know what you're doing, and you then inquire further down the report to see if it really is all right. Then eventually you get around to a hydrostatic check of the report. If everything passes, you're in business with that report. At the time, there was practically no discipline on the international telecommunication circuits. You would get non-meteorological traffic on them by mistake. You would get remarks in so-called plain language of whoever happened to be the originator.

Phillips: This was because other countries were still doing this work manually, with people?

Cressman: Everybody else was. This situation was quite intolerable and if you want a few good laughs, you could look up the paper Art Bedient and I wrote, called **Experiment in Automatic Data Processing**, October, 1957. This is worth a few laughs when I go back and look and see the condition things were in. But this was really not acceptable. At that point, I took a personal interest in the World Meteorological Organization. I got myself nominated to the codes committee of the Commission on Scientific Meteorology. Fortunately we had some people who came on at the same time who were very open-minded and very good, and I think that the initial work of that codes committee really was an earthquake in international meteorological communications.

Washington: Now that was aimed at trying to standardize things?

Cressman: Yes. Trying to standardize and regularize the exchange of data by codes.

McPherson: Who were some of those people, George, from the other countries, do you remember?

Cressman: We had a gentleman from France, M. Pone, who was very good. We had an excellent man from the USSR, but I can't remember his name now...anyhow, I think that was something we had to do, and I think in working through the

WMO, that we generated so much interest in the problem that we got pretty good results.

Phillips: Let me leave then the question of sources of computers and return to meteorology...

Howcroft: Let me say something about computers, from the trenches. We never have enough computers. We used to store fields in the deck of instruction just to try and stretch the core a bit.

Cressman: We had some very clever programmers, didn't we?

Howcroft: Extremely. But we never, despite what George said, from the trenches' point of view, had enough computers.

We would complain to Cressman about the fact [of] the slow turnaround and he would remark that really he had no problem with turnaround, he just handed this thing to the operator and--He couldn't quite understand our problem.

Cressman: Nevertheless, you did rather well, I think...

I really think that we had more clever programming in our work than you'd find any other place. That may be a biased view.

Phillips: This is a good day with that hurricane crossing Miami to ask you what you remember about the experience with forecasting hurricanes. This must have been a difficult question to face, as director of NMC.

Cressman: We had, first, a simple advective hurricane model. It was a non-interactive point that we'd designate and advect it with either the vertically mean flow or the 500 millibar flow. We got a hurricane forecast out of that. It was probably a better forecast than what was otherwise available for periods of over twenty-four hours. I don't think we established any interactive high-resolution, low-resolution models for years.

McPherson: I think the first time you did that, you were director of the Weather Service; that was probably in the early seventies.

Cressman: That was probably so.

McPherson: You authorized the establishment of the unit that turned out to be headed by John Hovermale.

Cressman: I do remember that back in pre-NWP days I did a study on hurricane forecasting from the following standpoint. You may remember a paper by Rossby on the movement of vortices in large-scale flow. His point was that an areal integral of the Coriolis and pressure forces over a given area was non-zero due to the variation of the Coriolis force with latitude. This leaves a resultant unbalanced poleward force on a cyclonic circulation. I became interested in this in 1950 as a result of an embarrassment to the Air Force Weather Service regarding a typhoon in the Pacific. I was able to show that this so-called Rossby force was of such a dimension that it had to be considered in forecasting motion of tropical cyclones. It depended on the radial velocity profile of a circular hurricane, which I analyzed under certain conditions.

This in turn came from a remarkable experience I had as a forecaster at Homestead Air Force Base. We had a hurricane to the south of Florida--Homestead is south of Miami--and down in the Caribbean there was a really serious hurricane. There was no very well-established steering current. All we tropical forecasters would look for steering currents, but there was no well-established steering current that I could figure out. In the meantime, the hurricane was growing and growing and growing, and it got pretty big. .It got really to be a large hurricane. And then it began to migrate slowly toward the north. The first thing we thought [was] that it was probably not significant; we would keep a close eye on it. We kept a close eye on it and it began to accelerate slowly toward the north. We had no idea what was going on, no idea how to make a forecast. The base commander there had the responsibility for all this hardware on the base, and he didn't know what to do. We were, let's say, ducking the issue as far as we thought we could get away with it. So finally he invited two of us to lunch. So we had a nice little lunch with the base commander. Finally he said at the conclusion of the lunch, "Now, I don't want any technical words. All I need from you guys is to tell me when the wind's going to exceed 45 knots at this airbase." I had a completely helpless feeling. I looked at the other fellow--his name was Taylor--and he looked at me and then the colonel looked at me. They were both looking at me. I crossed both fingers and said, "Tomorrow about this time." He said, "OK, we evacuate this afternoon."

I only missed it by an hour. It was a sheer utter uninformed guess. I never forgot that, so when I was in the Pacific, I saw this typhoon that was behaving rather obnoxiously and going north when it shouldn't have been going north. So I analyzed the problem from the standpoint of the radial velocity profile of a circular storm and saw that with a certain achievable velocity profile, there was a very powerful force for hurricane movement. Today's hurricane is like the second hurricane we had when I was at Homestead.

Phillips: [Now] we are going to enter on George's memories of what he experienced as director of the National Weather Service, but there are a few questions that some of us had relative to the National Meteorological Center.

McPherson: George, I wanted to hear your reminiscences about the development of the famous analysis system that bears your name and is still operational here at the Center after thirty-five years.

Cressman: The initial attempt along those lines, was due to Bergthorssen and Döös, who instituted a method of correction. I'm not sure whether or not they applied their method to winds, but in any case the main difference between what they did and what I did, as far as I recall, is the successive approximations as you lowered the scale. In other words, the biggest errors should be corrected first and then you gradually work your way down to the smallest errors, and you can take as much time as you like and correct errors as much as you like but at some point it doesn't pay to bother anymore.

Phillips: By being small, you mean in scale size?

Cressman: Yes. You know we had that problem with the geostrophic approximation that tended to give very large-scale errors coming in from the boundaries to some extent. And then we had other errors of a large scale, which were due to anomalous retrogression of the longest waves. So the system we had to have must be capable of handling these very large-scale errors, but since you use a previous forecast for first guess each time, these things can build up in an astonishingly short time and first thing you know you had what we used to call a "blow-up."

McPherson: Were other people involved in the development of the analysis system?

Cressman: Well, not directly. I think I did that pretty much on my own. The other people were the Scandinavians, mainly, and in particular, Bo Döös.

McPherson: And did you have contact with Döös?

Cressman: Yes.

Phillips: George, I remember John Brown told me when I asked him what his first work for the system here was when he was in the Air Force and he was reading off and maybe analyzing, but certainly reading off the gridpoint values of 500 millibar analyses. It was the first way to get the analysis in, to get the data in the computer. Did you go directly from that to the Cressman analysis system?

Cressman: No. There were two intermediate steps. First, we used for a while the method of fitting quadratic surfaces around grid points. That was slow and somewhat hard to control. That may be what brought me to the successive correction method, because we did have to do something about the large-scale errors which emanated from the boundaries. I'm sure you remember that sort of problem. A lot of these things seem to have originated at the boundaries.

McPherson: One other question I would like to ask George about your days at NMC: you mentioned that when NMC formed, it was a merger of the Joint Weather Prediction Unit and the old NAWAC, the National Weather Analysis Center. And it must have been shortly after that, or maybe in that process, that this concept of the man-machine mix arose. Someone must have had a hand in trying to, through that philosophy, persuade the forecasters that numerical prediction was a good and useful thing to do.

Cressman: I don't think that was a motivation in promoting the man-machine mix. The fact was we needed help in some areas and it's hard to appreciate now how rotten was the system of communications and data checking in those days. There were almost always data problems, like some gremlin was floating around the world with his tribe of ten thousand descendants and trying to confuse people who were doing a hemispheric analysis. There were innumerable problems of that type, so we had to get on top of the data problems. The first thing we had, I think we [would] print a very quick analysis on which was displayed the critical data and we had somebody go over that. That was first done by an analyst. That was one of the things that slowed down the whole production and also didn't work too well so it became a matter of considerable priority to get on top of this by program methods.

In those days, the encoding and communication of worldwide data was completely undisciplined. They had all kinds of plain language remarks in whatever happened to be the language of the originator; code forms were not at all scrupulously followed; originating offices were not identified in many cases. There was supposed to be an identification, but there was no circuit discipline, so anything could happen. The challenge was to squeeze all the information out of the legitimate data and not lose a lot of reports. Then, secondly, in the legitimate data there were all kinds of mishaps and errors. So then we had to try to squeeze the errors out of what we had identified as probably legitimate data. This was a most aggravating business, so that was really the reason I volunteered to get on the WMO-CSM Code Group. That was the only way to lean on them; that was the way the machinery worked--you could lean on them and get something done. Of course, making passionate speeches with really appalling examples at WMO meetings helped a lot. Nobody likes to be terribly embarrassed by what his own country is doing.

Howcroft: I have a question about the NWP. At the time I came here, you were working on your four-level version of the derived equations, using the derived equations and Shuman at the same time was working on his PE [primitive equation]. When was the decision made to go PE and how was it made?

Cressman: Fred and I were sort of on parallel paths at this time. We both had problems, which when formulated, were too much for our own machine, but it turned out that the Bureau of Standards either kept up with or had a machine one step ahead of us. They didn't use it very intensively; it was always possible to get plenty of time at night. So Fred and I were going up to the Standard's several nights a week and we took along a machine operator with us and we had free run of the place. Well, I wouldn't say free, I don't remember how much they charged us, but nobody was interested in interfering with us or competing with us for time. Fred was checking out a primitive equation model, and I was checking out a four-level filtered model in which the balance equation represented the wind law for the model. Now the way to do this, of course, is to carry your history in the stream function so when you go through the balance equation, you only go the linear way and not the non-linear way. You do the non-linear way at the beginning and after that you're home free, practically. Because after that all you've got to solve is linear equations. So I checked that out and it ran just beautifully. But even at that stage I couldn't get it on our own machine. So I thought, "Well, since there's still time, I'll put in latent heat," which didn't look to be too much of a trick at that point. I was checking that out when Fred had a glorious success with the primitive equations, so I just dropped it. I thought it was ridiculous to have competition on this because, given a choice, I'd pick the primitive equations every time.

So that's how it worked out. Since then, I'm proud to say that I've programmed and successfully run a primitive equation model myself. I did that a few years ago. It worked fine the first time around.

I just didn't want to retire without running a primitive equations model. But it was easy. And who was it who said this: It was Hinkelmann. One time I asked Hinkelmann why he was so intent on getting a primitive equations model going, and he answered me: "It's because I'm lazy." And what he meant was it's so much easier to program it, once you've established the stable, finite differences. Hinkelmann became the director of the German Meteorological Service, and his colleague, Reiser, is now director of the Service, I believe, or at least has just retired from it--one or the other. So it shows if you were in on early numerical prediction, you got to be director of a weather service.

Phillips: Is that a reward or a punishment?

Cressman: I've often wondered. It's a duty, Norm.

Phillips: Around 1964-65, George, there was a fair amount of re-shuffling of government bureaus concerned with atmospheres, oceans and space and in particular, the Weather Bureau. Robert White came in, I believe, as the chief of ESSA, and the Weather Bureau was combined with--

McPherson: I think that Dr. White replaced Reichelderfer as chief of the Weather Bureau and immediately proceeded to organize ESSA.

Cressman: If I may interrupt at this point briefly, a hidden factor in this that we haven't mentioned is J. Herbert Holloman. I don't know to whom I should attribute this notion of this re-organization.

Phillips: Do I understand that he was the scientific advisor--

Cressman: He was the Assistant Secretary for Science and Technology in the Department of Commerce, under which he had only the Weather Bureau and the National Bureau of Standards, I believe. Herbie Holloman was not a fellow to sit still and let many things walk past him.

He came up in collaboration with possibly Bob White, among others, with this idea of the re-organization and formation of ESSA. I can see the rationale for it and I suppose it made a great deal of sense in many respects. However, I didn't feel that it did much for the Weather Bureau. Jack Townsend once remarked to me, many years later, that the formation of ESSA was bad news for the Weather Bureau. I think Bob White believed in what he was doing, I think he still believes that it was right. We just have a difference of opinion on that.

Later on, it became not so relevant because they kept organizing themselves into grander and grander things. You know, ESSA became NOAA and eventually the Weather Service got back pretty much to where it was, organizationally-speaking.

McPherson: But at that time in the old Weather Bureau, what eventually became the Environmental Research Laboratories were part of the Weather Bureau...actually research and operations were part of the same organization.

Cressman: I'll say this in Bob White's favor, that I believe that the research activities got a lot better support once they were put in a separate part of the organization. However, in turn, I think that might have lessened somewhat the coordination

between research and operations. But I think, on both sides, we made a very conscious effort to cure that problem.

Washington: Now about that time, GFDL was formed--

Cressman: GFDL was formed at least ten years before that, I think. Not as GFDL, but it was called the General Circulation Project.

Washington: It was part of the Weather Service at that time.

Cressman: It was part of the Weather Bureau, formed under [it] when Reichelderfer was chief. Some people have thought I didn't think that was a good idea. I'm convinced it was a good idea, it should have been done. I think that what galled me was they got an upgrade on their machine well before we did, every time, it seemed.

Phillips: As the director of the National Weather Service, successor to several very honorable and capable people--Reichelderfer, Bob White and many others before that--what do you remember as your most difficult problems and your most heartwarming successes, and maybe even a failure or two...?

Cressman: Well, if we had a failure, it wasn't a failure--we were still working on the problem.

When I took over the Weather Bureau, there was one problem immediately needing attention and that was there was really rotten morale at the Weather Bureau. I think this was mainly due an external influence. The Federal Aviation Agency had, by its charter, also done a lot of things--they have cooperated with the Weather Bureau and Weather Service in taking observations in locations where they are and we aren't. They also brief pilots. So the kinds of work that they do in the field stations are very similar and in some cases overlapping completely. This doesn't mean there's waste; on the contrary, this is increased efficiency for the government. This coordination was arranged long before I even thought about it and it was proper and should have been done.
There was a catch, though. They got better grades than our people did for doing the same work in the field stations. So here you are out at Podunk, Nebraska, and you've got an office and here's the FAA and right out the door, maybe in the same office as the Weather Service and these are technicians doing almost interchangeable work. Our people brief pilots some places, their people brief pilots some places. Their people take observations some places, so do ours. There was no duplication, but they were doing the same kinds of work. The difficulty was the FAA got one grade better for their people.

Phillips: This is like the present argument in society as a whole about women getting paid less than men. The same work for jobs that have equal demand. It's that kind of thing you're pointing out.

Cressman: We didn't have any strikes, but on the other hand, our own technicians picketed Bob White when he went on a trip to New York. This was a really disgraceful affair and so when I was appointed director of the Weather Service, this was a very hot ticket item but cured only gradually. The first thing I did was I asked for and received invitations to address their union conventions. I tried to take every route to inform them that the management was on the same side as they were, it was a grave problem, we were going to get it fixed if we could. The other problem was that some of them were working in really poor facilities--we were going to fix up the facilities. There was also a problem that there was a mixture of duties. In some places in our own organization, some people were getting professional grades for doing work, and other people were getting sub-professional grades for doing the same work. I'm not saying this in criticism of Reichelderfer--he dealt with things in a different age and different situation and I've always admired him very much. How much of this happened in the interval between Reichelderfer and me I don't know, but that was the situation I found. So I had to devote a lot of time to personnel problems, sorting out the professional from sub-professional, giving the sub-professional some dignity, and not subjecting him to equal work for un-equal pay. This all took time.

Furthermore, the FAA had a good school where they sent their employees to learn what they were supposed to know. I succeeded in establishing a new training center at Kansas City, and they had pretty much whatever they asked for in the way of funds to fix up the center and for staff to do the teaching. We expanded it to include meteorological technicians at large and we got a nice new building and they could get all the furniture they wanted in a nice-looking place. We had one really hilarious problem. The technicians were used to going to our old maintenance school and they would draw per diem and then they'd rent two cots in Mrs. Murphy's basement and sleep down there for fifty cents a night. They were making a ripoff on their per diem, so what we did instead was to provide housing for them and they had to stay in it. So at least this cut off the ripoff they were making from the government. They forgave us for that after a while. I'm convinced that they planted bugs in the motel room I had when I stayed there once.

The grades [as compared to the FAA] improved, I wouldn't say equalized, but improved. We had a really good training center. Training centers had an up-and-down history depending on what was the need for them at the time. I understand now that they're way up again, with big training going on there now.

Phillips: At Kansas City?

McPherson: At Kansas City, well, several different places but in particular at Kansas City.

Cressman: Our problem was with the meteorological technicians as far as I could see right away. Here and there was always some little nagging problem. But we asked our technicians to do a lot of things that were not so easy, for example, at some of the early remote stations in Alaska. I understand that at Summit--Summit was a place between Anchorage and Fairbanks--we had assigned some met techs in there along with some FAA people, which included one or more Alaskan natives who were perpetually drunk. And once you get in there, you can't get out all winter. The snow gets twenty feet high. So without saying we had all the problems solved, I think we made a lot of progress and it got to be where my appearance at an all-union conference was more boring than anything else and those problems really went away as far as I could tell. But the met technicians were really in an uproar when I came in.

Phillips: There must have been, I'm sure, during this period, times in which there were pressures from Congress about station locations and things like that, is that true?

Cressman: Oh, yeah, but I wouldn't regard them as serious problems. Rooney had a longer tenure in Congress than most of us would approve of. Outside of that, I would say our actual relations with the House and the Senate were of sterling quality.

McPherson: And how were your relations with the Department of Commerce in those days?

Cressman: Well, I will say this for the ESSA-NOAA idea. They kept us somewhat removed from Commerce. Our contacts there were pretty much limited to Herb Holloman, our Assistant Secretary of Commerce.

Phillips: I associate him with large ambitions for weather modification.

Cressman: I wasn't very much vexed by that. Holloman was interested in a wide variety of different things and he was a very energetic individual. They say that the ancient German army classification of officers says that the incompetent but energetic officer is the worst of four categories. I wouldn't call Holloman incompetent, but he had a very wide range of interests, and he didn't have a whole lot of time to specialize in any particular question that I knew about.

Phillips: Were there any kinds of severe financial problems where you had to, for example, consider large-scale risks?

Cressman: In principle, yes, but in practice it wasn't all that bad. And I'll tell you why. The

Weather Service, with its stations, has representation around the country. The meteorologists and officials in charge of our stations understand very well that their duty includes first attention to the issues in their area of responsibility. And most of them, if not all, are very careful to cultivate the leading figures in the towns where they are and they do a beautiful job of it, at least they used to, as far as I could see. I can think of very few exceptions.

As a consequence, say when a proposed reduction in force comes along, coming down the line from the Office of Management and Budget, when we were going to change something in the field station, the first thing we have to do is notify the local people in Congress, the senators and the Congressperson--

Phillips: You mean "to have to" as a matter of tactics.

Cressman: No, we have to, as a government official, we have to go and tell them that, say, we're going to close Station X because of budgetary priorities or we have to make a change in the functions of Station X and add to its responsibility or something like this. Because Congresspeople really don't like surprises from government bureaucrats. They detest surprises and they can kill you if you give them half a chance. So the thing is you have to keep ahead of this game, and when you're going to do something, you have to tell them first and they appreciate that very much.

The second thing is they don't like to be left out of the loop in another way: if a government official reasonably high up comes to some area and pays a visit to a facility, the press will probably show up at this, and the next thing you do the Congressperson is reading in the paper that "XXX" from the Weather Service visited and made this and that statement about future services in the area. They'll kill you for that--they have a right to. So what you have to do is first go see the Congressman, you explain the situation, and you explain you're going to visit his town and you offer him an opportunity to show up and be assured that the press will probably be there.

It works like a charm. So then if I had a real problem, a nasty problem, I'd go down and see the Congressman. He used to give us hell in Congress, you know, but in the meantime he's got some beautiful press coverage in our forecast office. I'm not going to mention one because there have been a number of them. So I go and I say, "Look. We've got this real problem and I need some assistance and I need to consult with you about what we're going to do about this station in your district." So then he'll call in a couple of aides and we'll sit there and I'll explain the problem. All I have to do is keep them informed of anything before it happens because they really don't like surprises. This is what kills their tenure. That is what most Congresspeople are for: to represent small districts. There's a

little station in Nebraska, I don't know if it's still there or not, but it was not too long ago. It was called Valentine, Nebraska. That was a one-person station, a lady MIC in there for a long time. It doesn't look like much of a station and it stands out in a balance book, you know. Budget people always had fits over it. But this lady was there and she would furnish a really first-class service in this remote state with severe weather problems. She would work with the local people. She had her networks all set up and if there was going to be a storm, she would have everybody alerted and people who had their cattle out in some God-forsaken place would see to it that they got them to shelter. Those little people are representatives of Washington, as far as these local people are concerned. That's what really counts to them. Don't tell them about budget deficits or social problems, they want to know if it's going to snow so they can get their cattle in. If they sustain loss of their cattle, you know it's really bad news. They're broke probably, have to sell the farm.

And so the local meteorologist in charge, or official in charge, has got to keep his eye on the local pulse. That's what he's there for, to provide local service. We could put a machine in to take the observations, but we can't put a machine in to do local service. They want to call up a representative of the government and they want to get an answer. And they're entitled to, they paid their taxes. So our meteorologists or officials in charge in these country-wide stations are very important people in their communities and I think almost without exception, they understand their position and their responsibilities very well.

I guess that's what has made the Weather Service really loved out in the heartland. And those people will fight right down to the line to prevent anything nasty from happening to their local weather station. Valentine, Nebraska, has appeared on a retrenchment list interminably, but it's still there.
Virginia Smith was the name of the Congresswoman...

McPherson: I believe Mrs. Smith has retired and I'm not sure whether Valentine is still there or not, but if it's not it's only been a year or so since she's retired.

Cressman: This business in the heartland tells you what people are really doing for a living out there. We kept a fruit frost meteorologist in Brownsville, Texas. He knew by first name every big grower around and he was up all night and on the telephone in case of possible fruit frost, but people like that are obvious targets of retrenchments. If you go to a centralized WSFO, which has responsibilities for a whole state, the local people who are growing oranges and grapefruit aren't going to get the same kind of service. Nobody's going to call up and say, "We've got a fast change made in the forecast and you better get busy." A little station has a lot of useful functions. Now, I suppose in principle this could be turned over to private enterprise but in that case, it's going to cost somebody something.

The local people feel they paid their taxes and by God, if they can't get a little weather help out of it, it's a waste--somebody's doing things wrong in Washington.

Phillips: One of the things which may not have been started during your tenure as director, but certainly came to full fruition during your tenure, was the importance of the forecast game of statistical modification of the forecasts at the end of the forecasts. This was the unit that was originally headed, I gathered, by Bill Klein and was particularly useful in the beginning when stats were another part of the model forecast, they had to be derived from model parameters. When did that start up?

Cressman: Back in the early fifties, there was a lot of unrest about the notion, which was probably true, that weather forecasting hadn't kept up with the rest of science. And it was not up to standards. Of course, we discussed the gradual development of atmospheric physics and dynamics and numerical prediction, but at that time it hadn't come to fruition. To the average person, no improvement was in the wings waiting to present itself on the stage. This led to all kinds of difficulties, as you can imagine. The idea of statistical forecasting was nothing new in those days, but the advent of computers was and I guess statistics as a branch of mathematics had made some advances as well. There was a certain rumble and people writing papers about statistical methods in forecasting; they also used much more advanced statistics than the old linear regressions that everybody at a weather station knew how to do back in say 1940.

At one point, there was some discussion, I don't know who initiated it. I think the Air Force initiated it by saying "We're out of date, and we need a whole new upgrading." So I think it was devised pretty much up at Cambridge Research Lab, called System 433-L...as I understand it, this was sort of an envelope under which you could do practically anything that you thought you could do to improve meteorology.

Phillips: It was focused on _____ forecasts.

Cressman: No, it had a very broad focus. But the focus did not seem to involve face-to-face competition with dynamic forecasting. Perhaps that was implicit, but I didn't see that as a challenge. But the Air Force also felt that its instrumentation needed to be greatly improved. And so I think it was the Air Weather Service who put together a set of requirements for this system known as 433-L. Part of the system was understood by people who knew a good bit about it, to involve development of new and better observing equipment, new and better communications and better forecasting methods. In short, anything that could be done to improve weather forecasting. But the emphasis was heavily on

development of statistical methods for forecasting, and the main interest here was at Travelers' Insurance Company, where Bob White was employed at the time. Now, probably--and I say this because I don't really know--but I suppose that what happened was that Travelers' made a direct proposal for support in this area. Personally, I felt that a lot of the equipment communications certainly needed modernizing, but I personally didn't see statistics as a means for a significant improvement in weather forecasting. After all, people had been doing linear statistics on minimum temperature and all kinds of things in the stations for decades, but that's not what they had in mind, linear statistics of that sort.

Phillips: What I was referring to--I couldn't think of the proper name at first--was model output statistics.

Cressman: We'll get around to that.

In any case, what happened was the Air Force and I believe the Navy, possibly the FAA, possibly even the Weather Bureau, had some initial contracts with Travelers. And the impression I got from being on the outside of this but hearing about it was that this was going to be an all-purpose system for all kinds of forecasting. This may not be fair, but that's the impression I got from what I heard by casual conversations, which is probably not too reliable. But in the meantime, of course, dynamic forecasting came along and history was changed by then. But statisticians and other people interested in such things kept working on statistical aids to forecasting, and eventually new acronyms were invented, possibly for old things. But the difference came where the new statistical forecast methods being worked on and proposed involved use of, as input parameters, the results from numerical forecasts. So there you come to what you call model output statistics. Bill Klein was one of the leaders in this. So our other work began in that, and I'm sure you would agree with me that this can be used, say based on model output as a useful tool for all kinds of things that models aren't particularly good at. Like minimum temperature forecasting for tonight at Omaha, Nebraska, or fog or something like that. In a sense, that's not new at all. Joe George was publishing statistical fog forecasting algorithms in the **AMS Bulletin** way back into the thirties, I believe, and he wasn't the only one doing it. But I think Joe George made a special name for doing that.

So more talented people were brought in by the 433-L project that was established. I think it meant different things to different services, but anyhow the work emanating from Travelers or which started at Travelers found moral and financial support within the Weather Service, and we had a unit under Bill Klein producing what useful could be produced in this way. I think it's pretty much self-evident that there's a space for that kind of forecasting, especially now

using the output from numerical models as starting parameters.

Phillips: In my experience with it, the MOS is accorded an all-powerful position in that a model change cannot be initiated by an actual meteorological change unless it improves the MOS or at least does not degrade it.

Cressman: Has it got to the point where you can't do parallel running?

Phillips: No, they don't care what we do as long as it doesn't enter the circuits.

Cressman: Things were much more simple then, I suppose, than now, but we used to do parallel runs of the model. If we had a new model that was obviously superior, we'd run it.

Phillips: Except five years ago, they would want a year, maybe two years of parallel running.

McPherson: We do have parallel runs now and we still have MOS and they still wait a year or two.

Cressman: That beats five, doesn't it?

Phillips: There's an amusing sidelight on this. We have cable television so we can tune in and get the Weather Channel, and they reproduce what is presumably a literal Weather Service-prepared forecast. Graphically, it's a horrible production, but it always states: "P.O.P. 45." Now I know what "P.O.P." means, but my wife couldn't guess what it meant. It's always given after the statement about rain, so she gradually realized by association that it has something to do with the rain. That acronym, "Probability of Precipitation" (the "POPs") has become a television byword.

Cressman: Well, I had a lot of difficulties myself with the local precipitation forecast because of the extreme local variations we have, especially in the summer. And I had the impression, almost as an amateur in this case, that there are certainly systematic effects for cities that are subject to dynamic modeling, not on a daily basis but in order to help us understand the systematic effects. For example, with sort-of halfway, 50-50 precipitation forecasts for convective rainfall and the general broad current from, say, approximately west, it seems to me that two miles from my house they get five times as much rain as I get at my house. I haven't been able to get hold of detailed information and study it intensively, but I had the impression, for example, that the city heating is a non-negligible factor. If you combine local topography which is rather low, but with a marginal atmospheric situation and city heating, it's may make a big difference where the

thunderstorms go.

Phillips: Yes, and combine that with the fact that even under ideally homogeneous boundaries, the thunderstorms will still vary from here to there.

Cressman: Yes, but then on the average, they ought to equal out, shouldn't they?
Phillips: That would work in many events, yes.

Cressman: Mine don't. At least they won't until the whole regime changes. But with the situation we've had this summer, and possibly last summer as well, I fell way behind National Airport on accumulated departure from normal in precipitation. Time after time I can stand out in front of my house and I can look at intense thunderstorms going along about two miles from my house. And I got caught in one and I got caught in one hell of a hailstorm one day not too long ago. Right over there, and when I got home, it was dry. This has been the pattern all summer. I think modelling will show some very interesting local climatology under certain marginal conditions, and it would be very interesting to start a career over again and do that.

McPherson: Especially with new tools like the Doppler radar that also show you those kinds of details.

Cressman: Yes, that's a good verification...I don't think it would take one really enormous computer, either. I think home computers that some people have now would be good enough to give you a lot of fun with that and you might learn something.

I get very sensitive to this because I have to water and water costs a lot of money. I'm sure it's a useful subject to study.

Phillips: I remember on that subject every once awhile in the map discussions that we used to have here at NMC, someone, probably Harlan, would get-together all the local cooperative observers' twenty-four hour reports of rainfall per unit convective storm event in the summertime. And draw them on a scale of, I suppose it was about a half-inch to the mile, a map of that size, and try to draw isolines and then you could see that someone else might have drawn a completely different set of isolines. It was that variable.

Cressman: I think Harlan Saylor didn't get credit for what he knew that he didn't tell people. He was a lot smarter than people thought and that was one of his secrets of success. He was very interested in details of rainfall.

Phillips: I thought the secret of success was when you got people to think you were smarter than you really were.

Cressman: Some people are very good at that.

Phillips: I have one general question about George's stint as director of the Weather Service. But before that, maybe you should come in with any questions you have.

McPherson: I have two or three rather specific ones that I'd like to touch with you on. You were, I believe, just beginning your tour as director of the Weather Bureau at the time when the Satellite Service was formed and satellite data became available. Do you have any comments on the impact of meteorological satellites on weather forecasting?

Cressman: In the first place, it seems to me that they are very helpful in closeup, local forecasting. In the second place, what is completely obvious is that you've got some kind of data whether or not there are any other kinds of data and if you understand your data well enough, that's a big asset. I have had the problem that, in my opinion, the accuracy and utility of the vertical soundings was over-advertised for a long time. Whether it still is--I don't know.

McPherson: Yes, it's better.

Cressman: But one of the last things I did here at NMC was to write a very small report about a problem we had when it seemed that we had a lot of difficulty with satellite soundings. Apparently they went sour due to some change in the software or something--

Washington: George, can you say something about when you started trying to use the satellite soundings in analysis and forecast models?

Cressman: The cloud drift winds seemed to me to be quite good. But the problem, I think, had been with the soundings all along. I hope it's a lot better than when I left NMC, but when I left, there was, in my opinion, still a very serious problem with satellite soundings because we had an area in the Southern Hemisphere where there aren't many data to begin with, and then you get a whole avalanche of satellite soundings in this area--they're going to outvote everything else you've got, probably improving continuity. And if there's some difficulty with the instrument or some difficulty with the algorithm that's used to convert these to soundings, you're going to wait a long time before somebody finds out.

Phillips: Actually, the Southern Hemisphere is the one hemisphere where it's been rather easy to demonstrate a positive impact.

Cressman: Sure, this is as compared to nothing.

Phillips: That's right. It's the Northern Hemisphere where the countries paying for the satellites are located, where it's been very difficult to establish positive impact from satellite or temperature soundings.

McPherson: George, in the early years of the Satellite Service, though, there must have been relatively enormous flow of money into the satellite side of the organization as opposed to the Weather Bureau. Do you remember that giving problems, like the FAA and the Weather Bureau--

Cressman: Of course, it used to bother me. I thought they were sending good money chasing after bad in those cases. It's not to say I was anti-satellite, but I think that the satellite soundings among other things were puffed up in order to maintain an adequate constituency and adequate financial support and I had to deal with the consequences. People would say, "Now, look, you've got all these satellite data, how come you're missing forecasts?"

McPherson: I remember a time early in 1970 the director of the National Weather Service at that time made a decision to withdraw support for the ocean weather ships in view of the fact that we had satellite sounding data there. I also recall as I remember that you had to go to Europe and sort of defend that decision.

Cressman: Well, I want to correct your premise here. I didn't withdraw support for the ships. I objected at every stage of the process, and I was probably wrong in doing so, but I objected vehemently. This, of course, did not endear me to my bosses. Jack Townsend understood the game but he was a good actor and he would raise all kinds of hell with me for being backward and ante-deluvian and uncooperative. But it was I who was responsible for the forecasts, not Jack. Jack understood the situation very well, though. He clarified this much later at a party I saw him at. So I won't hold that against Jack.

But nevertheless, I could see the satellite problem. They had to demonstrate something good was coming from this very expensive program. And of course, they're going to see everything in the best possible light. I cannot say we were completely innocent of that type of thinking when we got numerical prediction started. But in the meantime we had become "truified" through age and fire, you see. And these guys were not supposed to commit the same offenses that we did. You're talking about much bigger money, besides. You see, I raised objections to practically everything that would cut our ocean observing capability. Finally, the thing that rescued us was the cooperation of the Air Transport Association, which gave us the aircraft-measured winds in large quantities around the globe. Of course there aren't air routes absolutely everywhere; there are problems over

the Southern Pacific Ocean, but by and large we get very good help from the airline industry.

McPherson: Norman, I just have one more question in this. George, the name "United States Weather Bureau" was around for about eighty years, and then around 1970, it was changed to the National Weather Service. How did that come about?

Cressman: Although I didn't like it, I have to admit it was a more reasonable political move than doing nothing, in that the word "bureau" has come to take on negative inferences in the political world of the last few decades. We're all bureaucrats, in a sense, or we were at one time, and this becomes a toy in elections. So if you say more what we are--a service--instead of a collection of bureaucrats in some fancy office someplace, it conveys a much more positive impression to the public. I guess my objection to it was that I hated to see the tradition disappear. But it was a good move in retrospect.

Washington: It is interesting to note that the FBI still uses the word "bureau" in their name.

Phillips: I guess police departments would conceive of them as a service.

Cressman: One of the things that influenced me in my negative attitude on this was you have all these employees out in the field who figured we imported a bunch of new high-priced people in Washington who couldn't forecast rain tomorrow, changing everything now including their name. What disgrace will happen next?

Washington: I would like to ask a couple of broader questions. You served as the president of the American Meteorological Society. Is there anything there you would like to mention or any memorable event, or events?

Cressman: No, my tenure was relatively peaceful as far I'm concerned. I love and support the American Meteorological Society. Good outfit.

Washington: Also, since you might want to mention something more about the World Meteorological Organization and your trials and tribulations. You've already mentioned some of them.

Cressman: Well, one thing was rectifying the data problem. And then of course they came along with this concept of "World Weather Watch." I was not an author of this term, but I think it has been a useful way to look at activities of the World Meteorological Organization. I think it was, in effect, a useful political term. It enabled governments around the world to get support for a new program, and more support for an old program and it enabled the U.S. to lay on new programs of support for areas where there were few data, for example, in developing

countries and oceanic areas and so by and large, it was a very useful concept. There was very little new that I could see except the various aid programs which came under one banner or another. Like World Weather Watch, you go to Congress and tell them this is for World Weather Watch and it's going to benefit the United States and we're going to get all this help from other countries and we couldn't possibly afford to do this ourselves. That's a selling point. In the meantime, we could sell the idea of the aid programs. They provided observing facilities for countries who would then operate them.

"Voluntary Assistance Program" was its name. It was a very useful program. It involved some of the smaller countries and poorer countries in providing data for meteorological services and in turn, certainly encouraged them in meteorology in general. But that was really under the surface a self-centered program for the U.S.

Phillips: Was that the program under which you would subsidize radiosondes in Mexico, for example?

Cressman: No. That was a long-standing program before that. In any case, it was essential for us to get soundings from Mexico. We financed it and they did the work.

Howcroft: At one time when we were developing a limited area fine mesh model, I thought you were about the only proponent of that besides myself and Desmarais. You have any recollections of that early time--about 1970-71?

Cressman: Let me say as director, I was under the impression that the agreement to do it was unanimous.

Phillips: This was the program where you and Desmarais did the work out at Offutt, didn't you?

McPherson: We were converting the primitive equation model.

Phillips: Six-level model of Shuman?

Howcroft: Yes, to operate on the new UNIVAC 1108's out there, and we found ourselves out of a job when we delivered the translation. And having done original work on the window model for the Air Force, we had experience in doing small-scale models. Now we had a knowledge not unique to be sure, but a rather in-depth knowledge of the six-level P.E. that was shared by not too many people. In looking for a job, I think this was invented in a bar at the Officers' Club at Ophutt. We decided, well, could we put these things together, and we mentioned to this--I was under the impression that Dr. Cressman was very much interested

in that. I wondered if I was mistaken.

Cressman: No, I was. I thought that was a logical and natural thing to do. In the first place, we were never satisfied in those days with the horizontal and vertical resolution that we had. And we were quite aware of the penalties of the low resolution in that basic model.

Cressman: It was a feasible way to do fine mesh modeling without paying the full penalty for it, and the fine mesh covered only a relatively limited area compared to the Northern Hemisphere. Therefore, it was much less expensive to run say than a full-scale hemispheric fine mesh. But this seemed to me like an excellent idea. I was very much in favor of that.

Do you get along now without a fine mesh model?

McPherson: Oh no, no. A regional model is still very much a part of the production suite and in particular, the LFM will celebrate its 21st birthday on the 29th of September of this year.

Cressman: I suppose you still feel a lot of problems or dissatisfaction with the Mexican network. Are they down, or are they just as good as they were before?

McPherson: Actually there are signs of hope there. The present Mexican president has breathed life into the hydrologic side of the Mexican government and they have made a deal with one of the Scandinavian countries for technological assistance, and they are in the process of implementing new radiosonde equipment at the stations in Mexico and eleven or twelve radars that will actually have a network together and many, many--I believe on the order of 2,000--telemetered rain gauges.

The new equipment for the upper air network also will be coming in. So many of the stations which, up until now, have had DLG & D equipment are being replaced by modern, computerized equipment.

Cressman: I don't see how you can get along in the winter without measuring upper air data from Mexico. You know, there was one time when the satellite sounding program was being put together. In its initial days, I felt as if there was a position in NOAA that this would allow us to cut way back on radiosonde stations. I felt that Jack Townsend was pushing this notion and I was in opposition to it, I was disgusted with it. With the activity that occurs in the upper air over Mexico in the winter, you've simply got to get real measured data down there. And it's not because it's Mexico, it's because Mexico has the good or bad fortune to be where it is. It's a heck of a good aircraft carrier for

soundings. Now to run an upper air measurement program in Mexico is not a simple matter. The Mexicans do things differently from the way we do, and you have to operate the Mexican way, or you're dead. This includes an adequate supply of liquor for the director and such things.

Washington: I noticed after you retired from the Weather Service, that you've been active in helping other countries--China, Australia, Spain, Brazil--by being a consultant. You must find this rewarding.

Cressman: In places. The two most successful activities I engaged in were with Australia and Spain...which is no accident that these were the most developed of the countries.

The job that I had in Australia was that the Meteorological Service had fallen way behind in its equipment and its operational capabilities due to lack of support from the government. So I was hired to go down and tour Australian facilities, tour Australia to some extent and make recommendations. Of course, this is a game we all know...

END OF TAPE 2, SIDE 2

Interview with George Cressman

TAPE 3, SIDE 1

Cressman: I will entertain you briefly with two of the things I found in Australia. One was the fact that they had no specific fire weather service, but forest fires are a bigger menace in Australia than almost any other kind of natural disaster. The other was that their equipment--the main reason I was there was to evaluate their equipment--was a hodgepodge of obsolescent equipment that they had managed to be given or buy cheaply elsewhere, from other countries that were getting rid of them. The most amazing thing I saw in this respect was that they had an old French radiosonde set. What this looked like was a cockpit out of a small private airplane. It sat out there in blazing, 110 degree heat, and had a lot of plexiglas over the top. So what they did, a fellow launched a balloon and popped into this thing and it had a joystick. It drove like an airplane except it didn't go anyplace. So he would fly this little gadget to keep the balloon in his sights. And then this gadget recorded where it had been, so to speak, and he got elevation and azimuth angles from it.
There was no ventilation in it, no air conditioning, and he did this in 110 degree heat. Can you imagine, getting balloon soundings that way?

McPherson: He rarely got above 700 millibars.

Cressman: The balloon would break just like that. The other thing that struck me as being odd was that forest fires are an enormous menace in Australia and they had no fire weather service. Evidently, it never occurred to them...We had a real fire weather service in this country.
Then there was another thing. That was that they had their hydrologic service pretty much as we do here, included with the regular weather service. And they had no automation whatsoever in the hydrologic service, but you've got to have automation to handle flash floods. So they did have real problems like obsolete equipment that was so out-of-date it just couldn't do the job. When they ran the radiosondes, they had a little pocket calculator about the size of mine and they had about 4,000 instructions that they put in by hand to evaluate the radiosonde flight. Can you imagine that?

Anyhow, it was very interesting to tour Australia. I wrote a really blistering report and the Meteorological Service was absolutely delighted with it. They leaked it to the press and the press ran all kinds of cartoons about how obsolete the Australian Weather Service was, people trying to make forecasts rubbing the fur on the back of the cat, all kinds of really wild notions. But they got a bunch of money out of it, and they were very pleased.

I went down and visited with the Council of Science and Technology and gave them my review of the matter too, which helped a lot.

Spain: I would put it in the First World category; it was where I had the most success. The Spanish got a substantial sum of money and they wanted help in deciding what to do to modernize their weather service. The culture in Spain is not entirely what we would consider as Western. I don't know what you would call it, but it's different. And so you have to be very careful with this sort of thing. Things will work here that wouldn't work there. Their hours at work are absolutely inscrutable. I never did figure that out.

Aside from the fact that if you want to go to a really good restaurant, if you get there at 10:00 in the evening, you're going to open up the place. But nevertheless, the Spanish, I felt, made a lot of progress with their program. I went back and forth for five or six years. My last trip there, they gave us a free weekend in Barcelona. That was about three years ago. Barcelona was just starting re-construction for the Olympics. Lovely city. I could go back to Barcelona anytime. The Spanish work habits are really beyond my comprehension; I really never figured them out. I know that some of the more enlightened people I dealt with realized they had a big work habit problem, and they complained to me almost from the outset of my work with them that they had this trouble with the old-timers who would sit there and draw a few lines on a map and then go out for lunch and show up again about three in the afternoon and that was it. They were unprotected against flash floods, nothing like continuous manning. They had no radar network, they had no automated rain gauge network. They had really rotten facsimile from one of the European centers. Not rotten in content, but inappropriate in content and rotten in quality of reception.

So they had a lot of money and we made pretty good plans. Many of them were carried through. I worked with them off and on for five years, and a program like that takes a long time to implement. Because first you've got to plan it, then you've got to find things. Then you've got to get them made. Then you've got to check them out. I spent a lot of time getting the radar consultants to go and check out new radars off the production line. In the end, there was a big quarrel in the Spanish Meteorological Service. My contact was practically sacked. He got his paycheck, but lost all his duties. I suspect because the Spanish character is rather unyielding; the tradition is unyielding, and if you think I'm difficult, you haven't seen anything yet. However, I think we did make a lot of progress, so that was not a lost cause.

In China, I ran into what I also encountered in Brazil: and that is to say, I don't think I really understood the local culture well enough to do what I should have

done for them. The characteristics of China and Brazil resemble each other to some extent, as far as I was concerned, in that words that are properly translated still have a different meaning from what you think.

If I may summarize briefly, my impression is that in countries that are considered Second or Third World, nothing is what it seems, and it's very difficult to communicate effectively, if not impossible. On the other hand, you take a country like Australia or Spain, which are first-class countries. People don't realize in the United States how advanced Spain is, at least until lately. But in countries like that you can do a lot of good as a consultant. In a Third World country, only under very special limited circumstances can a consultant from the First World do any good--that's my opinion.

Phillips: I have no more questions to ask of George. Have you got any?

McPherson: I have my list here.

Howcroft: One question has been forming during the time we've been talking with George. He's had a long and distinguished and very eventful career. I wonder what stands out in George's mind as perhaps his most significant contribution to mankind and the science of meteorology.

Cressman: That's a difficult question to answer, you see, because if I weren't here, would it have happened anyhow? The answer in 99.9% of the cases is probably yes.

McPherson: If I may comment on that, Jim, I couldn't help but notice at Harlan Saylor's memorial service, George introduced himself not as a former director of the National Weather Service, but as the first director of NMC.

Cressman: Well, that was a pretty good time, I have to admit. It was a very good time. I think we made a lot of progress.

Washington: Do you have any final statements that you want to put on this tape?

Cressman: I think I've said quite enough. I appreciate that you came all this distance for this interview, and I hope you have some fun elsewhere before going back.

Phillips: George, I want to thank you for participating in this. You have certainly been one of the prime movers in our science in the past several decades, and I'm sure that this tape will be very interesting reading for other scientists in the field, even if some of your comments were not put on the tape.

Cressman: Thank you. It's been a lot of fun, hasn't it, Norm?

Phillips: Indeed.

END OF INTERVIEW