# American Meteorological Society
# University Corporation for Atmospheric Research

## TAPE RECORDED INTERVIEW PROJECT

### Interview with Robert N. Culnan

### 25 October 1993

### Interviewer: Earl Droessler

Droessler: This is Earl Droessler. It's Monday, October 25, 1993, and I'm in beautiful Charlottesville, Virginia, on an absolutely gorgeous day in the foothills of the Appalachian Mountains, for an interview with Robert N. Culnan, who has been living here in Charlottesville for the past three or four years. We are going to be reviewing Bob's perspective on the development of meteorology over the past 40-50 years. He's been a longtime worker in this field.

Bob, it's a pleasure to be with you this morning. Thank you very much for inviting me to Charlottesville. You've had a wonderful career in meteorology and I think one of the highlights of your career might have been the days when you met Rossby and the time you spent with him in the summer of 1940. Would you begin by describing this meeting and what happened during that summertime?

Culnan: What happened was I was coming back to NYU for another year, this time as an instructor, and Spilhaus and Emmons were there at the department at that time. Spilhaus decided that I ought to do something special to get ready to be an instructor since I'd only had one year of graduate school and some Weather Bureau experience. He had talked to Rossby and asked him what he might have me do that summer. And Rossby suggested to Spilhaus that I ought to work with Harry Wexler that summer. Wexler was going to be out at the LaGuardia Weather Bureau aviation forecast center and helping the forecasters there to get up to date. So I came back to New York and went out and met Wexler and we started to do some analysis, and all of a sudden the upper air data came to an end--this was the new radiosonde program--and it stopped that summer because of contract problems in getting the instruments that they were going to fly on the balloons. So we had no upper air data, and I talked to Spilhaus to ask him what do I do next. He checked again with Rossby, and Rossby suggested I come up to MIT for the summer for a couple of months and they'd give me some special instruction up there. So that's what I did, and Rossby arranged for Victor Starr to teach me isentropic analysis. I spent the summer analyzing what we called "canned" weather maps--maps for all situations--and having them reviewed by Victor Starr, who suggested ways to improve what I did.

The rest of the time I joined the five-day forecasting group, which was at MIT at the time working under Jerome Namias and under Rossby's guidance.
This was a very interesting summer. I used to see Rossby every so often when he would come up to MIT and join in with the group. He always had interesting comments to make. This is how I got acquainted with him. It was a very happy summer.

Droessler: You certainly hit the jackpot that summer, in 1940. Hurd Willett, Jerome Namias and Carl Rossby. As usual, Carl Rossby wasn't around very much but when he did show up, he made quite an impact.

Culnan: Jorgen Holmboe was also there; he had just arrived, so I met him also.

Droessler: What sort of a person was Rossby at that time? Was he a warm, caring person? How would you describe him?

Culnan: He was a very outgoing person, very caring, and always very nice to all the people he met. He treated you like you were his best friend. It was just very pleasant to be around him and to listen to him.

Droessler: So then after that summer, what did you do?

Culnan: At the end of the summer, I went back to New York to start the instructorship at NYU. But at that time, the first war course was going to start. They decided during the summer to send aviation cadets to the universities that taught meteorology. We had a group of something like fifty cadets coming plus a few other students and those were the ones we were to teach during the academic year of 1940-41.

Droessler: And what was your task during this teaching activity?

Culnan: My task was really to do the laboratory instruction along with Jim Miller, who had also been brought in as an instructor when they found out they were going to have this expanded teaching program at NYU.

Droessler: Athelstan Spilhaus was the chief director of the--?

Culnan: That's right. He was the chairman of the department. Gardner Emmons was also a professor in the department at that time. Ray Montgomery was also there; he had just joined the department that summer.

Droessler: So there was Spilhaus, Emmons, Montgomery, Culnan and Jim Miller. Were there any outstanding stars among the young cadets in the first class?

Culnan: Yes, there certainly were. George Cressman was in that class. He really shone--he had an advantage of having some background from Penn State, where he came from.

Of course, he was already interested in meteorology. He wasn't coming to just get a commission; he really wanted to study meteorology. Then I think that Alfred Creasi was in that class, too; he became a very good meteorologist.

Droessler: We certainly remember George, though. He went on to become President of the American Meteorological Society and also Chief of the Weather Bureau.

Culnan: I should mention one other person. Herbert Riehl was in that class. It was interesting to work with Herb in the lab because he always had some special ideas on how he wanted to analyze the weather map.

Droessler: How long a time then did you teach these cadets? Was it an academic year?

Culnan: Yes, it was a nine-month course. It was one course after another; when we finished the first course, then another group came right away. There was no summer vacation that year. The second war course started in June of 1941.

Droessler: All this time, you were an employee of the U.S. Weather Bureau?

Culnan: No, I'd taken leave from the Weather Bureau to come back to NYU to take the instructorship, and I stayed on leave that first year. At the end of the year, if you wanted to stay on leave, you had to go back and work for a month for the Weather Bureau, but that wasn't convenient, so I resigned from the Weather Bureau at that time.

Droessler: How long did you stay on at NYU?

Culnan: I stayed at NYU through 1946. Actually, I was part-time during 1946; I went back to the Weather Bureau part-time in 1945.

Droessler: During this time, you earned a Master of Science degree at NYU in meteorology?

Culnan: Yes. I received my Master's degree after my second year there but since I'd been there one year earlier as a graduate assistant, I finished up my Master's in that first year.

Droessler: So the wartime program ended about 1945-46, and then you had to find another job.

Culnan: Yes. Actually the cadet program ended in about 1944 and we went on with a couple of more small courses training people--I think there were some Navy people in one of the courses and the last course was just Weather Bureau people. They were very short of trained people, so we trained another small group for them in 1945. That was the end of the war courses.

Droessler: Then what happened to you?

Culnan: That was when I went back to the Weather Bureau and I was offered a position in what was the so-called research division of the Weather Bureau--they called it Scientific Services in those days. I went down at the end of May, 1945, to the central office.

Droessler: Was Harry Wexler the chief of Scientific Services?

Culnan: No, Harry had left the Weather Bureau, I guess, around 1940 or 1941, and had gone into the Army Air Force, was commissioned and was serving in the Air Weather Service. Wexler didn't return until 1946.

Droessler: So there was a gentleman by the name of Cyril--

Culnan: Sarle. Charles F. Sarle. This was again some of Rossby's work. Rossby had been there for one year, around 1940, as assistant chief of the Weather Bureau for research. When he left, he suggested to Reichelderfer that they bring Dr. Sarle over to the Weather Bureau from Department of Agriculture. He came, and was given the title of Executive Assistant for Scientific Services. It was on the level of an assistant chief. He supervised the special scientific work that was being done all during the war.

Droessler: It wasn't long ago after that that Mr. Sarle moved away and Harry Wexler came in.

Culnan: Harry Wexler came back in 1946, and was immediately under Sarle to begin with, but I think Sarle realized there wasn't very much future for him when Wexler came back so he went back to Agriculture at that time. Wexler was then head of the Scientific Services work.

Droessler: What sort of a person was Harry Wexler to work for?

Culnan: Everyone enjoyed working for Harry because he was another idea man, always full of ideas and new things were starting. We really enjoyed working for him. I became his special assistant in helping get the program organized and then later, in running the program. This was the role I carried on for some time at the Weather Bureau.

Droessler: How many people were involved in the Scientific Services and who were some of the outstanding people you were associated with?

Culnan: When I came back, there was rather a small group. Roger Allen had been there as a division chief during the war and helped carry out the wartime projects such as the

Northern Hemisphere surface maps, the upper air maps that were analyzed and published, and other special projects to meet wartime needs. At the same time, there was some small research work going on; there was still the radiation program which was carried on up at Blue Hill under Irving Hand, and Glenn Brier was there as a statistician. Then, during the forties--probably around 1942-43--the extended forecast group, known as the Five Day Forecasting Group, had come down to the Weather Bureau from MIT, and that was part of that division.

Droessler: That was headed up by Jerry Namias?

Culnan: Yes, Jerry came down and was in charge of the extended forecast work.

Droessler: You have a most interesting group there in the Office of Scientific Services, Bob. I wonder if you'd talk about some of the activities and the work of this agency.

Culnan: This was all centered around Wexler, of course, and the first thing he did when he got back was to sit down and try to organize the research division. I remember that he wrote a memorandum to the Chief of the Weather Bureau outlining the things he thought they should do. It is interesting to go back and read this. Many of the things didn't happen, because time caught up with him, but it was the beginning of his planning, and he first re-organized the division. Roger Allen had been the acting chief while Wexler was away; he became head of a short-range forecast development group working with the field forecasters to develop techniques in forecasting, especially in the application of statistics in forecasting, which was one of his primary interests. Later that led to putting research forecasters at the main forecast centers and coordinating this group. They all did studies to improve forecasting.

Sigmund Fritz, who had been in the Weather Bureau before the war, came back and his field was radiation. He came in to what was known as the Meteorological Physics Section of Scientific Services. That became sort of the catch-all for people coming into the Weather Bureau research group. This was a good place to put them to start out. Fritz organized the radiation work and also started the ozone work when the Weather Bureau bought a Dobson ozone spectrophotometer. NYU had a Dobson also, which they had procured in the late 1930's and which was used to do some research based on the data collected. This instrument was given to the Weather Bureau. That was the beginning of the Weather Bureau Dobson network. Gradually, the Weather Bureau, through the years, bought some more Dobsons to establish a network of total ozone measurements from the surface, and this was done under Sig Fritz.

Droessler: After some forty-fifty years now, ozone studies have become central to the whole field of atmospheric sciences, haven't they?

Culnan: Yes, and this Dobson network is still important because it gives the ground truth for ozone measurements and it's been very helpful to NASA to use the information in calibrating what they're doing. In fact, I think actually they couldn't understand their satellite measurements without this program. We can talk later about how this program got changed during the IGY.

Wexler kept on enhancing the program, bringing in new people. Joe Smagorinsky joined us and he was put in the Met Physics group. Morris Tepper came later, and there were others. This was sort of the starting-off place in research and finally when they found their niche, they went into another group in the Division. But this was a great place to bring in people.

About this time, the numerical weather prediction group was established at the Institute for Advanced Study at Princeton under John von Neumann, and people like Charney came, and Eliassen from Sweden. Rossby was very much interested in this; in fact, he may have had something to do with the beginning of it and Wexler also became very interested. I think he very early saw the possibilities of this so he worked closely with the group and helped them when they had a model to test. Through Wexler and Reichelderfer they arranged for the use of the ENIAC for that first test. I always remember a picture that was hanging in Wexler's office of the group of people that went down there for that first numerical weather prediction run of the ENIAC.

Droessler: Some of the folks listening to this tape years from now will not know what ENIAC was. Why don't you mention--?

Culnan: I don't know much about ENIAC either, but it was sort of one of the earlier computers that was developed with the support of the military.

Droessler: Right, it was a large computer development.

Culnan: It was before the new approach to computing got started, which was really von Neumann's line of thinking. And that led to the IBM 701. I'm sure, in talking to George Cressman, that you received a lot of background on this because he later was the first director of the Joint Numerical Weather Prediction Group.

There were others who also came to the NWP group. Phil Thompson was there and Wexler arranged for Joe Smagorinsky to be assigned to the group from the Weather Bureau. He went up there and worked at Princeton, but also was finishing his PhD at NYU at the same time. When this project finally developed far enough, Wexler then began to think about NWP--numerical weather prediction--at the Weather Bureau. He organized a group with the approval of Reichelderfer, to study how we

could start it at the Weather Bureau. This led to a joint group of Air Force, Navy and Weather Bureau people--which was the Committee for Joint Numerical Weather Prediction. Out of this group, they developed a plan to establish the Joint Numerical Weather Prediction Group and George Cressman was selected to be the first director of this group.

As people came in, new projects were started. When Morris Tepper came, he got interested in severe local storms and tornadoes, and we established a severe local storms group under his direction and recruited a few more people to work with him. There was also a small group that did studies on air flow over mountains under Dever Colson, and that went on for some time. Glenn Brier was already there, and he was doing statistical studies of meteorological problems. We formed a meteorological statistics group. The Air Navigation and Development Board came to the Weather Bureau and wanted to do a study of objective forecasting on a very short range basis. The Weather Bureau established a group to study this and Frank Gifford came in along with some other people to carry out this project. That was Frank Gifford's entry back into the Weather Bureau. He had been a wartime meteorologist. Later he became head of our research group at the Oak Ridge National Laboratory.

There was a micrometeorological program that also got established in the late forties. We had been asked by the Atomic Energy Commission and also by the Air Force to do work in this field. Wexler brought Lester Machta in to head this work. This program really grew in to a quite a large program. Later, we needed somebody to help us work with this program, to coordinate it, and Fred White joined us. He had been in the Thunderstorm Project out in Illinois before that. So Fred was the assistant to Wexler for the coordination of this program, which was carried out under the direction of Lester Machta.

At that time, of course, rainmaking had come into play. The Weather Bureau, of course, was very interested in this. So it put together a small group to work on this problem and keep track of what was going on. Dwight Kline came in as monitor of all this work and then later Ferguson Hall also worked on this program. Ferg actually did a project out in the northwest part of the country, trying to measure the effects of cloud seeding. That was the one project that the Weather Bureau actually did. Ferg also worked on the Bishop Project, which involved seeding over the Sierras, and trying to measure the results in the lee of the mountains.

All through this period, Wexler and Rossby had a very close association. Of course, Wexler had been one of Rossby's students and he just continually kept a mentor relationship going. At one point, sometime around 1950 or a little later, I think it was Rossby's suggestion that we ought to meet with the National Academy of Sciences to see what could be done to help meteorology. A meeting was arranged

with the President of the National Academy, whom I believe was Detlev Bronk at that time. So on one afternoon, it was Rossby and Wexler and Reichelderfer and maybe somebody else--I don't know for sure--but they took me along to be secretary of the committee. There was a wide-ranging discussion of what the Academy might do to help meteorology. I think one of the suggestions was that we maybe needed an institute for meteorology in the country, and this was actually Wexler's suggestion. When the meeting ended and they decided that we would have further meetings in the future. I wrote up the minutes of the meeting and after that, they decided that the work was such that we probably needed a secretary in the Academy and Rossby arranged for John Sievers to come help. So he became the secretary for a group that was named by the Academy; I think it was probably called the Committee on Atmospheric Sciences of the Academy. If not, it became that committee later.

Droessler: It began as a committee on meteorology and then later on it became the Committee on Atmospheric Sciences. It had a long life in the Academy.

Let me go back to that first meeting for a minute. I didn't quite understand the nature of the committee makeup at that time. Was Bronk himself actually part of the discussion group?

Culnan: Yes, I believe it was Bronk who was president at that time.

Droessler: He was president of the Academy, but he was one of the--

Culnan: We met in his office.

Droessler: So it was just that small group.

Culnan: Just that small group...

Droessler: Just like a free-ranging discussion.

Culnan: That was sort of the seed that led to the development of the work the Academy did through the years, by the Committee on Atmospheric Sciences; they covered many things.

Droessler: Of course, John Sievers came out of Chicago, as I remember. He was just graduated with a Master's degree and Rossby was able to get him employment in the Academy as secretary of the Committee.

Culnan: I think Rossby felt he was the kind of man for that kind of a job. Rossby was always doing that, picking out particular people who worked in special places, and this was one of them.

Droessler: In this case, he certainly was an outstanding secretary for that committee for many, many years.

Culnan: To complete the picture, let's mention a few other things that were happening at that time. One of the things we did was to carry on further the publication of Northern Hemisphere maps, which was done for research purposes. They had done the forty-year series during the war and they wanted to extend it up to the present time, which was about 1950 at that time. So that was done through the Scientific Services Division.

At the same time, we were also supporting groups at several universities under what we called memoranda of understanding. They were sort of cooperative agreements with the universities. We didn't have very much money, I think it was less than $100,000. So we would put out these small contracts, to the tune of about $10,000 or so to help a project leader get an assistant or to buy some things that the project needed. But the university itself was paying for the principal investigator.

There were a number of interesting projects that were started. I remember one at UCLA where Will Kellogg worked on the upper atmosphere. This was Wexler's interest and he was looking at the meteor trails at that time to try to understand what happened in the upper atmosphere. There were other similar projects such as that. I don't remember all the details; they were all rather small and sort of passed away in time. But it was useful when there wasn't too much support for research.

Droessler: And all this time you were the principal assistant to Harry Wexler, and of course you had to carry out all of the detail work, arrange for the contracts and look at the proposals and things of that sort because he didn't have much of a taste for that kind of work, as I remember.

Culnan: Yes, he liked to make the decision and have somebody else do the work. That's where my role came in. It was an interesting role. He always kept his eye on everything and gave you all the direction you needed.

Droessler: Well, you really couldn't work for a more fascinating man all these years.

Culnan: There was always something interesting going on and that was the fun of being there.

I think it was somewhere around 1950 that the Congress passed a bill on severe local storms research. It authorized the Weather Bureau to conduct research in this area to improve forecasts and warnings. It also directed the Weather Bureau to report to Congress every year on the progress made in this. But the only catch was they never

appropriated any money to do the work, so we were stuck in writing a report every year on the work we did without any new funds. There is a whole series of reports under Public Law 657 on thunderstorms and other severe local storms. This went on for a period of at least ten years. They are in the archives somewhere. It records the history of the research in the Weather Bureau quite well, if somebody wanted to go back and check on it. We would usually assign this problem to one man to write and I think Ferg Hall may have done it once or twice. Finally, it was Jim Caskey who had this problem. Jim had come into the Weather Bureau to be editor of the **Monthly Weather Review**, and he helped build it up into a really very good meteorological periodical, which finally was turned over to the AMS in later years. In fact, Jim actually went up there later and was an editor of the AMS journal.

Jim Caskey headed up our editorial division, which handled all the output of papers coming out of the Weather Bureau. He did a very, very fine job. I think Jim Caskey probably has as much knowledge on the research that went on in the Weather Bureau as anybody because he reviewed all these papers and either obtained approval for publication or helped authors improve them for publication in outside journals or in the **Monthly Weather Review,** or sometimes as a technical paper.

Droessler: So the **Monthly Weather Review** was a responsibility of the Scientific Services office.

Culnan: Yes, it had been for many years. It was there even in Scientific Services during the war. It became sort of idle during the war because the only thing it published was climatological data. When I came back to the Weather Bureau in 1945, one of the jobs I had was to be acting editor. I edited a few papers at that time for the **Monthly Weather Review** or as a technical paper, before I became Harry Wexler's assistant.

Droessler: During that period of time, over about a twenty-year period or so, that was a major publication for weather research.

Culnan: Yes, it was and I think it clearly became well-recognized not only in the U.S. but throughout the world as one of the publications of practical meteorology, especially in the forecasting field.

Somewhere in the mid-fifties when Dr. Reichelderfer was away in Europe at some meetings, I think in Geneva or Paris--maybe the IMO, as it was known in those days before it became the WMO, the World Meteorological Organization. Delbert Little, who was the Deputy Chief, represented the Weather Bureau at the Congressional hearings, and somebody, I think it was probably at the House hearings, asked whether the Weather Bureau could do some work on hurricanes. He offered to add $1 million to our research budget to do research on hurricanes. Little said, yes, he thought we could do some fine work with it. Sure enough, it got into the bill and got

passed, and the Weather Bureau all of a sudden had $1 million to do hurricane research. This was the beginning of the hurricane research program in the Weather Bureau.

Reichelderfer got back, and of course he was very surprised to hear about this. We never did find out what his answer would have been if he had been asked the question about the $1 million, whether he would have said yes also, and taken it. He might have. That was followed the next year by additional money, because in the meantime the program was started and Dr. Reichelderfer appointed Bob Simpson to come in and head up an office for hurricane research. He also decided that we would start up work in the thunderstorm and tornado field, known as severe local storms.
So the project got established as a hurricane research project down in Miami, and a severe local storms project out in Kansas City, which later became the National Severe Storms Project at Oklahoma. Ed Kessler was recruited to come in and take this over at some point. I remember Chester Newton worked with the project for a short time when he came back from Sweden. Then he went out to NCAR after helping with it. There were others, I guess, that helped with it, too, but I don't remember.

They decided the main thing they needed was better aircraft data to understand hurricanes. So they went back to Congress the next year and requested an appropriation of $1-1/2 million to establish a research flight facility. That was also approved as the first airplanes at the Weather Bureau to use in research, but also to use in some weather reconnaissance to supplement the work that was done by the Navy or the Air Force.

That was the beginning of Severe Storms Research and all the time that Reichelderfer was there, the program stayed separate from the Office of Meteorological Research, the new name for Scientific Services. There was coordination back and forth, but this project was not under Harry Wexler. I guess this was the way that Reichelderfer did the divide-and-conquer approach, or something of that sort.

Droessler: So Bob Simpson would report directly to Dr. Reichelderfer as head of the Hurricane Research Project.

Culnan: That's correct.

We mentioned earlier the establishment of the Joint Numerical Weather Prediction project under George Cressman. Phil Thompson was in that group for the first year or so, and Joe Smagorinsky was there for about a year. This was still fairly early in the fifties. I think JNWP started in about 1954, and von Neumann was still active

and working on the subject. He thought the next thing we should do was to get into climate prediction, if we could, or at least understanding climate with numerical models. He and Wexler came to the agreement that we ought to start a group doing that, so we took Joe Smagorinsky off JNWP and put him with a small group to start a program of general circulation research. He started out with a group of about four or five people from here and there. I helped him find a few people and he found others and then the work began. He gradually built up the organization. Several people came from Japan who became very excellent research people and are still with the group. It later became known as the Geophysical Fluid Dynamics Laboratory. It started out next door to the numerical weather prediction at Suitland so they could use the same computer. They started out with a 701 computer and later, got a 704. That became pretty busy, and the general circulation research needed more computer time, so they finally got their own computer, starting out with a STRETCH computer which never worked too well, as everyone knows. IBM only made ten of those and quit, but there was a whole hierarchy of computers that went through that group. The group later moved downtown to a strange building across from the National Art Gallery on Pennsylvania Avenue. We always joked about their location down there, between a liquor store and some other store, in a small building that had been leased for them.

Then later, they decided they would take the group out of town and they looked around and considered a number of places but finally decided on Princeton, which was a good place to go. Princeton welcomed the group and was interested in the work of the group, wanting to tie it in with some of the work it did. I guess the fact that von Neumann had been there and the NWP work had started there must have been some sort of an influence. So then the Geophysical Fluid Dynamics Laboratory moved up there into a building that Princeton built for the Weather Bureau. And it's still there today.

Droessler: Well, Bob, you certainly have told a great story about the Offices of Scientific Services and what it was able to accomplish within the Weather Bureau and within our national interest. This program in the Weather Bureau also was present at the time of launching of the International Geophysical Year, which I remember began about 1957 and lasted for a year and a half. What role did the Office of Scientific Services have in the IGY?

Culnan: Actually in about 1954, the planning began for the IGY. Again, Wexler was involved in it because I think he saw the possibilities of getting measurements we couldn't get otherwise. It would help meteorology, especially on the global scale and in some particular studies. The National Academy of Sciences was responsible for the U.S. program and it established an office and a committee to oversee the program. The office where the IGY was centered wasn't far from the Weather Bureau down on M Street. There was a man by the name of Hugh Odishaw who

became the executive director there. As I remember talking to Hugh before he came in, he was working at the time for NBS, and he was interested in how we manage our research programs in the Weather Bureau as he realized the kinds of problems he was going to have. So we established a relationship very early with the IGY. The IGY committee established subcommittees for the various disciplines. There was one for meteorology, and Harry Wexler was made chairman of that committee. There were another eight people on the committee. We had representatives from the Navy and Air Force meteorological services. I don't know whether the Army was there or not. There were some university people and a few others.

Droessler: Did you serve that group with agenda and minutes?

Culnan: Yes, I became secretary of the committee on meteorology. We met regularly to develop a program for the IGY which was to begin in 1957 and go on through six months in 1958. It was actually an 18-month period. It was interesting how the program developed. One of the things they wanted to do was to get more measurements in the Southern Hemisphere. The Committee proposed a line of radiosonde stations that went down South America. I believe there were four of them, with the idea that the Weather Bureau, with the cooperation of other countries in South America, could do these observations. That was finally approved.

Droessler: So we had a sort of longitudinal line of upper air stations from the North Pole to the South Pole, almost.

Culnan: Yes. At the same time, there was planning going on for the Antarctic program and it was decided that this was going to be a major international program down there. There was a special committee set up for the Antarctic. Meteorology was in the picture, because they wanted meteorological work in the Antarctic at the stations that were to be established. At each station that was established in the Antarctic, there was a meteorological observatory there for observations and research.

There were some other special things that were to be done during the IGY. One of the things that Wexler was very interested in was carbon dioxide. His earlier work had been at MIT and had been in radiation--heat balance and things of this sort. This was still at the back of his mind. He recognized the importance of carbon dioxide and the fact that it was a gas that was changing made it of special interest. So he suggested setting up a station out on Mauna Loa, up somewhere around 10,000 feet--maybe not quite that high--to establish the observatory and then to make various measurements there of the atmosphere. One reason they selected this station was it was away from where people lived and so there was a minimum of local pollution. You were able to make measurements of the global atmosphere as it went by Mauna Loa. The only thing you had to worry about was maybe if some gases were given off by Mauna Loa, but it was dormant at that time, so it made a good point for

observations.

To do the carbon dioxide program, Wexler realized that there was a lot of work to be done to measure carbon dioxide regularly because up to that time, it had just been done occasionally. There was a man connected with the Weather Bureau by the name of Oliver Wolf who was working at Caltech doing magnetism studies and through Oliver, we found a man by the name of Dave Keeling, who was just graduating in chemistry from Caltech. Wexler arranged with Roger Revelle, who was also very interested in the IGY, to get a program going at Scripps. So Dave Keeling was hired at Scripps to do this program, with funding that was to come from the IGY and probably other places later.

Also, they wanted to do more ozone measurements from the IGY and they wanted to get a better network going. We recruited a fellow from Canada, by the name of Walter Komkyr to take over our ozone program. And this was really a very good selection because he was a very good physicist capable of doing very good, detailed measurements and he was good at building equipment. He went to work on the Dobsons and improved them and increased the Dobson network. We also arranged to have Dobsons go to the Antarctic stations.

**END OF SIDE 1**

**Interview with Robert N. Culnan**

**TAPE 1, SIDE 2**

Culnan: We were speaking about the ozone program, which was established during the IGY. This program went forward under Walt Komkyr's direction. The Mauna Loa station became the first of a group of stations that was established as a baseline for monitoring constituents in the atmosphere. Actually there are now four stations in the network that continued first in the Weather Bureau and then under NOAA when it was established. So Mauna Loa was staffed with a group of people willing to work up there on the mountain, and up very mountainous roads. They carried out the measurements during the period. Dave Keeling really managed the carbon dioxide program for Mauna Loa, set up the equipment and also established a laboratory back in La Jolla in which to carry out standardization of the measurements and do the analysis of data and put it in good order.

Droessler: Does that carbon dioxide observation at Mauna Loa continue today?

Culnan: It continues to this day and as you know it has become a very important program. It's the one place in the world where we now have a record starting with the IGY and going continuously. I think it's on this basis that we know the rate of change of carbon dioxide in the atmosphere, and this, with the computer modeling of the atmospheric circulation, has led to the basis for the global warming studies.

The other stations were added later when the Office of Meteorological Research set up three other locations for the clean air monitoring; this was done much after the IGY. It was done under Lester Machta's group in the Weather Bureau and later in OMR in ERL and NOAA.

The Antarctic program then became a main emphasis in the IGY, and Wexler really had a great interest in this, because he was interested not only in the atmosphere, but also in the oceans. He became the chief scientist in the IGY Antarctic program on loan from the Weather Bureau during the IGY period. He oversaw the development of the Antarctic program which was done of course in cooperation with the Navy, which provided the logistics. It was a very major program, and was also in cooperation with other countries. One interesting thing was of course the exchange of scientists between the Russians and the U.S. I know that this exchange program has been described by Morton Rubin and by Gordon Cartwright [AMS interviews], who were two of the people who spent a year at a Russian station in the Antarctic.

Droessler: Did you ever have the good fortune to visit in Antarctica?

Culnan: No, I never went to Antarctica. I just stayed home and looked after OMR while Wexler was away. He kept in touch with us. He was back now and then, but most

of his time was really given to the Antarctic program and also the meteorology program of the IGY during that period.

Droessler: Bob, you covered the carbon dioxide program, the ozone program, the Antarctic program, all of which got their start during the IGY. There was another program that got started during that time; this was a weather satellite program; as a result of the initiatives of the Soviet Union, we cranked up a satellite program in our country, and as I remember, the Weather Bureau played a critical role in the development and thinking through a weather satellite program. Will you talk about that program for awhile?

Culnan: Yes. This started I guess a bit after the IGY but certainly the thinking started when the first satellite flew and the U.S. then scrambled to get some satellites into space. Again, Wexler became very interested in the satellites because he thought this was a tool that could be very important to meteorology. One of the things he did was to get hold of an artist, and have him prepare a picture of what he thought the cloud structure would look like from satellites. Wexler sort of laid it out and the artist would sketch it and he kept working until he thought he had what was going to be a reasonable view of what we could get if we had satellites taking measurements of the atmosphere. I always remember this picture because it hung in his office afterwards, and was sort of the guiding light for the beginning of satellite meteorology. When Wexler died, why we gave the picture to Sig Fritz, who was one of the prime workers in the field and I guess Fritz has it hanging somewhere either in his house or in his office now, wherever he is.

What Wexler did was, he decided he'd start somebody actually working on the problem at the Weather Bureau. He picked Sig Fritz to do it because Fritz was engaged in radiation work and this was really a radiation problem. So Fritz started studying the problem and tried to find out things they could do, and the program just gradually grew into a small group. By that time, the first meteorological satellite was going to become available and Wexler asked Dave Johnson to come join Fritz and together they established the Meteorological Satellite Organization, which was part of OMR to begin with and later as it grew, it finally became an independent laboratory. But again, it was Wexler's early thinking that led to getting these things started and ready to go and pushed the program forward.

After the IGY, when the work was essentially done, Wexler turned his interest to meteorological satellites and what they could do for the weather services. I'm sure the first TYROS satellite was launched by that time. Wexler had decided that we should do something internationally using these data. He went to Geneva and sat down with a Russian by the name of Bugayev to work out an international program. They came out with the name of "World Weather Watch" for it. This later turned into the World Weather System. So again, we lost Wexler from the office for quite

awhile. We were left to run the place and saw him from time to time as he came home for a few weeks before he went off again on his international travels.

I think maybe this international work was Harry Wexler's undoing. He was having some health problems at that time and was on a diet and you know how hard it is to keep on a diet when you travel internationally. I think he was probably off it and that led to the heart attack that he had in 1962 when he was up at Woods Hole in the summer of 1962.

Droessler: Bob, after Harry Wexler's death, the Office of Meteorological Research continued and would you recall what happened during this period?

Culnan: This was sort of an interim period where we were without a director. There were two of us in the office of the director. Mort Rubin had come over to join the office and help out with the international work. Since Wexler had been spending a good part of his time on international studies, we decided we needed some support for that work too. So Morton and I were there and the division went on with the interim directors helping us. This lasted for about two years. Reichelderfer was apparently looking around for a replacement for Wexler, but it never actually happened during that period of time. But the work carried on all right and about 1964, it was announced that Dr. Reichelderfer was going to retire, and there would be a new director of the Weather Bureau. So there was a period then when they were searching for the new director to come in and finally Bob White was selected as the new director of the Weather Bureau. He arrived in 1964. We enjoyed his coming because he came before he actually took over the role of the chief. We had a month or so that he came in and worked in our office, getting acquainted with the Weather Bureau and planning ahead of time for the official date when he was to take over. We enjoyed having his ear to talk about research and such things: it was an interesting time.

When White became chief of the Weather Bureau, he immediately appointed an acting director. The first one was Lester Machta, who was also the director of the Special Projects Laboratory, which had been doing the work for the AEC and the Air Force and also some Weather Bureau work. This went on for about three months and we still didn't have a director so then the role passed to Frank Gifford down in Oak Ridge. He came up and was acting director for awhile. Then, after awhile, he'd been there long enough, it passed to Joe Smagorinsky. So we had three acting directors from within the division to help keep the work going that time. My job was to support each one and give some continuity to what was going on. It seemed to work out fairly well as far as I could tell.

Then, about that time, Bob White finally selected Jerome Spar to come down and take over as director of the Office of Meteorological Research. He came, I think,

about the end of 1964. He was with us for one year, and it was an interesting year. White was beginning to have his influence on the programs, and they were beginning to change. Vern Suomi came in as a chief scientist for a year, chief scientist actually to Bob White. So we saw some of him, but he spent most of his time working on various Weather Bureau problems and reported directly to White. Jerry Spar carried on with the research of the Weather Bureau. I don't think there were any radical changes I remember during that year. There probably would have been if Jerry had stayed on longer, but Jerry decided the government wasn't the place he wanted to work and after a year, decided to go back to NYU.

It was at that time the first re-organization was taking place. We were then to become the Environmental Science Services Administration (ESSA), with the research all going into what they called institutes--the Institutes for Environmental Research. Meteorology was to be one of the institutes, and the director of the Institutes was supposed to be in Boulder, Colorado. We did some preliminary planning to join with a group from the National Bureau of Standards that was coming over, the Central Radio Propagation Laboratory under the direction of Gordon Little. It was decided to move the majority of meteorological research--this was Bob White's decision--to Boulder. They selected out the parts that were considered to be research and they kept the parts that were development in a systems development division in the Weather Bureau.

At that point, I would have probably gone off to Boulder, but by that time, we had a new director of the institutes and that turned out to be Dr. George Benton from Johns Hopkins. When George came in, he asked me if I'd stay on as a liaison officer for the Institutes, working at headquarters which was then at Rockville, which I did. So I worked there actually for about six years as the liaison officer. It was supposed to have been only about two years, but they kept having trouble trying to get somebody from Boulder to come in to Washington. It seemed that nobody wanted to work in Washington. The only way I ever got to Boulder was that they finally closed the office after six years, also after Bill Hess took George Benton's place as director of the research arm at Boulder. By that time, we had become NOAA, the National Oceanic and Atmospheric Administration, and added oceanography to ESSA form NOAA.

Droessler: What year was it that you moved to Boulder?

Culnan: That was in 1971.

Droessler: So in 1971 you left Washington and headed to Boulder, Colorado. What were your duties out in the Boulder area?

Culnan: In going to Boulder, I joined the Office of Programs, which was Bill Hess' staff

office, Bill Hess being the director of Environmental Research Laboratories at that time, which was the name it took on when the agency became NOAA. Before, it had been the Institutes for Environmental Research. The Office of Programs was an office of about four or five people and each person in the office had several laboratories which he monitored and worked with to help with the management aspects at the headquarters level. Alan Shapley was originally the director of the Office of Programs and he went into data work, which he'd been doing on the side all through the years. He decided he would rather do that because he couldn't do both jobs.

The Office of Programs was then directed by Alan Thomas, who had come to the office about the same time that I came out there. He was formerly in the Office of Management and Budget, and he had worked on the budgets of the National Science Foundation. He was a physicist, by the way, so he had quite a background. He was recommended to Bill Hess by Joe Fletcher, who at that time was the assistant director of ERL. So Alan came and it was under his direction that we carried on the program office work, and it was a rather straightforward management program with various things happening, but no real highlights to point out.

I continued to work in the Office of Programs as deputy director of the office until about 1976 when I retired and then went into other activities outside of NOAA. They were non-meteorological-type things.

Droessler: Bob, that's a fascinating story you told about your work for the U.S. Weather Bureau, ESSA and NOAA. It all totals up to about thirty-nine years. You began in 1937, retired in 1976, and you had five years out of that period when you served as a lecturer at New York University. It was a great life that you had and you went through a lot of very interesting times and you worked with some great people.

What I thought we would do now is to move back in time to your early college education, and your opportunity to volunteer to work for the U.S. Weather Bureau. So let's go back to 1935-1936 when you were at San Diego State University and then moved on to the University of California at Berkeley.

Culnan: You will remember that those were the Depression days and opportunities were a bit limited and money was a bit short. So my first two years in college were at San Diego State. My family had just moved to San Diego when I graduated from high school. I had planned to go to UCLA, but we left Los Angeles so I went to San Diego State for a couple of years, mainly in math, chemistry and physics and then transferred up to University of California at Berkeley in the fall of 1935. Again, I was in chemistry and math and finally made my major mathematics. I graduated in 1937 from Berkeley and then had to decide where I would go from there. I needed a job, I had to have some money to keep going. I was around Berkeley until the fall of 1937. I'd taken a junior observer examination that they gave in the previous spring,

as I had some interest in meteorology because of a course in meteorology taken at San Diego State given by Dean Blake, who was then head of the weather office in San Diego. He was quite well-known for some of the papers he wrote there in the twenties and thirties. A very interesting gentleman with whom I was glad to get acquainted.

At Berkeley, I had come into contact with John Leighly, who was a geographer and also a climatologist and a very interesting person. He was very interested in meteorology also, and he gave me some advice where to go if I were interested in meteorology. He had suggested the route of starting in the Weather Bureau. So I took this examination, as that was the way you got into the Weather Bureau in those days--you had to start from the bottom. The bottom was anywhere from minor observer to junior observer. I chose the top--junior observer. At that time, the Weather Bureau was expanding its aviation service; it was establishing more stations and based on some studies done in California earlier.

I received an offer of a job as an observer in Sacramento, California, a new station. The job started in the fall of 1937. I went there and a fellow by the name of Hastings was sent in from Wyoming to head up the station and the assistant head was Jean Brown, who later did a lot of the work on Northern Hemisphere maps at NYU. He was a very good analyst and he and I became very good friends. I worked at Sacramento for about a year and then was transferred down to Oakland, which was a forecast center, still in an observer's status, but with a chance to learn more because there was forecasting and analysis going on there.

I hadn't been there very long when I applied to go to school. The Weather Bureau had advertised that you could apply for scholarships in the Weather Bureau and so I did. I applied for a scholarship to go to one of the universities to study meteorology. This was the first year for them, and they only selected only about four or five people to go. They selected people who had been in the Weather Bureau for quite a few years and I had been there only one year. Then I heard from the central office asking if I'd be interested in going to NYU as a graduate assistant to work there for a year and study on the side. I said this sounded interesting and so I received an offer from NYU to come there, which I did. I went and found the new department of meteorology headed up by Athelstan Spilhaus and the second man there was Gardner Emmons, who came there as the forecaster/analysis expert. Both were from MIT. Spilhaus was more a theoretician and also interested in meteorological instrumentation and oceanographic instrumentation as well.

Another person came in the same role as a graduate assistant and that was Charles Woffinden from the Salt Lake Weather Bureau. So the two of us joined the department which was really was in its first year. Spilhaus had been there the year before as a single individual and was a bit unhappy with the way meteorology was

being taught by another individual. He got the dean to change things and the new department really started there in 1938. In that year, I took the course in weather analysis and forecasting. We were allowed to take one course which we were busy working practically full time. I also audited the dynamic meteorology course. At the end of that year, I returned to Oakland and became what was called an aviation forecaster, making forecasts for the aviation routes out of Oakland going south to Burbank, north up towards Medford and east to Reno. We cranked out those forecasts regularly, day by day. That went on through the year--that would have been 1939-1940. Then, towards the end of the year, I realized I needed more education and I applied again for a scholarship and also wrote to Spilhaus and told him I'd be interested in coming back. He'd asked me at the end of the first year whether I really wanted to stay on. I said, no, I wanted to go back and do some more work in the Weather Bureau. By now, I realized the place to be was back at the University.

Pretty soon I got a call from Steve Lichtblau, who was working for Rossby at the central office at that time, asking me what was I going to do? Did I want a scholarship or was I going to NYU to accept Spilhaus' offer? I wasn't sure where I would go on the scholarship and I wanted to go back to NYU so I accepted the offer from NYU. I went back as an instructor in meteorology at NYU and that really began the next leg of my career.

It was very interesting. When I got there, I found that Ray Montgomery was there, an oceanographer who had joined the department and also--let me back up a bit. When I was coming to be there in the fall, Spilhaus had talked to Rossby, asking him what he might do to get me prepared to take on the job in the fall. Rossby suggested I ought to get some experience in upper air analysis during the summer ahead of time, so Spilhaus wrote and asked if I would come at the end of June and start my employment at NYU then and spend the first two months studying.

Droessler: Bob, I believe we've completed the loop now. We started this interview with your summer months at MIT, but let's go back to your six-year period at NYU and add something there that ought to be added to this historical account.

Culnan: That was a very interesting time. It was fun to work for Spilhaus. He was always full of ideas; things were always happening and Ray Montgomery was there as sort of the "steadying" influence. I finished my Master's degree in 1941 and so was spending my full time teaching. As the war courses went by, they got bigger and we got a bigger staff. We hired additional people to come in as instructors and we kept some of the officers who had graduated from the course also as instructors. We referred to them as "officer-instructors." George Cressman and Selmar Johnson were the first two of those. They were allowed to stay for a year, and then they were told by the Air Force that they could only stay that one year, but leave it to Spilhaus

and Rossby, they cooked up a scheme where we traded George Cressman for Josh Holland, who was in Chicago. So Josh Holland came to NYU and became an officer-instructor and was one of the very helpful people in the instructional work.

The next thing that happened was that Spilhaus decided that he was going into the military, and so he left us sometime in 1943. He took a commission and was assigned to Fort Monmouth in the observational-instrumentation work. When he left, he had to make some arrangements to keep the department going, and we ended up with a rather unusual arrangement. Jim Miller and I had been promoted to the assistant professor level. They were only allowed to have so many assistant professors at the university, so Jim became an adjunct professor and I was the assistant professor. Also Spilhaus had to figure out how the department was going to be managed when he was away. Gardner Emmons didn't want to be chairman, he was the next man in line; Ray Montgomery didn't want anything to do with administration--he was purely a science man. So we set up a committee of four: Gardner Emmons served as sort of acting chairman of the department and chair of the committee, and Ray Montgomery was on it. Jim Miller and I were on it. So whenever there was any kind of a problem or plan to be made, we did it with this committee of four. This was the way we ran the department for a year or so. It went on quite well.

There was one more thing that happened at that same time which probably had quite an effect on my career. Somebody had to do the administrative work for the department, working with the dean and so forth. Spilhaus, before he left, had decided that I ought to do that job. So they made me the executive secretary of the department, as well as being an assistant professor. I had to do all the work with the deans and other organizations in the college in managing the department. Also, I was responsible for the correspondence coming in and out, arrangements involving the Weather Bureau and the Air Force and students wanting to come; I was in charge of all that type of work.

That was where I got my first taste of administration. I guess I enjoyed it. That was probably the reason when I went back into the Weather Bureau and Wexler came aboard, I ended up as his assistant and helped manage programs. I thought I could probably do that role better than I could do research.

Droessler: That's an interesting experience you had in the academic community, too. I mean, you were part of the management of the department and you took over a very serious responsibility, and that is to work beyond the department with the rest of the university to be accountable for your students and your faculty, the budget and all of the other important details that go to make the organization work.

Culnan: There were a few other interesting things that went on during that time. We did a

few research projects. Earlier, we had done a couple of projects; one had been a study of tropical meteorology for the Air Force. It's interesting because this was one of the things that Rossby had mentioned earlier that needed to be done was to get more information about the tropics. So we had a contract and we divided up the work at that point. We brought Robert G. Stone, Bob Stone, the editor of the **Bulletin**, and a climatologist, to join us, to help on the climatological work of that project. Jim Miller decided he'd work on easterly waves, and I worked on cold air outbreaks going down into the Caribbean area, known as "northers in the Caribbean." Actually, that served as the basis for my thesis, and Jim Miller's thesis for his Master's was the easterly wave study.

We did one other study later. The Dobson ozone instrument had arrived at NYU back about 1940, and they were making continuous measurements. Louis Carstensen was at the Weather Bureau and he did the first ozone work, more a theoretical type thing, and also took the observations but other people helped. We managed to keep the observations going during the years, so after we had a series of them, in about 1944 we decided to do an analysis of them and that was my little piece of research: to try to understand the variation of ozone based on what went on in the upper atmosphere. We relied on the weather maps that the U.S. Weather Bureau had done at 13, 16 and 19 kilometers to get some data on the upper atmosphere. We found some correlations and I wrote up a paper under a small contract from the Weather Bureau. We sent the report down just about the time I went back to the Weather Bureau, and I actually received the report at the other end of the line! It went to the Joint Meteorological Committee, a committee of the Air Force, the Weather Bureau and the Navy at that time, because they were interested in the report and I guess they had given it their blessing at that time it was proposed.

Droessler: Did you have any opportunity to observe low density of ozone overhead, which we now call "ozone holes," either in the Northern Hemisphere or Antarctica?

Culnan: No, we hardly knew anything about how the ozone varied in the atmosphere at that time. We were measuring, of course, just the total ozone above NYU and we were interested in the big changes that took place. It was based on what would happen when low and high pressure systems would go by. It was quite highly correlated with temperature in the upper atmosphere. The thing we found was whenever we got a cold influx in the lower atmosphere with the warm air aloft, we ended up with the high ozone values above. There were some interesting correlations.

Droessler: Bob, I think we may be ready for my final question: who is Bob Culnan?

Culnan: If I don't know by this time, I should. I came up out of a family that was in the lumber business. I guess the reason my father was in the lumber business was because he grew up in Wisconsin. He was the oldest of a family of about five, and

when he graduated from high school, he went to work in the lumber business in Wisconsin and eventually wandered west, went out to the north woods and got into the lumbering work out there. He worked in the forests and the mills and such places, but he had a little business background because he'd gone to business school and somehow he'd worked up in the business of the companies and eventually was sent down to manage a retail lumberyard in Southern California by a big company on the West Coast. He worked his way up through that company and finally got into wholesale work. That's how we ended up living in Los Angeles. The reason I was born in Riverside was he was managing a small lumberyard in Riverside at that time, before we went to Los Angeles.

Droessler: Did you have some brothers and sisters?

Culnan: Yes. My mother came from Denver from a family of three, two twin boys and a daughter. She started out teaching school in Colorado, was born in Denver, came from a very conservative, restrictive family and she, being of a different nature, broke away from that and went to the West Coast to work out there as a secretary and met my father on a ferryboat one day, on a Sunday afternoon. I guess romance blossomed. Eventually came marriage, and they came to San Diego to start the retail lumber business career.

I had a brother who was born in San Diego two years ahead of me. Then I was born, when my folks got to Riverside, California, and I had one sister who was also born there before we left Riverside and went to Los Angeles. This was where we really grew up. I went through school from about the third grade on up through high school in Los Angeles.

Then at that time my father decided he'd go back into the retail lumber business and went to San Diego where he found an interesting position as general manager of a large retail yard down there. That was when I went to San Diego State for two years, and it was while I was there that I went through a course in meteorology under Dean Blake, who was the weatherman of San Diego in the Weather Bureau office. He made it interesting. I got interested in weather and I guess it sort of stayed with me because when I got to Berkeley, I also sat in on a course in climatology and weather given by John Leighly, who later became a well-known climatologist. In fact, he was with the Air Force during the war at Grand Rapids and taught climatology to the cadets. I saw him after the war, once in Washington, but he was really responsible for getting me into the Weather Bureau when I talked to him at Berkeley.

My brother had gone to Berkeley, and that's how I ended up there. He went to Berkeley after UCLA for a degree in electrical engineering. While he was doing graduate work, I came up there as a junior, stayed on and got my Bachelor's degree.

I was going to go on to graduate work, but then the opportunity in the Weather Bureau came and that's when I started the Weather Bureau career.

While I was in Berkeley, we had a group of us who were centered around one of the churches there and it was where our recreation was. I met a person who later became my wife. That was back in 1937, before I'd gone off to Sacramento and off to the first year at NYU. When I came back from NYU to Oakland again, why, I wondered what happened to her and I looked her up and a mutual friend arranged for us to come to dinner one night and that's when romance blossomed and we were engaged after a few months. Then I went off to NYU again, as an instructor that time. I came back a year later for a short period of time to get married and we started our lives in New York City. The "Cliffdwellers," as we called ourselves, living in the apartments, looking over the Harlem River. There was a little group of us from the NYU days in meteorology who lived down there in the apartments. Jim Miller was there and a bunch of officers who had to stay on as instructors and we had really a fun life among ourselves and in off-times. Our first daughter was born while we were in New York. This was before we left. We had two sons who were born in Washington, D.C., after we moved.

Droessler: Well, Bob, I want to thank you very much for inviting me here today to interview with you. I can't thank you enough for the very pleasant time that we've spent together. I've known you off and on for many years now, and I've always admired the work that you've done in a steady and consistent and very, very useful pattern to help advance our field of meteorology. You've served with some of the great people, and I'm sure you've helped them to carry out much of the work that they're credited for doing. Again, thank you very much. It was most enjoyable being here with you.

Culnan: Thanks very much, Earl. It was a real pleasure to see you again and a privilege to be interviewed; I never thought it would happen, but here it has.

Droessler: This is Earl Droessler, signing off on the interview with Robert Culnan.

**END OF INTERVIEW**