AMERICAN METEOROLOGICAL SOCIETY
UNIVERSITY CORPORATION FOR ATMOSPHERIC RESEARCH

TAPE RECORDED INTERVIEW PROJECT

Interview of Morton J. Rubin
14 December 1991

Interviewer: Gordon D. Cartwright

Cartwright: This is an interview with Morton J. Rubin on December 14, 1991. We are interviewing at his lovely house in Bethesda, Maryland, surrounded by many items of interest, particularly artistic and historical. I am Gordon D. Cartwright, now living in Geneva, but visiting with Morton in order to make this interview possible.

It is my understanding, Morton, in my conversation with Julius London in Vienna, that one of the purposes of these interviews is to record the historical developments of meteorology as much about the careers of the individuals who were involved. And so perhaps the interesting thing would be for you to give us a very brief account of why you got into meteorology and how. Would you now explain this, Mort?

Rubin: Well, it's a long story since I go back a long way. Let's say I was born in Philadelphia on the 15th of May, 1917. I grew up in Philadelphia, went to school there, my parents were what you might call the average middle-class [people]--they had no more than high school educations. There was no particular interest in science that I know of. My father had two younger brothers--their family were bakers, and these boys used to get up at 4:00 in the morning and run through the streets with pushcarts, delivering the bread, come home and have breakfast and go to school. Following regular public school, they'd go to a Hebrew school, then they'd come home and have a dinner. If they had time, they would study before they went to sleep. My father had various jobs as a clerk. He was a salesman who eventually ended up as a sales manager in a vacuum cleaner company. His second brother was a medical doctor; the youngest of the three became a teacher, but died at a young age in the flu epidemic at the end of World War I. I didn't know him, although there's a picture extant, a photograph, of me being held in his arms.

My mother's father was a tailor; there were seven children in that family. She was the third oldest and the eldest daughter. She did not work. She was a housewife after she was married. I think she did work in a factory somewhere before she married. At any rate, I went to the public schools and did the usual things. I had no particular interest in science, although I remember towards the end of my high school career, I was interested in chemistry--I had some physics and the math, of course, and the languages. I was a fairly good student. In fact, I had to transfer to

Harrisburg High School because my father went there for his work. I graduated first in my class. This was no big deal, I think, I don't know how demanding it was. I went to Penn State as a matter of course. I was the first in my family to get a university education, although I suppose my uncle became a doctor going to the University of Pennsylvania.

I entered Pennsylvania State University in the school of chemistry and physics. I was there for a year and a half, and this was from 1934 to the middle of 1936. In 1936, the family finances were such that I had to withdraw. I went to Philadelphia, got a job in a department store where I was responsible for the public address system. I had some friends at the time, close friends--there was a group of four of us--and one fellow had taken a civil service exam to be an apprentice joiner. He said, "Well, why don't you take one of the exams?" So I said, "Well, what do they have?" He said, "There's something called the 'minor observer' in meteorology." I said, "What do you have to know for that?" He said, "You need some math." I said, "No problem." He said, "You need physics." I said, "No problem." Then he said "Meteorology," and I said, "What's that?" And so I went to the library and got a book, the only thing they had at the time, a book by a man named Pick, from Great Britain, and this was an elementary meteorology book published by His Majesty's Stationery office. I read that. I took the exam and in a few months, I was appointed minor observer in the Weather Bureau in Philadelphia. This was a standard Weather Bureau office at the time. A number of people there: climatological summaries were put out there every month, and a weather and a crop bulletin every week, and my job was basically as general factotum, whatever I was told to do, I did. I didn't do much meteorology, I must say.

Mr. George S. Bliss was in charge at the time, and he had also written the booklet on weather forecasting that was published by the Weather Bureau. Eventually, he demanded that they withdraw that because they had changed it so much it was no longer his. He was from the old school; he used to come out and draw the weather map and things of that sort. There was a man there by the name of Richard Sampson, who was doing climatological/statistical work. He was very impressive in my mind. He had studied at Harvard with Robert Decourcey Ward, who was one of the very few eminent names that I had heard of at the time.

Well, in the midst of all this, Willis Gregg, who was Chief of the Weather Bureau, died; he was on a trip somewhere, and Francis Reichelderfer was appointed. Well, the uproar in the office in Philadelphia was something to behold. They moaned and groaned and said, "Don't know what the Weather Bureau's coming to, they're appointing a Navy man to be the head of the Bureau, and not only that, he studied in Norway and was a devotee of the polar front theory."

Cartwright: Yes, Morton, I recall being in the Weather Bureau, when there were these concerns. As a matter of interest, Willis Ray Gregg was attending an Air Transport Association meeting in Chicago when he died of a heart attack. Reichelderfer was a selection organized through the Academy of Sciences, with the famous physicist, R. A. Millikan, as the head of the group. The objective was to bring into the Weather Service a new beginning in the science and technology of meteorology, away from the old traditional type of director. Would you have any comment on that, Mort?

Rubin: Well, I was in no position to comment. I had no feeling for any of this because I had never really studied meteorology and I didn't know the personalities. But at any rate, there came an opportunity to take another examination. Oh, there's another interesting point here that really leads me to something that will happen much later. This was in 1939, so it must have been in November, when Admiral Byrd was leading his third expedition: it was called the U.S.-Antarctic Service at that time. That was funded by the government, largely. The previous two had been personally financed through donations. At any rate, Arnold Court was the meteorologist who was going with them, and they were on the North Star and left Boston and called into Philadelphia to pick up equipment. Apparently in the earlier transit, the mercurial barometer had broken, and I was designated to carry down a spare mercurial barometer to the Philadelphia Navy Yard, where the ship was docked. I carried this down very carefully, of course, and got onto the ship. It was absolutely exciting to me. I must have been 22 at the time, but somehow or other this feeling of all the equipment on board and the dogs howling and the men down in the wardroom--well, I finally found Arnold Court and turned over the barometer and of course got a receipt for it as I was required to do. This left a big impression in my mind, and I can refer back to that later on as to how that led me into some of the things I did later.

At any rate, there was an opportunity to take another exam for junior observer and I did that, passed, and was posted to Kylertown, Pennsylvania. This was in the mountains, in the Allegheny Mountains, with a population of 300.

Cartwright: Yes, I was posted at Cleveland, Ohio, in the forecast center there, and Kylertown was well known as a very critical observing point on the airway between Cleveland and Newark. You may remember that Newark was at that time, the main airport for all of New York City.

Rubin: Yes, that's true, and the reason I was so eager to go to Kylertown is that it was within thirty miles of Penn State, where there was a meteorology department at that time under Helmut Landsberg. Hans Neiburger was there also and one or two others. But at any rate, it opened up an opportunity for me and it was an interesting spot. It was a town of 300, there were five of us observers, many of us went on to do more important things than to be a weather observer in Kylertown, but it was interesting. And I used to work on the midnight shift and drive the thirty miles (I had a Model A Ford at the time) to take a course or two during the semester at Penn State in meteorology, and other subjects. I would take a few weeks off in the summer and do some summer course, and during that period I was married in 1940. I had occasion to meet C. G. Rossby. Landsberg had asked him to come up to Penn State to give some lectures to the students. So Rossby was at

Penn State for three days or so; he was assistant chief, or deputy chief, of the Weather Bureau at the time, for research and development probably. He was at the point of leaving the Weather Bureau and going to Chicago to establish the department there. He was a very inspiring man, very easy to be with and to talk to. I approached him specifically about career prospects because I had thought I would take a leave of absence from the Weather Bureau and finish my degree at Penn State and then come back to the Bureau. But at the time, the Bureau was rather hidebound; I think it was much influenced by a man named McDonald, who had very little interest in or regard for research or, I think, professional education. He possibly, himself, had not had very much of it; I'm not sure.

At any rate, I spoke with Rossby and Rossby said, "Well, there's no doubt if you want a career, get your degree." So that satisfied me, and my wife said [that] she would go to work at the college and I would do the studying. That was a good idea for me, so I did resign from the Weather Bureau and went on to Penn State and finished my work there.

At any rate, Penn State was an interesting place to be at the time. Some of the students were exceptionally gifted. There was Harlan Saylor, for one, who later went on to be, I think, a premier weather forecaster and analyst in the Weather Service, a man of high reputation and tremendous professionalism. Then there was George Cressman. Those two had been boyhood friends from the time of kindergarten, and George, of course, we know very well from his capabilities and his contributions. He, of course, later became the Director of the Weather Service. At any rate, they left in 1941; they went in as cadets into the Army Air Corps program, and went on to greater things. I continued until 1942.

The head of the department was Landsberg. He was a very interesting man, inspiring in a very quiet way. He was not anyone who would dominate, but he was very good. He'd gone originally there to do studies on ventilation in mines. This was in the School of Mineral Industries at Penn State, and after that he established some minimal program in meteorology, then Hans Neiburger came over from Germany. His field was atmospheric optics, but also he taught courses in instrumentation and one of his projects that he demanded that students do in the course in instrumentation was to design and build an instrument from scratch. Well, that was quite an interesting process. One learned a lot from that. I myself at the time built a hair hygrometer and tested this out in a small wind tunnel that we had. (Neiburger had brought along a lot of long, blond German hair which had never been permanent waved, so it was suitable for making hygrometers.) At any rate, there was another man who taught dynamic meteorology, Meir De Gani. He had just finished his doctorate at MIT, so he brought along that element of expertise to the program.

The time was 1942 and I had choices. I could either go into the Air Corps, as others were doing, or there were two offers of jobs that I had--one was with Pan American Airways in Brownsville, to do forecasting for Central America, and the other was Pan American Grace Airways in Peru, in Lima, to keep the airline going in South America. The reason the airline had to have a meteorological service was that they had airmail contracts with the U.S. Post Office, and this required that they follow a certain, I guess it was FAA procedures, and have weather reports and forecasts and so forth.

As you can imagine, in 1942 there was minimal possibility of getting the kind of information and analysis and radio communications and everything else in South America. They were government services, but they were mainly concerned with collecting climatological data. I spent a year and a half in Peru and during that period, we managed as best we could. We had weather stations along the coast, all the way down from Panama through Colombia, Ecuador, Peru, Bolivia, Chile, Argentina, and there were forecast offices in Lima and Santiago and Buenos Aires. So we learned as we went. There were no data, absolutely none, from the West, over the Pacific, the wide Pacific, and of course it was wartime and a lot of information was classified and coded in cipher. After a year and a half there--oh, I must say that Arnold Glaser was there at the time, and Arnold had just come out of MIT also. Arnold was very bright; he had come with a good background of physics and math, and he very quickly did all the required subjects and examinations for his PhD at MIT. Henry Houghton suggested that before he do his thesis, Arnold had better get some practical work, because he had never done any meteorology. Arnold took the job. We met before he went off to Bolivia. He ran the weather service for Panagra over there. He finally went back to the U.S. after a few years, went into the Army and taught at, I think, Chanute Field, teaching Army cadets.

I went off after a year and a half to run our Service in Chile. And there, basically, we had to do everything that a National Weather Service would ordinarily do. We established the stations, we operated the stations, we trained the observers, and we had our Central Forecast Group. It was a very difficult process to keep an airline running because the west coast weather, particularly in the south, produced a lot of fog, a lot of winter storms and the airways were such that they never flew at night--they could fly until half an hour after dark and that was it. Sometimes weather was so bad we would have planes lined up every which way, seven or eight planes lined up not able to get into Santiago. We had learned very quickly though --We had two stations up in the mountains, very high, over 3,000 meters, and we watched those very carefully. Pressure changes were very important at that level; we had no understanding at all about jet streams, we had no upper air soundings, we had a few pilot balloon stations. I think in 1945, when the war ended, I was commissioned by the company to go back and purchase equipment: mercurial barometers, electric anemometers, and we also brought hydrogen generators and theodelites so that we could have a pilot balloon program. That helped a little bit, but of course not too much.

There was very little that we knew about modern developments in meteorology at the time. We were mainly sort of seat of the pants people. We were far away from the centers, and of course this was still in the immediate postwar era. So many of developments in meteorology, modern as we knew them later, hadn't come. We didn't get any of the journals of that sort.

But we did learn, and at any rate, after seven years in South America, I thought that this was enough. There was no future and I was as far as I thought I would go. I wasn't learning very much. In 1949, I decided I would resign and go back to the United States. I put in several applications for Civil Service jobs--nothing had come through at the time, so I went home anyway. Basically, I had a wife and two children at the time with no real job to go to.

It turned out that Arnold Glaser, after he left the Army, had gone back to MIT to finish his thesis and fortuitously, the U.S. Weather Bureau--and it was Harry Wexler who was director of research at the time--had established a Southern Hemisphere Analysis Project. On my way back, the director of our activities in South America had told me about this and he said I should apply there once he knew that I finally was leaving. So I did and I got a job at MIT as a research associate. This project had been established at MIT with Weather Bureau funds, and Arnold Glaser was the one who was running it. When I applied, he recommended me and I got the job and he left. So I was at MIT for three years. That was an extremely interesting time and opened up a lot of perspectives for me in meteorology. I went there in 1949; Hurd Willett was ostensibly the supervisor. Henry Houghton, of course, was department head; Tom Malone; Ed Lorenz--in fact, we all used to play bridge at lunchtime together; and Alan Bemis was there; Polly Austin in the radar group.

So I was able to study at the same time and get my Master's degree. My thesis topic--and this brings me back to my experience with the Byrd expedition--the thesis that I took was to do a study to explain the so-called disappearance of the tropopause in the Antarctic, postulated by Arnold Court--he had done the first radiosonde observations during that expedition in 1939-41. He recorded the fact that the tropopause seemed to disappear at certain elevations. Well, I decided that would be my thesis topic, and I looked upon it as caused by radiation, rather than, as we know now, there's a lot of dynamics in there--but anyway that was basically the first individual research project that I ever did.

At that point, as I said, it was interesting to me. Hurd Willett, who was my thesis advisor, was at that time very much involved, and had been for many years, in large-scale synoptic analysis and he had developed certain indices for determining the fluctuations in the energies in the atmosphere. He was not a theoretician in a sense, but he had a very fine understanding of how the atmosphere worked in a descriptive way. He used to teach the course in five-day forecasting. Of course, Namias had been with him in earlier days and of course, Harry Wexler had been at MIT as well. So that was it, and then of course they had this very fine research program in radar meteorology with Allan Bemis and Polly Austin, Herb Ligda, who later went to Texas--he died unfortunately not long after that. Ed Lorenz, of course, was a dynamicist of very high standing and Victor Starr had come over from Chicago. He was a very interesting man, very quiet, reserved, but I remember he used to come in to look at the work that we were doing in the Southern Hemisphere Analysis Project. By the way, Harry van Loon had come over from Denmark to work with me as an assistant. He later went on to achieve quite a lot of recognition in his analysis of large-scale circulations.

Victor Starr used to come in and say, "Oh, that's very, very important work." In fact, a lot of the students used this Southern Hemisphere material for their thesis. Bill Gibbs, who had come over from Australia on a Commonwealth fellowship for a year, did use those maps for his thesis. Even Patrick Obassi, who became the Secretary-General of the WMO; his thesis was based on those maps as well. Other people who came through and looked at these were people like Harald Sverdrup from Norway, an oceanographer who was very interested in the interactions between the large-scale atmospheric dynamics as related to the oceans, and Walter Munk as well from California, from Scripps. He was interested in that time in studying very long waves in the oceans, and his idea was that they were generated in the Southern Hemisphere and actually came across the Equator into the Northern Hemisphere, so he was looking at the maps. There were a number of people who were there.

Now the problem with that project was that originally the attempt was to make it synoptic, in a sense, keep it up to date. But that was absolutely impossible because the kinds of data we had to use to fill out the oceans (and the oceans were very, very sparsely observed). At least we didn't get the data on a regular basis and the ship lanes were not where we needed them. The Southern Hemisphere, particularly in the higher latitudes, was devoid of ship reports. So we had to go back historically in a sense, and through a cooperative activity--at that time the Weather Bureau of the Union in South Africa had, along with the Australian Weather Bureau, a system where they would give secret ciphers to the whaling ships that would be going into the high latitudes during the summer and fall season to catch whales. They would then send weather reports. They would come back to those two centers by radio. The forecast would be made and the whalers would get the benefit of it without having to reveal where they were catching the whales.

Cartwright: I see. It was a commercial secret in a sense, so that they wouldn't give away their position where they were catching whales.

Rubin: Exactly.

Cartwright: I recall when the IGY was going on that they also produced a Southern Hemisphere series as part of the global analysis...

Rubin: That was ongoing, and I'll come to that in a moment. I had something else to do with that. At any rate, we would get those by mail and they would come back very late so we were really not keeping up, not by far. I think eventually we were able to do only about two years of analysis. However, that was a period, I think, when people were beginning to think more globally rather than regionally in terms of analysis and the development of forecasting models and so forth. Although at that time already, people were beginning to think in terms of mathematical models in that sense.

Cartwright: Did this project continue at MIT or where did it go after--?

Rubin: Well, it was three years at MIT, and then the people there thought it had been there long enough. Wexler used to come around pretty regularly, too. Wexler himself was an enthusiast, a man of wide talents. Again, full of enthusiasm, but never dogmatic or overbearing. He was a very easy man to know and to like and respect. He had a wide-ranging understanding of more than just meteorology, too. At any rate, it was transferred to the Weather Bureau and we kept it at Boston for two years, and it went on there until 1954.

Now at that time, the Weather Bureau decided that they had put enough effort into this and it turned out that the South Africans wanted to carry it on. Now Harry van Loon had left us about a year before that to go to work in South Africa as a meteorologist, and an arrangement was made for me to go down to South Africa to help them initiate their continuation of this particular project. That was a wonderful opportunity and my activities were jointly funded by the Weather Bureau and the Air Force Cambridge Geophysical Research Directorate, I think it was called at the time. And Bob White, who had gotten his degree at MIT during the period that I was there--we'd become friendly then--he had some money to put into this, so I went to South Africa for a year. That was again an experience where a Weather Service was in the stages of modernizing itself. The previous director had been Schumann, who had had a good reputation meteorologically. The people who followed him, unfortunately, did not have his standing as a scientist. But at any rate, they had an ongoing service and when I was there, I was able to interact with them. There were a few Germans who had come out of Germany after the war--Eberhart Vowinckl was one--he was a dynamic younger person; he had been in the German Navy during the war. He later went on to McGill, to become a professor. There was an interesting man named Nagel, who was an instrumentation specialist. He had been involved in developing automatic weather stations to put out at sea during the war. They were put out by submarines, and he used to go on these hazardous journeys in submarines to put these out. And there was Andrew Drummond--he was a British radiation instrumentation specialist. He later went to work for Eppley. But the year that I spent there was interesting for me from the point of view that I was able to devote myself to some additional research and I did one paper on the relationship between the dynamics and the synoptic distributions and the rainfall patterns over South Africa. They of course are very dependent on rainfall and some of the crop areas are very marginal. So this was an interesting paper; no one there had thought to do that and the year I was there I did that. And I did another similar one for South America as well. That ended my year in South Africa.

September of 1955 was my first posting to Washington, and Harry Wexler said that he wanted me to go to work in what became the Geophysical Fluid Dynamics Laboratory under Joe Smagorinsky, who had come down from Princeton from that group with von Neumann and Jule Charney. They were going to do global analysis and studies with their models and computers. This was not a field that I was particularly knowledgeable about or interested in because I'd been so far behind in some of those new developments that I felt that it was nothing I really wanted to do, although, had I been forced to do it, I would have, I imagine.

So I worked for about three or four weeks with Jerry Namias and Bill Klein. They were the Weather Bureau's long-range forecasters at the time, and I had of course to get used again to Northern Hemisphere analysis. I'd been completely turned around in the Southern Hemisphere ever since 1942. So that was a bit tricky, but nevertheless I was beginning to fit into that milieu. Oh, I remember what I told Wexler when I came back: I'd heard about the International Geophysical Year and I knew there was an Antarctic program so I told him that this was what I really wanted to do. He said, "No, no, you've got to do this other one." So I said, "Well, all right, if I have to." And he went off to a meeting in Paris, or perhaps it was Belgium (I'm not quite sure). It was at that point that the United States offered to establish an Antarctic Weather Central in Antarctica to collate data and do analysis and make forecasts. This would be manned by people from a number of countries, but run by the United States. Well, once they offered to do that he realized he needed someone with experience to do it and he came back and said, "OK, you're in the Antarctic." Well, that was like having a Christmas and a birthday all together! I started from scratch in that, of course, but there was still a fair bit of information available in the literature from earlier expeditions, from various countries, and some of the Australian Southern Hemisphere analysis, from New Zealand and so forth. I got together a background, a library, and we had people coming in from France from Argentina, from the Soviet Union, from New Zealand and other countries.

We had some of these people in Washington, and of course, I had to give them some of the background I had had in forecasting without very much data and the experience of others in the literature. We went through, in a sense simulated data and did analysis and made some forecasts. That was quite an interesting thing that we had to do. In the interim, Harry Wexler had asked me to do a trip to South America prior to the IGY to try to get weather services in Ecuador, Peru, Bolivia, Chile and Argentina to undertake programs of upper air soundings so that we would have these data during the IGY. It's an area that I knew well, of course, and I was able to speak Spanish with no hesitation. It was successful in the sense that we got countries to take on the responsibility, and the Weather Bureau provided the equipment. U.S. people went down to train them. There had been a cadre, actually, of people trained during the war at a school that the United States ran in Medellin for weather observers. And these people were available. In fact, when I worked in South America before, a number of them worked for me.

Cartwright: That must have been, Morton, when I was in charge of the observation system in the Weather Bureau. I remember Hugh Odishaw coming in and discussing some of these prospects. Also, Sverdrup came in on one occasion to review the observing possibilities in that part of the world. So there was a major effort on the part of the U.S. in trying to build that up before the IGY program actually started.

Rubin: I think, really, that was a stimulation for so many new projects to be started for--an outreach program to get people involved in these international efforts who never had any feeling for it. Many of the South Americans were willing to do this, but they didn't have the capability and very often they didn't have the trained people. And that was unfortunate. I recall at one time when I had come back to the Weather Bureau, Dr. Reichelderfer asked me to give a paper at a meeting of the U.S. Committee on the United Nations, and the thesis that I took was exactly this problem. In South America where you had sub-professional people doing work that requires trained and skilled professional meteorologists. Consequently, the value of the work did not cause the government to fund them adequately. This was a vicious cycle. It seemed to me that the IGY opportunity gave them the ability then to develop these infrastructures and the people to take off from there. And I imagine that has gone on over the years since then. And the World Meteorological Organization has had a lot to do with this, with the training, and you're the expert on that of course.

At any rate, we had this first group that I took down to Little America in the Antarctic. Well, I should go back and say I first met you, Gordon, in 1956, I think, I came to Washington in 1955, and it was soon evident that you were going to be the first exchange scientist with the Soviet Union at their station in Mirnyy . I think we interacted quite a lot on that and you possibly called on me to give you some ideas about the meteorology of the area. So that's when we first met, really, and it's been a good, warm relationship ever since.

Cartwright: Who were they? Do you remember?

Rubin: There was Bill Morland, I think, was the first one in charge of our group. We had Jean Ault from France was there, and there was Arruiz, an Argentinian naval officer. He later became an admiral and was their naval attache here in Washington. We had two young Americans, Bruce Lieske and Ron Taylor. There was a Russian there, Rostroguyev. That was pretty good, and also one of the jobs that I had to do—

END OF TAPE ONE, SIDE ONE

Interview of Morton Rubin

TAPE ONE, SIDE TWO

Cartwright: Would you continue now, you may remember, Morton, you were at Little America and the International Weather Central?

Rubin: That was at Little America, and that would have been 1956. It was late '56, early '57. Gordon Cartwright, you of course at that time had gone down with the Russians to be at the Mirnyy Station. You'll tell us about that later. And this was at Little America. I had been there the year before (1955-56) with Deep Freeze One, before the IGY stations were open.

Once we got our analysis started--we had essentially thirteen radiosonde stations; we were doing an upper air analysis. When I'd been in South America and doing the Southern Hemisphere program, we did no upper air analysis. It was just impossible. At any rate, on the basis of that, we had a station at the South Pole and one at Byrd Station, and the Russians had put one in part way up to where they eventually were going to, Vostok, and this was called Vostok I. They had a line of stations there. So on the basis of that, I was able to do an upper air analysis and to determine, to my satisfaction, the heights of the Byrd station and the South Pole station--there had been no ground traverse up there to determine that.

An interesting feature there was the planes had radioaltimeters. They were flying back to me and telling me that I had given the height of the station as too high. It wasn't until later that we realized that the radioaltimeters were penetrating the snow and they were getting reflection from some layer below the surface of the snow. This, of course, later came out to be a very interesting tool in probing the depths of the ice; instead of seismically, they could do it with radio waves.

Cartwright: Would you correct an impression, Morton? I thought you were back in Washington when I went with the Russians. If that is so, then they had not yet established Vostok I, because that was done after I got to Mirnyy sometime in 1957.

Rubin: You're right, Gordon, you're right. We did not have the Russian station there, but I think they had them at Komsomolskay and Pionerskaya, which was part way up. But nevertheless, you're right. This was the austral summer of 1956-57 that I was there. And I left the group there to continue some of the other work that I was doing in Washington to get ready to train the next group that was going to replace them at the International Antarctic Weather Central.

The group that I had left there were basically doing synoptic analysis over the Antarctic and the Southern oceans. They were not primarily a forecasting center; they were an analysis center and a number of interesting papers came out of that work: a joint paper by Ault, Alvarez and Rostroguyev, which was a very good international cooperation--a Frenchman, a Russian and an Argentinian. Then there were a few others-- Bruce Lieske and Ron Taylor had a paper. It's a bit vague in my mind now, but basically, they were there doing research on these things, in fact, that were published in a number of places.

In the meantime, after I left them there to come back to Washington to prepare for the next group, I got involved in what was called the "SCAR," the Special Committee on Antarctic Research. It hadn't been established as a full scientific committee of the ICSU, and I had to go to Europe at the time for one of the meetings. It was in London, and Hurd Willett suggested that I get in touch with Sir George Simpson, who had been the meteorologist with the second Scott Expedition to the Antarctic. He had been the director of the met service in India, and later on was the director of the met service in the U.K. He was a very gracious man who was quite old at the time--this would have been in 1957, I guess, and he very graciously invited me to meet him at his club, the Athenaeum, in London, and we had a very interesting to me and possibly to him too, time at lunch and afterwards talking. I was regaled with his stories of what went on with the Scott Expedition and some of his meteorological activities and later work. So that was a very eminent meteorologist who had contributed a lot in terms of understanding the climate at the time. I think, also, he had done a global radiation study that had to do with the climate. He died, I think, a year or two later.

These activities got me more involved in international meteorological concerns of research and particularly on the scientific side. When I left South America, that was the end of my direct operational activities. I was mainly involved in research and the administration of research. So we had the second group go down. But those things are all on record. I didn't go down with that group; they went on their own because one of the jobs that Harry Wexler had given me was to find a replacement to go down with the Russians where Gordon Cartwright had been for the year.

Well, this was an interesting problem. It turned out that all the people who wanted to go I didn't consider qualified. And those who were qualified didn't want to go. So about six weeks before we had to send somebody, we had to notify the USSR Academy of Sciences who that person would be. We were stuck, so I went in to see Harry Wexler and I said, "When it comes right down to it, there's no one better qualified than I am." So he said, "Well, you're right." And I don't know whether he had been thinking along those lines himself. So I said, "Well, I'll just have to go." This was a difficult decision to make because--not for me, necessarily, because the Antarctic was in my blood and I think once that's in your blood, you're never cured of that particular disease. But I had a wife and three children, one of whom was a teenager and they aren't necessarily the easiest people in the world to get along with, so I called my wife on the phone. She knew that I'd had this problem and I said, "What would you think if I went to the Antarctic for the year?" (Actually, it turned out I was away for seventeen months, but it was a year basically.) There was a sort of a pause, and I think I heard sort of a gulp over the phone and she said, "Well, if that's what you want to do, then of course I'm behind you." So that's the way that worked out.

I guess those things are very important. The give and take between a man and his wife and the ability to recognize that what one has to do at a certain time is important, and people on one side or the other will make a sacrifice.

Cartwright: I can tell you that she really did make a sacrifice. When I came back from my stint, I called her on the telephone to tell her that I was back and that you were well-established there and she broke into tears for a brief period. She certainly did miss you, Morton.

Rubin: She never told me that. I mean, not that she didn't tell me that she missed me but that she had cried. But that period also was interesting to me because Harry Wexler was the chief scientist for the Antarctic; he was, as I may have mentioned, an exuberant man full of ideas and never abrasive, never overbearing, and he used to bring special people into seminars. We'd had John von Neumann, for example, from Princeton; he would come down to Washington and give us a seminar. We had an interesting research group under Harry Wexler. There was Morris Tepper and Lester Machta and Glenn Briar, Jim Angell was there and all with their respective fields of interest. Don Peck was another one working with Lester Machta on air quality and in that environment and with those seminars, one was very well-educated, I think, as things went along. Of course, Bob Culnan was there as a good right-hand man to Harry Wexler, who moved things along very well.

At any rate, I went down with the Russians. The head of the expedition, the land expedition, was Yevgeny Tolstykov, who was a meteorologist and he later became the deputy director of the Hydromet. Service in the Soviet Union, a man with whom I became very friendly, very, very friendly. And as is the Russian custom, when we used to see each other out in the Antarctic at meetings or so, we always embraced and kissed each other so that indicated to me that they don't do that ordinarily with people who are not of their own culture. And the head of the meteorology group there was Victor Antonovich Bugayev. He had come from Uzbekistan. Although he was a Russian, he was the head of the Hydromet Service in Uzbekistan and he became a very eminent figure and worked very closely with Harry Wexler later on under the aegis of the WMO on the satellite program, the international satellite program.

Cartwright: And the design of the World Weather Watch in the way which is pretty much still in existence, Morton. You might note that all three, that is, my director, my chief of the expedition--Alexei Treshnikov, Tolstykov, and Bugayev--are now all dead. It's a loss to the whole system, I think, because they were splendid people, very active, very dynamic and really great leaders in their way.

Rubin: Treshnikov, of course, was an oceanographer and he later was the head of the Arctic and Antarctic Research Institute. I didn't know that he had died; this must be very recent.

Cartwright: About a month ago.

Rubin: Ah, yes. A shame. At any rate, we had some young meteorologists there. Bugayev was a very, very easy man to be with, very erudite, very cultured, sophisticated man, not what we ordinarily think of perhaps, an uncouth Russian. He was an excellent person. And we, and I know you too, Gordon, kept up a good friendship with him for a long, long time. There was a young meteorologist who Bugayev had brought along named George Gruza. He was doing observing mainly, I don't know what he did in the way of research. There wasn't too much research that went on there. I think there was some that I perhaps wasn't aware of, but Byeloff was doing some atmospheric optics, and there was some ozone measurements, as I recall, and Gruza later became an important figure in the Hydromet Center in Moscow doing data analysis and developing models for extended forecasting. He recently has been in the United States on an exchange that people at the National Weather Service Climatic Center are interested in models that he has developed. He was happy to come because he didn't have the computers that are capable of doing what needed to be done. So there's a very good exchange there.

Cartwright: Could I ask a question at this point, Mort? You are certainly well aware of the ozone hole over the Antarctic. I recall that when I was with the Russians, at the time of the actual beginning of the International Geophysical Year, they began to apply a very large correction to the high levels of the radiosonde data. Now, my recollection was that it was between fifteen and twenty degrees at the very top of the plate. The reason that I ask that question is that I do not remember temperatures as low as those now being recorded over the Antarctic, where -78 and -80 seem to be fairly common temperatures now at the very high altitudes. These temperatures are partly responsible, I believe, for the diminishing of the ozone.

Rubin: Well, it's not a field that I know too much about. I think the depletion of the ozone is rather due to a chemical process and the fact that they have to have some ice crystals, water vapor and water in some form--

Cartwright: Stratospheric clouds.

Rubin: Right. I don't know. I'm not so sure that the radiosondes we had, although some of them did get to pretty good heights, I don't know that they got to the point where we're getting data now. I don't know that -78 or -80 would have been unusual, even in those days.

At any rate, I was interested in ozone myself at the time, and I did a little bit of research on it to see what had been done, but not ozone at the vertical, but ozone at the ground, and I had an idea that some of the drifting snow and the static discharges as a consequence of that, with the breaking up of the snow crystals would cause some increase in ozone levels at the ground.

Cartwright: I can confirm that in another way. I was in Texas during the end of the Great Dust Bowl period, and one of the professors at SMU did a paper on ozone in the atmosphere created by the interaction between the dust and the atmosphere.

Rubin: I think it was a static discharge. You could get it from dust or particularly snow crystals still in the Antarctic. But there were a number of things going on. I must say the Russians were interested in their work, they were dedicated to it, they were easy to be with. I never had any problems at all with them, except once a mild disagreement with a fellow, but he had had a bit too much to drink. That was understandable. But the environment socially was very good for me and it was interesting.

While I was there, of course, I prepared upper air maps at 700 and 500 millibars, and the data that I collected there and worked on resulted in what was, I guess, one of the first determinations of the heat transport across the Antarctic. I developed two papers from that, one was that and the other was a heat and mass balance study of the Antarctic.

Cartwright: Were you able to establish clearly the strength of the circumpolar vortex with that data that you had?

Rubin: Oh, yes, sure, to the extent that we could reach the high levels. But in fact we went in, I think, with the Russians earlier than we had ever gone in before or since. We got down there at the end of October or very early November, you remember that, Gordon, you were there.

Cartwright: When you say you got in, you may remember that you got within only about thirty kilometers of the continent itself. And that I flew out in a helicopter to meet and properly welcome you.

Rubin: Yes, you did welcome me very warmly, I must say. But we did go in by helicopter, you're right, and did get into the base. Eventually, the ship was able to get close enough when the ice broke up. But on the way back, I left in February of 1959, and I'd been there for fifteen months, and as I was going back to South Africa where I was to leave the ship, I had a radiogram from Harry Wexler saying he wanted me to go to Australia where there was going to be an Antarctic meteorological conference. Well, I very quickly had to get my thoughts together on this week-long or ten-day trip back to Capetown and write a paper (fortunately I had a typewriter with me), and I had to make some slides. I had a camera and some drawings and so forth and made the slides for this paper. They were very crude, but they worked. So that paper was given there, and there was a publication that came out on Antarctic meteorology. I think it must have been one of the first compilations of internationally-produced research on the Antarctic.

Cartwright: Had the Australians set up the Antarctic Analysis Center by that time? I thought they did, because you may remember that one of the aftermaths of the Little America center that you established was that the Australians finally picked up that idea.

Rubin: I was coming to that, but I'm glad you jogged my memory about it. That publication came out. Meanwhile, as you say, Gordon, the Antarctic Weather Central had closed down; this would have been at the beginning of 1958--no, the Antarctic Weather Central continued until 1959, so they were there until 1959. Then the Australians decided that they would continue that analysis program because they'd had a very good program in the Antarctic for a number of years.

Cartwright: Who was the director of that?

Rubin: The director of that was Henry Philpott, who was a meteorologist from the Australian service and we assigned Tom Gray there, from the United States, and there was a Russian whose name I can't remember, a few others and then the Australians. That went on for a number of years until it became amalgamated some years back with their general synoptic service. They were doing certainly hemispheric, and maybe even global analysis. So that was in a sense a seminal project, which again indicated to people that it could be done, and with the increase in better communications and more stations and satellite data and so forth, the whole world opened up in that area. I sometimes sit and ponder what I could have done with all of those data in 1942, when I was in South America for the first time working with a very small array of surface stations along the west coast of South America.

At any rate, these are the things that happen. And you plant a seed and they grow and the technology then helps you to advance.

Cartwright: One of the outcomes of that seed, Morton, you may know, is a new Southern Ocean and Antarctic Analysis Center set up in Tasmania and headed by our good friend, Dr. Garth Paltridge. I talked with Garth in Vienna during the IUGG meeting, and it looks like this could become one of the very important analysis centers in the Southern Hemisphere.

Rubin: Actually I think it's not so much an analysis center as a research center. It combines a number of groups. I just recently came back from Australia, came back in October, and I had a chance to talk with the people at the University and at the Weather Service about this. It's a program of the Australian government to establish centers of excellence all around the country, much like we have with our cooperative groups that NOAA establishes. That group under Paltridge, as you say, will have university people from the University of Melbourne, Bill Budd basically and a few others, and they have a group from the CSIRO, which will be doing oceanography and an Antarctic group of glaciology from the Australian National Antarctic Research Expeditions. So it is a fine center; the government intends to fund it, and I think this mix of all of these disciplines oriented to geophysics will be very good.

So that continued, and the United States of course was supportive. For a few years, we had people there. So we come now back to 1959 when I got back to Washington and the Weather Bureau established a polar research group, of which I was the head. I had some of these people who had been in the Antarctic with me, and some who had not. Bill Weyant was one, Bruce Lieske went back to the university and I think Ron Taylor went on to get his doctorate at UCLA. That was the activity then.

I did forget one thing about the Navy group at McMurdo. They had established themselves at McMurdo where they had their large facility and the planes and the ships came in and so forth. John Mirabito was the meteorologist then. He went down with Deep Freeze I and did a fantastic job with, again, very little data, making forecasts for these flights which flew nearly across the continent and back. It was nerve-wracking for him, I'm sure, but he did a very good one. He had people working with him, Navy aerologists, of course.

Cartwright: This was before or during the IGY?

Rubin: This was Deep Freeze I, just before the IGY, at the beginning of the IGY in 1957. He was followed by others--Bill Lanterman was one and the others--I lost track of that activity.

Cartwright: I think they continued the assignment of Americans and Russians to each others' center, and I recall that Dr. Tarakanov was based at McMurdo for awhile as the Russian liaison scientist.

Rubin: The year that I left, apparently they did not have an exchange at Mirnny, but the year after that, with the lapse of a year, the exchange person, who was a glaciologist, was Gil Dewart. He was there during the time when the meteorological houses burnt down and there were seven people killed. The house that you lived in, Gordon, and that I had lived in burnt down during one of these Antarctic hurricane winter storms. When I was there, we had five periods of wind velocities of over fifty meters per second. The houses shook, and it was just impossible. It was during one of those storms that there was an electrical short-circuit and the house burnt down and seven men were killed, including your friend, Oscar Krichak.

Cartwright: The chief of the meteorological group when I was there, Oscar Krichak.

Rubin: And one of the observers when I was there, a young man named Popoff, he was killed at that same time.

Cartwright: I remember him. Fine young man.

Rubin: Well, the Polar Research Group was composed of a number of people who had been in the Antarctic and we had Ed Flowers, who spent the first year at the South Pole observing in the radiation studies, and the Dobson--they had a Dobson spectrophotometer there.

Cartwright: And I remember they had an ozone sonde that had been developed by Vern Suomi. It didn't work very well the first year, so they only got a very small amount of data.

Rubin: Was that Suomi's development? OK, well at any rate, Ed Flowers had come back and when Kirby Hanson came back, he had been the second year at the Pole and did a very definitive radiation balance study in the lower levels and he and I wrote a paper on that. There was Fedor Ostopov who was an oceanographer, and he was looking for oceanographic data. Bill Weyant was with us and his field was more synoptic and dynamic. We did a number of papers together; I can't remember all the ones that I did myself, but I certainly did a number.

We were basically utilizing the IGY data, and that went on for three years. We were learning all the time; in fact, the one paper which I did, based on some glaciological data, snow accumulation data, some of the transport data that I had, determined basically what the distribution and mean precipitation was on the Antarctic continent and the mass balance. It turned out that simultaneously a Soviet scientist, Ketlyakov, his name is, was doing a similar paper. When I had finished my paper and submitted it for publication to the AGU, his paper was called to my attention. And of course I acknowledged in the paper as an addendum that this had happened with absolutely no understanding on his part that I was doing one or vice-versa.

Cartwright: You may remember that I met him just this year in Bremen at the SCAR meeting and he recalled his association very warmly. Still a very active man.

Rubin: Well, that's good. I'm glad to hear that. Our figures were essentially the same. I think we came up with something like 19 cm. overall per year. Of course, Antarctica is a desert and something like 2-3 cm. at the Pole--the fact is, it just doesn't melt and run off, that's why it accumulates. So we were learning as we went and the other interesting aspect of that was the activities through the Scientific Committee on Antarctic Research, which brought together--Harry Wexler was one of the prime movers in that and I followed after he died--in 1962, I took up his activities there and I was for many years the chairman of the working group on meteorology under SCAR. These meetings brought scientists together from many countries, all the countries participating in the Antarctic research, sharing ideas, developing programs and making recommendations. I remember one of the first recommendations we made very early on was that we needed at least ten years of data before we should consider cutting back on anything in the Antarctic. Well, as you know, of course, this has gone on for thirty years and we still don't know everything that we have to know about Antarctica and its role in the global circulation, which is a very important one, of course.

Cartwright: You will tell us later, I hope, Morton, of your work at ?SCAR where you made a comprehensive review of the very early meteorological data and its implication for climate change. You'll pick that up later.

Rubin: I hope I can remember what I did, Gordon. We found that stimulating each other brought out ideas and I think even in those very early days, in the early sixties, the recommendations we made are still valid today, and we even in those days brought up the idea that we had to know more about the atmospheric chemistry in the Antarctic, and here you have the ozone problem, of course. A lot of the deficiencies in any activity at the time in was the lack of technology to accomplish these. The satellite came in later, of course, and they had ozone measurements from satellites and vertical soundings and so forth. So we had a very exciting period and it was wonderful to be in from the very early stages because anything you touched turned out to be gold, in a sense. There was always something new to find out and it was relatively easy, although I don't know that we were able to do everything we could have done because we didn't have the technology.

Cartwright: Could I comment on this point, Mort? I attended the recent assembly of SCAR in Bremen. "Antarctica and Global Concerns" was the title of this assembly and as you say, it is becoming every year more clear the importance that the Antarctic plays and the number of global processes particularly with respect to climate.

Rubin: This has been evident and understood by the people who provide the funding for the scientific programs. They realize that the globe is a unity, and you can't separate--

Cartwright: That's the apex of it, Morton.

Rubin: At any rate, those considerations went on and as time went by, although I continued my interest in what was going on in research, as inevitably happens, I became more and more responsible for organizing the research of others, and seeing that proper balance was maintained. After Bob White came in as the last chief of the Weather Bureau or when the Environmental Science Services Administration (ESSA) was formed, it was a very positive concept, I think, bringing together the Coast and Geodetic Survey, the Weather Bureau, some elements of the Wave Propagation Laboratory that had been under the Bureau of Standards, I think, people of that sort. This again was reinforcing the research that was going on and even the operational activities. I found myself working with some of the oceanographers in the Coast and Geodetic Survey that although at first there was a little bit of hesitancy on the part of one or the others in the groups, we realized that we were engaged in a common pursuit and the cooperation fell right into place as it should.

Cartwright: Good. There will be a question that I will raise later regarding the further joining of oceanography and meteorology, but I'll leave that until later.

Rubin: Well, then of course, beyond that then there were additional developments with the formation of the National Oceanic and Atmospheric Administration (NOAA), and I was involved in that. Of course, Bob White was a prime mover in all of this. He came in 1963, appointed as the chief of the Weather Bureau when Reichelderfer retired. And he brought again a whole different level of activity and understanding with him. He is a dynamic person, very supportive of other people, understanding, and again one of these men who is a high producer but not abrasive or antagonistic in any way. He is a wonderful person to work with and work for. And of course having known him at MIT as students together, it was easy for me to be in good relations with him.

Another one of the interesting activities I had during this period--these are all ancillary to some of the research and some of the research administration I was doing--we had a program that the United States carried out with funds obtained through the sale of surplus agricultural commodities. It was known as PL-480. Some of that money, in addition to being used for expenses of the embassies and missions in these particular countries like India or Poland or Yugoslavia or Israel were used to fund research projects carried out by the people in the country at some of the institutes. And of course this was a good program of mutual cooperation because it brought about understanding and it helped provide funds for people who, in those countries, might not ordinarily have had enough money to do the things that they wanted. I visited a number of these countries on these missions, say Poland, Yugoslavia, India, and determined that there were a few good projects and these were of course basically meteorological, although some had to do with hydrology, as well.

There was one that I was hoping to fund in Yugoslavia, at a nuclear research center, that had to do with some aspects of nuclear pollution and the atmospheric role in predicting and determining where the fallout might be. Of course, on that I called a lot on my friend Lester Machta, who had been involved with the program during the testing period at the Nevada Test Grounds.

Cartwright: And as you know, that became a critical element following the Chernobyl accident when the IAEA joined with the WMO in comparing a whole plan for prediction of, distribution of, diffusion of fallout in the atmosphere.

Rubin: During that period, I still kept up my interest in the Scientific Committee on Antarctic Research and had my associations there. And, sometime, it must have been in the middle sixties, I believe, the International Association on Meteorology and Atmospheric Physics decided to establish a commission on polar meteorology.

And I was asked to be the convener of this, the first organizing committee, which I did and I called together people from the Antarctic community and the Arctic community as well. Sven Orvig, from McGill, and Tornje Vinje from Norway were two of the members. We established this commission which was different from the SCAR commission, which focused only on the Antarctic. The Polar Commission focused on both poles and had a slightly different purpose. I was elected president of that. I served two terms for eight years.

Then, I think by the early 1970s, I felt that I was no longer really a researcher and I was administrator of research, and I was involved in working closely with Bob White, who was the administrator of NOAA, and his immediate assistant and associate administrators in formulating policy and programs and establishing requirements and ensuring--in fact, the job that I had was to try to reconcile and bring together programs in meteorology, oceanography and upper atmosphere, work that NOAA was responsible for in a coherent and coordinated way.

Getting into 1974, a lot of this responsibility that I had--I had a group of about seven people, each with a different specialty looking at these various aspects of meteorology and oceanography that I had to deal with--I realized that I wasn't doing very much myself, but just seeing that other people were doing things, and assessing what had to be done. I heard a lot, and of course knew a lot about the Global Atmospheric Research Program (GARP). There had been the BOMEX before, and then there was the GATE program, so I thought this was something I would like to get into. I had spoken with Dr. Bo Döös, who was in Geneva as the director of the office within the World Meteorological Organization--that was a joint activity of the WMO and the International Council of Scientific Unions. Bob White had a lot to do with this, too, by the way. He was very innovative in his approach to getting people to work together on important scientific problems. Basically, we were addressing world issues, and the atmosphere was and climate was. So I was talking with Döös and he was interested in having me go over to Geneva and this would have been in a sense giving me interesting work to do, to develop some of the scientific rationale for these programs that were being developed and with my background, particularly in the Southern Hemisphere and the polar regions, I had something to bring to them--

END OF TAPE 1, SIDE 2

Interview with Morton Rubin

TAPE 2, SIDE 1

Cartwright: This is tape two of the interview with Morton J. Rubin on December 14, 1991, in Bethesda, Maryland. G. D. Cartwright, interviewer.

Do you have something more to say, Morton, on the further developments of your career?

Rubin: Yes. I think I was saying that I talked with Bo Döös about this Global Atmospheric Research Program at the WMO under the joint aegis of WMO and ICSU. There was a joint scientific committee that had been established which consisted usually of, I think, eight or ten or so members appointed by both organizations and these were primarily respected scientists in the various fields having to do with the program. There were meteorologists, climatologists, people who were atmospheric physicists generally maybe, people who also were heads of some research organizations.

Cartwright: I recall that the original of that committee was called the "Joint Organizing Committee," which I think is an important distinction from what followed under the climate program, which is called the "Joint Scientific Committee." So it was JOC versus JSC.

Rubin: You're right, Gordon. Your memory is better than mine even though I was sitting right there. At any rate, Bo Döös was the director and he had some senior scientists under him. He was very jealous of the prerogatives of this group, and kept it to the extent that he could away from the regular ongoing activities of the WMO Secretariat, which had its own important work to do in terms of establishing standards for training and education, meteorological services around the world, hydrological services and programs of that sort.

Cartwright: Did he not succeed Professor Rolando Garcia who I recall was the first director of the program under Davis?

Rubin: You're right, Gordon. Again, your memory is better than mine. I didn't come into this until the very end of 1974, and Döös had been there for several years. But I did later learn the earlier history, and Rolando Garcia, in fact, had been the first director. An eminent scientist from Argentina, he had studied at UCLA. I believe he now is in Mexico, if I'm not mistaken, at an institute there.

We had the responsibility of helping this joint organizing committee develop the scientific rationale for the various programs that were going to be carried out under the Global Atmospheric Research Program. The one in which--this, of course, had been, earlier on, the "GARP" Atlantic Tropical Experiment. This was the second order acronym, "GATE," "G" standing for GARP, which was an acronym in itself. But at any rate, that was a very ambitious observational program across the Atlantic, because contrary to what had happened in the IGY--and that's an interesting idea to bring up at this point--I remember being told, although I wasn't involved in the discussions, that Professor Herbert Riehl had insisted at the time that the IGY was being organized, that the tropics were the important element in this whole global circulation. But there were many other equally and perhaps stronger enthusiasts for the Antarctic because the Antarctic was unknown. They prevailed, so the major effort was in the Antarctic.

That was a very ambitious program and finally attention was being paid to the role of the tropics and there were surface observations, there were ships, there were aircraft, there was a lot of increased observational activity in Africa, because basically this is where the genesis of the hurricanes are in the Atlantic. Of course, the test case for that was BOMEX, the Barbados Ocean Meteorological Experiment that was back in the early sixties. Arnold Glaser, whom I knew first in South America, was the prime U.S. focal point for that activity.

Cartwright: I thought it was actually headed by an NYU professor who later died in the course of the experiment.

Rubin: He did die, Ben Davidson, that was the man. Glaser took over for him, but I had never known Davidson.

Cartwright: Speaking of GATE, Morton, reminds me of a very interesting comparison. It's my clear recollection that GATE was originally designed to be held in the Pacific and it was initially built on that premise. But as the proposals were being considered at the higher level, it was decided by the U.S. that the security situation in that part of the Pacific was just too tricky at the time. The reason I mention that is that right now the biggest program under TOGA, the Tropical Ocean-Atmosphere Experiment, is now in the Pacific called COARE. In some ways, it's fortunate that this series of events happened because now the instrumentation and the availability of knowledge of the modeling is so much greater that COARE will be a much more successful experiment than GATE would have been...

Rubin: I think it's logical to go from small-scale experiments, tentative ones, which help you understand your capabilities and some of the problems, like BOMEX to GATE and now GATE to COARE, as you say.

At any rate, we were the scientific staff supporting the Joint Organizing Committee. Bo Döös was our director and a very good one at that. He was a scientist in his own right, of course, who had been involved in global dynamics and modeling. And those of us who were there by and large brought a specific kind of expertise to this and I, from my Southern Hemisphere experience and the polar aspect of it and the research that I had done. So that when I got there, I was immediately assigned to looking at those aspects of what was called the first

GARP global experiment. This is FGGE now, so you now have another second order acronym. And I was looking primarily at the polar regions and also I had a lot to do with organizing the observational program in the tropics and see what shipboard activities we could carry out and what countries could provide ships. Also, there was a drifting buoy program which was important in the southern oceans and around the Antarctic. And then a separate activity called POLEX, which had to do with the polar regions and there my expertise came into play not only from the point of view that I knew the players, but I knew the problems and how we had addressed them in the past and so forth.

Cartwright: Was POLEX actually carried out then, Morton?

Rubin: POLEX was carried out in the sense that it was something that would have been done anyway through the activities of the various countries in the polar regions, but we simply put this together as another focal point to give it some priority so that the scientists who wanted to carry on work in the polar regions did have something they could relate to and use as a basis for seeking funds and support in other ways. We had very little in the way of money to provide the support. The whole concept of the program is a joint program with each country producing and giving what it can to support the overall activities within the program. And we provided guidance and we sometimes provided help in getting technical assistance from one country to a lesser developed country who might have an important role to play, but didn't have the wherewithal.

Cartwright: Morton, is my recollection correct that at one stage in the planning, the Russians decided that the program was going too quickly, and that they said that they wanted it postponed for a year or two? My recollection is that Dr. White said, "Well, our plans have gone so far that if you don't come, we will have to go ahead anyway." Is that more or less correct? Do you remember that--?

Rubin: I'm not so sure that that information was made known specifically to me, but I know there were always problems not only with the Russians, but with other countries getting them to come forward at the time and producing the equipment that was needed in this whole schedule of events. Eventually, it did work out and it was an excellent, excellent program. I remember I made a presentation in Monaco, at the International Hydrographic Organization, which is an inter-governmental body but not under the U.N. And making the presentation, the countries under that organization have control of hydrographic ships. Those go out anyway and we had very good response. We had ships from India, we had ships from China, we had a lot of the Soviet ships--they of course had very big fleets of that sort. There was an Italian ship, a Spanish ship, a number of ships arrayed around the globe in the equatorial regions. And of course there was a buoy program in the Southern Hemisphere; this required coordination. Countries going to the Antarctic could release these buoys, but again the schedule was very important and many times they had to accommodate their schedule to ours and that was a very hard thing to achieve because they had problems with ice, they had problems getting there on time to the Antarctic, getting their people in and out and so forth, but it worked out very well.

I think the kinds of problems we addressed mainly had to do with getting the ground truth that was needed to provide a basis for assessment of satellite data that were coming through and to go into areas where very little information had been available on say, ocean currents, or an interaction between the ocean and the atmosphere, and the El Niño activities came out of this and so forth. So I felt that although I wasn't doing research anymore, I was involved in something that I thought was significant and had to do with moving forward the whole boundary of our scientific knowledge about the global atmosphere and the oceans and the climate.

Another program that we started while I was there was an alpine experiment. This had to do basically with this phenomenon of very serious storms developing in the Mediterranean at certain times of the year, and the relationship that the circulation around the Alps had to do with this. The mountain problems, of course, are very tricky problems to deal with and we developed a program there; the one who was the head of our alpine experiment committee was Dr. Joachim Kuettner, from NCAR, a man with vast experience in this sort of work. He was one of the premier glider pilots in the world, and a very, very sound scientist in his understanding of these problems. There again it was a question of getting countries together to put ships into the Mediterranean to establish small-scale networks within the mountains and around the mountains. This included the Germans, the Austrians and the Yugoslavs and the Swiss and the Italians and the French. That was sort of my next-to-the-last thing that I did there, at the WMO. That was very, very satisfying.

That went on after I left the WMO, and the last thing that I did was to help organize (this was under Professor Eagleson from MIT, a hydrologist) a symposium in Washington in January, 1981, on the role of land surface processes in atmospheric circulation models. We brought together agriculturists, soil scientists, hydrologists, meteorologists to look into how one could develop the kinds of data that one would need to put into atmospheric circulation models, to reflect the role of the soil and the ground surfaces. This is a very, very complex problem, because soil characteristics and water retention characteristics and evaporation characteristics vary so much with the vegetation with the type of soil, with the character of the terrain--I don't think to this day they've been able to get a definitive model on a large-enough scale so it could be handled by the models to be helpful. But I think they're on their way to that. That symposium's results have been published in a rather large volume by the WMO.

Cartwright: Wasn't there a satellite program that was tied in with that? ISELSKIP, or something of that sort?

Rubin: No, but satellite data certainly were--

Cartwright: ISELSKIP is cloud satellite--

Rubin: Well, that was another aspect of the climate program. Of course, the whole cloud radiation budget--

Cartwright: Yes, that was Bob Schifter, and still is, as far as I know.

Rubin: Anyway, I had reached beyond the age of retirement from the WMO, which was 60, but through some delicate administrative maneuvering by Arthur Davies, who had been the secretary-general when I got there, made it possible for me to go to 1981, when I was 64 years of age. At that point, I took the opportunity to go back and do some research and I spent a year as a visiting scholar at the South Polar Research Institute at Cambridge University.

Having been interested in Southern Hemisphere data and the Antarctic for many, many years, and having looked at a lot of the more recent data, say from the expeditions from the early 1900s, even the late 1800s, I thought it would be interesting to go back and see what had been accomplished by the very early expeditions. And at the Scott Polar Research Institute, which has a wonderful library, original material even, I decided to look at the first expedition to circumnavigate the Antarctic, the first approach to the Antarctic that is known, was Captain James Cook. That was a British expedition, jointly organized by the British Navy and the Royal Society. It was interesting, in reading the accounts leading up to that expedition, that one could go back to the annals of the Royal Society, which go back even almost 100 years before that. Some of the administrative and logistics elements reminded me very much of what went into the Deep Freeze discussions between our academy and the Navy--some of the petty jealousies, some of the real concerns of one party or another but at least Cook's expedition was a well-founded expedition. Of course in those days the ships were very small, the quarters were cramped, but it was a revelation to see some of the equipment they took along. It was modern, of course, for those days, and in fact, they even had a way to measure subsurface temperature in the ocean. I think, if I'm not mistaken, they were able to bring up water samples from as deep as 200 meters. Of course, one didn't know exactly if it was 200 meters or not because of the cable they would let down and so forth, but nevertheless, to the extent that they could, I think Cook, in his two years' circumnavigation, took three or four subsurface temperatures.

Well, anyway, what I did was to go through and their observations, which were very complete, the log was complete, the remarks were complete and the kinds of instrumentation they had were adequate, although I couldn't say for sure that they had been calibrated. They didn't seem to be unreasonable in the temperature they did, and I was able to, on the basis of that, come up with some average data over a say a five-degree square for particular periods of the year, and especially very close to the Antarctic, which was what I was interested in to try to determine where the ice edges were in those days. We have satellites now, of course, which do this very accurately. Those ships, of course, were not powered other than by sail, so it was not easy for them to get into pack that had any sort of concentration at all, although at times they did and they were beset for awhile. So I was able to compare the cloudiness, the temperatures, the winds, the set of the ocean, currents and swell and so forth to get a picture of what kind of weather conditions they were meeting. Now that expedition was from 1772-1775. That article was published in the **Polar Record** of the Scott Polar Research Institute.

I did a second paper during that year and this was on the Russian expedition, the Bellingshausen Expedition. And again, that was a purely naval expedition, as I recall. And they had better ships, that was from 1819 to 1821, they had larger ships. And they also, interestingly enough, provided some heat for the crew between decks, which was an innovation in the navies in those days. They too had good instrumentation. A lot of it they bought in England on the way to the Antarctic, including maps, barometers, chronometers and they too were able to do some ocean temperature measurement--below the surface, that is. Comparing the data that they got with Cook's and when they were in the same areas around the Antarctic, there wasn't any considerable change in climate that one could notice in the thirty, almost forty years between the expeditions, but any variations I'm sure just would be natural climatic variations.

They had scientists with them, both Cook and Bellingshausen, not necessarily meteorologists, but people who were knowledgeable about weather and oceanography.

There is not much else I have to say at this point. I'm retired and keep an interest, I would say a passive interest in what's going on in my field. I go to meetings occasionally, and symposia. The last one of any consequence, I guess, was the IAMAP meeting in Reading, England, of about three years or so ago. I go occasionally to meetings here in Washington, and read some of the literature, but not very much since my eyesight has been defective for a number of years.

So I guess that is the end of this. My allotted time, more or less, has run out--at least on the tape, and you can take it from there. Do you have any questions, Gordon?

Cartwright: Morton, that was a fascinating review, covering a period of an active lifetime, and lots of interplay between the whole international community and certainly in meteorology and its related sciences. I will perhaps make some references to this in my interview. I hope we may not have to go back and redo any of the tapes because of that. Is there anything you'd like to say about the career and in relation to the AMS? After all, you've been active in the AMS for many years. They are instituting, as you know, a major educational program that should help spread the news of meteorology as a science and as a profession, and encourage younger people to come into the field.

Rubin: Well, in respect to the AMS, I don't suppose I've been active--I've gone to meetings and subscribed to the publications and I've had the honor of being named a Fellow of the AMS, which pleased me very much. In terms of associations with professional societies, I've been more involved with the American Geophysical Union and possibly that's because they are the representatives to IUGG and because of my association with IAMAP and the polar work and also I guess I neglected to mention that I was also the Chairman of the Board of editors of the Antarctic Research Series of the AGU for a number of years. So I guess those, in terms of professional activities, occupied my time so that I couldn't be active in the AMS, but I certainly support all the educational programs of all the societies because it's through those that we bring in new members, if we make known not only the objectives and the purposes, but the excitement of the profession in which we've been involved. You, for a number of years more than I--probably a decade longer--but it is important, because the new blood has to come in and I am reminded of this very forcefully when I look at the journals and see what is going on. 90% of the names I don't recognize anymore and I used to content myself with reading the abstracts finally, but now I don't even understand some of the titles, so I guess it's just as well my recollections are put in the form of a historical context rather than as an active scientist.

No, Gordon, I say I'm pleased that the AMS does this and I support it, of course, so I think I better end at this point.

Cartwright: Well, Morton, this has been a fine experience here on this cloudy morning in December to hear this review of a very active life, and one which I was most fortunate to have many important contacts and derive lots and lots of pleasure from a very strong and loyal friendship. So with that, we'll conclude this interview and look forward to hearing about it from the powers-that-be. May it be helpful to the young scientists coming into the business. Thank you very much.

END OF INTERVIEW