

**American Meteorological Society
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TAPE RECORDED INTERVIEW PROJECT

**Interview of George S. Benton
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Interviewer: Earl Droessler

Droessler: This is Sunday May 26, 1991 at Johns Hopkins University at the Office of Professor George S. Benton. We are going to have an interview this afternoon and probably continue it tomorrow morning on the early development of meteorology and the early development of the American Meteorological Society and George's role as President of AMS. Then we will look at the early development of NCAR and UCAR.

Droessler: George, it is very nice to be with you. Thank you very much for the invitation to come and talk with you.

Benton: Well it is nice to see you again Earl.

Droessler: George lets begin by getting your view point and your perspective on the early days of meteorology on how you came into the field.

Benton: Well I guess I go back a fairly long way in meteorology, at least it seems long now. I was on the periphery of the field when I was a under graduate student out at the University of California at Berkeley. I was involved with the Department of Geography there, although I never did finish my Bachelors at the University of California at Berkeley. A number of the faculty there were interested in atmospheric problems. One summer around that time I did some observing for one of the cooperative U.S. Weather Bureau stations that were on the Berkeley campus. But I guess my first real association came somewhat later. I dropped out of the University of California, and in very early January of 1938 went back east to Washington where I worked for a short time in a temporary post at the Soil Conservation Service in a division that was under the direction of Thornthwaite. There were a number of people who were well associated with meteorology in that group; Dave Blumenstock, Ben Holzman, Kathryn Clark Halfsted. Ben Holzman was later very much involved in weather forecasting and weather service elsewhere. And I went out to Ohio for the Soil Conservation Service to help organize a Works Project Administration program and was looking at the climatology and rainfall history over the Muskingum Valley for the development of soil conservation and water conservation. At that time, I began to carry out at Ohio some studies of drought and some statistics of drought occurrence, which

were later published in the Department of Agriculture publication, which was really my first publication. Just a little bit later I think it was, still 1938, if I remember correctly, I took a Civil Service Examination and became an Observer in the Weather Bureau office in Akron, Ohio. I think that was my first real immersion in Meteorology. It is rather remarkable in how many changes occurred in the lifetime of one person. When I went into the Weather Bureau in 1939 (I think it was), there were still airplane observations for upper air observations. Radiosondes had not been used operationally with the United States. We used a word code, not numbers that were transmitted by teletype. It was a very awkward code, but I was told the reason for it was that Western Union did not charge as much for transmitting words as they did transmitting numbers, and we had a whole code system.

I still remember the code. The consonants were the first digits. I think B was 1, D was 2, F was 3 and so on, and the vowels were the second digit; A was 2, E was 4, I was 6 and so on. And then you got the odd digit by adding an S. So by transmitting a word with 2 symbols you could get 4 numbers and this was the official code which transmitted weather information all over the country. And of course, we used to write everybody messages at New Year's time, making up words which relayed the information, but were not the intended ones that were normally used. But going back to those days, it seems like a totally other world. We sent aloft a pair of Japanese lanterns with candles in them to follow them with theodolites for upper winds. And when there were low clouds the balloon disappeared into the stratus or whatever the low cloud was and we lost all observations for upper air winds. Often the weather was too bad for the airplanes to go up. They were very light airplanes that could not land in bad weather and so you got very good airplane observation when the weather was fine and when the weather was bad, you did not get very many. It was a totally different world than that which would become after the war. The huge watershed was at the time of World War II when the tremendous growth in meteorology developed to support the emerging Air Force of the United States and other countries of World War II.

Droessler: And you were there at the beginning, at that increase in activity of the whole science of Meteorology and the development of a much larger educational framework in which to train the men needed for the World War II?

Benton: Oh, I was there very near to the beginning certainly, and my time in meteorology included this. I think I left Washington or left Ohio to go back to Washington, I think it was 1940. But maybe it was earlier than that. I was only in the Weather Service for 6 months or maybe it was the fall of 1939, and I went back to the Soil Conservation Service. It says something about how the Weather Bureau operated in those days. That I was a sub-professional observer SP-3, as it was called in those days, working for the princely salary of \$1,440 a year (\$120.00 a month),

which was a huge salary. There were people during the Depression years that were raising families and keeping them together on \$75 a month. So as a single young person at \$120 a month, I was rolling in money. I had an apartment, I bought a car, I was living high on the hog, which says something about inflation. When one now goes out for dinner with one's spouse and spends as much for dinner as he used to get in a month, it is quite a change. But I went back to Washington in the Soil Conservation Service as a professional, but did not have my Baccalaureate at that time and was still a little short of my degree. Thornthwaite wanted to do work for his division in Washington and was able to get approval for me in terms of my other experience as a professional-level Soil Conservation Service employee. And I continued to work on some of the statistics of drought, some of the problems related to research and development. At that time a good bit of my time was spent over at the old Weather Bureau office on 24th Street (just north of the Circle). And I got to know some of the people in the Weather Service quite well.

Well, I guess my best friend over there was Harry Wexler, who was a bit older than, and a bit more senior, but who I think I got to know quite well, certainly a very great help to me. I didn't know Reichelderfer reasonably well as I started to say earlier but got side tracked. When I was about to leave the Weather Bureau, Rice ? had me in his office in Washington and urged me to stay with the Weather Bureau and promised me he would push me ahead as a bright young person as quickly as he could, but all he could offer me was still a more advanced sub-professional position at \$1,800 a year. And I decided to leave and go over to Thornthwaite's group but didn't know Reichelderfer in those days, and I didn't know Jerry Namias and some of the other key people. And yes, this really was just before the "explosion in meteorology". I think that going back into the heart of meteorology came about in part because of Harry Wexler and very much because of Rossby. Rossby had left his position as Associate Director with the Weather Bureau for Research Associate Chief, I guess it was called in those days. And [he] had gone out to the University of Chicago to start an Institute of Meteorology and later became the Department of Meteorology, and then ultimately the Department of Geophysical Sciences. And at that time they were just beginning the programs for training Navy & Army Cadets, Navy Ensigns and Army Cadets for meteorological service and what is now the Department of Defense. They were looking for young people who could come out and teach. I had been deferred from military service because of some problems I had with my left lung, which had caused me to drop out of the University of Chicago. And I started at the University of Chicago in 1934. And in the spring of 1935, I developed some problems with a lung which forced me to drop out of the University, and when I came back and went back to the University of California, the problems with my lung had caused me to be deferred from the military service. And sitting in Harry Wexler's office one day, Harry suggested that

perhaps I ought to go out to Chicago and go through the 9-month intensive training program in Chicago and then stay on there and teach. I thought that it was an interesting idea. I remember very well, Harry handed me the phone or rather picked up the phone and called Carl Rossby at Chicago and told him, reminded him of my existence, because I didn't know Rossby nearly as well as I knew Wexler and he told him, he recommended that Rossby have me come out to Chicago and first take the course and stay on and teach. And I remember very well Harry handing me the phone and Rossby saying only one thing to me, "When can you be here?" And I told him, it was probably on a Thursday or so, I said; "Well, our group in Soil Conversation Service is breaking up," and the war just began shortly before Pearl Harbor [*sic*] and I said, "I could probably be there a week from Monday." And he said, "That is too late (which was 10 days), we are starting a new class and I would like you to be in on it; Can you be here next Monday?" (which was about 4 days). I said I didn't think I could be, but that I would be there by the middle of the week. He said; "Fine, come on ahead". I remember he didn't even talk about the details of exactly what I was going to do or how much he was going to subsidize my education or what. I did know from Harry Wexler that they would pay for my tuition at Chicago and I did know that they would provide me with some kind of assistantship, which would enable me to keep myself in food until I finished the course. But I decided this was something I ought to do. So I told Rossby, fine, and up and left with two or three days to go to Chicago, and that was really the beginning of my professional association with meteorology.

Droessler: Well, George, let's continue then with the early days at the University of Chicago.

Benton: Well, this was the real beginning of a large program. We were in what was called the third program. I was in the third program which began in March of 1942, which would be just three months after Pearl Harbor. It was called the third program but was really the first large program. There were at that time—which I think you well know—teaching programs at UCLA, at Chicago, at NYU and MIT, and Chanute Field for the Army Air Force as it was in those days. Chicago had two earlier programs, I think the first one of which it started sometime in the previous year and there was one that started a few months before ours. But there were only a handful of people in each of these two courses. Our course that began in March of 1942, was, I think, a rather special one. There were some very impressive people taking the course, and very impressive people on the faculty at Chicago. We had Carl Rossby of course, Horace Byers (they were the two mainstays), Victor Starr was an Assistant Professor and Helmut Landsberg was there in climatology. And I think that was the entire staff when I first went out. Others were added later. Herbie Riehl became an assistant professor in later years. But in those days the only people that had their degrees were Rossby, who had an honorary doctorate, he never did have an earned doctorate. Horace Byers,

who had his doctorate from MIT. Victor Starr did not have his doctorate in those days. Helmut Landsberg, who trained abroad, did have the advanced degree. But most of the people who were there and who were teaching in one way or another were fairly new in the field and didn't have advanced degrees.

Well, I didn't have any at all when I went over there. I had finished my baccalaureate while I was taking the intensive program in Meteorology. But the people who were there who were students in that first class were an extraordinary group. Herbert Riehl had been there and in the earlier class they had Dave Fultz; they were in Class #2 if I remember correctly. But in Class #3, were in addition to myself were Vern Suomi and Reid Bryson, who later developed meteorology at the University of Wisconsin; George Platzman who stayed on at Chicago as Professor; John Bellamy who went out to the University of Wyoming and developed meteorology there; George Haltiner who came as a Navy ensign and who was later a Senior Professor at the Naval post-graduate school on the west coast. Let's see who else: Werner Baum and quite a number of others whose names I am sure would be very meaningful to you. It was quite an extraordinary group, and there was a very good group of civilians. We worked very closely together, studied together, worked together. It was quite an interesting time.

I would like to say a few words about Rossby because he was my senior professor. I did my doctoral dissertation directly under his supervision and a very remarkable man he was. He of course, by the time he came to Chicago, he had a rather long distinguished career. He was one of the bright young men in the Bergen School of Meteorology, working with the elder Bjerknes and then he came to the U.S. and started the group in meteorology at MIT. Then he went down to the Weather Bureau as Associate Chief for Research and hired a very good group of people around him including people like Harry Wexler, Jerry Namias and others. And then he went off to Chicago to start the Institute for Meteorology at the University of Chicago. He was also directly involved in starting a little bit later the group at NYU. He was heavily involved in bringing Jak Bjerknes and Holmboe over to this country to start the program at UCLA. Oh, I forgot to mention Caltech when I was talking about groups. And of course, after he left Chicago, he went back to Sweden and started the Institute for Geophysics at the University of Stockholm in Sweden. And in his final years he was talking of getting restless again and was thinking of starting a new international institute in Istanbul. But then he died and never did do that.

When I really got to know him well was when I went out to Chicago, although I had meet him previously. I still remember the first time I went into his office at Chicago. We had only this very short conversation which was over the telephone in Harry Wexler's office and then I came to Chicago and walked into his office, not knowing what to expect. We sat down and he told me that the course had

started two or three days earlier and I would of course have to catch up. He told me that I would have arrangements where I would be an assistant, though I never did much work because we were too busy taking the course. It was quite frankly that, it means subsidized future teachers. But I was nominally called an assistant and nominally working for Helmut Landsberg, and I forget my salary but it was in excess of \$100 a month, which was enough to live on in those days. And I got to know Rossby quite well over the succeeding years.

After I got my training in Chicago, I stayed on as instructor and then, after I got my doctorate at Chicago in 1947, I started on briefly as assistant professor before coming to Johns Hopkins. And all throughout these days, I knew of course Rossby, Byers, Starr, Riehl, the whole group from the University of Chicago quite well. I don't think that when Rossby was a very, how shall I say, "he got things done", but he was also a very dynamic person. A little absent-minded. I remember when I went into his office, sat down at his desk and we were talking about what I would do and the phone rang and he picked it up. It was someone in Washington wanting some information. He said, "Oh, yes, I'll get that for you," and put the phone down and went off to get the information for the person on the other end. I don't know who it was. He was gone a long time and he came back and I think he was surprised to see me there. He had sort of forgotten that I was there and then he looked very quizzically at the telephone and said; "What's this?" and he picked it up and said, "Hello! Hello! Oh yes, I was going to get that information for you", and he put the phone down again and went out. That time he did come back with the information that was requested. But that was typical Rossby. He was a dynamic person, always living for the moment, full of ideas. He could get absorbed in a subject and forget everything else with the excitement of the intellectual challenge of the moment.

Droessler: I've often wondered, was he skilled in providing thesis topics for his students and thesis work themes or did he leave that pretty much to themselves?

Benton: He was and he wasn't. He was quite content to having you do your own thing. I took on a topic which was very much in hydrodynamics of flow through porous media, which I had gotten fascinated with and I wrote a dissertation on the hydrodynamics with mix of flow through porous media which Rossby knew very little. But he never the less volunteered to be one of the reviewers on my dissertation. He didn't really contribute to it because it was a subject he knew not much about, but he did read it critically, and made some comments on it, some suggestions but, as I say, he was quite content to have someone who wanted to do something and have the gumption to do it and to work on his own. On the other hand, he was full of ideas and full of things he wanted to see done. And he was a master-mind at getting people, and recognizing people to do things he thought ought to be done and so yes, he was very dynamic and was good at feeding ideas

to people. He was extraordinary at feeding ideas to people and that was in fact one of his major roles. I don't think that the people of tomorrow will really appreciate the role that Rossby played in those days, because if you look at his publications they were good but they were not so tremendously outstanding. If you take his early paper on Rossby waves for example; it was an interesting paper, one that was quoted over and over, and over again, and one that is regarded as a "key paper". It was full of over-simplifications mathematically that simply did not have to be taken. He did everything on a beta earth. He did everything he first analyzed, Rossby wave which was a wave system which didn't vary in the radial direction. None of these mathematical simplifications were needed. But this was the way he worked. He had an idea he wanted on paper, he got it on paper in the simplest way and went on to something else and immediately afterwards of course dozens if not hundreds of papers were written extending what he did making it more rigorous mathematically, putting it on a rotating sphere instead of it on a beta earth on and on and on. But they all followed directly from the germ of the idea that Rossby had so that he was very much a source of ideas. But more than that, he was a very dynamic person who I think more than any other person except possibly Reichelderfer was tremendously influential in guiding how meteorology developed during these war years.

Some very key decisions were made at that time and I give Reichelderfer and Rossby primary credit for this. Let me take a few of them: the decision was made to train the meteorologists for the Air Force and for the Navy by sending the select group of ensigns in the case of the Navy, cadets in the case of Army Air Force—to send these students back to school for nine months. This was not a necessary decision; they could have put these people in the crash/pragmatic training course to teach meteorological forecasting as it was known in those days. It would have been a natural thing to do but Rossby very firmly believed that meteorology was a science and that the young people who were coming in to the field, even those who were going out to do forecasting for the war effort, should have a good solid scientific background and should understand the scientific fundamentals of the subject that they were working in, and I think that Rossby and Reichelderfer both played very key roles in convincing the Army and the Navy to send their students back for this intensive nine months of training, which involved mathematics and physics and the basic dynamic meteorology that gave them a background which they never would have gotten if they had gone into pragmatic teaching programs for forecasters.

Droessler: And also convincing the leaders of the day in the universities like Karl Compton and others that meteorology was a subject that had a proper place in the university structure.

Benton: Absolutely. And he was very influential as I said in starting the program. He

started the program at M.I.T. and was very influential in getting Bernhard Haurwitz in to start the program at NYU, and getting Jak Bjerknes to start the program at UCLA. He was only peripherally involved in Caltech but he was involved some degree in the Caltech program. And Rossby was sort of the dynamic person who on behalf of the academic committee really got this program going. He played a role, a programmatic role that was important. And he also played a role in terms of people because his students, his formal students, flared out all over the world to lead meteorology in different countries.

I really should not leave Rossby without mentioning some of the people who were so important in those years, who worked in government. I think people in the academic communities have a tendency to neglect the fact that major advances cannot be achieved without counterparts in Washington in government who carry the program forward and get the necessary funding. I say this particularly because in later years as an example the academic community feels that Jule Charney was the father of the World Weather Program. And to some degree he was. He was certainly a scientific leader of tremendous importance. On the other hand that program would never have got going without Bob White as Chief of the Weather Bureau in those days, and later as administrator of ESSA & NOAA. If it had not have been for Bob White fighting the interior battles of getting the money through the Washington bureaucracy, that program would have never had gone anywhere. So you cannot forget the people who really carried the ball in Washington. And this includes people in both the Air Force and the Navy. "Shorty" Orville, as we always called him in the Navy. There were a lot of people; Reichelderfer was a key person with a tremendous entree in Washington to the meteorological world and people who mattered to the meteorological world. And then the giants in the civilian community who were not meteorologists but who led the whole organization of science for the prosecution of the war effort and who lent their support to Rossby in getting the work done that needed to get done in meteorology, but I think Rossby was the spark. He was the person who made things go. He was the dynamic heart of many of these activities and I think that coming generations simply will not appreciate all that he did and all that he contributed.

Droessler: And you might want to mention Don Yates and what he did for the Air Weather Service.

Benton: Oh, indeed, indeed. Yes, I should have mentioned Yates.

Droessler: You see that was the group in Washington that provided the nation leadership for meteorology and for the development of what we later called the atmospheric sciences.

Benton: Right. Right.

Droessler: There was Reichelderfer, Yates and Orville.

Benton: Absolutely.

Droessler: And they were combined in a group which was called a Joint Meteorological Committee that worked within the Pentagon. And then they provided a spark and the leadership there, which reached to the White House and to the chambers of Congress.

Benton: Oh, it was an exciting time. That third class at the University of Chicago was about 90 people. There were about 15 or so civilians and the balance were Air Force cadets and a comparatively small number of Navy ensigns. But then the classes really began to roll. They got up in the hundreds. Five hundred and more, and we turned out two classes a year from then on. And as I said, I stayed on and taught at the University of Chicago. I was teaching and first teaching in the laboratory; later on I became in charge of teaching the laboratory which meant I had a whole group of instructors working for me running the laboratories for the cadets and for the Navy ensigns. And I stayed in that role until close to the end of the war when our whole teaching program was going down and I went off to do some work in hydrology and then came back to Chicago to organize a program in hydrology and hydrometeorology at the University of Chicago. But that was in 1945 that I came back to Chicago, just before the end of the war.

Droessler: But where did you go from Chicago, George? I mean after you left and resigned your position there.

Benton: Well, towards the end of the War in 1944, our teaching program at the university was winding down and I was beginning to feel that I wasn't contributing adequately to the war effort. Joe Kaplan, Professor Joseph Kaplan, Professor of physics at UCLA, was organizing an operations analysis group for the Army Air Force. Operations analysis in those days was a group which existed in several key places. Within the Army there were offices manned by civilians which were used very creatively to help the war effort. The advantages that civilians had over Air Force personnel was that they were not bound by the confines of being in uniform and having to follow military protocol and they could be used much more in interesting ways to find out what was really happening to bring information back to the necessary headquarters office to short circuit the bureaucratic chain of command, to make special studies of one kind or another and it was really a rather extraordinarily activity in the war and later became a more mundane activity in postwar years. But as I said, Professor Kaplan was organizing an operations analysis group for the headquarters of the Army Air Force Weather Wing in

Asheville, N.C., and he was looking for young Meteorologists that he could recruit and that were civilians. He came by Chicago and talked to Rossby and Byers and they recommended me, and Joe Kaplan talked to me and asked me if I would come to Asheville with him to set up the program as operations analysis there. I agreed, although I did say I wanted to get out in the field. At that time we had some rather fancy plans for me. Kaplan had been told that they needed some people of this type, operations analysis group in western China. The whole coast of China in those days was occupied by the Japanese but we were regularly flying the "Hump" with men and material and they were setting up some Chinese meteorological activities to support Chinese Air Force and what U.S. Aircraft there were in that area. And Kaplan wanted to know if I was interested in flying from India to Burma and helping to organize the Chinese meteorological services in western China, and I thought that would be a very interesting job, and I agreed to do so. So I went with Kaplan to Asheville, N.C. with this intent, but one thing and another developed and we never really got that started although it was still planned toward the end of the war and the war ended very suddenly in 1945 and I never did get to China at all. But I continued to serve as Joe Kaplan's deputy of the operations analysis group in Asheville and since Kaplan was a very peripatetic individual who was often away from home base, I was quite involved in running on a day-to-day basis the operations analysis group.

Droessler: Do you remember, was Joe Kaplan at that time associated with UCLA?

Benton: He was on leave from UCLA as a professor of physics. He went on leave from UCLA, and running as I said on a fulltime basis, that is, the operations analysis group. I did have a few interesting months in Washington before I went to Asheville because there was an operations analysis group in Washington that was analyzing a problem that was associated with bombing of Japan and that was attached to the 20th Air Force Weather Wing that there was the proper number that or the group that was bombing Japan out of the Islands there. And my friend Helmut Landsberg from Chicago was in this operations analysis group for a few months until Kaplan and I started the Asheville. I was working in the Pentagon in the late months of 1944 to work on some operations analysis problems with Landsberg in the group there. Then I went down to Asheville to start the program there with Kaplan.

Droessler: So you stayed with Joe Kaplan a year or two?

Benton: I was with Kaplan until the end of the war, until just before the end of the war as a civilian I could resign. I didn't have to wait my rotation turn and I was in Asheville when the first atomic bomb was exploded in Hiroshima and I remember that very well. It was a fascinating time; at that time there was a full colonel who was commander of the Army Air Force Weather Wing, as it was called in those

days. I am not absolutely positive of his name. I had forgotten it over the years, but the senior person in meteorology was Ben Holzman, who was also a full colonel and Ben was stationed there, Harry Wexler was stationed there, Ken Spengler came there towards the end of the War and George Forsythe, who was a mathematician and got his training in Meteorology from UCLA and later co-authored a book with Holmboe that was used extensively in dynamic meteorology in the early years. Forsythe was there, we had quite a group. In operations analysis, we worked very closely with these people. I remember very well the occasion of the explosion at Hiroshima. I had some inkling of the fact that there was a program related to the effort to create a bomb to release nuclear energy by fission. Because I had been at the University of Chicago and there was this huge program which existed at that time, which was headed by Enrico Fermi, who I took a course from at Chicago, who was a remarkable man but that is another story. And I knew that something was on, Mike Ference who as a young assistant professor of physics and who came over to teach hydrodynamics, the Institute of Meteorology at Chicago did know the physical background of the problem of fission as it was known before the war years when everything became classified. He was not directly involved in the Manhattan project. And Mike Ference and I had some extended discussions about the physical background and Mike had told me what young modern young physicist who the concept of fission was entirely possible and they had put two and two together and produced this major research project that was going on at Chicago and other locations that we did not know the exact location of. It had to do with the release of nuclear energy.

So I was aware of this. And I remember one time in Asheville, N. C.—this was after I married and my wife and I were in Asheville, we had a small suburbs place where we had rented a cottage and Ben Holzman, who was a colonel in the Air Force. Harry Wexler was a lieutenant colonel working with Ben, came over to our place for dinner one night and we were sitting and talking and I remember we were talking about the Japanese program of sending over balloons with fire bombs which were dropped rather randomly across mostly the western part of the United States, but occasional balloons went all the way to the eastern part of the United States and we had conducted a little bit of a study of this for the Air Force and we were talking about the balloon bombing, as we called it in those days, and speculating why the Japanese were doing this and I remember saying perhaps they are working on release of nuclear energy and that they hope to be able to use these kinds of balloons to drop much more potent balloons over the United States. Harry Wexler and I got into a spirited discussion about fission and Ben Holzman sat there very quietly and not saying a word—which was very unlike Ben—but then Hiroshima came a short time after that and I remember dashing in Holzman's office and he sat there and sort of smiled, and it turned out that he had been out at the Alamogordo test and the reason he had been sitting so quietly was he was just

back from the Alamogordo test of the first atomic bomb in the United States. When he came over at our house he was not about to indulge in any speculation about the atomic bomb. But we had a fascinating evening that night.

Ben Holzman, Harry Wexler, Joe Kaplan and I and my wife all went over to Ben Holzman's and talked about the Alamogordo tests. Holzman had been ordered out there to Alamogordo to assist in the forecasting for the first explosion on the atomic bomb and he didn't know where he was going. Of course he didn't know anything about the Manhattan project on the effort to release of fission energy, but he went out there. He flew to Alamogordo and as he arrived he was picked up by a staff car and a driver. And his orders had said to go to such and such a base. I forget what, and the car that picked him up started heading out in the wrong direction and then Ben, I remember he said; he leaned forward and said to the driver; we're not going where I thought we were supposed to go, why are you going this way? And the driver said, I have my orders sir and I'm taking you where I'm supposed to take you according to my orders. So Ben, who was a rather phlegmatic guy, sat back and waited to see where he arrived. And he arrived at Alamogordo and went up to the proving grounds where the bomb was later exploded and found that meteorology was playing a very important role. They had a young forecaster who had gone through the program at Caltech under Craig and his favorite way to forecast, which he had been taught to forecast was this analog system which didn't work worth a damn of course, where you take a weather map and try to compare it historically with a map that looks similar to it some years previously and then used the previous set of events to make your further forecast and Ben, who was an old Weather Bureau man, was very disenchanted with this method of forecasting and he was also very disenchanted with this officer's inadequacy of the forecast he was providing and with his inadequacies in evaluating the effect of the bomb. Ben was a very bright guy and when he found out the amount of energy that was going to be released in this explosion, he knew perfectly well that the bomb would blast a hole in the atmosphere and the tropopause and go well up in the stratosphere as this Caltech forecaster had been forecasting that the cloud would rise only to the tropopause, which Holzman didn't believe at all. So Holzman, who outranked the chap who was there, essentially took over the forecasting. Holzman wasn't one who had been heavily involved in the forecasting for D-Day in England. One of the two key people in those forecasters, and he was fairly senior so he took over the forecasting for Alamogordo. Alamogordo in terms of the weather. Of course it was raining and there was this very great doubt as to whether they could explode the bomb or not, but they did forecast the letting-up of the rain and the bomb was fired during such a period when the rain had temporarily ceased. They were of course concerned about the rainout of nuclear products. And again Ben told me how unfortunate that might be in great detail. He wasn't of sufficient rank to be in the dugout from which the explosion was being viewed. Ben and a number of

others were about seven miles from the blast and lying down, heads away from the point of explosion of the bomb and when the bomb went off, Ben told me that heat was so intense that he felt as though his skin was burning and all he could think of was that perhaps they dropped the decimal point in the energy output. And of course the light from the explosion was tremendous but the heat rather quickly lapsed and they waited for the shock-wave which they knew was going to roll over them from seven miles away and the blast of the shock-wave came past them and then he raised his head and turned around and looked at the explosion, at the cloud and that was quite an occurrence for him. And seeing, sitting and listening to this, the night of [the] Hiroshima explosion was quite a memory.

Droessler: Well, George, you want to finish up that, your duties at Asheville and then move on to your next station of opportunity?

Benton: Well, I'd like to, let me go back to Asheville. There was another story that I'd think I'd like to tell you and again shows the state of early meteorology just as the forecasting for Alamogordo tells us something about the state of forecasting in those days. Let me give you another one. The morning after Hiroshima, the morning after the five of us had sat and listened to Holzman talk about his experiences, I was in the office and Ben Holzman came into my office this time and told me that he had a call from Washington and that some general (he didn't tell me the name of the person) on the Joint Chiefs of Staff, whatever its equivalent was in those days, and called and asked for a forecast of the dilution of the Hiroshima cloud by the time the radioactive material passed the coast in North America, and Holzman had tried to explain to the people in Washington that we really weren't able to make forecast of this sort. We didn't know enough about how the pollution dispersed through the atmosphere to make an accurate forecast, but he was given the order that we should come in with a forecast of the dilution of the; oh pardon me, this was, I said something wrong here. I don't know if you want me to go back in correct it, but it was Ben Holzman. This was the day after Hiroshima. I think it was Wexler that came into my office, but that doesn't matter. At any rate, that problem was turned over to Harry Wexler to come up with a forecast of the dilution of the Hiroshima cloud by the time the material passed the coast of North America. And Harry got three of us together to _____: Harry, George Forsythe (a very capable, bright, later prof. of mathematics from UCLA) and I. And told us that we each decided that we would independently come up with a prediction of the dilution and we used three different techniques and each one of us dreamed up our own technique and I think that says something about our personalities. Harry Wexler who used _____ coefficients, mixing coefficients, which he didn't have any confidence in but he didn't have anything else to use, and he used an estimate based on mixing coefficients extrapolated, bad as they were. We had nothing to say on how the mixing would occur vertically much less horizontally, but he came up with a dilution factor over

a period, oh we had an order to do this by the way and when we got the order and we all went off to work, I remember when Harry Wexler; kissing his fingers and saying "there goes science out the window".

So Harry came up with this forecast based on mixing coefficients. George Forsythe took some chemical warfare tables, which had to do with the diffusion of poison gases and extrapolated them many, many, many orders of magnitude but he didn't know what else to do so he just took the curves which were valid for very short distances and times and extrapolated them out for thousands of miles and huge time. I just didn't know what to do, so I simply tilted my chair back and looked off into space and made a very crude guess as to what the width of the swath might be just based on intuition by the time the cloud passed the coast of Canada, and then used a roughly equivalent dispersion for the direction of flow, assumed that it would be distributed over, about half of the depth of the atmosphere, then got the volume and simply used the volumetric different dilution. I haven't the slightest idea which of these techniques was best because I don't even remember what we, we got together, we each took our forecast number, we averaged the exponents, we _____ all the various exponents and took that back to Washington as our forecast of the dilution and I think I may have been involved in what possibly was the first forecast of the dispersion of radioactive material in the atmosphere in the real-time basis. I haven't the slightest idea what the annual value was or what the forecast was.

Droessler: That is a most interesting story in many and particularly from one way you mentioned before, and that is to demonstrate the performance of the science of meteorology to that on this kind of a question. We were really at the very beginning stage weren't we?

Benton: That's right. And the intensive work of Lester Machta and associates and the diffusion of radioactive contaminants that took place in later years under the auspices of the Weather Bureau and later ESSA and later still at NOAA, Air Quality Laboratory. I think we called it in later years the Environmental Research Labs. All that work lay in the future. That was the beginning of a more scientific approach to diffusion. We didn't have anything, we had—the only thing we had were these very crude mixing coefficients that none of us trusted.

Droessler: Let me ask you another question that I have been wanting to get an answer to for a long time. During this period of time we discovered the jet stream in the upper atmosphere.

Benton: Yes.

Droessler: And I was wondering whether that operational analysis group that you were

associated with, had anything to do with the discovery of that jet stream.

Benton: We didn't have anything directly to do with the discovery of it but we were involved in trying to figure out what might be done about it and trying to understand what it was. I remember very well when the B-29's came into the war zone. The earlier planes flew at a lower elevation over Japan and they didn't encounter that jet stream problem, so it was a B-29 problem. And our Air Force operations analysis group was heavily involved in the B-29 program. As a matter of fact, one of my early assignments in Asheville was to go out to the Air Force base in Clovis, New Mexico, and see how the program was coming along for training weather observers to fly in a B-29 weather reconnaissance outfit that was being put together to fly in the Pacific in support of what was coming forward as the B-29 bombing effort over Japan. And may I detour to show something of conditions of those days. I went out to Clovis again, I was a civilian, I had an officer's adjutant status so I stayed in the bachelor officers' quarters, but I very quickly found out that things were a mess. The B-29 crews were being trained for the weather recon squadron, but the weather observers did not have authority to fly. So the B-29 weather reconnaissance aircrafts were going out on training missions without the meteorologist which made no damn sense at all, and the commander of the base was tearing his hair out because he had been sending messages through the chain of command to get permission to allow the weather observers to fly on the B-29's to no avail. That was one case where Operations Analysis helped during the war because I was able to get this "nonsense" information through the hierarchy of the war effort of the military and get it directly in to Operations Analysis headquarters in Washington where they were able to solve the problem and get the orders coming down from above to give the observers the authority to fly within a week or so. I was only there for a few days but within about a week of my departure, they finally got the authority to allow the meteorologists to fly in these squadrons.

Well, to get back to the jet stream, the aircraft bombing over Tokyo in those days and over other Japanese cities flew bombing patterns where they would fly northward to the east of Japan and then come in flying westward over Japan, where they evaded detection until the last moment. And that had been a very successful bombing technique with the planes that previously preceded the B-29, but the B-29 flew up around 30,000 feet and that was getting up high enough to get in the jet stream. And as I remember it, this is all from memory and my numbers may be wrong, but I think their top air speed was around 285 knots and what happened of course was that on several occasions the B-29 bombers would go up to their bombing elevation and start coming in flying westward over Tokyo and they would run into tremendous head winds and sometimes made virtually no headway at all. They were moving so slowly that they were sitting ducks for the anti-aircraft gun barrage from below. So this was not an appropriate flying pattern

for the B-29's. Well, this was the first time that the existence of the jet stream was really confirmed. I think I was aware of this; it had been detected earlier but nobody believed it. I remember when I was an observer back in the 30's in Ohio, occasionally you would get a balloon in very fair weather that would go up to extreme elevations and the sun would be at such an angle that you would be able to follow even a small balloon (this was not a radiosonde, it was a pilot balloon) to sometimes 30—even higher: 30,000-35,000 feet. With the sun glinting off the balloon, you could follow it in the outer light and sometimes we ran into extremely high winds at those high elevations. We never believed them. These balloons often suffered pinholes, and at high elevations they sometimes began to leak right after you let them go and of course, with only theodolite measured angles, you were assuming a rate of rise for the balloon. But if the balloon started to leak and lose lift then it would rise more slowly than you thought it would and its angle would appear lower on the horizon than the angle at which it appeared. You would assume the balloon was further out because all you had was the angle of elevation. And whenever in those days anybody ever came up with 200 miles an hour winds, I guess somebody thought it was a leaking balloon and you throw it out. So I'm sure that the jet streams had been observed indirectly but nobody had believed it until we got planes up there and found out that the B-29 sometimes were flown backward by the jet stream. And then the question of course was how to change the bombing pattern, what kind of meteorological conditions might be appropriate and we did have some involvement in the analysis of that problem. But that was really the first of the jet stream.

Droessler: And then when did you leave Asheville and where did you go after that?

Benton: As I say, I was a civilian and I could leave quickly. Hiroshima came off, we turned in our forecast that I mentioned and then the Nagasaki bomb was dropped and shortly then after the Japanese surrendered and the war in Europe was winding down and I decided that there was really not that much need for me to stay in Asheville and I made arrangements to go back to the University of Chicago and resume my position of instructor at Chicago. I was one of the very senior instructors there, since earlier I was in charge of the laboratory for instructing military students in the laboratory courses. I had a permanent fulltime post as instructor at the University and I wanted to go back and finish my doctorate. So I went back from Asheville in early '45 I think it was, and in '45, clearly in '45, I went back to Chicago and my wife and I got a place to stay near the university and I became an instructor, was still in the Institute of Meteorology [which] rather soon became a department and I embarked on a parttime study for my own doctor's degree, while continuing as an instructor.

There was a continuing flow of students in meteorology; for example, there were 12 students who came over who were sent over by China. There was quite a

number of foreign students who came to the university in those days, some American students, some students who got out of the Air Force or the Navy and decided they wanted to finish their studies in Meteorology. So we had a group that was moving ahead quite effectively. Some of the people were George Platzman, who came back to the university, Vern Suomi, who was still at the university in those days. A lot of these people came back and got their doctor's degree. I think I was the sixth doctorate to get Ph.D. at Chicago, which probably makes me among the first twenty or so in the United States. The ones who preceded me I remember very well; they were all post-war. The first one to get his degree at Chicago was Morris Neiburger from UCLA. UCLA had qualms about giving a degree to one of their own people. So Morry came to Chicago, I think he was there for one semester, perhaps it was one year, I think it was one semester, and got his doctorate. We were all waiting for Victor Starr to finish his doctorate because he was the senior person among all of us at the university who didn't have degrees, and we all knew that Victor had to be the first to get his degree. Victor was such a retiring person and so unaggressive that he was taking his time about it and it didn't really matter to him. He didn't need a degree to know that he was an effective contributor to meteorology.

And I remember one day a group of us getting together and they elected me as spokesman and we went in to see Victor. I told him that he had to get his degree because he had to do it out of loyalty to us because it was evident that none of us could get our degrees until he finished his and therefore he should get his and get out of the way, which he did. So he was the second and Herbie Riehl came in there somewhere with a degree and John Bellamy was the fifth and I'm missing someone. I think there was somebody else before me, but I think I was the sixth person to get their Ph.D. and I got it in 1947. And then I stayed on and became assistant professor in the fall of '47 and stayed at Chicago as assistant professor in the 1947-48 academic year.

I finally left to come to Johns Hopkins University in the fall of 1948. But in Chicago I specialized to some extent in hydrometeorology and hydrology. I had gotten interested in this subject. Rossby was very good about sending his students off to broaden themselves. When I finished my nine-month program at Chicago in late 1942, he sent me to UCLA for three months just to go to a different environment and to do some work in the department of meteorology at UCLA. And Jak Bjerknes had put me to work analyzing weather maps for the 40 year historical North-American analysis. I had very close contact with Jak Bjerknes in those days. He was a remarkable man. He could certainly analyze weather charts a great deal better than I could, because I would sweat over these maps. Of course the objective was to put on the charts a coefficient and intelligent series of possible weather systems. And I say "possible" because out over the Pacific especially, we had practically no observations. So you would have an observation

from Hawaii and some from Quay and a few scattered observations, but vast expanses of hundreds of miles with no data. Therefore you could dream up the development of a cyclone quite properly and where absolutely no data existed. And the first indication that it existed would be when it came ashore in the Aleutians or something of this sort. And, so therefore, you had to work back and fourth between the charts and get a reasonable series for the development of weather systems and I remember very well sitting and working and working on these and sometimes having a devil of a time getting everything to fit with the observed data. And Jak Bjerknes would come in and sit down and say well, let's see what you have today and he would begin to look through to see what I had done and then he would begin to sketch things—well, possibly it is this and work away and pretty soon he would bring order out of chaos and he would have, I won't say it was a correct analysis, because none could say that, but it was a reasonable one.

Droessler: A plausible one.

Benton: A plausible one, which is all you could hope for. Still later when I went back to Chicago after Asheville, Rossby sent me down to the Tennessee Valley Authority because I was interested in hydrology and he wanted me to spend some time in Knoxville and in Washington. The Hydromet Service was the Weather Bureau's effort to learn something about hydrometeorology and hydrology. And I remember spending several weeks, six weeks or so, in Washington with the Hydromet Service and then I was down in Knoxville, Tennessee, at headquarters of the TVA to study how they use weather forecasting and weather observations in managing the TVA. That was fascinating because this was the pioneer days for radio observations of rainfall. TVA pioneered this under a man by the name of Frye, who was one of their chief engineers for forecasting river flow, which was of course used to manage the entire hydraulic electric power generation system. And with the cooperation with the Weather Bureau and the Hydromet Service, the TVA placed into the mountains of the Blue Ridge and other remote areas, a whole group of radio rain gauges, which could be interrogated either by phoning in or listening to radio signals to get data on current rainfall. They were using that observational data to predict rainfall and runoff estimates for the efficient use in scheduling water usage for power generation. This of course became a much more developed field in later years. This was really the pioneering days of hydrometeorology.

Droessler: You mentioned Hydromet Service. That was part of the U.S. Weather Bureau?

Benton: Yes. I think it was. Yes it was. I'm trying to remember the names of the people there. The names sometimes escape me.

Droessler: Well even today they have the responsibility for riverine flood forecasting. Which is in a part of our same service extended into our same water—

Benton: I did teach hydrology and hydrometeorology at Chicago for a couple of years and I had a one year course in hydrometeorology in 1947, 1948 and I remember some very well-known people who went through that course. One for example; Roscoe Braham. I remember when Roscoe came back to work for his doctorate. He was one that I think was a student in one of those two courses, I think it was the one in 1947-48. I also had some interesting experiences at that time in the administration in the department. Carl Rossby went off. Let me stop and get my thoughts together here. Let me go back just a little bit. Rossby went away for a year, I don't remember quite where he went, quite possibly to Sweden because it was not too long after that he went to Sweden to start the Institute for Geophysics at the University of Sweden.

Droessler: Yes, that's right, he went over there for another visit.

Benton: And he was gone for a year, I remember. And Horace Byers, when Rossby was away ran the department and ran it very efficiently as a matter of fact. I quickly realized Horace Byers ran the department when Rossby was there. Not in terms of policy because he was a very dynamic person who had a million things on his agenda. But the day-to-day operations of the department was really Horace Byers' bailiwick. And he was a very methodical, thorough person and Rossby was a very _____ scattered person who scattered his ideas all over creation and the two of them made a very good combination. What happened one year, I remember, the Thunderstorm Project, its first year came up in Florida and this was shortly after the end of the war. Byers went off for a year of leave of absence to run the Thunderstorm Project. I did not yet have my degree. I think this was in probably 1946-47, I think it was or 1945-46, I'm not sure which. But at any rate, Rossby was gone, was there, was back, but Byers had gone away for the year. The last thing Byers said to me was, when he left, was that I should take over the administrative responsibilities for the department while he was away and I said, "But I don't even have my doctor's degree and I am only an instructor." He said, "That's all right, nobody else can do it and I'm going to be gone and you've got to be responsible for seeing that things get done because Carl doesn't often follow through on things."

Droessler: It's not his forte.

Benton: So then Byers disappeared and I was left with this monumental task trying to see the department operated smoothly with Rossby as chairman and with no other senior professor who was willing to take on this duty. It was quite a job. The dean, I think it was Bartky who was an astronomer, but at any rate, the Dean of

Physical Sciences would call and tell me we had to do so-and-so and I would go in and ask Rossby what should I do. And it was virtually impossible to get him to make decisions because he hated administration and he didn't want to make any decisions about mundane things like operating a department. If it was a scientific decision he was all over it but if it was an administrative decision he didn't want to be bothered. And I was getting ulcers. This must have been in the fall of '45 because it was shortly after I got married and I was, and maybe it was the spring of '45, it doesn't matter. I would go home and tell my wife I didn't know what to do because on the one hand, the dean was beating on me that he needed certain information; on the other hand, what did the department want to do about x, y or z and on the other hand, I couldn't get Rossby to make up his mind. And finally I decided on a strategy and it was really one of the first of my administrative decisions in life. I decided that I was in an impossible position and the only way that I could handle it would be to go in and tell Rossby what I was going to do. And I know he would tell me that I couldn't do that, not to do it, that he hadn't made up his mind yet, and he told me that I wouldn't accept that for an answer. And I remember the terrible day when I tried this out. I went into Rossby's office and I cornered him and I demanded, I told him that we absolutely had to have an answer to a certain problem that the university needed an answer to. And I told him what I was going to do, and he said, don't do that. He said, I'll tell you what to do tomorrow. And I said no, I heard that story before. I said no, I'm going to come in tomorrow morning and I'm going to do this on behalf, in the name of the department unless you come in tomorrow morning and tell me not to do it or unless you tell me something different today. And he said, no don't do it, I'll think about it tomorrow. And I said, Dr. Rossby, I won't take that for an answer, I'm telling you that tomorrow morning, unless you tell me to do something different, I am going to do what I think the department ought to do. And he said, well I don't want you to, and I said well that's all right cause I'm going to do it anyhow. Well he said, I've got to go off to a meeting and I cannot think about it now. Well I said, I'm just warning you. He said, I'm going to go off to my meeting, and off he went. And so the next morning I went ahead and did what I said I was going to do and the subject never came up again between us because he had forgotten all about it. So when things got difficult that was the technique I used with him. It was a pretty nifty technique for a young instructor who didn't have his Ph.D. to work with a distinguished head of a department but it was.....

Droessler: And working under Rossby for your Ph.D.....

Benton: The only thing I could do, believe me (laughing), but at any rate I didn't want to stay at Chicago. I had been at Chicago a long time. I was well known to everybody there. I was comfortable. They knew me well, I knew them well. I didn't think I had that much more to learn at Chicago and I wanted new horizons.

I had the idea for a long while of getting a background in engineering because I'm interested in application. I'm a bit of a dual person in my own intellectual life. I like theory and I've done some papers that are purely theoretical and on the other hand I'm a rather practical person who rather likes to work on applications and who has been involved in later years I think more effectively than in science, in administration.

As a matter of fact, looking back, I think that the contributions that I've made to meteorology over the years have much more been in the field of administration than in the field of science. My contributions in science are not perhaps totally negligible but they don't rank with those of many, many others. I think I have made some good contributions to the operation of meteorology both academically and government. But at any rate, I wanted to get involved in more practical things and I'd thought for many years about getting into engineering. In fact, after I had my doctor's degree, I went down to the Illinois Institute of Technology and talked to them about the possibility of getting a dual degree and going on and studying in engineering; it was a very negative experience. In those days, many of the engineering schools were quite narrow and I found out very quickly that this was not the place for me. For example, I had a good background in thermodynamics and physics and yet they informed me that that was not acceptable in engineering; that I would have to go back and take a course in heat engines. And I was not about to go back and study heat engines. I thought I knew thermodynamics reasonably well and I thought I could learn what I needed to learn about heat engines to the extent that I had to learn about them. I had no particular desire to do that and I had no appetite whatsoever of going back and learning engineering thermodynamics after having taken it from a more scientific point of view. So that idea fell through.

But then I got the idea of going to a school of engineering and combining my work in hydrology and hydrometeorology with water problems at a school of engineering and in those days I got my doctorate in the early part, I think it was spring 1947. I don't know if it was in '47, but any at any rate, in those days those of us who were finishing our degrees were getting offers from universities by the dozens practically and I was busy saying no to offer after offer that I wasn't at all interested in. But there were two that I was interested in particularly. And I got two offers—one was from the University of California at Berkeley where they had a very strong program in water resources and the School of Engineering. And they wanted me to come to the School of Engineering and play a role in hydrometeorology and hydrology as assistant professor. And the other one was from Johns Hopkins to come to the School of Engineering here. Hopkins also had a program in water resources. Abel Wolman was the senior professor here and he was a remarkable man. A giant in the field of water resources and water resource management and he was chairman of the committee which was searching for a

bright young person with a meteorological background to join the University faculty in the school of Engineering.

I think there were two things that turned me to Hopkins instead of Berkeley. I liked Berkeley, I'd been there as an undergraduate for several years. It's a wonderful place. I was there from '36 to the beginning of '38. (loved Berkeley). But one thing was that at the University of California, meteorology was being developed at UCLA not at Berkeley and it was rather clear that if I went out to Berkeley and joined the staff there that we had a rather limited future in meteorology. I could do work in hydrometeorology, I could do work in hydrology, but it would always be as an appendage to the engineering program in water resources and couldn't really taken on a life of its own because we would have to compete with UCLA. The other reason was that as a product of Chicago I was quite comfortable with a small private institution and somehow Hopkins appealed to me with being a little bit more flexible, a little bit more desirable, perhaps a little more scientific in their engineering department than Berkeley.

So I turned down Berkeley and came to Hopkins. And again illustrating how things were in those days, they offered me \$4,500 as an assistant professor for the academic year. And I told them that I thought that was a little low. They told me that at \$4,500 I would be the highest paid assistant professor in the School of Engineering. And nevertheless, they made an agreement to raise my starting salary to \$4,800 a year. So I came to Hopkins at the very princely salary of \$4,800 for the academic year in the Department of Civil Engineering and that's where I have been ever since. Although I have certainly taken two extended leaves of absence during these years and have had a varied experience during these 43 years that I've been at Hopkins now.

Droessler: And both of these extended leaves of absence were associated with the further development of meteorology?

Benton: Absolutely! I had several tours of duty I'd like to say in meteorology, in government. The first was as an observer in the Air Force, the U.S. Weather Bureau which I've talked about before and following that with my work with Thornthwaite in the Soil Conservation Service. But it was in a program that related to meteorology and climatology. The second tour of duty in government was when I was in operations analysis in the Army. And the third tour was an extended leave of absence to be the first director of the Environmental Research Laboratories under Bob White and when ESSA began many months later. And then the fourth was the associate administrator of NOAA in the Carter Administration. So all of my periods of time in government were at an increasingly senior levels that related to meteorology.

- Droessler: While as I remember, ERL was really the first time that that ESSA tried to put together in an organized, coordinated way the research capabilities in-house.
- Benton: That's correct.
- Droessler: And you were asked to be the first director out there?
- Benton: Yes, yes.
- Droessler: Was it to be established at Boulder, Colorado, right from the beginning?
- Benton: Yes. I established it out there actually. What happened was that ESSA was formed and Bob White who was director—oh, pardon me—chief of the Weather Bureau, became the administrator of ESSA, the director of the Coast and Geodetic survey, became the deputy administrator of ESSA, and ESSA was formed with a variety of operational wings.
- Droessler: And we are just getting in to the move of one of George's major, his first leave of absence from Johns Hopkins to work under Bob White as the first Director of ERL. And I was going to ask you, George, whether or not you already had personnel there and laboratories already formed or whether you had to begin ERL from scratch?
- Benton: Okay Earl. As I remember it and I'm saying this without going back and checking, but as I remember it, there were three major line components; operational line components of ESSA. One was the Weather Bureau, one was the Coast and Geodetic Survey, one was the Fisheries, if I remember correctly, and one was the Research Laboratories because it was decided to put the research programs together. Now in order to get the research laboratories started it was also decided to put research programs together. Now in order to get research laboratories started, it was also decided to move some of the activities of the Bureau of Standards over to the new ESSA. So ESSA was really formed by the Weather Bureau, the Coast and Geodetic Survey and some part of the National Bureau of Standards.
- Droessler: I think we should mention what the acronym means, ESSA.
- Benton: Environmental Science Services Administration.
- Droessler: Right.
- Benton: (Laughing!) And the part that was moved was the part that had to do with the effect of the atmosphere on communications. It was essentially the

telecommunications laboratories that I think they were called that actually, telecommunications research labs of some sort. There were two of them as I remember it. And these were moved over at the same time, fortunately there were some very capable people out in Boulder where these activities were being carried forward. The telecommunications people would come in to the research laboratories and actually in those days it was the research laboratories not the Environmental Research Laboratories. The E was later added but I always like to think of it as ERL. The telecommunications people that came over to ESSA were housed in the main building in Boulder, the main Department of Commerce building on Broadway. And I remember having some extended conversations with Bob White who was taking on the job as new Administrator of ESSA. Bob in the first couple of months kept the title of acting head of the research laboratories as well as the Administrator of ESSA while he was looking for someone to take on the job of Administrator, as head of the research labs. Bob and I had many extended discussions about this. He had felt very strongly that the headquarters of the research lab should be set up outside of Washington were they wouldn't be subject to the alarms and excitements and frustrations of the budget cycle and to all the kinds of crisis that arise in Washington.

Droessler: Ya, the day to day ones.

Benton: Right. The day to day ones. So he conceived the idea of setting this up out in Boulder. And I agreed that this was a good idea. Of course, Bob was a motivating person (forcer). He was much more knowledgeable about the ways of government than I. I had never really been in government at that kind of a level of responsibility before. When I went out to Boulder we had a variety of responsibilities. We had a group of research laboratories which were moved from the Weather Bureau. One of the things that was done at that time was to take the research labs out of the Weather Bureau and move them into the Research Laboratories. That was good in a way because it enabled these research labs to stand a little aside from the day to day problems of the Weather Bureau and it also enabled them to build better contacts with the other Research Laboratories from other groups. It had its negative sides because over the years the Research Labs that had formally been a part of the Weather Bureau tended to drift too far away from the Weather Bureau and the research and the operations arms in meteorology became too distant. And that's a problem which even today haunts NOAA, but that's getting way on in the story. The research labs that came from the Weather Bureau were the National Severe Storms Laboratory with Ed Kessler, Joe Smagorinsky's Geophysical Fluid Dynamics laboratory, which in those days was on Pennsylvania Avenue in Washington, D.C. The Air Quality Laboratory which Les Machta dealt with problems of diffusion primarily of radioactive materials but it also dealt with other diffusion problems. And let me see, I think

those are the three research laboratories in meteorology. Then there were research labs over from the Coast and Geodetic Survey. We had a Geodetic Research Lab, we had a laboratory that dealt with solid earth problems and that got earthquake forecasting and was heavily into earth quake forecasting. And this was one of my responsibilities. I knew nothing about earthquake forecasting. There was a chap named Aldridge who was head of that laboratory. He was fairly knowledgeable about that subject, but we were responsible for earthquake forecasting. We were also responsible for the elementary efforts in predicting solar flares that have come over to us from the Bureau of Standards and we had a group doing this. At this time I forget, when I had a agronomy laboratory headed by a very talented person named Elden Furgeson who was doing very pioneering work in early chemistry of the atmosphere, pioneer work. Elden was a tremendous man with a excellent group of people around him. I think perhaps there are two outstanding laboratories were in those days, Joe Smagorinski's Geophysical Fluidid Dynamics laboratory which was meteorology and Elden Furgesen's agronomy laboratory. The telecommunications activity, telecommunications research laboratory. I think I did a good bit to help here. I'd say I think I made most of my contributions in administration rather than in science. Although I am proud of the things I've done in science. And one of the things I've done rather early on in administration was to push for and help Gordon Little set up the wave propagation laboratory. This started as an new laboratory when I was head of the research labs with Gordon Little as a Director. It was quite clear that we had some very gifted people in understanding the atmospheric transmission of both electromagnetic and acoustic wave phenomena and this seemed the natural thing to do and Gordon was interested in. And I think the wave propagation lab has performed admirably over the years and has spun off today in which a way other groups have continued this work. Another thing I did was help set up the weather modification program. We had a weather modification group under Helmet Weickmann which I think we had set up as a separate laboratory. I think that was done at that time, bringing all the people from Weather modification out to Boulder to work. So we had a rather fare fluidng group of laboratories running from the east coast; Joe Smagorinski first in Washington and later I helped Joe relocate at Princeton. We had the Air Quality Laboratories which in those days were in Washington. We had the National Severe Storms Laboratory in Oklahoma. We had the Fishery's Laboratory out in Seattle, University of Washington. There was quite a group of laboratories that I was responsible for. We set up others. We set up a small group out in Hawaii which still exists today. And I was kept pretty much on the run. I got back to Washington every other week and we had administrative problems that required this. I would try to get at least once a month to the laboratories outside of Boulder to teach.

Droessler: I thought one of the most innovative things that you accomplished as the Director

of ERL was the establishment of cooperative institutes with a variety of universities and therefore a bridging between ESSSA, NOAA and the university community.

Benton: Yes! I'm proud of that too. I'm very good at acronyms and I think I was one of the ones if not the one who was responsible for CERES. We were trying to get CERES first and couldn't get it so we decided on CERES Cooperative Institute for Research in the Environmental Sciences. And Earl, I wish I could take more credit for this. I take some credit because I fought for this, and fought for it budgetary in Washington, got the funding for it and did alot of the battling that needed to be done to establish CERES. But I cannot take to much credit because we had a prototype setting there right under our nose namely JILA (the Joint Institute for Laboratory Astrophysics) which was a joint institute between the university of Chicago and the National Bureau of Standards. And this was a very successful academically run Institute which worked beautifully to flesh out the relationship between the academic community and the Bureau of Standards and I remember talking to one of the early directors of Bureau of Standards. Oh dear, what was his name? (stopping the recorder for a moment), Ed Condon of course, I had talked to Ed Condon a couple of times about the Univeristy problem and what we could do to improve our relationship with universities and Ed suggested to me that we ought to set up something with JILA inside of ESSA. And that led to the development of the cooperative institute series with the university of Colorado. And I am very proud of that because over the years that has turned in to a very successful organization and sort of was a pattern for later implementation of institutes of other universities which you'll remember well Earl, because we were together in Washington at a time we were building further institutes and further contacts. Let me say a little more Earl about building contacts for universities. I think one thing of the years that I have stood for in many, many different ways over the years is trying to bring about the enhanced cooperation between the academic community and the government. And while I was out in Boulder at the Research Laboratories I did a number of things to bring this about. CERES was the big step, setting up the first cooperative institute and getting this concept accepted but beyond this I determined that we need to have a program of research in the community, directly sponsored by the research laboratories. Now, You'll have to remember that we didn't have any charter for grant research so that we had to lean a little towards the applied side then going out for research but I felt that there was alot to be done here and a real need for building a connection between the academic community and ESSA and the research labs in particular. And therefore, I began hoarding money and setting aside money to use for contracts to the Univeristy community. And this was a rather revolutionary idea. Certainly new for ESSA.

Droessler: Absolutely!.

Benton: There were about thirty. Our budget in those days was only about \$30 million dollars in the research labs and I remember well. And over a period of two years I was able to put aside about a 1/2 million dollars for research at universities. And I put together a whole series of contracts. Now I insisted with that research contracts have a science monitor in the research laboratories who would work with the faculty member of the university. And we built bridges this way in a number of different universities. A half a million dollars in those days was a lot more money than it is today. And I had to fight like the devil to get that money together because the only way I could get it was out of the hides of research laboratories and there doesn't exist the very many directors of individuals of research laboratories who would willingly give up resources to hard dollars which there could spend in-house to build an out of house program but I had to turn the screws. I think that that was a good achievement. Unfortunately, when I left that program it did not continue to expand.

Bill Hess is an old friend, one of very fine characteristics. Good manager, excellent manager, well rounded scientist, but this whole idea of University relations with ESSA never appealed to Bill, to the extent that it did to me. And I remember going back to fight it out with Bill Hess for some technical reason. This was perhaps 6 or 7 years before I left government and went to Hopkins. And I remember inquiring about research programs and there was still a lot less million dollars in it and with inflation that meant a lot less dollars, real dollars that existed when I left. And then when one of the budgetary hard times came on the laboratories, most of that was cannibalized so that the program pretty much disappeared. You write your initials in the sand and the sea comes and washes it away. I did my best.

Droessler: But one of the initials that you wrote in the sand it was called Cooperative Institutes.

Benton: Right!

Droessler: And while you were the Director of ERL you established a hand full of groups.

Benton: Yes, a small group.

Droessler: And all of them supported with association with ERL I believe. But that concept continued and grew within the ESSA and later on with NOAA and today there must be

Benton: It's 8 or 9 I think, something of that sort.

Droessler: And it included not only direct contact, and the budget for ERL but also the satellite part of NOAA and the fisheries part of NOAA and the National Weather Service part of NOAA.

Benton: Right.

Droessler: Each of these parts of NOAA have Cooperative Institutes which they nourish and which they value today.

Droessler: Congratulations George, that was an excellent idea I think.

Benton: Well let's see where we went from here. I guess that we, I think that while we are talking a little, we ought to mention Herb Hallaman. One who was at a very crucial time with ESSA and immediately before was assistant secretary of commerce for, which an area that included the Bureau of Standards and ESSA. Herb was rather small in stature, very dynamic man, very, very bright, very quick, very abrasive. He was not a very good Washington politician because he was too abrasive. He got people unhappy by kicking them too hard in the shins, speaking figuratively not literally. But Herb would get his counterparts in other agencies irritated with him by being too aggressive, by being too sure of himself, too absolutely certain about what needed to be done. On the other hand, he was a very good person to work for because if you worked for him he was, he would do battle for you. He would listen to you, he would get ideas from you and give ideas to you. And then he would go out and do battle for you. And I found Herb to be a very good person. Of course, Bob White was the one who worked directly with Herb and Bob could tell you much more about Herb than I can. But he was, he played an important role in the evolution of meteorology in those days and was a very good supporter of my efforts when I moved out in Boulder to build bridges to the academic community. He and I would see things eye to eye in these things and in those days things like this, I could, I was about to do some of these things, it was that I had Bob White who was very knowledgeable and very broad in his sense in what needed to be done. I had Bob White who I had reported to directly and then from Bob the chain went up to Herb Hollaman who also was able to go to bat for these kinds of things. Without that kind of support of higher levels, we wouldn't be able to do, we wouldn't have been able to get a CERES, we wouldn't have been able to put together the program of research at the universities. And I think that needs to be said. Because, again, academic people don't understand how this line of command is in government if you're really going to get something done. You know that full well Earl. Well I finally left Boulder. I left in the summer of 1969 to come back to Hopkins. I think there were a variety of reasons for it. One reason was that I thought that I had done about what I could do and partly it has to do with my temperament and way of working. I take off my hat to those dedicated people and I could name some of them who operated effectively

year after year within government. Earl, you did that for many years. Bob White for many, many years. I really take off my hat to people who can do that. My temperament is somewhat different. I like the university. I'm comfortable at a university and I like government. I found it exciting but my period in residence in government would typically be in the order of three years. Going in to government and giving it all I have for three years and then I'm kind of burned out and I'm ready to retreat to a university and being a little more intellectual environment and a little less abrasive environment.

Droessler: And I think your training under Rossby may have influenced you along this line. Rossby had that same fundamental philosophy that guided his work. You see that once you're not staying too long in a job and he told me, Earl 5 to 10 years in one job and after 5 years if you find it that you wanted moving out of that into another responsibility he said, reach out in front of you and put a obstruction in your way and then you'll have to go around it or over it. (Earl laughing, George laughing) And he probably talked to his students out in Chicago and got this message across in the same way.

Benton: Yes. Yes. Well it's partly temperament and it's partly that background as you say. But at any rate, I felt it was time for me to come back to Hopkins. I left in the summer of '69, came back to the university and I should of said that just before leaving Hopkins, I had been involved in creating a Department of Earth and Planetary Sciences. Oh, I should say that at Hopkins, in previous years our interest in earth and planetary sciences were pretty much distributed around. We had a geology department which was separate and which was hard earth geology and alot of structural geology, and son on. We had a meteorology group in the department of civil engineering which later on moved in to the department of mechanics which formed in 1960 and which I was the first chairman of. The department of mechanics took over all of the fluid mechanics oriented disciplines including self formed?. See I've been involved in mergers and reorganizations all my life. At Hopkins in the 1960's we put together the department of mechanics which was formed of mostly civil engineering, fluid mechanics came out, the mechanics department came over, in mass because they were mostly fluid mechanics. The aerodynamics people came over, for instance Kosher, who was later Dean of Engineering at CALTEC, and Stan Corssin who was the chairman of the Department of Mechanical Engineering and the expert in turbulence came over. I was acting chairman as Civil engineering of all things before the Department of Mechanics was formed and we put these groups together. This didn't include geology, this was in the engineering school so we had a department of mechanics which was a very distinguished department. I remember well the day when Francis Kloser, who later went to be Provost at the Santa Cruz Univeristy of California and then of Engineering at CALTEC and Stan Corssin

who was Chairman of Mechanical Engineering, came over to see me. The three of us had been plotting the department of mechanics for some months and came over, and told me that they had decided between them, that I should be chairman of the department of Mechanics. So I started the Department for Mechanics in 1960 and that became a world-class department. We brought in some tremendously fine people. Gerry Erickson who went on to be a senior Professor of Mechanics at the University of Minnesota. We brought in Clifford Grusdale from Indiana who was a world famous in computational rational mechanics. We had a great department. Kobosni Corssin in Turbulence, Kolser in Aerodynamics. It was a great bunch. And we had a group in material sciences, Green & Bob Pond who were senior professors and later on Department of Material sciences at Hopkins. It was a wonderful group, really wonderful. And I was chairman of that for 6 years. But towards the end of this time it had become evident that we needed to pull together the people that had common interest in the earth sciences. So we put together a different group. We put together some of the geology people, all of the geology people. And those of us who were specifically interested in geophysical fluid dynamics. And meteorological problems such as myself and Bob Long. And we moved over to the department of earth and planetary sciences and that happened just before, in fact it was planned when I went to Boulder, but took place immediately after I went to Boulder. Well when I announced that I was coming back to Hopkins in the fall of '69 they asked me to be chairman of the department of earth and planetary sciences. I guess by that time I had established a bit of a reputation as a department chairman because I had chaired the department of mechanics for 6 long years and I worked as chairman of the department of earth and planetary sciences for a year, and then, a long story, another long story, but not one that immediately relates to meteorology. I was to become Dean of Arts and Sciences and by that time we had phased the engineering school out and the engineering school of arts and sciences joined to form the faculty of physiology and the faculty of school of engineering together formed the school of art and sciences and then the engineering sciences as well. And I became Dean of that school for two years, 1970-72 roughly. And then I became the Vice President at Hopkins responsibility for Alwood Campus under a new President, Steven Molar and I did that in 1972.

And I was essentially for five years Vice-President responsible for the University Campus in Baltimore. Included also the School of Advanced International Studies in Washington. So I had by then a fair amount of administrative experience and academia to go with my previous experience in government and in 1977, I think was, in the late 1976 or very early 1977, I was asked if I would come over to Washington, in the Carter administration, as a presidential appointee as Associate Administrator of what was then NOAA. NOAA was formed after I left Boulder. And so my second long leave of absence from Hopkins came when I was over in Washington during the Carter administration. To correct matters, I

actively went over to Washington to be assistant administrator in charge of the Oceanic and Atmospheric Services. And there was no permanent associate administrator. Dick Frank was busy looking for an associate administrator and he asked me to come over as assistant administrator for the ocean and atmospheric sciences. And then, then he, Dick Frank and I got along very well together and he asked me to take on the job of associate administrator on a permanent basis and so I moved in to his office as associate administrator, and stayed on at that until the end of the Carter years. So I was gone for about 3 1/2 years to Washington and another extended leave from Hopkins. This was a continuation of the kinds of interest I had before, also a much broader set of interest. You know, I think I made a mistake before in saying Fisheries were in ESSA. I get confused between these two tours of duties. I think the Fisheries joined in when NOAA was formed.

Droessler: I believe so. I think Fisheries came in later.

Benton: Yes. I think Fisheries came in later. So I was wrong about that. But this meant a broader set of responsibilities. We had the National Weather Service, we had the Ocean Survey, we had the National Environmental and Satellite Service that Dave Johnson headed, we had the Coastal Zone Management Organization, we had the Research Laboratories. Some of the harder earth work had been spun off in the formation of NOAA over to the Department of Interior, so the old earthquake forecasting labs had disappeared. But there was still a pretty far fluid group with a lot of responsibilities and I found that my job was essentially being the in-house person. It's not that my interests were totally inwardly directed, but we had two other presidential appointees.

The administrator of NOAA was Dick Frank, who was a very bright young lawyer, extremely bright young lawyer who had worked on a variety of environmental legal issues, but who knew essentially no science whatsoever, virtually none. And simply couldn't appreciate from a technical point of view the kinds of task that were being undertaken. His interests were related to some of the legal aspects of international law, international seas, some of the legal problems associated with coastal zone management, that's where his interest lay. He simply didn't understand oceanography or meteorology in the slightest, and knew it, and therefore tended to be willing to delegate in these fields a lot of responsibility. As deputy administrator, was also a bright young lawyer and capable man who came in from the Hill where he had written some important environmental legislation. But again, had no background in science. And I was the third presidential appointee.

So there were basically three presidential appointees, two lawyers and me and neither of the lawyers having technical background. And so it rather naturally fell

to me to be responsible mainly for the internal administration of what was largely a scientific organization. And some of the things that I pushed at this time were continuations of my previous interest. I really pushed hard to try to improve relationships between NOAA and the academic community which I think had fallen into somewhat disrepair. The research labs had really washed out most of their sponsoring of work at the universities except for a limited number of cooperative institutes. And the cooperative institutes had not been fully implemented. There were a couple of them, but there weren't many. And one thing that I did was to help revive the cooperative institute idea and build new cooperative institutes and get them accepted at a variety of additional universities. And that is a continuation of something I had started years before in ERL with Bob White's help. Another thing I did was to try to revive a sponsored program in the universities. We didn't have much of a program with the university community, where dollars flowed from NOAA to the universities and I thought this was terribly needed. It was needed to build understanding to the academic community in NOAA and it was needed to help stimulate NOAA scientifically. And in order to do this I prevailed on my old friend Earl Droessler who sitting right here today, to come to NOAA and to take on the task of being the chief honcho for University relations in NOAA and Earl and I worked together but Earl did 90% of the work to try to build an enhanced program of contact with the universities. And I think, Earl, we did a good job for a limited number of years.

Droessler: Well, on two fronts we made some real progress. One, we established a NOAA fellowship program and let the other universities know that we were interested in bright young boys and girls to come into the NOAA sciences' areas. And I think that went on successfully for several years. And then of course, you already mentioned that we got very, very active in bringing together the cooperative institutes and pursuing the opportunities for more cooperative institutes which I think we have made another important mark there.

Benton: And the third one was putting together some funds that we used directly for contract work. And that required some stiff-arming.

Droessler: That's right.

Benton: I would literally give quotas to the various line components, operational line components of the amounts of money that they would be expected to come up with. We didn't seek to take the money away from them we just told them that we would expect them to spend million dollars in sponsored research at universities and that they jolly well had to do that or they could look upon being treated unfavorably at the next budget going on. And I think they got the message. But as I said, Earl Droessler did most of the work. A few years we made good progress, but after the Carter years when I left and then Earl left and then the

whole program just came on bad days and it essentially went backwards and another period of backsliding. So actually I see that program, as I say I've pushed it in Boulder and it disappeared after I left, and mostly disappeared and we pushed it again in NOAA and then it largely disappeared in the Reagan years.

Droessler: There are two other areas that I would like to cover before we complete sort of the add-development of the meteorology of the atmospheric sciences. One, I would like you to reflect for a little while on your association with the American Meteorological Society.

Benton: Before I do that, let me try to do another one Earl. I think that there was another thing that I take a great deal of pride in that I was related to administratively and that I would like to mention because I think it's interesting historically. The grand daddy of all big cooperative projects that involved government and universities, I think was the Thunderstorm Project at the University of Chicago, which was participated in by the Air Force, the U.S. Weather Bureau, the universities and I guess that's it. Several universities were involved and it was a big cooperative.

Droessler: Had quite a few universities.

Benton: Yes, quite a few universities. And this ran one year in Florida and a second year in Wilmington, Ohio. Now I was not working on that directly. My wife worked on both of the years of the Thunderstorm Project. I remember quite well, we were married in 1945 and on our anniversary on June 21st in 1946, our first anniversary, she was off in Orlando with the Thunderstorm Project, but she had worked for Vern Suomi at that time and they were building equipment to measure vertical velocities in the atmosphere. But at any rate, I was thoroughly familiar with the Thunderstorm Project and how successful it had been. And when I found myself out in Boulder in the Research Laboratories I was really involved in the revival of the large program, large complicated research program.

In fact, Earl, if you look up there, there's a plaque that's labeled BOMEX, which was the Barbados Oceanographic and Meteorological Experiment. And if you read the fine print there, it one that I'm proud of, it was given to me in Boulder, it calls me the grandfather, or the father, I forget which, of the BOMEX experiment. And I think the reason is that I was very, I've always been interested in measuring large scale evaporation, and realizing how totally inadequate our knowledge of the hydrologic cycle was quantitatively. And getting my head together with some of the people in Boulder, we conceived the idea of the Barbados experiment, which was designed to be an experiment to use a huge government array of ships, aircraft, university community resource people, to try to measure weather, evaporation and precipitation, the whole ball of wax over an area, in the vicinity of Barbados. And I think I was certainly the foremost proponent of that and the

pushing that through. Again, it wouldn't have been possible without Bob White, it wouldn't have been possible without others in Washington. But the idea for BOMEX really originated with us out in Boulder.

And other activity which had repercussions over the years, which we'll come back and talk about later, I'm sure, that I take pride in, is a mesoscale program today. Gordon Little had a lot to offer here. He was very interested in acoustics and using the electromagnetic spectrum, and probing the atmosphere. And he had some very gifted people worked with him. And we set up the Wave Propagation Laboratory. And we realized with the kinds of things that were beginning to occur, the time was becoming possible when we could really make major advances in, what is know called mesoscale meteorology but in those days that term didn't exist. And I remember that we had a retreat in Boulder, which I called, and we got together some fine meteorologists, research meteorologists from our labs, and Gordon Little and some of his associates. And we plotted a mesoscale program of research which we thought should be put together. And what it was, was to take an area in the vicinity of Denver, and experiment out of it the most advanced remote sensing equipment possible and dense network of surface stations in cooperation with the Weather Service, Weather Bureau at Denver, we would build this intensive research effort, in what is know called mesoscale meteorology in the Denver area. It didn't come off. Bob White was sympathetic and he asked me to present it to a meeting to a meeting of the line component directors.

And I remember going back to Washington, and one of Bob White's extended meetings, and presenting, very passionately and in great detail our plans that we had jimmied up for this major experiment which would have required the act of collaboration of the Weather Bureau. And bless his soul, George Cressman was exceedingly negative. George Cressman was terribly negative and there was a generally negative reaction from the operational people who thought we were far too visionary, that we had thought wouldn't work and so on. And it never flew, it just collapsed. But that was an exciting time and we had together a plan, which many, many years later developed in to PROFS and again I was again involved in developing PROFS. I cannot say that I, when I was in Washington in the Carter years had the idea for PROFS. It surfaced from Boulder and they brought it to me when I was associate administrator and I thought that was a great idea and pushed the PROFS budget with Dick Frank and got the PROFS budget funded and the Department of Commerce and through the Office Management and Budget and we got four million dollars for PROFS. And now of course it's turned into a laboratory, the FSL. So PROFS was very successful in the Carter years and going on into the Reagan years and now the FSL was a research laboratory in Boulder as I think is an extremely successful laboratory, and the history of that goes way back to the time in Boulder when I and the group of very gifted people, Gordon

Little and others put together this first idea of mesoscale; although we didn't call it that research program that never, never took fire. Although, of course later the whole Denver _____, the joint program between the Weather Bureau and the research laboratories and a lot of other groups to do essentially this many many years later.

Droessler: Well, the FAA is in to it now.

Benton: The FAA very much so.

Droessler: And we had those crashes of aircraft, you know.

Benton: And downburst studies that were carried on by UCAR. So, these were the early germs from which some of these things developed and you know there were gifted people there in Boulder. I keep going back to that as a job that I particularly liked. I remember when I was there in 1968, we had this very bright person in the telecommunications laboratory, no, pardon me, he was in the Wave Propagation Lab. He came in to see me one day and was telling me about these new push button telephones which were very new and were just being installed at that time, beginning to be installed. And he had in connection with this whole program that we were going to undertake in the Denver region for mesoscale studies using modern terminology. He had the idea of setting up a local weather information system. Where we would convince the telephone company to put in push button telephones on a priority basis to anybody that wanted them in the area. And setting up a scheme that he had developed where you could use push buttons to find out what the weather was any place in the area by simply dialing in an entirely automated code with push buttons. And it sounded very visionary. And it was very visionary. And it's been about 20 years before that became common usage. But that wasn't my idea. That was an idea of very gifted young man in the research laboratories. But he foresaw the future very effectively. And I think our ideas were frankly, they might not have worked in those days. The remote sensing equipment that we wanted to install as part of the study was not really adequate to do what we wanted. Perhaps we were a good idea and in order for a good idea to work it has to be not only a good idea but timely and we were rushing it. The remote sensing equipment wasn't good enough in those days to do the job that could be done many years later.

Droessler: But after 10 years, 10-15 years of development but we have some excellent equipment now for mesoscale meteorology.

Benton: Oh, of course, of course. The mesoscale research programs that came on later were tremendously successful and led to STORM and other things. But at any rate, I think the germs existed back then. And I think that in the largescale

studies, BOMEX renewed and set a pattern of international cooperation and some of our thoughts on mesoscale were applicable in later years as well.

Droessler: Well George, I believe we can get on with your comments and your perspective on the American Meteorological Society. And you have served in the Society, and the Society affairs for a long time, including presidency of the AMS. So if you would just talk about AMS, its importance, and how you see the role of the American Meteorological Society, and some of the people in the Society who helped to shape it and move it along, including our good executive director, Kenneth Spengler.

Benton: Well, as you well know Earl, the Society is a very important force in meteorology and the United States and the world. My association with the Society goes back into the very early years of the war, perhaps to some degree, even before that, World War II, that is. That is the war to most of us old timers around here. But I guess my very close association with it came in the postwar years. And I think this isn't entirely related to my development as professional scientist; it's in part related to changes which occurred in the American Meteorological Society. I didn't have much awareness of the Society as a major force until after World War II came to an end. And I think in those postwar years as the Society grew and developed and strengthened that a lot of the beneficial things that occurred could be traced to Ken Spengler. I knew Ken when he was a major in the Army Air Force, as it was called in those days. And he was stationed for a while in Asheville, North Carolina, the headquarters for the Air Force Weather Wing, near the end of war this was. And I remember Major Spengler very clearly. And I remember very close to the end of the war his telling a group of us that he had decided to take on the post of Executive Secretary, I think it was originally called and later became Executive Director of the American Meteorological Society. And I remember thinking to myself, What is a bright young guy like this want to do taking on a post like that? It's a bit of a backwater and he's too capable to drown himself in, and shut himself away in such a position. And I was wrong. I like to talk about the times I was right. This was one time I was wrong. I think the position that he took was a bit of a backwater. At least that's my impression of it. But he certainly made it in to something different over the years and I think none of us can do anything but feel a debt of gratitude for Ken for all he did for the profession and for the Society and building it to a very strong and active group with a major impact on our field.

Droessler: I don't really think he would have taken it on except that he was encouraged by Houghton and who was at that time president of the Society and by Rossby who was the incoming president and of course the emerging power in meteorology in our country. And I think these two people, they were very hard to resist when they both get together to do something.

Benton: Yes, that's true. Either one of them was hard to resist. But I'd like to just say I think Rossby was in the merged power and Henry Houghton was an emerging power. But they were both very very important people in these years. And I think they had a vision of what the Society could be and should be which I was too young to have at that time and I certainly didn't realize the full potentialities, but at any rate the Society did grow and strengthen over the year. the acquisition of the Headquarters on Beacon Hill was a very major step certainly. But the membership has grown steadily, the publications have grown steadily and become more and more important, the number of meetings and the quality of the meetings and the breadth of the subject material covered has grown. The activities of the Society have grown over the years. All one has to do I think, I've never done it but I think if you prepared a chart of the numbers of people that attend the annual meeting and plotted it over the years it would pretty much be a monotonic graph. But it would demonstrate the huge growth and the impact and importance of the Society over the years. Under Ken I think the Society was oriented towards, first of all for broadening itself. It tended to try to make itself a home for a variety of groups and I think this was a very wise and forward looking point of view that Ken brought to the Society. There was a broadening of the Society, services such fields as hydrology, hydrometeorology, upper-atmosphere physics, and so fourth. In addition to these the emphasize to this was primarily on meetings and publications. And it began as I said before in both of these efforts, the Society has been very successful over the years. Having said these things and having said how important the Society has become, then let me say something that I don't want to be interpreted too negatively and I certainly don't want to indicate that the Society as been other than successful over the last many decades. But there was one area where I think the Society didn't contribute as much as it might have. And that was the area of being a spokesman for the profession of meteorology and policy issues in Washington, and also representing the profession in Washington. Ken, I think, had his eye very firmly on building the Society through meetings, through expansion of scientific scope, through publications. He did not pay as much attention as he might have to the role in Washington, which has of course emerged as a very very potent role as the primary funder of science in the United States. It's interesting looking back that I think that the role of being a spokesman for the profession in Washington sort of went a little bit more in the direction of UCAR. Perhaps again because of personalities involved. Walt Robert's was very much involved in understanding and trying to understand the problems of science, the funding of science, he was very involved. And after this when Bob White came in as president, he of course was an old Washington hand and as president of UCAR, played a very significant role in Washington. And had a very strong presence in Washington. And I think that over the years, UCAR has sort of moved in to the vacuum that was in some degree left by the American Meteorological Society and started to play a more important role in Washington.

And looking at it in prospective over a long period of time I think that pendulum is swinging now, swinging back more towards the American Meteorological Society. Well this is now 1991 and lets see when was it when Dick Hallgren became executive director?

Droessler: About 3 years ago.

Benton: About 3 years I think, something like 3 years ago. But Dick Hallgren of course is an old Washington hand. He's an old associate of mine from ESSA days. He first came in to Washington as an assistant to Herb Hollowman at the Department of Commerce back in the days when I was head of the Research Laboratories and ESSA and then later he moved over to ESSA and later NOAA and I worked closely with Dick for the first time I think in connection with the BOMEX experiment, Barbados Oceanographic Meteorological Experiment, where Dick was special assistant to Bob White. He played a very major role in organizing that very successful experiment. And then later years of course, Dick was in NOAA and played a very key role, first he was my deputy when I was for a short time, director of Ocean and Atmospheric Services and then later he was the Director of the Weather Service and very successful in that role, Assistant Administrator of NOAA. And when he came in as Executive Director of AMS it was only natural that that one area where I think that the AMS had perhaps lagged a little behind was strongly strengthened so that the pendulum is swinging. At the same time the leadership of UCAR has perhaps been a little less involved in some of these Washington matters. So that the role of, if you look at the perspective of precedence in Washington, spokesman for the Society, influence in the halls in Washington, the American Meteorological Society is on the ascendancy and UCAR is perhaps a little weaker today that it was when Bob White was president. This isn't bad, this is not bad for our Society. We need both the influence of UCAR and the influence of the AMS and I cannot see anything better for our profession than UCAR and the AMS both to be effective in this area and to be working in cooperation and seeing to it that the importance of our profession and the needs of our profession are understood in the Washington community of decision makers. So in retrospect in looking at the AMS I would say we've had many long and very successful years and under Ken Spengler he built the Society in a very positive way. He gave us a great legacy to build on, a very strong legacy to build on. But I've been very pleased in the last few years of how the Society has moved in to a somewhat different role and to a new arena with Dick Hallgren. This is good. You get two good leaders and if you can follow then with a third good leader each one bringing something a little different to the Society that is the best of all possible worlds.

Droessler: During the term when you were president, or president elect president, and past-president, immediate past-president. Who were some of the people that were associated with you in helping to guide and lead the AMS and what was the

nature of some of the problems that were about at that time?

Benton: Well you know Earl, my period as president I don't think was a terrible eventful period. I was president in 1969. Ken Spengler was running the Society very capably. He had things well under control. We were, I won't say there were no problems but in taking the Society in view of, in the long term view, we were growing, we were expanding our influence, our membership, and our scope. They were terribly eventful years in the sense of policy issues that came roaring to the floor. And quite frankly, during my year as president I was busier as president in trying to handle some difficult problems at the Research Laboratories in Boulder and the year kind of went by. Vern Suomi said that being president at the AMS for one year and I was the second one year president I believe, was sort of like being on the Tokaido Express in Japan that it goes so fast that the world slips by you before you know it. And that year went very very quickly. There were some excellent people that I had the opportunity to work with. Vern Suomi was president immediately before me and Vern continued of course on the Executive Committee as past-president and Vern was, as he always is in policy matters, extremely imaginative contributing person. I'm trying to think who succeeded me I think it was Gene Bollay and he brought a different set of capabilities. Gene was very capable person, more practically oriented, more oriented towards the private sector with a strong military background. It was a good group to work with but I cannot say that, I cannot really point with any major pride, any tremendous steps that occurred during this period. We had a comfortable feeling that things were going well. That a long term set of objectives had been identified and that we were moving in a reasonable manner towards these objectives. I won't say there weren't local crisis's. We only had problems with the funding of the MGA and it looked as though it was going to disappear from the scene at one time and we had to work like hell to get the funds to keep it going. But that was a recurring kind of problem in those days and from the long term point of view and looking back at it over twenty years later, sure there were minor crises but nothing major.

Droessler: But the publications of AMS continued and continued with excellence and increased probably in numbers and the meetings went on splendidly and the AMS grew under your presence. And I think that a happy a lot that you left the AMS.

Benton: Well, one likes to have the feeling to be able to say, well I did this or I did that! We're all egotistical to some degree. It would be nice to say that. I look back at my period as president and say well I functioned as president and let Ken continue the growth of the Society with the assistance from all of us.

Droessler: Well thanks George for your comments on the American Meteorological Society. Lets move on to your activities with the National Academy of Sciences and its

committees and boards and what was accomplished there for the advance of the atmospheric sciences.

Benton: Well that's a little more difficult to make comments on this in perspective, because I haven't played that key role in the academy as a whole. I've been on a substantial number of committees and panels and for a while I was the chairman for the National Academy of Sciences Committee on Atmospheric Sciences. I've certainly played my role here but I really haven't the perspective on how the activities and the atmospheric sciences fit in with some of the other scientific disciplines so therefore it's a little difficult for me to evaluate the contributions of the Academy in a broader sense. So my comments pretty much have to be restricted to the more narrow discipline of meteorology and what I say may not be more broadly applicable. Although, I think in one or two cases it may be but I'm not sure. Let's look then at the meteorological activities. I think my association with the academy began, oh a long time ago. I think perhaps one of the first groups that I was a member of and quite active in was the air sea interaction activity. When I was on the panel for air sea interaction and later chairman of that panel and wrote one of the publications, principal author of one of the publications of the panel air sea interaction. But I went on from there to work on a variety of other activities such as the national committee for the international hydrologic decade, the national committee for the global atmospheric research program, a committee on atmospheric sciences itself and later as chairman of the committee of atmospheric sciences. My general perspective here is that the contributions of the Academy in the atmospheric sciences have been weakening over the years. I don't know whether this is characteristic fully of other scientific disciplines. I suspect it may be but I'm not sure. Not as certain as I am in meteorology. The committees aren't sufficiently strong. They don't compare to the contributions they made many many years ago. I think the Academy back in the days when Rossby, and Berkner were involved was a very potent force. And as I remember it it was a report of the National Academy which was very much associated and involved in the whole process in developing UCAR and NCAR (the National Center for Atmospheric Research) but more importantly than that, my impression is that in the early days of the committee of Atmospheric Sciences and its predecessors, the membership was composed of the very top leaders in our profession and they used these committees as a vehicle to work out a consensus about what needed to be done in our profession and to work away to get it, by working together. My impression is that they were a very effective group and in recent years, you know I'm not even sure who are members of the present Board, they come and go and I don't pay much attention to them anymore. And the reports of the academy come in the mail and you glance through them and toss them into your stack of publications and they don't seem to have much effect. I saw this to some degree from both sides of the fence because when I was in

Washington, especially when I was there as Associate Administrator of NOAA, we encountered some problems and I turned to the academy to set up a special committee to help advise us with regard to a particular problem that was occurring. Got good cooperation from the academy but I cannot really say that the process pushed or helped us particularly or was terribly important. I don't know if the academy can be revived. Maybe again I'm using the words too broadly, but unless you can convince, unless you can really somehow strengthen the membership of this group, strengthen its impact, it's going to continue to be less important. We've been making these detailed studies for a long time. I think the first one was 1970. I may have my dates slightly wrong but it was at a time when I was very much involved in the academy. And we had a study out in Friday Harbor in the state of Washington, the summer meeting. To try to put together our view of what meteorology would be like and should be like in the coming decade or two. I forget the title of it now over the years. It can easily be ascertained what it was by going back in reading the record. But we had a very large number of atmospheric scientists from all over the country come to Friday Harbor, broke down into groups, had discussions and then tried to put the results together in a coherent report. It was only marginally successful I would say. The report made a little bit of a splash but it wasn't as important to report as something that had gone earlier but, it was more important than some of the more recent reports but I don't know if it, it did have a value in helping to build a consensus among meteorologists from many different parts of the country. I won't say it was without value but it was more as a consensus builder, as an opportunity to exchange ideas than as a result of anything that really happened as a result of the report publication process. And this regard again, UCAR had become the intellectual center for meteorology in the United States. We now have meetings out in Boulder and we hammer out our consensus in Boulder meetings to a very higher degree. And perhaps that's another reason why the academy had slipped especially in the atmospheric sciences. The need for the academy the consensus builder has diminished as a result of the importance of UCAR and of NCAR.

Droessler: So the child created by the academy has now become the ruler of the scientific activity.

Benton: To a considerable degree that is so and yet it isn't so. It has I think outshone the academy in its committee activities, but I think it's going too far to say that UCAR has really taken on the leadership. UCAR is more than an organization. It runs the national center which provides the intellectual center for atmospheric science activities in this country and a lot of the consensus building that I mentioned comes about as a result of meetings that are held in Boulder, very often sponsored to some degree or helped by UCAR but, the actual work of hammering out a consensus is very often led by scientists from other institutions not from UCAR so that it has provided a home where we all go to meet others from our profession

from all over the country and from all over the world and where we have at it and where we try to decide what needs to be done in our discipline. And we don't need the Academy as much therefore. Well you know Earl why don't you make that remark on the tape. I think it was a very useful remark.

Droessler: The academy has got to attend to a wide range of scientific activity and we in meteorology - atmospheric sciences were very fortunate to have the concentrated attention of the academy and the leadership of the academy, Dr. Detlev Bronk and others for about a ten year period and I don't think we can ask for the academy to be concerned about our field in a focused way for a longer time than that. And I believe, we meteorologists must be forever grateful to the academy for what it did in helping us to make that step function increase in the activity in our field.

Benton: Well that is a very perceptive remark and I think it's a very valid one Earl. It brings me back to something I said earlier however. And that is that I'm not at all certain whether my perception that the Academy is much less important in the atmospheric sciences that it used to be. It has a parallel in other disciplines. I do have the impression and it's only an impression that the academy has slipped somewhat overall in its impact. That the relationship between the academy and the executive branch is not as interactive and as strong as it was in the early days when science was growing. Science is not in the dynamic growth phase as it was in the years immediately after World War II. And the Executive Branch doesn't listen to the academy as extensively as it did in the 50's and early 60's. Therefore, it is my impression that the academy has a weaker effect in disciplines far broader than the atmospheric sciences. And I would be interested in knowing whether you agree with this.

Droessler: Yes I believe that the leadership in Washington which really is the Congress, with some activity from the administration from time to time. It is now listening to a broader voice that's coming from the universities and the industrial committee and the federal government itself where scientific leadership has emerged. And back in the 50's and 60's that scientific leadership was very very scattered throughout our country. And the academy was successful in focusing it and bringing it together and coordinating within the academy. And that was the voice that Congress and the administration listened to at that time. And the strength of industrial science and technology, government science and technology and university science and technology, was far far weaker that it is today. And there are many strengths now in our country and the Congress listens to as many of them as can effectively get to Congress with their message.

Benton: Well this is true but on the other hand that has been a weakness too, because one of the problems that we have today is the cacophony of science. You take any major issue that comes on the agenda, right now one of the big ones as you well

know is global warming. We were talking about it in the car earlier on the way up here. Global warming is very much on the agenda today. It is part of the global change problem which has been perceived very widely as one of major importance for the future. Congress and its various activities and its very hearings and its deliberations and the executive branch as well, tends to listen to large numbers of people. Congress finds that there are many different points of view. The hearings that are held have scientists from different parts of the country, each of them expressing different opinions. One saying that global warming is going to be a major catastrophe for example and another one putt-putting the effect of global warming entirely. And the layman doesn't know, and the intelligent layman, the policy making layman isn't really a really a scientist, doesn't know how to listen to today. He got an array of information and misinformation beating down on him. He usually has in Congress, every congressman has a scientific staff member now, who very often is not a terribly perceptive young person and often doesn't really know who to listen to either. And the congressman really often winds up listening more to his own science assistant than he does to anyone else. And the result is that we are not getting a clear statement of what this country needs to do from a scientific, or what the scientific background is for policy. We aren't getting that message through the decision makers in a clear and concise way. There is a conscience on a lot of disciplines and a lot of times that consensus gets lost in trying to convey the information to the nonscientist decision maker. And that is a real problem Earl.

Droessler: Well, back in the last 50's and early 60's when I worked at the National Science Foundation, we got the message from our leadership that when the National Academy of Sciences spoke on a matter which the academy had considered and brought together the difference points of view and then arrived at a consensus and the, hopefully the best scientific basis for that consensus that we were to follow that and we were not to question it. That was the authority. And so if our leadership in NSF looked upon the Academy that way I can assume that leadership of science throughout the country and the leadership in the halls of congress and the executive branch did the same thing or something very close to that. And this is now lacking, the authority for speaking on behalf of science in a voice that is a measured scientific basic one and at the same time that is not immediately questioned and ripped apart by other elements of the Society included in our media.

Benton: You're right, and it's a real problem. And one of the problems also is that very often that the people who turn out to be spokesmen are not terribly strong scientists. They are people who have managed to put themselves in a position where they represent something that they don't fully understand. Let me give you an example of a case where I think that the Academy was leading us down the garden path. When I went back to Washington as Associate Administrator of

NOAA it was at a time when the whole problem of precipitation enhancement through artificial insemination of clouds was being pushed very very strongly. And one of the first things I encountered that there had been a report come out, I'm pretty sure it was from the academy, quite certain it was, which insisted that we should play a much stronger hand in efforts to enhance precipitation through cloud seeding. And as the science member at the head of NOAA, I was very much bombarded with demands that we should follow this and move in this direction. Follow the lead of the Bureau of Reclamation which was moving in to operational seeding in the west trying to increase the precipitation. My view was that this was much too quick. I just didn't believe that we should get into operational activities. I didn't think that this was right for NOAA to do. I very strongly supported the idea of a steady increase in the funding for the basic cloud physics because there were so many issues and so many things that we didn't understand about how clouds nucleated, how they developed, how precipitation formed. There was so much we needed to know and a strong program of research in cloud physics would have been a very fine investment. Instead we were being pushed to take on an operational role. And I remember very well going up on the hill and testifying one time for a senate committee and Senator Bellman from Oklahoma, who has long since passed away from the scene. I don't know if he's alive or dead today, I assume he's no longer with us but I don't know. But he gave me a terrible time beating me over the head, because there was a drought in Oklahoma and he wanted to know why NOAA wasn't out there seeding the atmosphere and producing rain in the drought. And I simply couldn't convince him that first of all we couldn't make rain when there were no rain producing clouds, that the best possible thing we could do was somewhat to understand precipitation, not make it. And the second thing was that I couldn't convince him that there was nothing that NOAA could do to alleviate the drought in this state. We were under terrible pressure and I think that, I have the impression that all too often the Academy is now under the influence in a particular report or a particular effort that is too much under the influence of scientists who have an axe to grind. And the reports often tend to be somewhat suspect because we feel that they're not sufficiently impartial. They're not made from a group by a group of people who are not sufficiently above the fray to be able to evaluate objectively the opportunities that exist in that particular area.

Droessler: George, let's move on into international atmospheric sciences and the role that you played there.

Benton: Well this is an area that I've taken a great deal of interest in and had a great deal of pleasure from. I'm very interested in science in other countries and I've been fortunate over the years in having been able to make some very good friends in other parts of the world and to play a role, although not a tremendously major role in the international cooperation between the United State and other countries in

the atmospheric sciences. One of my first activities here was, occurred when I was director of the Environmental Research Laboratories when I had a rather extraordinary opportunity to head the scientific delegation to the USSR, which was the scientific delegation on weather modification., because in those days the Soviets were operating the hail prevention program in the Caucasus and also in parts of southern Russia and in parts of Asia, which they claimed was tremendously successful in reducing the damage due to hail storms in these important agricultural areas. And the kinds of results that they were obtaining simply hadn't been able to be replicated although no major reference had been made at that time, has not been replicated in other countries. Now this visit came about at a very interesting time. The cold war was very cold between the Soviets and the United States. This was shortly after Khrushchev pounded his shoe on the podium in the United Nations and denounced the United States for over flights of the Soviet territory by spy planes, the U2. The situation was rather grim, but it did alleviate sufficiently for following this for scientific interchange to begin again and our delegation was really the first delegation to go to the Soviet Union after this very intense period of cold war and after the thaw had began. But we were greeted with care and the whole trip was planned with great care. Our trip to the Soviet Union in '69 was to be followed one year later by the scientific delegation from the Soviet Union and weather modification whether it was to come to the United States and it is indicative of the hostility that existed at the time and the way the two countries were looking at each other, that itineraries, the statement of where two delegations would be allowed to go during their visits were negotiated between the State Department and the Administration of Foreign Affairs of the Soviet Union and every prize ? that they gave us in terms of allowing us to go some place that we particularly wanted to go, they demanded a quid pro quo to go some place in this country that they wanted to go. And of course the negotiations were hot and heavy. I wasn't directly involved in those. That was conducted by the State Department. I was indirectly involved because of courses we were attempting to coach the State Department as to where we thought we wanted to go and what kind of permissions we would like to have granted. But it was very interesting trip. It really introduced me to some of the key people, key players in the Soviet Union at the time. We spent a few days in Moscow then we went down to Tbilisi in Georgia and visited the areas of hail prevention activity out there. It was quite a very interesting experience. Then we drove by bus over the Caucasus into the north part of the Caucasus and visited some more areas, some research groups in that area. And then we went over to Moldavia and visited some hail prevention activities there that we went up to Kiev, Leningrad and back to Moscow. We were there for a month. It was a very fine trip. We learned a great deal. It was written up, two reports in the Bulletin. I gave a very short report as chairman of the delegation. Lou Battan from the University of Arizona who was a specialist in the area of cloud physics, gave a much more elegant and more detailed report of the trip and so we do have a

permanent reference to it. I think it might be useful for me to make some very broad remarks which might be of interest in the historical perspective. The details of the trip aren't that important but let me give you some of my impressions of how science seemed to function in the Soviet Union and of the level of scientific wariness with which the two countries approached each other. In the first place, I found it almost impossible to come to any intelligent judgment about whether the hail prevention program of the Soviet Union was really effective or not. It was quite clear that a party line had developed with regard to what was acceptable to say about the hail prevention program. We attended for example, the meetings of farmer cooperatives, we visited with local groups, agricultural groups who told us over and over again how important the hail prevention program was to them in terms of them preserving their crops. But one had the feeling that this was the party line and that whether or not these people really believed it or not, was very hard to ascertain. Certainly, no hard data was shown to us and I tried my best to find out what statistical evidence there was that this program was really effective and I was never presented with any evidence that seemed to me to be clear proof that the program was indeed successful. So my perception was that Sheverenz and a few others who were quite important at that time and Federov, who was an academician and an important one of the very powerful. I remember he was a member of the Polit bureau, a very powerful man who was very interested in this field and the scientists who were working in hail prevention had his ear and his support. And one had the feeling that the program had taken on a life of its own which was totally unrelated to science and was related rather to perceptions and convictions of some of the leading politicians who were pushing this field for one reason or for another. However, the community, the group came back to this country and we did recommend that the United States try to check out some of these things. Not in an operational program but in a research mode. And our report on the Soviet experience and what we thought the United States might do led later to the development of the National Hail Research Experiment in Colorado which went on for several years under the leadership of Dave Atlas. There is nothing that has happened over the intervening years to cause me to change my evaluation. I think the success in hail modification in the Soviet Union, if it existed at all, was vastly over-stated. With this taste of international activities, I continued my involvement in other areas, most particularly when I went to Washington again with NOAA during the Carter administration. Bob White had for many years, been the so called permanent representative of the United States to the WMO. I also have to smile at that term, Permanent Representative, because we were very impermanent but it drew a distinction between us and those who were present in Geneva and some community because we were the official representatives of our country to the WMO. Bob White had been the representative for the United States for many years but when he left at the end of the previous administration and the Carter administration came in with a new group of appointees, Bob gave up his post, in fact, necessarily gave up his

post as US perm rep to the WMO and I took his place. Essentially four years I was the US permanent representative to the World Meteorological Organization. In this role I was elected as a member of the executive committee of the WMO which is a fairly large group which meets annually and of course led the US representation to the Congress which occurs in the WMO every four years. It was a very active and interesting time in the WMO for these four years. There were a number of issues that came up that we were forced to deal with. Let me just tick off a few of them; the whole problem of weather modification was important in the international scene and we launched during this time the precipitation enhancement program which was a program of the WMO, and involved field experimentation and field studies in Spain. I was chairman of the WMO committee on precipitation enhancement and in this capacity made several trips to Spain to review the work that was going on there at the Spanish field site. Harry Orville during the later years was working in Geneva at the WMO as a head of a small group which was involved with the program. Another activity that was always with us in the WMO was the effort to assist third world countries in their development. We had a major program which went under a separately different program, which names were changed during this period. Where the United States and other developed countries advanced support of one type or another; educational support, advisory support, in some cases helping countries procure equipment and so on to help strengthen meteorological activities in other developing countries. This was an important problem and one that took a good deal of our attention and the executive committee and one that I had a great deal of sympathy with. I think the United States gained a great deal from having scientists from other countries come to the United States from developing countries come to the United States to study. The present Secretary General of the WMO, Ubas, studied at MIT and the same can be said of many other representatives to the WMO scientists from other countries who had been in the United States at one time or another. And these interactions made me a very strong supporter of the role the United States I think must play in helping developing countries and in helping develop the scientists from developing countries. This was driven home to me and I made an effort when I was in Geneva to continue the tradition that Bob White had started, inviting key representatives from African countries to dinner during the executive committee meetings and also during the congress. And it was rather heart warming to me to find the high positive feelings that were held towards the United States by many scientists from Africa, many of whom had studied in the United States at one time or another. Another issue that was of great importance at this time was the developing climate program, the world climate program. Which was really the antecedent of the global change program. And here we were very active, the WMO was very active, cooperated with UNEP, United National Environmental Program which was part of UNESCO. And under the aegis of, the joint aegis, I remember it of both groups, there was the First World Climate Conference was

held in Geneva. Bob White was co-chairman of this along with a Soviet co-chairman. Let me see what other issues came forward, it was really at the WMO that I began my association with Chinese Meteorology, which had been a very important set of exchanges and a very important part of my international activities. The People's Republic of China and it was represented at the WMO by Zou Jing Meng, a very fine person and a very old friend by now. He and I met for the first time in the WMO and at that time the United States and People's Republic of China were very distant. This was before the ice had been broken by Nixon going to the People's Republic of China. And my relationship with Zuo at that time was not very close but we did get to know each other. And later on that relationship flowered. In later years the United States of course built very strong bridges to meteorology in China. I might say here, because it goes back to something that we had talked about in the past. That the first opening to China came through a visit of the American Meteorological Society to China. A group of past-presidents of the American Meteorological Society visited the People's Republic of China. The American Meteorological Society had broken the ice by sending a delegation to the People's Republic in 1974 I believe it was, 5 years I know before our delegation went. This was a group of past-presidents of the Society. Unfortunately, I was president too early to have been included in this small delegation. That delegation was entirely composed of past-presidents but it was a small delegation and it was headed I believe by Dave Johnson and I did not go on that trip. However, that started to break the ice and it was a very important step in the warming of relationships between the People's Republic of China and the United States. And following Nixon's visit to China and later on Frank Press went as the presidents' science advisor and negotiated a very broad agreement with China for scientific exchange. Well following this, again, meteorology had the first delegation in a particular discipline to a foreign country, in this case China. And I think Earl, that this indicates how international meteorology is. I think in a way we're the most international of all sciences. The atmosphere knows no boundaries, the weather systems move around the globe without regard to political barriers.

Droessler: That's right!

Benton: And it says something for our field and for the international nature of our field. But first of all there exist at WMO which was a power of strength and is the only, UN, well the international telecommunications union is science oriented but, in turns of a specific science such as atmospheric sciences or physics or chemistry or biology, meteorology is the only discipline that has a entire UN agency, specialized agency devoted to it. And it had been a very potent force in our field and it also is related to the fact that ours was the first delegation to the Soviet

Union following the weakening of the cold war and ours was the first delegation to the People's Republic of China following the cold war period with China. And I had the fortunate chance to head both of those delegations in part of my career. But before I go on let me finish up with China. Our delegation went there in 1979. It was a fascinating trip. I've always been interested in China but I had become much more so as a result of my visits there. I think I have been in China now six times since 1979. But we were met at the airport, the old airport, its now been changed, not its location, but there's been a new terminal developed. Given the VIP treatment, not taken through customs of course, we were taken directly to VIP rooms while our baggage was whisked away. And we had very long and extensive discussions with our Chinese counterparts about how the United States and China could cooperate. It was to a very large degree how the United States could help China but there was good will on both sides. And we mapped out some plans which led to some time later to, I forget the exact timing of it, it can easily be determined by going back and looking at the record. But it led some time later to the signing of a formal protocol between the United States and China, which I negotiated bit by bit with the Chinese. The actual signing took place in China and Dick Frank who was Administrator of NOAA went over for the signing. I held the fort down in Washington but, it has always been a satisfaction to me that I negotiated that agreement and it is interesting to me that the history has some what gotten rewritten because my friends in China now say that I signed that agreement, although I didn't. And just not to long ago, in 1989 the Chinese held a celebration of the 10th Anniversary of the signing of the agreement between the United States and China and they invited me to come to that signing. And it was a great opportunity to visit China again but, I was a little amused although I didn't say anything that I was continually referred to as the one who signed the agreement not the one who negotiated the agreement and that quite literally was not true. But we set up a plan for cooperation between the two countries, we set up a committee made up of representatives from the two countries, I headed the group from the United States Zuo Jing Meng headed the group from China and we met in alternate years, meeting one year in the China the next year in the United States, back and fourth in that kind of exchange continues to this day. We also made plans for taking a small group of Chinese scientists, Chinese meteorologists into the United States and we took in NOAA, a group of about ten people from what is now the State Meteorological Administration and helped to train them. And now when one visits China, I've been there many times, one continually meets people, meteorologists who have worked in the United States who are familiar with the United States who speak English reasonably well and there is a tremendous reservoir of good will between the two countries and again it's one of the things that I'm proud of and that I had a hand in helping to build that relationship which I think is a very special relationship today. I shouldn't close in regard to China without saying two things; one that I wanted to say is that on one of the trips to China that I have made my

wife and I spent four and half months in China. I spent two months at the University of Nan Jing giving daily lectures, in fact I lectured for two months, 5 days a week for two hours a day. But not only this, I wanted to make closer contact with the students who were in the class so I had about sixty students and young faculty in the class which was largely a review of where the United States was going in mesoscale meteorology. And I broke the class down into five groups of about 12 each and followed the lectures with a meeting for about an hour and a half with one of the groups of twelve each day. So, it was a rather intensive program, one that I enjoyed a great deal and one where I met and made many friends. From there we went on to Beijing and I lectured for two months again at the State Meteorological Administration. It was a very interesting time for me and it gave both of us an opportunity to strengthen our friendships with our Chinese colleagues, including our old friend Y? who was at that time Vice President of Academia Scenica but, still a very important participant in the activities for the institute of atmospheric physics in Beijing. I also had the opportunity to visit there several times and gave a lecture over at the institute as well. So for me personally, this has been a very heart warming and profitable professional relationship. Earlier Earl we were talking about the fact, even as we set here today, the Congress, the WMO which meets once every four years is on. The United States is now represented by Joe Friday who is Assistant Administrator for Weather Services in NOAA now. And it's interesting to note that Joe is, I believe, only the fifth permanent representative of the United States in the history in the WMO. And so perhaps that title "Permanent Representative" isn't so impermanent as I made it sound. There was Reichelderfer, he was followed by Bob White, Bob White was followed by me, I was succeeded by Dick Hallgren, and when Dick Hallgren went to become the Executive Director of the AMS, Joe Friday took on the task. So, there's only been five of us over the long history of the WMO and I think that in itself is a pretty unusual stability in international affairs which again is indicative of how our field has worked. But again, thinking of the WMO, again brings me back to the American Meteorological Society which over the years has been very important in the WMO. One thing that Bob White did when he was Administrator of NOAA and representative to the World Meteorological Organization, was to work cooperatively with Ken Spengler in presenting a positive view of American Meteorology in the WMO. One of the problems we had was the fact that in the WMO and during the Congress it was always fashionable to, for the various countries to hold receptions or lunches or dinners or some kind of entertainment. And this was done not only during the Congress when a tremendous number over the month of the Congress, tremendous number of these receptions and dinners took place. But also during the executive committee meetings when there was a continual round of receptions and luncheons and so on. Well it was quite interesting to me when I took over. Countries which were received, most of their tremendous amounts of support from the United States always had very lavish

receptions. Egypt which was a recipient of huge amounts of U.S. money always had an extremely lavish reception and the same thing could be said of many other countries who were beneficiaries of U.S. aid. On the other hand, we got virtually no resources from the State Department for entertainment. I think that I got something like \$250.00 dollars from the State Department for entertainment. And for a very bare bone luncheon for the members of the executive committee it always set us back well over a \$1,000.00 dollars and to run a reception was even more expensive. NOAA had a small amount of money which it had in a special fund which we all contributed to when, the way it was done in those days that if I was invited to speak somewhere and offered an honorarium, I was at liberty to accept the honorarium but only if I turned it over to this special entertainment fund which was then used to entertain foreign visitors. And I think I got \$250 dollars more from the administrators entertainment fund each years. And then it still left us short with \$500 dollars in Geneva you couldn't do anything and the American Meteorological Society stepped into the breach and it become the fashion and I didn't start it, it was started under Bob White for the reception and the luncheon to be held jointly by the U.S. Delegation and the American Meteorological Society. And this might have had in part its origin in the financial needs, but it very soon took on a life of its on and the AMS began to be accepted more and more as a representative of the United States and the international community. And I think it was good both for the U.S. Government and for the American Meteorological Society and that tradition has continued to this day. Right now Ken Spengler is over in Geneva representing the American Meteorological Society and Bill Bonner the current President of the American Meteorological Society flew over to Geneva for several days to participate in the AMS role at Geneva. I think this has been a very positive thing for all of us. Although, as I say, it probably got started for other reasons. Before closing that off, perhaps close this discussion by remarking that this problem of entertaining foreign visitors was one that bedevils us in this country and something needs to be done about it. I'm pretty sure that the situation is about today as it was when I was in Washington. But, as a U.S. Government official, I traveled abroad, I was always wined and dined in Japan, in China, in other countries all over the world and when visitors from those countries came to the United States, we needed to entertain them. And there was no way to do so, and this is of course a common problem. But coming from a university where as Vice President I could simple take any body to lunch that I choose, it was a bit of a shock going to Washington as Associate Administrator and finding that I had absolutely no resources to entertain for foreign visitors or prospective NOAA senior people that I was trying to recruit to Government service. I well remember a distinguished young oceanographer that I was trying to attract to come to NOAA to head oceanography from a scientific point of view in NOAA. And of course I brought him to Washington, took him out to dinner on more than one occasion, tried to sell NOAA to him, had a very comfortable post at the University and come to

Washington, come to NOAA. In this particular case, I wasn't successful, although I did recruit Ferris Webster to come later, but in a situation like this, you simply paid for the dinners yourself. I think those of us in Washington, a role like this wound up planning to spend \$500 or \$600 dollars a year on expenses that rightfully should have been paid for by the U.S. Government. Perhaps there is no way out of this bind, perhaps that just comes with the territory, but it's a little ungracious and I remember occasions when I paid for a dinner, personally for a dinner with representatives from African states to the WMO and at an expensive Geneva restaurant. And I just don't think it is right to expect this of Federal employees.

Droessler: George, I would like to ask you, who is George Benton? It is very important that we get some biographical information on you so that students of the future listening to your remarks will have an appreciation of who you were.

Benton: Well, let me say something about myself. I come from a family which has a rather varied background. My mother was born in the United States in 1876, but her family came to the United States from Edinburgh, Scotland. She came from a Jewish family. Her family however, many many years ago, somewhere around 1100, immigrated from somewhere in eastern German area to Ireland actually. Her maiden name was Davieson. And some of the families simplified the name to Davis, so there were some interrelations here and she always was fond of telling me she was dimly related to the leader of the Confederate rebellion, Jefferson Davis, but I think that might have been a flight of fancy of hers. I'm never quite sure if it was a relationship. It was a very very distant one. But part of the family later moved from Dublin to Edinburgh and then later still where they had a place on King Street. I had a chance when I was in Edinburgh one time to walk past the old family residence. The family had been gone from there for many many years, but it was a rather well to do part of Edinburgh so apparently they were doing quite well. The family came over to the United States in the mid 1800's and my mother was born in 1876. My father I don't know as much about. His family immigrated from Rega when my father was three and settled in Michigan. And later my father moved to Chicago. The family name was Blumenstock, and I was originally George Blumenstock Jr. I never really was comfortable with the name Blumenstock and somehow I didn't like the name Junior, being a Junior. And some years later I changed the name to George Benton, that was many many years ago however. I was born in Oak Park, Illinois, September 24, 1917, brought up in Chicago and was raised mostly on the north side. I went to Sullivan Junior High, now a High School on the north, but my family moved down to the vicinity of the University of Chicago at the time I entered High School. And I went through my three years of High School at High

Park High School in Chicago. A very remarkable High School in those days. I graduated in 1934. There were some very fine people there some of those who I am indebted to. I still remember in particular, my math teacher in High School. It is remarkable how one person can stick in your memory so far. We had a very remarkable teacher by the name of Ms. Shoesmith, Buella Shoesmith. My goodness that name came back to me all of the sudden. And she taught mathematics in a most creative way and she turned out a string of students who were indebted to her. Dave Fultz was a student at Hyde Park High School who finished about two years after I did and was also one of Ms. Shoesmith's students. George Platzman and his brother Bob Platzman were from Hyde Park High School. And George, I always remembered as being a very young man with a big head, and I don't mean that in terms of being a popish, he literally, physically in those days when he was smaller had a head that seemed disproportionately large compared to his body. In later years I didn't have that impression but I remember him before he was fully grown. And George was a Hyde Park High School graduate as was his brother. Bob Platzman knew better than George in those years. And Bob and I were almost contemporaries, we were only a half year apart. George was a bit younger. But I remember years later, George Platzman and I, and Dave Fultz all went over to an celebration in honor of our former high school teacher. And I remember very fondly what she did for all of us. After I graduated from High School, and I did graduate at the top of my class, I went on to the University of Chicago on a scholarship. I really did want to go on to school, to college immediately. I was very young when I graduated. I had skipped class, skipped a year and I was sixteen when I graduated from High School and I didn't want to go to college immediately. But I had a full scholarship from the University of Chicago for \$300 dollars which covered my tuition and one was not about to turn down a full scholarship and I was not sophisticated enough to realize that if I wrote the university and asked them to postpone the scholarship for a year they probably would have been willing to do so, and I would have been much better off. But I didn't realize that in those days and I thought that if I didn't take up the scholarship that I would lose it. So I went on to the University of Chicago where I promptly began to grow up and in the process paid very little attention to studies and as a result lose the scholarship. I didn't have a terrible record but it was not a distinguished one; there were a lot of B's. And I spent more time socially than I did academically in studying. But my career at the University of Chicago came to a rather abrupt end. When I had some problems with my left lung, which caused me to drop out of school and at the time I thought this was terrible. I went to my parents; my parents at that time were in Saint Louis. So I went to Saint Louis and stayed at home and recuperated. I thought this was a terrible blow. Years later I thought of it as a blessing because actually there were no lasting problems as a result of the difficulty, but it did keep me from being drafted in to the Army in World War II and therefore from my point view, it was a blessing in disguise in retrospect.

Because I would have made a very poor soldier I think and probably would have wound up losing my life on some battlefield. But at any rate, after a period of recuperation, I went out to the University of California where I was bound and determined that I was going to be a geologist. My brother was a graduate student in the Department of Geography and I did take some courses in geography. Became well acquainted with Karl Sauer and John Laley both of who were professors at Berkeley and are now giants in the field of geography and joined field trips Sauer and Laley. But my main interest I decided was geology and more particularly in mineralogy and crystallography and I studied with Pap's who later became a very famous person in this area in the department of geology at Berkeley. But after a while that began to pall on me and early '1938 I dropped out of the University of California before I had finished my degree and went back to Washington where my brother was working in the Soil Conservation Service and from there on my career pretty much followed the paths that I think I've already indicated. Going from Washington to Muskingum, Ohio where I had a WPA program for the Soil Conservation Service. It was a growth experience. I was just 21 years old at the time and I was supervisor on a project of about 150, mostly middle aged men who were working on a statistical program that I had to supervise. I had very little experience in running and directing people, running a program and directing people. I was a babe in the woods in fact when it came to administration. And these people were very honest, hard working people, but they were desperate people. They were getting salaries of \$55 and \$60 dollars a month on which they were trying to support families. And I remember we hired these WPA employees as Junior Clerks at a salary of \$55 dollars a month and in this they were obliged to prove their skills at clerical statistical work, and then they graduated to being senior clerks; an increase of \$40.00 dollars a month to \$60.00 dollars a month. And as you can well believe this was of tremendous importance for these people, getting that extra money a month was a major thing for them and their families. And here I was a young man, 21 years old and having to make decisions in regards to careers of livlyhood. I probably would have had a much more difficult time if it hadn't been for the fact that there were really two projects in Ohio. They did almost the same sort of thing, the other project was run by a much older man by the name of Paul Kaiser. I remember him well. Paul was not a scientist, his school was not that advanced, but he was a very experienced in personnel. He was very good at getting the most out of people and in smoothing things over and making things work in terms of getting people to work together. And the way we operated was, Paul who had no background in science, technically we were co-workers, we were each director of a project, but in practice I helped him very exclusively with the scientific questions that were relevant in his project and he helped very exclusively in many personnel issues that arose on my project and I learned a great deal from Paul. He was a remarkable man although, certainly not a technical man. From there I went to the Weather Bureau as an Observer after taking the national exam and I ranked third

in the country I think, and was recruited and stayed in Akron because they needed a person there. And then I came back to Washington and then out to the University of Chicago. I should mention that it was at the University of Chicago that I met my wife. She came to Chicago as one of a small group of women, three survived the course, I think it was the fifth course, and it was a huge class of about 500 hundred people who perhaps 60 or 70 Ensigns and Army Air Force Cadets and three young women. And I've always said my filter worked very well because I wound up with one of the women. But, I didn't know my wife Charlotte at that time, I saw her, I knew she was at the University but, I was one of the civilians who was recruited by Rossby to take the course and stay on and study up with the education of the future classes. So after we finished the 9 month program and I didn't not enroll in other courses, my wife-to-be Charlotte spent a short time out at the Weather Bureau office at the office at the Airport in Chicago. At that time, Joanne Malkus who later became Joanne Starr and then Joanne Starr Malkus and then, no what a minute, I beg your pardon her name was Joanne Gerould who was also a student at the University but then stayed on to teach. And Victor and Joanne went out with Charlotte and myself on a double date. As I say, Joanne later married Victor Starr and is now Joanne Simpson, past President and a very distinguished member (AMS) of our community. So my associations with Joanne go way back. But, we were married in 1945 and Charlotte came with me and joined me in Asheville where I was working for the Headquarters/Air Force Weather Wing. We returned to Chicago after the war and then moved to Hopkins after I finished my degree and stayed a year at the University of Chicago. And shortly after we came to Baltimore we had the first of our children, we have four children. Our first daughter Sandra, was born in 1948, our second daughter Barbara, was born in 1952, our son Jeffrey was born in 1954 and our daughter Lauren, always calls herself Laurie was born in 1956. And I don't really need to say much more about this except I like to sort of look back in my career and say "that I've had four different careers, and I think I've done reasonably well in three of them and not as well as I would have liked in the fourth. Let me say what they were. I had one career in Government. I have been in Government and as a advisor to Government. I think that this has been a fairly successful career. I been pleased with what I've been able to contribute. I think my skills are fairly good in administration and I've mentioned already some of the things I think I've accomplished here. I won't repeat them. But, I know I was one of many working in these areas but I think my contributions have been positive. I feel comfortable about what I have been able to do.

Droessler: I think you should.

Benton: A second career has been at the University. I had been very involved in the university governments. When I first came of course, I was a very young Assistant Professor but, I have known all of the Presidents beginning with Isaa

Boman whom I personally knew because of his interest in geography. Isaac Boman insisted that I spend part of my time in his beloved department of geography as a condition of my appointment because he wanted to introduce more science to the School of Geography as it was called in those days. So I got to know him personally. Brock was one of the Presidents and Milton Eisenhower who I got to know very well and who I think was the most remarkable person that I ever worked for, a very very remarkable man that I don't think was fully appreciated. So many of his activities were carried out entirely within his administration. He used his administration very effectively when he was president of Hopkins to handle problems at the lowest possible level and I learned a good deal from Milton Eisenhower. I always remember in the earlier days when I didn't know him as well as I did in later years, we thought of him as being somewhat distant, because he would very commonly say with regard to a problem "Well that hasn't reached my desk yet or that hasn't come to me yet". And I didn't fully appreciate it as I did in later years, he knew everything that was going on on Campus. And he was actively talking to on Campus to his Deans, and his Vice Presidents and his Associates and seeing to it that problems got handled properly and never got to him. So many of the things that he would disclaim any real knowledge of, his was really the stage managing behind scenes to see that the problems were handled well. I learned a good deal from Milton. I treasure my relationships with him. Steve Moller was another fine president that I worked with for many years, but I don't think he quite had Milton Eisenhower's tremendous abilities to get the most out of people and orchestrate a successful administration. Steve was very bright, very capable, but was not as good in attracting around him people who could really support him adequately. He was a great one man show, but he didn't put together as strong as an administration as Milton did. But I was very involved in the various reorganizations at the University. I was one of the active people who led as an acting Chairman of the Department of Civil Engineering, was very active in phasing out the School of Engineering and bringing it together much closer with the Engineering Science departments, bringing engineering science and the engineering school closer together with physics and chemistry by making them all part of one activity. I became Dean of the Arts and Sciences in Engineering, in fact all of the day time activities on the Homewood campus for two years. Later I became Vice President responsible for the Homewood Campus and for the School of Advanced International Studies in Washington and held that role for five years before going to Washington in the Carter administration with NOAA. So I had had a long career in University Administration here and again I feel, I feel good about this. I think the changes that we've made over the years have been good for the University. I'm happy with the role I played and it hasn't always been one directional. I mentioned that we phased out engineering and merged it with the science and other faculty of philosophy activities to bring about a closer net campus. Some years later I was also active in reestablishing the School of

Engineering because times had changed and some of the advantages of that collaboration with the science departments had been won. And I think in the present School of Engineering, which is now distinct from the School of Arts and Sciences that the advantages of that being together for some years still persist. And one shouldn't hesitate to move in one direction and then back again when the time is right. So I feel pleased about the contributions that I have made to academic administration. And that includes the role of Hopkins and UCAR, which was very much in my ball park during all the years at Hopkins as participated in UCAR. A third area that I would say I had a career in was that as a creative scientist. In here my contributions have not been to my satisfaction. I've done a few things that I've been happy with. I think I've made some contributions but, if this were all I had done I would have not felt that my life was successful. I wasn't that great a scientist. I think I had good capacity for reasoning. I was a very good workman. When I understood a subject, I understood it well, I could teach it well, but I wasn't creative enough as a scientist. I didn't really have those inspirational flashes of new ideas that draw distinction between the really creative scientist who makes a change in the world and one who contributes in a much more minor way to the development of this field. I think I was in the latter category not in the first. So I would say this is one of the four careers that I don't think I was as successful in. The fourth one is in my personal life and in here I think I determined very early in my career that I was going to have a rounded existence. I was not going to forsake my family for the workplace. When my children were small I always took a full month off in the summer and we all vacationed together for many years in Michigan in the shore of Lake Michigan, so I got to know my children and I participated in their development and their growth. And that was a satisfaction to me. We had been a close knit family and our children all grown now but we're very close to them. We're friends now and it's been a very happy association.

Droessler: George on behalf of the American Meteorological Society, I want to, and myself personally thank you very very warmly for these several hours of quite an experience. It been a marvelous experience for me, an educational one. I thought I knew you fairly well, I thought I knew many and most of the things that you did but it just turns out that no one can walk in another man's shoe. And I think that you have accomplished a great deal in the world and your footprints are there on the sands of time in the advancement of our great profession in meteorology and also in the advancement of the University community.

Benton: Well we've lived in a very privileged time in many ways Earl. I don't know that I would be happy coming up today as you and I did. We lived in a time of opportunity. I remarked once about how many doors were opened to us and those of who had some gumption could have walked through them. And it's harder for the young people today, we lived in a very privileged time and it's been very nice

to be able to go through these things.

Droessler: Well thanks again George. And this is Earl Droessler concluding the interview with Professor George Benton at Johns Hopkins University in his office and this is the 27th of May, 1991.