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Interview with Warren M. Washington:

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Interviewer: Earl Droessler

[UCAR Tape 73-74]

TAPE 1, SIDE 1

Droessler: This is Earl Droessler, and I'm at the National Center for Atmospheric Research, where I will be interviewing Dr. Warren M. Washington, who is the director of the Climate and Global Dynamics division of NCAR. It's Monday, the 8th of October, 1990, and the first snowfall in the Denver/Boulder area is just outside the window, about three inches on the ground. It's nice to be here Warren, to sit with you for some time and talk with you about NCAR, and the atmospheric sciences, and yourself, and I'd like to begin the interview by asking you to talk about your early days in NCAR.

I understand you came to NCAR about twenty-seven years ago. What was NCAR like at that time?

Washington: At that time, NCAR was an exciting new place, which nobody seemed to have a good idea about what would be carried out. I was just finishing up my Ph.D. at Penn State under Hans Panofsky, on a project dealing with analysis and numerical weather prediction. And I went to a meeting up in Stanstead in Canada. At that meeting were a number of very prominent meteorologists who helped me decided about going to this new place called NCAR. At that meeting was Bernhard Haurwitz, Julie London, Hans Lettau, Aksel Wiin-Nielsen, who was here at NCAR as the deputy director for, let's see, I think it was called the Laboratory of Atmospheric Sciences under Phil [Thompson], and Aksel was the deputy. I remember one afternoon after the sessions we all went to a pub down in town, all drinking beer, and I asked, where should I go? I apparently, as I recall, I had at least one offer for a job at the Navy group in Monterey, and the other was, I had one here at NCAR. The difference in salary was twelve thou...well, at the Navy it was twelve thousand and at NCAR it was nine thousand, so that was a pretty big difference for a

young scientist to look at, and I was strongly encouraged by everyone there at that table as we were drinking beer, to go to NCAR. I still didn't quite know what I would be doing there, except they had indicated to me that they wanted to get into numerical modeling of the atmosphere, and try to build models. So, I came to make the decision with my wife, to come. At that time, I came in August of 1963, and that was the year that there were a lot of new hires into the Laboratory of Atmospheric Sciences[LAS]. Just to give you some idea of the people that were here, Chester Newton, Harry Van Loon, Doug Lilley, Akira Kasahara, Jim Deardorff.

Droessler: These people all came in 1963.

Washington: Yes, all of them I think, came in 1963. I remember at that time that I needed to finish up the writing of my thesis, even though it was all done essentially. Just before I came I had to make the changes in my thesis as well as prepare a paper for publication. And at that time George Platzman was around, and I think he was just visiting. I essentially used him to bounce some ideas off of, and show him a draft of my paper. The instructions I was given after I had arrived was basically to spend half time of what was the big project and that was to try to build models, and the other half was to just follow any research interests that I had.

Droessler: And these instructions came from Phil Thompson?

Washington: Phil Thompson.

Droessler: The Director [of LAS]?

Washington: That's right. Because Aksel Wiin-Nielsen had just left. Apparently they ran into some difficulty at Michigan, and even though he had helped hire me, he suddenly became the chairman at the University of Michigan in the Department of Meteorology and Oceanography.

Droessler: What sort of contact did you have with Walt Roberts who was then the director of NCAR?

Washington: I didn't have a lot of contact with him, but Walt had a procedure whereby he would invite you to come by and talk with him every month or so, or every two months. I don't remember how often. It wasn't a structured meeting. He just wanted to get some sense of how things were going and he'd often give words of encouragement and that sort of thing. I guess it was his way to kind of keep in touch with what people were doing. We didn't have a general circulation model at that time, and so on the work that I started principally with Akira Kasahara as the more senior person, was to start to put together a general circulation model.

Droessler: Well, we didn't have an NCAR on top of the hill at that time, on top of Table Mountain--the Mesa, plateau--where you were located. You and the people associated with you.

Washington: Well, all of us were located in a dormitory on the campus. It was called Cockrell Hall. In fact, it was interesting, it was named after, I believe, he was a botanist, or something of that sort, but a man who was extremely productive. Because I remember one of the interesting things about it, he had published something like five thousand papers in his lifetime. Which I think most of us feel is, would be an impossible task. It was a very interesting place, because we had a ping pong table which was used quite a bit by the staff. We had a very nice lounge area, where I can remember we had lectures by people like Fjortoft, Ed Lorenz and others, a very informal setting. Nobody wore shirts, I mean ties or sport coats or anything of that sort. A very informal setting. We could talk to anybody by just walking into their rooms; these were essentially dormitory rooms. It was a place where a lot of productive science got on, and it was great for a young person like myself, because you could rub shoulders with these legends in the field, and they were very friendly and always anxious to give advice. The atmosphere was very much campus at that time. There were lots of informal hikes in the mountains that were being organized, things of that sort. Taking advantage of the Boulder, Colorado, surroundings.

Droessler: Then After Cockrell Hall you moved to...

Washington: In 1965 we moved to the Colorado Building. Apparently we couldn't keep the dormitory. Apparently the university needed it. Probably to accommodate newer students who were coming in. The new building was set up, or the ground breaking was in 1964, and I remember hiking up the primitive road up to the top of the mesa for the ground-breaking ceremony. I believe I was walking along with Sydney Chapman and some others at that time. The building was not completed until 1967, I believe.

Droessler: This is the NCAR building?

Washington: The mesa building, up on Table Mesa. So during those two years we were at the Colorado Building which was downtown, it was a business office building, actually a fairly nice building in some ways, but certainly not a campus type building.

Droessler: Well, after the Colorado Building you moved into the new NCAR building up on Table Mesa. What was it like moving into this new building designed by I.M. Pei?

Washington: Well I think there was a great deal of excitement among the staff about the building. It was really very pleasant, it was in a beautiful setting, it still is. A lot of the old timers like myself have a very close association with this building, maybe too strong in some people's opinion, because as many people know we have purchased a second

building out north of Boulder, and that's not ideal because the idea was to keep everybody at the same site with the building of a new building, and that would have been accommodated had we built on the mesa. However, that was not possible and so you can imagine an organization where there's two different sites, and strong diff...sort of disagreements about core location.

Droessler: Well, when you moved into the NCAR building, did you move into this delightful office or was it another place?

Washington: I've only moved three times, amazingly enough, three or four times. We were in the other tower, the theoretical people were I believe, on the fourth or the fifth floor of the B tower, the south tower of the building. Then sometime later, a few years, and I don't remember when, we all moved over to the north tower. And we've been here I'm sure since the middle or the early seventies.

Droessler: Did the "Blue Book" influence your activity or did you share that vision that was in the blue book about what NCAR might become?

Washington: Yes, and the Blue Book probably didn't have an immediate impact except in this one sense. When NCAR was set up, it was clearly meant to be a center for carrying out some extraordinarily large activities. Activities that sometimes cannot be easily handled at individual universities. Now the carrying out of building a general circulation model that would be used by the academic community as well as the NCAR scientists, for research had been met. I'm glad that I've taken a part in that essentially from the first beginnings.

Droessler: When was this accomplished, or when did you feel this was met?

Washington: Well, I think about 1967, 68. At least on the aspect on which I am familiar with mostly, and that was the building of the General Circulation Model.

Droessler: At that time, what kind of computer did you have to practice with the General Circulation Model?

Washington: Okay, well let me just finish up one aspect on the computer, I mean on the aspect of the Blue Book. Now the Blue Book was not a perfect document, but I think it was sort of a guiding principle. And we still have this problem at NCAR even after all these years, of a mixture of having a cadre or people working on a wide range of topics, but at the same time having some large projects such as the General Circulation Model. So, it isn't all small science or all big science, there's some kind of mixture of the big and small that is needed in order to carry out these large projects. I think that struggle, which was probably even right after the Blue Book was formed, is probably still going on in NCAR. I mean, we're obviously, we don't want to be just a sterile organization that only has big

projects, I mean no small and interesting type science, small sciences going on. For example, Ed Lorenz. He comes every year, and greatly enriches our program. And I think if we were able to, if we ever took the steps of eliminating the interactions with people like Ed Lorenz, I think that that would be a terrible mistake for this place in terms of intellectual excitement. Now, getting back to the General Circulation Model in those early days. I'm not quite sure when we had our first computer. In the early days we were using a 709, an IBM 709, which was a vacuum tube machine at the University of Colorado. And that's where we first got some computer time. Around about 1965 we acquired a CDC 3600, which was a very small computer and we pretty much beat the thing up, because we were trying to run a general circulation model on it, 24 hours a day, seven days a week, and I essentially had computer time available after ten o'clock at night until six o'clock in the morning, or eight o'clock, and we really beat the thing to death. They didn't anticipate for people to use computers in that way. Since that time, we upgraded to 6600 and 7600, and then eventually we got into the Cray series of machines. But I believe it was the CDC 3600 which was our first computer, and that was actually installed over on 30th street in an NCAR building there. I think RL-2 or -3, I can't remember. And that machine was actually moved up top the mesa when the mesa building was completed, and became the mainstay of the NCAR computing center. It was an interesting highlight at that time. In the early days, the computing was actually part of the research division. There wasn't a clear separation in between the computing facility and the research facilities.

Droessler: Well, your General Circulation Model must have kept you in contact with a variety of universities, and kept you working with them, and having influence from these universities, and you influencing them, there must have been a lot of interaction there, between your activity at NCAR and some of the university centers. Where were some of the particular university centers that you worked with?

Washington: Well, it was interesting in those early days, it was say 1968, '69, we started making the model available to other groups outside of NCAR because we had finally gotten it off the ground and had some publications showing on the model and so forth. The early users of the model were the groups at Chicago, at Wisconsin, John Kutzbach and people like that. And they were mostly trying to help us improve the model. It was clear in those early days that we had a model that could be used for a wide range of atmospheric problems. At that time it was basically an atmospheric model. It wasn't coupled to an ocean, so in some ways it wasn't a true climate model. We have now, just in 1990, something like fifty-five university users. When we have a workshop, we have over a hundred people come. So, the enterprise of essentially making a general circulation model test bed for climate and atmospheric research has been very successful for over twenty years we've been making available these models.

Droessler: Yes, that's one thing that I've really noticed. Your work has really been a sterling example of what NCAR can do working with the universities. University relations are

excellent in this area, general circulation models and research.

Washington: Well, I shouldn't say just me. Obviously, general circulation model, and something we call now, on the community climate model is a tool, that a lot of people who contributed to here at NCAR. We have something on the order of fifteen staff people, a number of Ph.D's, who spend part time or whole time on trying to improve the model or use it in various ways. Something on the order of probably twenty or thirty people. So it's a tool that has grown in complexity and usefulness for the whole community.

Droessler: During your twenty-seven years at NCAR, quite a few changes have taken place, and naturally they would. I'd like you to go over some of these changes with us.

Washington: Well, I think in the early days of NCAR, and UCAR, there was more of a feeling that good science should reign supreme, not that we've sacrificed that in later years. It's just that now we have lots more structure to things, a lot more committees, a lot more direction from on top about what's important and not important. Whereas I think in those early days it was, maybe this was from the National Science Foundation attitudes, or from ONR, or wherever all that started, a more of a feeling of, we have a scientific problem to do, let's get the best people we can, and just do it. Whereas I think nowadays there's a little bit more of a feeling that we need to have an organizational plan.

Droessler: Well, don't you think that Walt Roberts too, was very influential in establishing this family-like environment at NCAR, where everyone was very important, and your work was very important, and he tried to get the associations established between and among the scientists, and the scientists and the universities.

Washington: Yep, I think that that was true. And I think that Walt's style was obviously something that impressed me as well as many other young scientists in coming here because he had a great deal of respect for the individual scientists and what ideas he had to pursue. And I think that's somewhat changed, and I'll come back to that a little bit later. The early style of administration in the NCAR side of things was very laid back. We would have very seldom, group meetings in terms of trying to plan activities in great depth. But what we had was retreats. Walt would always pick out some retreat in those early years, up in the mountains, sometime in October I believe, and we would go up every year and there would be short presentations by the scientific staff about what was going on in each of the parts of NCAR. People would offer comments and suggestions. There was lots of time to hike and interact between individuals who maybe didn't normally interact at the NCAR site. We would have dinner at an informal setting, so you would talk to people that you wouldn't normally talk with. So I think that was very, kind of a laid back style which I thought quite frankly in retrospect, was very productive, but that's not on the new world. Now, on the new world is a lot more structured to our scientific programs.

Droessler: So after Walt, then John Firor became the director of NCAR.

Washington: That's right, and I didn't have a lot of interactions with John in those days. It just turned out that he was much into the day-to-day operations of NCAR, but my interfaces were with Phil Thompson, principally, and Will Kellogg. I think Will came about 1965. Just to give a portrayal, Phil was a scholarly scientist. Very quiet, unintrusive, he would probably, he didn't interact on a personal level a great deal with the scientific staff, but seemed to have tremendous wisdom in terms of knowing what was important and what to anticipate and that sort of thing. When Will Kellogg came in, I believe it was 1965, Will was much more interactive. A little bit more assertive on what direction the research program took. However, he didn't change the basic style of NCAR, but he was certainly more assertive than Phil Thompson.

Droessler: After John Firor, came Francis Bretherton and I believe that in large part, Francis came to UCAR and NCAR as a result of the, as one result of the Joint Evaluation Committee [JEC]. In what ways did the recommendations of the Joint Evaluation Committee affect you and your group and the others associated with you?

Washington: Okay, well let me just kind of back up a little bit and say that there was a lot of interesting little stories about that, and of course some of us were quite sad to see the change at the top, but we knew that that was in store. I remember being out at Monterey, I believe it was Monterey, and Doug Lilley was in a telephone booth and there was a number of us out there attending a conference on numerical weather prediction, and Doug Lilley was calling back to find out who the new director of NCAR was, and he slumped down in the telephone booth with the knowledge that he'd just gotten on the telephone that Francis Bretherton was the new director of NCAR. I think that the reaction of the staff was not that Francis was not an extremely brilliant and capable man, he certainly is and still is. I think it was the fact that Francis has a kind of a style about him that was a little bit hard for the rest of us to take. He was extremely assertive and wanted to kind of direct things to a great degree. In fact, one of the interesting aspects was that when Francis became the director of NCAR and the president of UCAR, and the director of our division that Will Kellogg, but he also was chairman of the, something called the "General Circulation Steering Committee." So he had a tremendous number of hats on, and I can remember Francis, in his vigor of those days, would have us meet in the Damon Room, I don't remember how often—every two weeks or so, and he would, you know as Francis can do, he can stick out his hand and slow...and point it at everyone in the room, and slowly indicate who he wanted to do something. And people would kind of try to avoid eye contact, hoping that he would not assign...ask them to be assigned some certain duty. It wasn't a pleasant period, because I think people kind of felt that maybe there needed to be a change from JEC, but this was a change of a magnitude that we hadn't anticipated. And that Francis, quite candidly, even with all his brilliance and so forth, was not up on a lot of different aspect of the field. He just didn't know those areas. He was basically a theoretician, and a very good one. However, there were some areas of the

field, like numerical weather prediction, general circulation modeling, these things I know about, where he did not know a great deal, although he understood the basic principles of course. You got somewhat of an unbalanced sort of thing, because Francis being very aggressive and very bright, he probably put a lot more attention on things like general circulation modeling, and the theoretical parts of NCAR, whereas things like HAO and chemistry and the facilities—things that he had less knowledge about—I don't think he worked quite as hard to find out what the heck was going on. But there was some good that came out of that. It essentially disciplined us a little bit more, it made us re-examine where we are going, which are good things. But it was hard on the staff. I think there was a period where people tended to shy away from confrontation with Francis. But some of us liked the place and stayed around and we kind of went through that period. Now Francis also changed. Francis changed into a more understanding person as he got into the complexities of the organization as well as into the types of science. So we all grew from that, including Francis.

Droessler: Well, that's a very interesting story. During the joint evaluation study itself, which took a year or eighteen months, the committee would visit quite frequently out here at Boulder. What was the feeling among the staff and among your people and at NCAR about the way that the joint evaluation committee inquiry was going on?

Washington: I think some apprehension basically, but you know there are some very good things that I still cite back to the joint evaluation committee. NCAR had grown from those early years, until the 1967 I believe it was, and grown very rapidly during those years. There was a little sense of not knowing where the organization was going, and I think that was reflected in the Board and the foundation taking the action and carrying out the review. Now, I remember one thing that was interesting as part of trying to rate people in terms of what they are doing, and their contributions to the new NCAR, is that there was a kind of a rating sheet. I don't remember the exact details of it, but it was supposed to have some categories in there. And there were categories, things like, science, how you rated on your science, how you rated on your contribution to the universities, how you rated in terms of contributions to larger projects, on your contribution to the community at large. And I remember there was some kind of threshold score, something like twenty-five, or something like that was the maximum score you can get. And there were a number of people at that time who had gotten high scores in their own particular field, and their contributions to it, but they didn't interact with people, they didn't make a contribution in other ways that should have been part of their NCAR job. And they came out fairly low in terms of their total score, because these things just added up and it was the total score. I scored fairly high, and I think partly due to the fact that I was doing all these other things, and interacting with the universities. One little anecdote out of that, was, you're supposed to list on your best publications, and Bernard Horowitz had to fill out this thing, he couldn't think of any good publication he had, so he didn't put anything down. He said well, I have a couple of papers I like, but nobody ever reads them, and he put those down I guess. And I guess in spite of the fact, he didn't, if you know Bernard he's always kind of

self-effacing. He still became a senior scientist, in spite of the fact that he kind of felt the whole thing was kind of silly. But I think the good that came out of that is, it's part of this reevaluating. Where NCAR's going, and where UCAR's going. And I think that the organization needed that.

Droessler: It also gave you a more structured personnel order.

Washington: That's right.

Droessler: Senior scientist, junior scientist, and so forth. Something similar to what they have back on the campus.

Washington: Yeah, although I think though Earl, that there has been some good and bad out of that. Because as you can imagine, people that often get the senior appointments, the senior scientist appointments, I look at my own division for example. I have a number of people who are extremely good scientists. I mean really superb scientists. But they sometimes don't understand that NCAR needs to work on these large, unrewarding in some cases, projects. And so someone who does work on those kinds of projects tends to have fewer publications, fewer citation counts, things of that sort. And gets somewhat penalized by this more rigid appointment structure, because they have to pass mustard with these people that don't get involved in these things, but who have gone up the appointments ladder quicker. And so we still have some kind of problem with that. You know it's not...when I have a young scientist come in and he says I really want to work on a general circulation model, and so forth and so on, I always have to temper him and say, yeah, well that's fine, but make sure you don't get so caught up in it, even though you may do brilliant work and be extremely important, don't get so caught up in it that you're not paying attention to publishing papers in an orderly fashion. And making sure that your visibility to the inside and the external people is well known, because otherwise when the appointments come forth, I may tend to use academic standards, not importance to the institution, but the importance of their work to the external, visible community. And I think that that's a mistake. I still think that as a director, I face this all the time, because clearly our division needs to work on global change issues, climate change issues, that may require you to spend years without a publication of significance.

Droessler: On the other hand, your advice is well taken, because if a young person did not reach out and publish from time to time, he would not have good standing within his own scientific peers, whether they're at the universities or in the industrial field.

Washington: That's right.

Droessler: Or at NCAR or any other place, and so he has to balance his scientific and professional life.

Washington: Well, that's why Walt's original advice to me, and I think to the other young scientists was, spend half your time roughly, on something that is of great importance to the institution, and the other half doing what you think is important as a scientist. And I think that that advice still is the advice I give to these young person. And I look after them. I think Earl, you have some of the same characteristics. I'm a nurturer by nature, and I look after young scientists, I make sure that they are getting the help, and support, and encouragement, because I got that when I was young, at this institution and elsewhere, and I think that we all have that responsibility to the next generation.

Droessler: You're absolutely right, and life goes on at NCAR, and after Francis came Bill Hess.

Washington: Right, yeah, and let me say something about Bill. He had very strong, there were very strong feelings about Bill. I had never any problems with him at all. He was very encouraging of our work on the general circulation modelling, very interested, always willing to see us, or see me and sort of talk about things and I had no problem as a director with him. But I know there were parts in the organization who felt his style of management was not good for NCAR, but I guess I didn't see those points.

Droessler: Well, he was brought in by Bob White who then took over as the president of UCAR, and gave UCAR an expanded vision and expanded opportunity. How did that UCAR expansion affect your group or yourself as a working scientist and leader within NCAR?

Washington: Well, I think you can still see it sort of going on. It didn't have any direct effect, except in a very major way, it had to do with many of the UCAR functions taken on had to do with modelling. Climate modelling, weather forecasting, and so forth and so on. And as one of the attractions for people coming to visit this national center and interact with UCAR on a variety of topics, is the fact that we are a national center in the area of climate and global modelling. And so one of the impacts is, almost every visiting fireman that comes through wants to learn about climate research, changing climate, modelling, weather forecasting. So, over the years I've sort of done my part of carrying the NCAR, UCAR banner in meeting and taking part in all these various activities. And I've done this of course, with other people here in the division. But it is sort of draining.

Droessler: Yes, and you continue to grow now in years here at NCAR and in importance as a, now a division director. How do you sense this expansion of UCAR's activities has influenced NCAR's arrangement with NSF?

Washington: I think it's been good, although NSF is very concerned about the growing number of things which UCAR is doing that don't contribute to NCAR in any direct way.

Droessler: In the atmospheric sciences.

Washington: That's right. And you can see it when I go back. In fact I was in Washington: last

week talking to program directors, including Gene Beardly and everyone there. And we were having a meeting of the directors of NCAR, and the program directors of the National Science Foundation, and you could see the strain. Because they see UCAR doing less of management of NCAR. In fact, we've gone almost to a, if I can be rather extreme, where a division director, such as myself talks to the program director about our budget, then you asked the question, where's UCAR in that loop. Whereas it used to be NCAR prepared a budget, submitted it to the trustees, let the trustees forward it on to the National Science Foundation. That's only done in name only now. It's basically short circuited. And I think that that could present some long term problems for UCAR.

Droessler: The next director of NCAR was Rick Anthis.

Washington: Right.

Droessler: How do things fair under Rick as director of NCAR.

Washington: Let's see, this is a tough sort of situation, I've been on the search committee for all these people in the latter years. I have to be kind of careful about what I say. Rick is an activist. He is a man that is capable of taking on a task, and aggressively trying to put together a plan, or a budget or an action, or item. He doesn't let the grass grow under his feet. He's constantly, quickly doing things, and sometimes he does things too quickly, and we knew that when we made the selection. So we always hoped that the trustees would make sure that UCAR didn't go off into wild blue yonder doing too many things. Turns out that the expansion that went on in UCAR, started out with Bob White, then it went to Cliff, and then went to Rick Anthis, has not slowed down. I thought that they would slow down with Rick, but he's taking on more and more, more projects. Some of them with marginal interest to the atmospheric sciences. For example, the Institute for Naval Oceanography started out as a great idea. It was the Navy would fund this new center to take advantage of the new things that were going on in ocean modelling. At the same time, it would fund a number of university researchers. And so that seemed like an appropriate activity, it wasn't quite atmospheric science, but you know, these days with climate questions that we're dealing with, the artificial boundary between atmosphere and ocean is disappearing, as it has to, when dealing with the science. But I think what went astray with INO, as you can imagine happens with many things, things were overly ambitious. The budget was overly ambitious, and they had to cut back on the funding of university researchers, and so there's very little funding for the university researchers. And I would think that that would be, I'm thinking maybe out of turn here, that that's when UCAR should have said, wait a minute here, that is not a university driven program, and maybe our participation, or management of, we ought to cease. But anyway, I think it's some of those sort of things UCAR needs to take a very hard look at along with the trustees. Let me continue on with the future of NCAR, UCAR, and talk about the changes that seem to be taking place. I think some of these changes are being driven by two factors. One of course, is the fact that the 1980's have been difficult years

for NCAR in terms of NSF budget, but clearly I don't think we've been necessarily prejudice against by the National Science Foundation. The grants program is under the same sort of strains, the stress and strains that NCAR is, and I think you're seeing all of science in fact, under siege to a certain extent, from limited resources in the 1980's. The other big factor that took place very recently is the fact that we have something called the U.S. global change research program. And what this is, is an attempt to better organize research throughout the federal agencies, and internationally to add...(tape ran out).

Side B

Washington: Okay, now the way it's being done is because it is being somewhat managed through the white house science advisor's office, the office of science and technology. And in B, that there is much more in the way of what I would call organized science. In other words we have a master plan for all the federal agencies, we have a number of research topics that are going to be given emphasis, and the way that even NSF's budget in terms of our field is starting to be broken down to is something called core science, which essentially is what we have been doing for the last thirty years almost. And the other is these named programs; TOGA, Wolf, FIGI, no, I shouldn't say FIGI, but all of these various named global change programs which will have identifiable budgets with them. And, now that puts a real problem towards us because the core program has grown at roughly two percent per year, which because of the normal inflation of salaries and other things, is really a cut every year. And so we've had to kind of shift more and more of our research into these named programs. And, take advantage of the fact that people are leaving, so the number of Ph.D.'s at NCA has gone down. We have gotten funding from a number of different agencies. For example, our division used to, three or four years ago, eighty percent or ninety percent of our money came from the National Science Foundation, now fifty percent of our money comes from National Science Foundation. The rest comes from DOE, NASA, NOAA, EPA, Department of Defense, and we have probably something on the order of twenty different grants. And we're supposed to interface those somehow into the NCAR, NSF.

Droessler: Well, does that mean you have to be on the road sometimes to sell you proposals?

Washington: Oh yeah. So...

Droessler: So you have to depend on your scientists to write proposals.

Washington: That's right. that's right.

Droessler: And write this specifically for NASA, or specifically for the Air Force or...

Washington: That's right. That's a much different atmosphere. Now it still has to be approved by the National Science Foundation. Other words there's a step, an initial step to make

sure that what we're proposing to these other agencies, meets with our program plan and is agreeable to the National Science Foundation. In other words, and we obviously...and that puts some limits on the sort of things that we do. But clearly, it's a different ball game than it was. You know now we have many different masters to serve. The National Science Foundation is still the most important one, but it means the complexity of funding, make sure everyone is on the payroll, there isn't any gaps between grants, all those things are much more difficult than they were a few years ago. And it used to be we got a budget, we settled, we kind of made whatever adjustments that we needed to, then we could go on and do our research. Now we're constantly innervating with other agencies in terms of budget.

Droessler: Well, you got to spend a little more time then, in management of your division which includes the interaction with these various federal agencies.

Washington: That's right, that's right. Now we have our new director here, Bob Serafin, has a style somewhat different than our previous directors, where we have a lot more meetings, a lot more meetings to coordinate, which I must admit gets a little tiresome for someone like myself who tries to do a number of things on the outside, a number of organizations, as well as try to do some research on my own. It puts a strain on the overall system.

Droessler: Warren, I believe we're through with NCAR, UCAR for the moment anyway. Let's go back in somewhat of a reflective way to your earlier days, the days of your youth, and tell us how you got into meteorology.

Washington: Well, let me just go back into a little bit of family history because I think it does have some aspects that say something about how I got into the field. Going back, way back to my grandfather's time, he was born in Richmond, Virginia and his brothers had gone, he was born in 1871, but his brothers were...he had older brothers, much older than he who went out to take part in the California gold rush. He joined them in California a bit later, and essentially went to San Francisco and worked as one of these people that fixed bells in doors. You remember those old doors where you turn the bell? And he's living there, and then the earthquake came, and he moved to Oakland with a lot of other people after the earthquake, and worked on the Union Pacific, the Southern Pacific Railroad running between Oakland and Portland, Oregon, and that's where he met my grandmother. He worked as a pullman car porter, but was a man that was very interested in education. All the kids went to college, all of his children went to college at a time when it was very difficult to go to college. And he always had these books of knowledge, I believe, I think they were called "Books of Knowledge," set of books, and he read those and talked about it. He encouraged me to go to college, as all my brothers also who went to college. I went to...I wasn't a terribly good student I don't think, in high school, until in my junior year, I took a course in chemistry and I had a lady teacher who just was fascinating. She taught me all kinds of things and got me into a project dealing with...'cause I asked her one day, why is corn yellow? And why are egg yolks yellow?

And she said well, research it. So she had all of us go buy this big thick book called "The Handbook of Chemistry," I think we paid eight dollars for it as I recall.

Droessler: A tremendous sum in those days.

Washington: That's right. And it was a fascinating book, it was kind of a compinium of what was known about chemistry, plus have all the chemical formulas and all that stuff. So I started a doing a project of trying to find out why are egg yolks yellow instead of some other color and so on and so forth. I did a lot of research on that, that was my first research project. I think what turned me on was this lady teacher who said, well, why don't you find out for yourself. Instead of just giving me the answer.

Droessler: What was the locale for this?

Washington: This was in Portland, Oregon. And then the next year I took a course in physics, and suddenly I had been transformed. Science was what I wanted to do. There was a Mr. Woods, was the physics instructor, and I was turned on. Mathematics and everything kind of fell into place, and when I graduated, 1954, I wanted to go to be in physics, major in physics, but my advisor from high school said well, you should probably go into business and not go into science. I don't know whether he had some reason to think I couldn't succeed in science, I guess so. Anyway, I ignored that advice and got my...at that time I was working at a hospital in the evenings washing dishes. I knew I...my parents didn't have a great deal of money, my father was a waiter on the railroad, as were with waiters who were working for the Union Pacific. They were all college graduates. But they were all working these jobs, and that was the middle class in much of the U.S. of course. We're college educated but are working as kind of service jobs. In fact, my father went to a small college in Alabama called Taladega, which was very good school, but he was very fair skinned and always felt uncomfortable living in the south as a black person, so he immediately went out to Oregon. Wanted to be a school teacher, but Oregon didn't hire it's first school teacher until the 1950's, so it wasn't possible to be a school teacher, even though you had a degree in those early days. So anyway, I was working at the hospital, and I asked the dietician, are there any other good samaritan hospital around where I can maybe go to college. She said yes, there's another one down in Corvalis, Oregon, Good Samaritan, somewhat affiliated with the one in Portland. So she called and I got a job. Enough to help me through college, I worked in the evenings washing dishes. I did that for five years actually, even through my first year of my masters. Working at the hospital. It was a very small hospital then, I moved patients, and cooked and washed did some cooking, so kind of a general job. I did that for five years. All of the time I was studying physics.

Droessler: Now this was now at Washington: State?

Washington: No, Oregon State.

Droessler: Oregon State, excuse me, Oregon State at Corvallis.

Washington: Oregon State College, it's called a university now, but it was a small school, about five thousand students. What was kind of interesting at that time, there were something like twelve or fifteen black students on campus. I was one of the few that wasn't on the football team which was quite interesting because I weighed about a hundred and forty pounds. I told people that I played tackle, so they knew that I must be awfully mean, which I wasn't of course, but it came in very handy when getting into athletic events. That was another story, but I had many a football players as my roommates who seemed to regard me as a nerd because I was taking physics, and they were taking somewhat different courses. But anyway, I thrived on it, I really thrived on the challenge and the fact that I had some hardship getting through school only because I had to work all the time to pay for the tuition and room and board. But in those days it was easy to do that. At Oregon State I remember the tuition was forty-seven dollars a quarter, and the tuition went up to fifty-one dollars a quarter there were student protests about such an increase which seemed unconscionable. Well anyway, I went there, I study in physics, I got a summer job, I got interested in a job after school where I worked for Fred Decker, professor Fred Decker, who was in the physics department, but he was essentially teaching meteorology courses. And I worked as a radar operator on top of Mary's Peak. I had to hike up there on Mary's Peak and operate this radar. And so I learned a little bit about radar and meteorology at that time, and I felt that I really didn't want to do that, because I really did like using mathematics and sort of theoretical concepts. In fact, one of my first talks was at a meeting where Jule Churney was at. He came to some triple AS meeting in Eugene, Oregon which was close by, and that's where I talked about some theoretical work dealing with mountain waves. In the summer of '59, I got a job as a research mathematician at Stanford Research Institute in Palo Alto, California and I worked with Manfred Hall and Herb Ligda. Herb Ligda was a very prominent radar meteorologist. But I worked mostly with Manfred on a project dealing with numerical weather prediction. He encouraged me to apply to schools for my Ph.D. I applied to a number of schools and got offers from some, several. But I was intrigued by one from Penn State where I could work on this new budding field of numerical weather prediction and analysis. And Hans Penoski had this small grant from the Air Force. In those days everybody was, not everyone, but there was a lot of funding from the Air Force in terms of sort of theoretical work. It was very good for me. I came out to Penn State, I worked with Hans, he was very kind of laid back, very gentle, very supportive, positive person, very bright, capable guy, kind of left you alone, but if you were going off course he would kind of talk to you and send you back on course. But he felt that a Ph.D. was something that the student had to do. And that he would help them along rather than give them a project. And I worked on this project dealing with numerical weather prediction, something about how the tropics work, and high latitudes, and whether you should use winds or temperatures. It involved some theoretical work which I still get cited for in some very abstruse paper on geostrophic adjustment theory, which is something that Karl

Rosby and several others had made contributions to. So anyway, I got into the field in a very kind of strange way.

Droessler: Well, you now have your Ph.D. from Penn State which is certainly one of the great universities in meteorology in our country, even to this day. And we did cover in our previous taping here, your offer from the navy postgraduate school of Monterey, and your offer from NCAR, and you accepted the offer from NCAR, and had this wonderful twenty-seven year career at NCAR. I'd like now to ask you now to cover some of the other things you did besides NCAR. Your work with the committees of the National Academy of Science, your work with international meteorology, your work with the white house and these other activities. Would you tell us something about...

Washington: I think it is probably best if I do this in a chronological fashion, because up until about 1968, '69 I was pretty much was a working on improving models here, had a young family which I was trying to help along and get started with. In 1969 when Walt Roberts was the president of the triple AS, that was kind of a turning point at least in my career in terms of helping out with things externally. Also at that time I was an affiliate professor at the University of Michigan. I had gone out there to work with the Department, actually I substituted teaching for Axel Wayne Neilson, who was at that time was doing lots of other things. The triple AS was an interesting experience for me, because Walt Roberts being the experimentalist that he was in terms of structures and organizations and so forth, asked each of the trustees to bring a young scientist to the board meeting to observe the board, which at that time met on Saturdays and Sundays, and then to observe the board and to criticize the board. Remember this was a time when lots of students activism, the Viet Nam war was starting to heat up, the science politics, should science be doing this or that, was very prevalent. The triple AS meetings were very hectic in those days with lots of demonstrations and so forth. Well, Walt invited me back to the board meeting and there were a number of other people that were invited to come back, and in fact some of these are very prominent sort of young scientists who aren't young any more. They're like me, getting a little older. But, we observed on the board operate, and we saw them kind of talking about some issues that they had dealt with and so forth and so on, but they were kind of out of the main stream. So, the next morning, on Sunday morning, we caucused and then came to the board and let them have it from all kinds. We said the meetings weren't running well, they weren't being very effective, they weren't dealing with problems of women and minorities, they had an appalling record in terms of their participation in the society, the journals weren't terribly relevant, they didn't have programs dealing with handicapped scientists. So anyway, after they had met, we left the room. We sat out in the hall for an hour or two, then we were called back in by the chairman of the board, and they took our strong message to heart. And they said, we'd like to ask you young people, and I have to admit, when they said young people, I was thirty-three years old at the time, to give us advice on an on going basis. They set us up with something called the youth council. And the original members of that were the people that were there at the board, that each one of the directors had

met. They made me chairman of that young body, and we led, I think, to some substantial changes to the triple AS, bringing it up to date. So I did that for several years, then I was chairman of the committee on women and minorities for something like four or five years in its various forms.

Droessler: What a marvelous way to launch your external activities.

Washington: Well, in fact it was shockingly quick because when the board called us back into the room, they admitted that they had, that they needed to make some changes. I'm sure that Walt had a lot to do with that. But they immediately gave us a budget of twenty-five thousand right then, and staff support. So we had money for us to travel, have frequent meetings, and to further develop ideas to try to bring in youth into the affairs of the society. So it was a marvelous experience, absolutely marvelous. From there, and I should kind of mention that that is going on, I'm running now for the board of triple AS, I don't know whether I'll get elected or not, but anyway I'll find out in a few months. In 1977, I was called by the white house, my first association with the white house. I was...let me just kind of back up just a bit. In the period from 1970, early 1970's until 1975 or so, I was on the first committee to look at the global change, or look at the climate issue for the academy, and I believe we wrote this report which Yale Mentz and Larry Gates were the principle authors of, essentially a blue print of what needs to be done in climate. In fact, on that same plan was taken over by the international and led to the international plans for climate research and so forth.

Droessler: Do you remember who was the chairman of that committee?

Washington: I believe it was, I believe the chairmen were Larry Gates and Yale Mentz. People on the committee were people such as Ed Lorenz, Sutki Minobi, oh, I can't think of other names, but they were very prominent people that were involved in climate research at that time. In 1977, I was called by the white house to be on the NACOA, which was the National Advisory Committee for Oceans and Atmospheres, and I was appointed by Jimmy Carter. That was an exciting time, because here again, we had a chairman by the name of Donald McKernan, who was the dean of the marine ocean, of the marine scientists at the University of Washington:, who apparently knew Senator Magnuson, who had enormous power at that time, and also Senator Jackson. He knew these people personally. I can remember going up on the hill and testifying with him on a number of occasions, and everything was kind of done in the back rooms, so to speak. The hearings went on, but they were almost kind of window dressing to what was negotiated. Now that was a committee that was offering advice to the government in general, but mostly NOAA. Although I had served on a number of committee, of things dealing with the Coast Guard because they oversee the Coast Guard and any other things, law of the sea, and all those sort of things. It met, at that time, twice a month. I remember I took eighty-six trips to Washington: during that period of time. My term ended after Reagan was elected, but the day before Reagan took office, I was reappointed by President Cater, I

was something called a midnight appointment. So I served another few years. It was a marvelous set of years, because we had very astute people from the science community, from industry, from the academic community, who were offering advice on all kinds of issues dealing with oceans and atmospheres. There's a little tendency later on, in the later years in the Reagan administration, for people to be appointed who weren't extremely well qualified. I can remember one person who I got to know very well on the committee, name was Charlie Black. Charlie Black, I think to the general public, is better known as Shirley Temple's husband, although he had a career in his own right. Very competent, very gentle, very nice person. I worked with him on an interesting little project dealing with the fur seals in Alaska, where the only known...I think they store these things in North Carolina. I don't know if you knew that. But they then have been storing these seal skins in North Carolina since 1905 or something, in a warehouse. And the only people that buy those fur skins are the Russian army. They use them for their officers uniforms. And this treaty that we has signed a hundred years ago or so, was coming up for renewal, and we had to deal with all of the intricacies of the Indian population, the changing lifestyles, the environmental issues of killing all these seals, and it was a fascinating little study that I did with him. But anyway, that's another story. I think that that was an example of learning how to do community service in a much broader context that I was used to. And I think out of that I still have this kind of strong sense of being asked, and typically not saying no to things I should probably say no to. But anyway, a lot of that spirit came out of the early NCAR, UCAR days, heavily influenced by Walt Roberts who had a high sense of community service. In continuing on with my service to the, on the community. Interesting, just a few years ago, I was honored by being appointed, or by being selected a black achiever in science. An exhibit of that is being, is coming through. I don't know if it's come through your state yet, but it's on tour. It was put together by the museum of Science and Industry in Chicago, and they picked something like twelve scientists, six of them alive, six of them dead. People like George Washington: Carver, people of that sort. The one in physics is Walter Massey, the new president of the triple AS, and I represent the earth sciences. And it gives kind of a life story of us, and gives something for the kids to deal with. It's purpose is essentially a role model, in fact, when the director of the museum of Science and Industry called me, he said that three hundred thousand black children had seen that exhibit when it was in Chicago. A similar number had seen it when it was in New York City.

Droessler: Isn't it marvelous that these young black children now are seeing role models, other than the sports?

Washington: That's right.

Droessler: I mean the sports are fine but, there's more to life than sports.

Washington: that's right, in fact...

Droessler: There's more to leadership among black community that has to do with the kind of work that great historian who lives down there at Duke, and the other leaders of the black community.

Washington: Oh, yes. In fact, last year I was honored by Penn State with alumni of the year award, one of the alumni fellows. And I spent one evening with the black students in science at Penn State, which is a rather large number on the campus. I was impressed. All of these young people came to this dinner, and they were all dressed very nicely, and so forth. One thing that brought tears to my eyes, was after I talked, I asked each one of the students to go around, to tell me what they had done, what they were majoring in. And one young lady said, that one of the reasons she went into science was that she saw me in the museum exhibit in Chicago, and I was impressed with that. She was in some strange field, petroleum engineering or something like that.

Droessler: Just by coincidence, I was at Penn State the night you were honored.

Washington: Oh that's right, that's right.

Droessler: I went to your lecture, and I was deeply impressed by the number of students. They filled up the whole hall, and they were standing and then listening to your wonderful lecture that evening. It must have been very gratifying for you to see that number of students and your old faculty colleagues there all celebrating your distinguished alumni award.

Washington: Yeah. I'm impressed by those things in an emotional way sometimes. Last year when I went to Michigan, they had Martin Luther King Day, and I was invited to give a series of lectures with people like [astronaut], and he and I would give a talk, and he would talk first about the satellite, and I would talk about the climate system and green house research which I have been engaged in for a number of years. Just to go to a place like Michigan and give a talk, and there is six hundred, seven hundred, eight hundred students in the audience, it's just mind boggling to have that kind of experience. You know, normal scientists like myself, speak to small class sizes, graduate school courses, where you may have twenty in the audience plus faculty. So it's a different experience to kind of get that kind of attention. I mean it's flattering but it's kind of awesome to kind of get that experience. Let me just kind of move on. A few years ago, a few years ago, I became intrigued with the idea of trying to start a foundation, and try to get a number of black scientists, in the environmental sciences involved. And, there was a young lady who lives here in Boulder, a very articulate young lady, who ran our program dealing with trying to get minorities interested in the atmospheric sciences. She had joined a foundation, a small foundation down in Denver run by an oil man. It's called the Peton foundation. She also worked with another foundation, a small one down there, run by, I'm not sure I got first name, Swaney Hunt, of the Hunt Brothers. She was the sister in

the family who had left wherever, Oklahoma, wherever they had made their fortune, and moved to Denver, has been involved in women's programs, community service dealing with home, homeless and that sort of thing. So she has a small foundation that she ran, and Michele Simpson worked for that foundation as well as this Peton foundation. So she had me come down and talk to these foundation people, and they were really facilitator. And they said, well, we'll give you support, we'll help you have Michele's time on this, and we'll help you get a foundation started. But I didn't know much about foundations, but they felt that the foundation concept is an interesting one because it kind of gives you freedom that you don't have with normal, getting funding from educational organizations and the government and that sort of thing. And so they set us up so to speak. Gave us foundation support, got us lawyers, very gifted foundation lawyers, lawyers who knew how to set up foundations. And get us started. And they have been very supportive as we've tried to get this foundation off the ground. It's called the Black Environmental Science Trust, BEST. We have people on the board such as Charlie Anderson, a colleague of your at North Carolina, and he's given us some good advice. Walt was a founding member and gave us all kinds of advice about it, how to get it started, just before he passed away. In fact, he said, well, you know, I'm doing so many things, and I'm getting a little tired these says, and so forth and so on, but I want to help you with this. And so he would sit down with us for long periods of time and just kind of brainstorm about how to get started and so forth. And also, another person who you know, Joe Goldman, who has been very active on the UCAR trust.

Droessler: Bill Goldman?

Washington: Yes, I'm sorry, Bill Goldman. Old friend of mine, in fact, Bill was on that original triple AS thing that Walt got me involved in.

Droessler: Was he a member of your foundation board of directors?

Washington: No, no. He felt that...

Droessler: Took a lot of time, huh?

Washington: Yeah, he felt that he couldn't put that kind of time in, but he did give us a small amount of money to help get things started, and he's been very supportive of the whole concept. In fact, he serves on the, as chairman of the American Museum of Science, let's see, on the American Museum of Natural History in New York, and when the exhibit which I am in, this black achievers came there and was on display for three or four months, he wrote me a very nice note. I have been in touch with him. So anyway, we don't know how it's going to go, u but we have some expert advice, and people out, because one of the trustees is a black lady who was with the NAACP legal defense fund for something like thirty years, and she got the grants maker award, so she is an expert on foundations and grants and all that sort of thing, so she's helping us to. So, anyway, it's

going to start slowly because we want to raise endowment initially before we start giving out substantial financial grants. So it's kind of a dream, I don't know, I hope it works out, but we're working at it.

Droessler: It's really a marvelous idea. It's really a new, independent, self-standing organization, in a non-profit area that can have marvelous results, you see. And I think that you began something very, very worthwhile.

Washington: Well, I hope it takes hold. We have a lot of interest in it, which is one of my problems. People want me to come and give talks and that sort of thing, and I have to limit my time to getting the foundation on a solid financial basis. Let me move on to some recent things that I have been doing that are of some interest. I read a Newsweek article sometime in November about my greenhouse research, and I was quoted by John Sinunu in the article, and I think he misunderstood my research a bit. He said something about the greenhouse effect, doesn't look like it's as large as, from the NCAR study, as some of the other studies have indicated, and would I...I think what he misunderstood was that in our study, we had put in the full ocean, even the bottom of the ocean into the greenhouse problem, where as in earlier studies the ocean was only the top part of the ocean. And when you put the whole ocean in, as you can imagine, from the thermal inertia of the ocean, it would slow down on the effect of the warming as people anticipated. Anyway, I sent off a telegram to the white house to inform him of that and suggested that he call. Within one day he called. And I, then I started having a dialogue with the white house, mostly with John Sinunu. It turned out he had done a thesis of research at MIT on fluid systems with thermal inertia.

Droessler: Is that right?

Washington: And so, his first question to me was, Warren, are you using finite difference methods, spectra methods, or finite element methods for solving the equations? Then I knew that this was not the normal sort of white house advice that one gets. He actually gets into very technical issues. He asked all kinds of detailed questions about how the radiation was done, and dam strengths, and things like this that you wouldn't expect from the white house.

Droessler: Amazing.

Washington: The person. Well, the next thing I knew...

Droessler: Imagine having this right at the right had of the president.

Washington: Right. The interesting thing is that one thing led to another and I went and spoke to the cabinet, with Darmond, with Sinunu, with Michael Boston, the counsel for economic advisors.

Droessler: Has this been recently?

Washington: This was in March.

Droessler: March.

Washington: And so, our talk with the cabinet, the president wasn't there, but almost everyone else in the science establishment was there. And it was a real dialogue. I didn't lecture to them. They were just asking questions and so forth. There's an interesting little history to this, but I'll leave that for some other time. The next thing I knew I was being asked to brief various cabinet officers in more detail. I was appointed by Admiral Watkins, the secretary of energy, who actually had gotten his fame in national circles as the author of the so called AIDs report, which he did an excellent job on. He's a straight forward kind of guy, and he asked me to be his part, of his advisory committee, which I think was a great honor, because as you know, we are in the midst of the Middle East war now, it isn't a war, but the Middle East sort of situation, and everyone complains about the nation, doesn't have an energy strategy.

Droessler: So you're an advisor now to the Department of Energy?

Washington: Right. And the...Admiral Watkins is a very straight forward guy. You can get the feeling that he does not play politics a lot, and he's come up with an interesting strategy which he's going to have the president present at the state of the union address in January, but these are going to be some hard things for the nation to kind of deal with.

Droessler: And your part is to try and look after the environmental effect?

Washington: Yes, I guess. I'm not sure. He calls me back, and to stop in every month or so to give him a briefing on certain aspects of the problems. For example a few weeks ago, or a month or so ago, I brought in Wally Broker to talk about the carbon cycle with him, and also Tom Karl who's an expert in Ashville at the NOAA center there on temperature trends and the observed data which he seems to be intrigued with. Now that the reason he's interested in this is apparently he and John Sinunu are good friends which I didn't know until very recently, but they have great arguments at the cabinet meetings about kind of what's known about the greenhouse problems. So Admiral Watkins wants to be brought up to speed so he can argue articulately, in an articulated way with John Sinunu. I'm caught in the middle. In fact, one of the interesting things, which by the time you make this public, there's something that appeared in US News and World Report, about a month ago, that John Sinunu has a climate model. Well, he kept badgering me about wanting a climate model and Allen Bromley, the president of the Science Advisor, finally get fed up and asked me to give him a climate model that he can run in his office there. So, after getting some help from people here at NCAR, we put together a climate model

that John Sinunu can run himself on his compact 386 computer in his office. That's another kind of story, but it's...

Droessler: Let's see if one of those walk into NCAR.

Washington: Well, in fact, Bromley did call me one day and asked me, he said, Warren, I'm kind of fed up with this situation, give him whatever he wants including your password. So I'm kind of stuck in the middle of the white house politics to a certain extent, because I have different cabinet officers who are asking me to brief them. I'm scheduled to have a meeting with Riley, the head of EPA, who also wants me to tell him, I suspect, on what I'm telling John Sinunu. Because all of them are trying to grapple with this very difficult problem of global change and greenhouse effect. And you know, this is going to lead to some hard choices for the nation.

Droessler: This is a very difficult area, and we don't have agreement among the scientists.

Washington: No.

Droessler: And so it's very harkening to me, and I'm sure all your colleagues in atmospheric scientists, to know that you have a seat there in the white house and are helping to form the general perspective and policy. It's so important that we do it right.

Washington: Yeah, and I limit myself to what we know. Okay, in other words, there's often some people who have particular points of view, and want to use the scientific facts to support those in a selective fashion. I tend to want to follow kind of what the various international bodies are saying, to explain it to them in a way that they can understand, so that the science that we get out and tell them is accurate, because I am very concerned about, kind of what's in the newspapers. Usually what's in the newspapers isn't entirely wrong, but you know, all journalists and all editors have points of view that they are trying to push.

Droessler: And there's a general tendency among the printed media I think to, especially the newspapers and the magazines, to want to tack on a little of the adventure.

Washington: To hype it up a little bit.

Droessler: That's right, and project beyond where we really are scientifically.

Washington: That's right.

Droessler: Because they feel that's the kind of fascinating twist that they want to give the story.

Washington: Well, there was a diff...

Droessler: It's unfortunate. The public is not being well advised and well educated on this greenhouse effect.

Washington: Well, it was interesting and this is just one little facet of this white house meeting I had with, which Darmond was at, because he was very unconvinced, you know I thought I was losing him because...(tape ran out).