

**National Center for Atmospheric Research
University Corporation for Atmospheric Research**

ORAL HISTORY PROJECT

**Interview of: Dr. John C. Calhoun
15 June 1988**

Interviewer: Earl Droessler

File 1

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Droessler: This is Earl Droessler. I'm at Texas A&M interviewing Dr. John C. Calhoun, one of the early pioneers and contributors to UCAR/NCAR development. We are here in his office on the campus on a lovely day in June in Texas. John, I wanted to begin by asking you when you first became acquainted with UCAR/NCAR? Where were you working at that time?

Calhoun: I was at Texas A&M University. I came to Texas A&M in 1955, and I found that there was a Department of Oceanography and Meteorology, which was headed by Dale Lieper. Dale Lieper had a fair amount of external research work and carried on quite a productive program which was the envy of those of us who were in engineering, which is what I was administering at that time. I got to know Dale, and I also got to know another person, Archie [M.] Kahan, who was in the Texas A&M Research Foundation, which was a relatively new organization that had been set up to handle external contracts through which Dale Lieper carried most of his work.

We had very little of this in engineering, so we envied Dale and Archie for the programs they had. I looked into it in some depth in order to try to stimulate the people in engineering, but I became associated with the Texas A&M University system in 1957 as a vice chancellor, and as such took on a little broader scope of responsibilities with respect to the development of programs that weren't in the field of agriculture. This included a concern for water, and we held a number of water conferences. One of the principal things that concerned us was the droughts situation that had hit Texas, and a part of our program to contract droughts was

weather modification. This was not under my jurisdiction, but being responsible for a broad overview on water, I naturally knew about it.

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But I did not specifically become associated with or become aware of the UCAR/NCAR effort until some time in 1959, or it may have been late 1958. One meeting had already taken place concerning this activity, and my recollection is that it took place at Penn State. A representative from Texas A&M that attended the meeting was our vice chancellor for finances, a man by the name of Clyde Freeman [?]. Dale Lieper, of course, was our institutional representative from a technical point of view. I do not recall why the chancellor decided to transfer the administrative assignment to me rather than to Mr. Freeman, but he did some time prior to the meeting that was held in Tucson, Arizona, which as I recall was the first time I attended a meeting dealing with UCAR/NCAR. I went to that meeting, along with Dale Lieper, as the institutional representative of Texas A&M University, and went with instructions as to how I ought to consider it from a university point of view.

My background in the atmospheric sciences was very minimal, so what I brought to this assignment was fundamentally a concern for how to develop programs. Shortly after I became involved in this, my assignment changed and I became a vice chancellor for program development, which means that I had an institutional responsibility to look at the development of all kinds of programs within the university's structure.

Droessler:

But that meeting in Tucson in April 1959 was a very important meeting, John, because at that time the board of trustees for the University Corporation met for the first time and elected a chairman of the board and a vice chairman and set in motion the corporate affairs. So I think that you came in at a very auspicious time, just as the trustees were being formed and began to march off as the leaders for this new corporation and for the national center which was to come. So welcome on board, John Calhoun.

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Calhoun:

It may have been very auspicious for me. I'm not sure how auspicious it was for UCAR. It was indeed my introduction. I learned, of course, from the early discussions that there had been an Academy of Sciences study out of which had developed a plan for a national program. I don't know who were the ringleaders in this. I recall that those whom I met in this early UCAR meeting that struck me as having and have had a strong hand in the development of that program seemed to be people like Horace Byers and Henry Holton [?] and Tom Malone. They seemed to know all of the background, and when questions would come up, they would refer back to the Blue Book, as I believe it was called, and to some of that early study. But I came into the situation without any of that background. I might say

that as a meteorologist, my exposure was limited to a couple of courses I took as an undergraduate at Penn State from Helmut Landsberg. He joined the faculty of Penn State in the mid-'30s as a professor of mining engineering, and he introduced two courses in geophysics, one an introduction to geophysics and the second an application of geophysics, and in these courses he included a discussion of oceanography and the atmosphere and the interrelationships. This was essentially the background in atmospheric sciences that I brought to this, other than a concern for some of the water resource problems that the state of Texas formed.

So what I brought into this group, auspicious or not, was a concern more for an institutional position, for a program development posture, and for putting together a scientific-type program rather than a concern for anything that was particularly unique to the atmospheric sciences.

Droessler: Do you remember when you were elected a member of the board of trustees? It must have been shortly after 1959, perhaps in the early 1960s.

Calhoun: I can't tell you when that was. It's one of those things—I have a memory early on of a meeting in Chicago which probably occurred shortly after the Tucson meeting. Prior to that time, the location of this proposed center had not been decided upon. And in fact, one of the things I recall about the meeting in Chicago is that there was quite a bit of discussion as to what kind of criteria ought to be set up for selecting a director of this program.

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There was also a great deal of discussion in the early days as to where this national facility would be located. From Texas A&M's point of view, I was quite interested in this. I can't put it in context with any meeting, but I do recall that early on, there was a committee established to set up some criteria that would apply to a location for this national facility that was to be brought into being. These criteria were quite detailed, so being a young program developer, I naturally said, "Gee, this is something we ought to be able to capture," and I came back to Texas A&M and we went to work on this, and along with a lot of other people, we put together a proposal.

I recall that among other things there were specifications that there had to be no mountains around, that the radar were presumed to be one of the most valuable tools and you had to have a line of sight that was quite considerable, so you wanted a relatively flat terrain. I recall that as one of the primary things. Well, as it turned out, these proposals all went in and I think they were being studied along about the time that the selection of a director was made. I always remember this and used it as an example in later periods when talking about the decision-making process, because as it turned out, when discussions were held with Walt Roberts as to whether

or not he would be director, his answer was, yes, he would be director if the facility were located in Boulder.

Of course that immediately decided everything about location, so all the criteria went down the drain. I often wondered how much manpower went into preparing the proposals from various universities. I'm sure there must have been 15 or 20 proposals from various universities as to why this facility ought to be located at their location. This decision was made when a decision was made to hire a director. So I learned a lesson there right off the bat, that you make decisions in a certain order, and once you've made one decision, that kind of shuts the door on other decisions.

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Droessler: Well, you were certainly involved in these decisions, if not all of them were made in a direction that you were interested in. The selection of the site for the national center and the selection of the director were two of the early decisions that marked the development of UCAR and NCAR. When Walt Roberts came on board as the director some time in 1960, then I believe things got off to a very robust beginning.

Calhoun: Yes. I think this was probably the most critical decision that was made after there was the decision made to go ahead with this organization and with this program. Had Walt not been selected or someone other person of equal dynamic, capability, and stature, the organization could well have floundered. What Walt gave was a purpose, he gave a presence, he gave a vision which everyone could rally around.

Droessler: And he could articulate this.

Calhoun: Oh, and he could express it so well! Not only that, he was in a position to gain the support needed from a local point of view to make it go in the situation where he existed. So that was very critical. In that respect, I recall sitting on an airplane, and it runs in my mind it was after the Chicago meeting. I cannot tell you where we were going. Walt Byers was one of the persons, I was another one, and I don't recall, I think there were two more. And as we were riding in the airplane, Walt Byers observed that he thought—

Droessler: Walt Roberts?

Calhoun: No, Horace Byers, I'm sorry. Walt Byers wasn't there. These were people that were involved in the UCAR discussions, either as institutional representatives or board members, and I can't put it in context. It runs in my mind it was after the Chicago meeting, because at the Chicago meeting we were talking about how we would go about finding a director and all these things. It runs in my mind that it was right after that meeting and we

were going somewhere in an airplane. And I recall that Horace Byers made the observation that he thought Walt could be had for this job. I don't know whose influences were strongest, but I've always had the feeling that Horace Byers played a big role in it.

Droessler: Well, he was chairman of the director selection committee, so he had to play a big role. He was the person that was chairman.

Calhoun: I didn't recall that.

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Droessler: He was most responsible for trying to develop a list of names and getting people like Jim Van Allen and others to at least give a look in to see whether or not they wanted to be the director of NCAR. So that was really a very important decision, to selection a very good man to head up the organizations.

Calhoun: Yes.

Droessler: How important was the decision to establish a national center itself?

Calhoun: Well, I think that was—next to having selected a director and Walt Roberts as a director, I think the next most important decision was the one to actually build a facility and a center. I have observed a number of consortia-type organizations. I have led two or three. At one time I felt that the consortia route was the way to go, and during the '60s, Texas A&M belonged to 10 or 12 of these consortia. In my job, I was the institutional representative to many of them. They all had similar problems, and the one that I took over as executive director and president was called the Gulf Universities Research Corporation, focused on oceanography in the Gulf of Moscow. We made a fundamental mistakes, as did many of these, in not building facilities or providing some kind of a physical entity to rally around.

It is true that UCAR had the atmospheric sciences as an important area of study and a public concern to rally around and to focus on. But that in itself is too diffuse an objective. You have to have something more tangible. What the National Center for Atmospheric Research represented was a very tangible—not just a place, a very tangible atmosphere of research, a place where one could expect questions to be—people to gather and questions to be asked. It was built in a way to cause some excitement from an architectural point of view and from an environmental location point of view. And all of these things brought into being a presence.

Droessler: A program and a commitment and a presence.

Calhoun: Well, not even a program, but a presence, a presence around which one could hang a program. As good as the program might have been, a program can always be parceled out to many places. If you've got a facility, there's something tangible. It has to be kept going operationally, and I feel that the building of the center was next to having picked Walt Roberts or they could have picked perhaps somebody like him, but after having picked Walt Roberts, the next most important thing was having built that center as a national facility.

Now, fortunately, the whole idea came into being at a time when it could be done. The federal government and the funding sources were there. The window that was allowable for building that facility was very short. Had the idea not been conceived, the monies been allocated, and the center brought into being within a two- or three-year period of when it actually happened, it could never have occurred.

Droessler: That's an interesting perception.

Calhoun: The total climate for support of this sort of thing changed in the late '60s, as you know, and the questions being asked were much greater. So I think we were very fortuitous, call it luck if you will, the fact that the whole UCAR idea got started when it did, that the organization was born in the '60s allowed one to take advantage of that—call it the Great Society, and the rising role of research. Science occupied a much different posture, took a much different posture in our federal structure at that time than it did six or 10 years later.

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Droessler: You mentioned the building NCAR has and its architectural values. Did you have anything to do with the selection of the architect?

Calhoun: I was a member of that committee, and that was a very interesting experience. We had two or three meetings. It seems to me that Walt Roberts had his mind made up long before the selection committee finally came around to selecting the architect, but yet we did go through a rather long, involved process of interviewing the architects and having them present their material. We brought them into Boulder to discuss the matter.

Droessler: You showed them Table Mountain, or Table Mesa?

Calhoun: Oh, yeah, we had a couple of very nice expeditions to Table Mesa, picnic lunches, we went up.

Droessler: How did you get up there, by helicopter? Did you truck up on four-wheel drives?

Calhoun: As a matter of fact, we walked up. There may have been—I'll have to jog my memory here. There may have been a vehicle, there might have been a vehicle that went up around, but I don't recall a helicopter, although there might have been. I walked up.

Droessler: With the architect? With one or more architects?

Calhoun: I cannot say whether we had architects with us or not at the time. I'm sure the architects went up there, but I can't recall whether the times I'm thinking of, whether there were architects there or just the committee and members of the staff, but I can recall two distinct times when we went up on the mesa and had picnic lunches and looked around and talked about the various aspects of having the facility up there.

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Droessler: Looking back now, do you think the decision was a good one, to locate the NCAR on Table Mesa?

Calhoun: Yes, I think it was a good one. I don't see any negatives.

Droessler: It's an attractive and useful laboratory?

Calhoun: Oh, very attractive. How useful, I have no firsthand knowledge of this, although UCAR/NCAR seems to be carrying on pretty good programs, so I'd say it must be useful. The only problem we had with that facility, as I recall, after it was constructed was a roof problem. The roof leaked, and there was the question of who was responsible. We got into a three-way discussion as to whether it was the architect, designer, the contractor, or the people who supplied the roofing material. That lawsuit went on for quite a while. I think it was finally settled. I'm not sure who did take the blame, but the roof did finally get fixed.

There was another problem early on, as I recall, having to do with the color of the cement and how it blended and how the cement finishing was accomplished. But I didn't really pay much attention, wasn't paying much attention to those kinds of details. The thing that intrigued me was that I.M. Pei came to this job and this assignment and opportunity with such a great deal of vision. He captured almost everybody's imagination right off the bat. I think first of all, as I indicated earlier, I think he sold himself to Walt Roberts. Whether Walt sought out Pei or Pei sought out Roberts, I have no way of knowing how they came together. But it was clear almost from the beginning of our discussion that Walt Roberts favored I.M. Pei. And of course we were all very sensitive to Walt's views on these sorts of things.

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Droessler: Pei was an important architect at that time, but certainly not of great national fame.

Calhoun: Not as well-known as he is today.

Droessler: Certainly today he has moved on up to become not only national, but an international figure.

Calhoun: I think NCAR can be very proud of the fact that its facility was designed by I.M. Pei.

Droessler: I have a feeling that you came on the board some time in the early or mid-'60s, because you served a term, perhaps as the vice chairman of the board under Dick Cassander [?], and then after Dick left, in about 1967 or '68, then you became chairman of the board.

Calhoun: Yes, that is true. As to when I became a member of the board of trustees, I think it may have been in the early '60s. I recall that I was serving on an early committee dealing with organization. There are several themes that ran through the whole UCAR/NCAR development.

One of these themes was, "Hey, are we here to do a scientific job or are we building a bureaucracy?" This was simply a reflection of the fact that the people who had this vision in the first place wanted to see advances made in their profession and wanted to see their profession serve society in the best way, and they were anxious to get on with some projects, some scientific jobs. But on the other hand, there were those who said, "Wait a minute, you can't do this unless you do it in the right way, because you've got to have the support of lots of people. And furthermore, you have to do it in a way that's going to be acceptable to everybody, and we can all live with. We can't get you in the position of taking away the money from our own universities' programs. We're all competing for the same kind of federal money, and we can't get you in the position of taking away all of our fine professors and bringing them up here. We've got to keep some at home to run our own programs. You fellows can't just go out and say you're going to do something in the wild blue yonder, it's got to be organized."

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I was among those who, because of my background, tended to look as much at the housekeeping problems, or shall I say the organizational, I prefer to say organizational and management problems, rather than the scientific problems. So many of my early comments and discussions with the representatives had to do with how the group was organized and whether provisions had been made for one contingency or another as it might evolve. And this I think pegged me in the minds of some as being

interested in things that somebody had to do. So I got put on an organizational management committee fairly early on. As I recall, this was some time I think in the early '60s.

This, of course, involved such things as how you present your material to the federal people and how you have the program organized, but it also got into such things as personally matters. And this thread continued to run through the entire organization and in fact I think is the primary thing that brought the organization finally to move from Walt Roberts to another director. Walt Roberts's talents and attributes show up the best in a small group, that is, in a group where there's a lot of informality and as it gets to a certain size and people don't know Walt so well or there is not that close interpersonal relationship, it becomes a little more difficult for the organization to function.

So unless you're careful as you build organizations, you can run into problems, and I think basically the organization just outgrew the kind of style that Walt Roberts expressed, and so the organization found that it had to go on and get a successor. And this happens in all organizations as they develop. The person who has the talent to put it together and hold it as a family group or at a certain size finds eventually that it's not his kind of organization any more.

Droessler: Do you remember one of the rules that your groups established associating UCAR and NCAR with the universities that NCAR would not attract more than one member of the faculty from any one of the member institutions?

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Calhoun: I don't remember the rule, but I do know that that was an item for discussion. As I say, this was one of the threads that kept running there. Of course, there were some other threads. One thing that was present in the organization right from the beginning was, what is UCAR anyway and how does it differ from NCAR? There were times when people would say, "Wait a minute, are we talking about UCAR or NCAR?" There was a confusion in many people's minds. This was part of the organizational question also, and until the balloon facility was built, or until there were operations carried on that were aside from the center itself, this question always came up again and again.

What I'm trying to say is that as long as the thrust was to build a facility and to bring that facility into being, this occupied most everybody's time. So in that stage, it was hard to differentiate between UCAR and NCAR. Now, once that facility got built, the attention could be turned to things like, "What about a university relations program? What about some young scholars that we can bring in here? What about this balloon facility we're talked about? What about these airplanes and hail programs and things

that get away from this facility?” That made it clearer then as to why there was a UCAR organization a little different from NCAR.

I can't really say when that particular issue was finally resolved. Of course, eventually it led to separating the job of the director of NCAR from the president or director of UCAR, whatever the name is called. And as I recall, John Fierohr [?] was the first person to succeed Walt, and Walt was made president and we built that small extra building so that there would be a separate identification for UCAR. But that was another one of the themes that ran through continuously as part of the growing pains.

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There was another issue that kept coming up all the time, and that was, who should be in this family, anyway? It started out as a very much closed corporation. I think that number was 14 when it was originally brought together.

Droessler: That's right, mm-hmm.

Calhoun: Now, I don't know and I wasn't privy to the rules that set those up, but as we wrote the first set of bylaws, we were very strict in what we wrote down as qualifications as to a—

Droessler: These 14 were the universities of the land that had graduate programs in meteorology.

Calhoun: I think in the first bylaws we wrote in some very strict membership criteria, that they had to have a graduate program and so forth. Well, this was always a sore point, right from the beginning. “Haven't we closed off this fine national program and its opportunities from a lot of scientists who are really interested and capable of doing work in the atmospheric sciences but they just don't happen to be in meteorology departments or places where they have a bona fide Ph.D.?” That finally got resolved, I guess, in more than one step. It seems to me, as I recall, that the door was opened a little bit, and then the door was opened a little bit more. I guess now it's quite wide open. I don't know what the ground rules are now. And I don't know when that took place. I think it—well, it must have taken place in Cassander's term or earlier, but that was a continuing argument all the way through.

Now, there was one interesting consequence of all of this. When the corporation was first formed and everybody was brought together, we had a set of bylaws. I'll bet you that one of the primary agenda items at every annual meeting for the first 10 years was revision of the bylaws, because they were written so rigidly, either with respect to who could qualify as an institution, how the institution representatives would be chosen, how the

trustees would be chosen, how the voting would be carried on—we were changing those bylaws continuously, and this was almost a full-time job for Jerry Hart [?].

Droessler: Almost a full-time job, too, for Jerry Hart with the help of some of the administrative representatives from the universities. And going back to some of our early conversation, you said that one of the important decisions you made early on was the selection of Walt Roberts, and another very important decision was the establishment of the national center itself as a facility and an identity. And then perhaps there's a third important decision that was made early on by the UCAR organization, and that was to have both scientific and administrative representatives from the university, and you being an administrative representative from the university would approach the problem of UCAR, the organization, the management, and the carrying out of the functions of UCAR and NCAR quite different from the scientists' point of view. Do you agree that that was an important decision that was made by UCAR in the early days, to have an administrative representative there?

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Calhoun: Yes. and I don't know what gave rise to this. There were very few consortia of any kind in existence when UCAR/NCAR was formed, and I didn't have a role in the discussions that set up this format. When I became aware of it, there was already an institutional representative from the administrative side as well as the technical side. So I don't know how they came into being, but I would say yes, that was a quite important step, and it's one that's been copied by many other organizations.

I think I agree with you that the contribution of the institutional representatives was large. They put in a lot of time and effort in helping this young organization stay out of trouble. I don't mean trouble in a real nasty sense, but I mean out of trouble with respect to those who would nitpick on things like budget or salary structure or even the image that was being put out. These people were from institutions, MIT, University of Chicago—

Droessler: Do you remember some of them?

Calhoun: Oh, yes. The one that stands out most in my mind, I guess, was Gill Lee [?] from the University of Chicago. But Carl Floe from MIT was also quite dominant. I believe Dave Patrick from the University of Arizona was very—contributed quite a lot. What these people brought was a wealth of experience of how they had dealt with the federal government and with large programs in their own institutions, some of the kinds of problems that they had faced and then had to solve. They could alert Walt Roberts

and his staff of things that were going to come up that they had to avoid. They contributed a great deal.

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Another one that contributed a lot in helping Jerry Hart was Roy Wilkinson of Penn State, who essentially took over from Jerry Hart ultimately, but there were a great many legal problems, particularly in the early days, when the University of Colorado and the state of Colorado and the city of Boulder had to clear all the rules and regulations with respect to using Table Mesa, putting water lines up there. There were many, many areas in which I'm sure Jerry Hart played a key role, although I don't know them in detail.

Droessler: Let me toss a bouquet in your direction, John, because I think you deserve one, probably a dozen or more. You were the first administrative representative to be elected chairman of the board of trustees of UCAR.

Calhoun: I didn't know that.

Droessler: All the other chairmen had been from the scientific side, from meteorology. So congratulations to you for being chosen. Do you remember some of the activity that was carried out at the time that you were the chairman of the board, say, in 1968, over the next three years?

Calhoun: Well, it must be that all of these fellows did such a good job at getting this thing organized and put into being that I didn't have to worry, because I don't really remember anything that it seems to me was a big milestone. It was during this period that we finally came to grips with the total question of UCAR/NCAR separation or difference, distinguishing one from the other. As I recall, one of the principal things that we were wrestling with at the time was the funding. The National Science Foundation, along with other federal agencies, was feeling the pinch that follow the Johnson administration, the cutback in support of science. There were many more questions being raised about these scientists spending public dollars freely. We had to make a number of presentations to the National Science Foundation in defense of our budget, and I feel that they were much more critical at that time.

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They were raising questions about the difference between NCAR and UCAR and what our long-range plans were. That's the principal thing that I remember during that period. As I say, we must not have had too many problems. I don't remember any real fights. I'll tell you, if I would have anything to observe about the organization, it was that there were so many good people on hand. After all, when you have a fellow like Bill Golden, and I believe he was at the time I was chairman on the executive

committee and looking primarily after our financial business, and when you have a fellow like Gill Lee, with all of his background and experience, supported by the people from the National Science Foundation, yourself and others, you don't have to have very much on the ball. You just let these people work.

Droessler: That's a nice way of saying it, John. You're right, the National Science Foundation—I moved out of the National Science Foundation at about that time. I believe that Dr. Thomas Mulholland [?] came in as an assistant director.

Calhoun: Tom Owens.

Droessler: Tom Owens, that's right, came in as assistant director at NSF, and really led the questions of NCAR and all other aspects of NSF program management and support, because as you said, the funds plateaued and became less and less for the development of new initiatives in science. The NSF was very critical of all of the programs that it was looking after, and by that time, too, the NCAR budget became large enough so that it became very noticeable a part of the national scene.

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Calhoun: That's right. It started out as a program that was small enough that it could be hidden, if you want to use that word, or not deliberately hidden, at least it didn't stand out like a sore thumb. As it grew, it commenced to represent an appreciable part of the budget, and different sets of questions get answered when you get into that kind of a situation. I feel that there were other actions going on that contributed to the growth of NCAR and UCAR. You may recall that during the Johnson administration, the Congress set up a national council for the study of the oceans. They had a big staff in Washington which was—in which Ed Wink [?] played a key role as staff member. Jay—I forget his name—was the chairman of this commission. Anyway, the commission for the study of the oceans.

Droessler: Jay Straff [?]?

Calhoun: Jay Straff.

Droessler: From MIT.

Calhoun: Anyway, this commission for the study of the oceans had a big piece dealing with atmospheric problems, and this helped to bring out the importance of atmospheric problems. There was also a considerable interest during this period of time in weather modification, but Congress had latched on in the early '60s to the idea that weather modification

might work, in fact, there were some people claiming it would work, and there were Western Senators who were very much interested in putting money into such a program. And in fact, when I was in the Department of the Interior, I had to give testimony or answer questions from the committees of Congress concerning the advisability of such a program.

Well, it was brought into being and funded, and this represented a reasonable amount of effort. So it just wasn't that there was NCAR/UCAR growing. It's that there were a lot of other things that were also in this same sphere, and there was more competition for the money. And there were questions. For example, if there was a hail project, should that be done by NCAR or the Bureau of Reclamation or somebody else? The same with weather modification. So there were a great many forces—

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File 2

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Droessler: We've come a long way in our interview, John, and it's been a real pleasure. I would like to ask you to reflect on this question: has the presence of UCAR/NCAR advanced university activities in significant ways as the atmospheric sciences has matured in our country?

Calhoun: Well, I think the answer to that question would have to be a very strong affirmative, but I don't know that I'm the person who can give you all the details on it. In the first place, I think one would have to note that today there are some 60 memoirs, are there not?

Droessler: Yes.

Calhoun: Of UCAR, which is a considerable increase from the 14 that had programs. Now, not all of these 60 today have programs of the same depth or breadth or specialization that the original 14 had, but nevertheless, I think you would have to say that the total attention in the university community to the atmospheric sciences is visibly a great deal more than it was when UCAR/NCAR was formed. Coupled with this, I think you would have to note that UCAR has brought in a considerable interaction from overseas. A number of scientists were brought into the NCAR organization from other countries, and this has spilled over into the universities.

I think UCAR/NCAR can be proud of its role for the universities as a training ground, particularly with minorities, but with others also. Early on there was a committee formed dealing with university relations, and specific attention was paid to how the facilities and the presence of NCAR/UCAR could benefit the member universities. So there were

opportunities for faculty members to go into the NCAR laboratories for summer employment and special research projects. But more important, students were able to do this. There was a scholarship program, and as I say, some of this went into the minority institutions.

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I think one would also have to take note of the presence of special facilities which might have come into being without UCAR/NCAR, but it's highly unlikely. For example, the National Balloon Facility. The computer.

Droessler: Computers, particularly.

Calhoun: That large computer must be quite an increase in competency for the individual universities. I read in *Science* continually of things that are being done, and there's always mention of the UCAR people coming forth with better prediction methods. There may be—it may be well worth looking at the total federal budget going into atmospheric sciences now compared to what it was before UCAR/NCAR came into being. I don't know what it would be. It's certainly larger now. How much larger it is in real dollars, I do not know, or how it fares as a percentage of the total federal budget I don't know. But I would venture to say that there is more federal money relatively speaking now going into atmospheric sciences, and I think you would have to credit UCAR/NCAR for stimulating part of this.

But aside from the money, I think there's a more important thing. I believe there is a collegiality in the atmospheric sciences community today that's much stronger than it was before, because the individual faculty member, individual university people can work through an NCAR program. Indeed, the situation is not unlike that in my own field of petroleum engineering. In fact, I wish in my field we had the equivalent. What one is likely to have without an NCAR/UCAR are a bunch of individual university projects. Instead, what you have today are a bunch of programs.

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Now, there's a difference between projects and programs. I think what the presence of UCAR/NCAR has forced on the national academic community scene is the presence and existence of a national program in which they can all participate, rather than a bunch of discrete projects. This is one of the better elements of big science versus little science. You hear a lot today about the big science/little science, and there are many who say, "We'd like to go back to the good old days where I get an idea and I put together my project and it's my baby for it." There's a certain attractiveness to this, but with such a conglomeration of little projects, one never built a comprehensive program.

I am not familiar enough with the details of atmospheric sciences to know how much of a program existed prior to the UCAR/NCAR presence, but if I can rely upon what I heard about the Blue Book and all of the reasons why NCAR was justified, I would have to say there wasn't much in evidence in those days.

Droessler: We had a program in those early days, but it dealt with the operational aspect of meteorology, which was the forecasting under the National Weather Service. But in the research and development community, there was no national program at all. There were just—there was just the beginning of a handful and then more than a handful of research projects at the universities dealing with aspects of meteorology and atmospheric sciences.

Calhoun: Now, you might argue that a national program would have evolved without UCAR/NCAR, but on the other hand, I think UCAR/NCAR can certainly claim to have been a considerable driving force in seeing that such a focus came into being. So I would say that without question the contributions of UCAR/NCAR to the academic community have been tremendous.

Droessler: You also hear that another aspect of the UCAR NCAR contribution is the expansion or extension of the atmospheric sciences and the sciences related thereto. For example, the melding of HAO into NCAR and how that expanded the atmospheric sciences into aspects of solar astronomy.

Calhoun: Yes. That was a very interesting argument in the very early days. There were some who had their doubts as to whether HAO ought to be brought into the family or what it would do to it. In fact, HAO was an ongoing organization which Walt Roberts founded and was running, and it was part of the reason that he was attracted as a potential leader of NCAR. And there were those who felt that he would continue to be so absorbed that NCAR wouldn't really get off the ground. So there was a great deal of discussion. Fortunately, Walt had the very good second in command, Walter Fierohr, whom he was able to turn HAO over to. But it did indeed represent a first step in making truly atmospheric sciences rather than meteorology out of this animal.

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Droessler: John, do you think we have sketched ourselves out of the UCAR/NCAR activities? Do you have more comments and perspective to add as concluding remarks here?

Calhoun: I might offer a couple of general observations. I think it was very fortuitous or lucky or maybe it was really good planning that the

meteorologists and the Academy of Sciences or the leaders in the 1950s did get together and start this movement when they did. Had they waited another half dozen years or so, I don't think they would have been able to pull it off. I think it came at the right time, and it illustrates so much how important timing can be.

I feel we haven't touched on another aspect that's very important to me, and this is how the public views this whole thing. Weather, as we all know, occupies a good bit of the conversational time of people, and it bears on everything we do in our daily lives. I guess I look at the weather information on TV more than I do anything else. There is a great public interest which is far deeper and broader today than certainly it was in 1960. Now, what role UCAR/NCAR played in this I'm not sure, but I have seen many references to the national center and many of their publications that leads me to believe that the contributions of this organization to the public understanding of the importance of the atmospheric sciences is very high.

I guess this is a spinoff more than it is anything else. I don't know what NCAR/UCAR is doing these days to promote a public interest. Again, if I might make reference to my own field of work, it's rather difficult to get the public excited about putting public monies into research for oil production, because the public thinks right away of all those big oil companies. So the image which the public holds is very important.

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With weather, it's a little different. But fortunately, UCAR/NCAR goes beyond weather, and there are so many things now—like, take the El Niño and all of its ramifications. Take these long-range predictions, the whole question of global warming and the greenhouse effect. I suspect that as we go down the road, UCAR/NCAR will loom larger in the public mind in this respect, as in the past. After all, when you get that big ozone hole over the Antarctic and maybe over the Arctic, when you get that five-tenths degree warming since 1851 or whatever it is, you've got things to worry about.

Droessler: John, what has your experience with UCAR/NCAR meant for your own development?

Calhoun: Oh, it's been tremendous. In the first place, the people that I met, came in contact with, learned from, it's tremendous. You don't meet a Walt Roberts every day, or a Janet Roberts, for that matter. [laughs] You don't run into a Gill Lee or a Carl Floe as you go along or have a chance to listen to a Horace Byers. As a matter of fact, Horace Byers became dean of geosciences at Texas A&M and vice president of this institution because of our interaction in UCAR. When Nowell Rudder [?] was

president, he was looking for a dean of geosciences. I gave him the recommendation.

Droessler: So a plus for Texas A&M and a plus for Horace Byers!

Calhoun: I recommended that he look at Horace Byers. Fortunately, we were able to attract Horace here, and he made a tremendous difference in the institution. People like that you don't meet every day.

It was fortunate for me also in another way. During this period of time we're talking about, I spent two years in the Department of the Interior as science advisor to Secretary Udall. Of course, many of Udall's problems are environmental in nature—many of the Interior's problems are environmental in nature, I should say, and Secretary Udall himself was very interested in the water problem. One of his organizations was the Bureau of Reclamation, they were very much interested in the water problem and weather modification. And one of the jobs that was handed to me was to respond to a congressional interest in the weather modification program.

So I immediately turned to the people I knew in UCAR and put together an advisory committee of some of the leading experts to try to tell me, advise me as to whether or not there really was anything to this weather modification, whether the Department of Interior should support it or not, which allowed me to go to Congress with what I felt was a reasonable scientific position as well as a political position on the matter when it came time for testimony.

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I think that I learned an awful lot about putting together an organization and watching people interact I was subsequently involved in many other consortia-type activities, and my experience in UCAR stood me in very good stead. I guess more than anything else, I got some visibility from it and certainly my circle of acquaintances is much wider for having been involved in UCAR/NCAR. It was just a great experience, Earl, in more ways than one. I knew Earl Droessler and would have known him in other ways because of our interaction in NSF and the general scientific community, but I can't say too much, and this is all of course selfish on my part.

Droessler: Thank you very much, John. Who is John C. Calhoun?

Calhoun: [laughs]

Droessler: Who were your parents? What did they do? Where did they live? Where were you born? What year? Tell us something about your early life.

Calhoun: Well, I just passed my 71st birthday, going on 72. I grew up in north central Pennsylvania, in what might be called northern Appalachia. My father was Scotch-Irish, my mother came out of a Welsh family. I'm 50% Welsh, actually. Seven brothers and sisters.

Droessler: What did your father do for a living?

Calhoun: My father was in the lumber business when I was born, in my very early days, but the lumber business came to an end in northern Pennsylvania when I was about five or six. He decided not to move with them. They wanted him to go to—they moved to South Carolina, and they wanted him to go. For family or other reasons he decided not, so he became really a local tradesman and entrepreneur. I really grew up in a country grocery store

Droessler: What day were you born?

Calhoun: What day? March 21, 1917.

Droessler: What kind of an education did you receive? You went to elementary school, I presume, in northern Pennsylvania?

Calhoun: I grew up in a ghost town. By the time I entered first grade, there were eight complete grades in two buildings and there was a three-grade high school. By the time I graduated from eighth grade, the school was down to two rooms, four grades each, and the high school had moved to a township basis.

Droessler: This is because the industry had left the town?

Calhoun: Because the lumber business had gone out. By the time I graduated from high school, there were eight in my graduating class, and a few years later, the high school moved to a county basis. I was valedictorian in my high school class. I took the exams on a county basis and was beat out by the gal who was salutatorian. [laughs] But she had to turn down the scholarship, so it came to me. I was second.

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Here was a high school in the county, I don't know how many total students. We had eight in our graduating class, and the two people who competed in that countywide examination from our little high school took the first two scores on a countywide basis. Well, anyway, I went to Penn State. My father managed to scrape enough together, and I took a degree in petroleum engineering, which fortunately at Penn State is in the College of Mineral Industries. So I got an orientation for the earth sciences. As I

indicated, as a junior and senior, I took two courses in geophysics from Helmut Landsberg, but after I graduated and went to work in industry, I got an opportunity to go back and work as a research assistant. I had wanted to do graduate work and didn't think I could afford it, but they offered to take me on as a full-time research worker and let me take six credits a semester toward a graduate degree, so I came back and took it, stayed till I got my doctorate, and from there I went down to the University of Oklahoma after the war to head up a petroleum engineering program. I went back to Penn State to head that petroleum engineering program and came to Texas A&M in 1955 as dean of engineering and director of the Texas Engineering Experiment Station. And I became a vice chancellor or deputy chancellor.

When Dr. Williams became president of Texas A&M, after Horace Byers left, he asked me if I would be academic vice president, so I stayed in that role for seven years. There was a change in president, and I decided to go back into the engineering program, and then I retired from that in 1983. Along the way I put together the sea grant [?] program at Texas A&M.

If you ask who I am or what I am, I have my degree in engineering. I'm not really an engineer. I consider myself to be an educator. But as far as technical competency goes, it lies in the engineering field, and that's where I've done my research, and that's what I'm doing now. I'm back working in petroleum engineering on a part-time basis.

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My broad interest, and one of the things that attracted me to UCAR and has always been a part of my thinking, is I have developed, tried to develop a total philosophy concerning resources. The resource field that I got going in, of course, was oil. I moved from there to water, and that's what really brought me into the Department of the Interior, because they had a big water problem of concern. That got me interested in oceanography. Along the way I got some atmospheric sciences interest. In fact, I once gave a lecture up at the University of Minnesota on resource engineering. I put together some ideas, some resources. If I'm—I don't know whether I'm—I don't know what I am, really, when you say, what is John Calhoun? But that's the sort of thing I've done.

Droessler: Certainly Penn State was very important in your formative years, as a young college person.

Calhoun: I have 13 years on the faculty there in the retirement system.

Droessler: And then more as you were a graduate system. You mentioned Helmut Landsberg as one of the outstanding teachers there. Were there other

outstanding teachers that influenced you as you came through that marvelous institution?

Calhoun: Oh, sure. Any person is lucky, Earl, if they can count on the fingers of one hand the people who have made the difference. That's about what I can count. One of them was a physical chemist, J.H. Simons. He was a fluorine chemist, had a big part in developing fluorine chemistry. He's the person who really gave me a philosophy of science, if I have any. I took chemistry as a minor. Of course my major professor in petroleum engineering had great influence. This was at the graduate level, however. At the undergraduate level, you remember silly little things. I guess maybe one of the most influential persons on me was a professor of mineralogy, a man by the name of A.P. Haas [?]. He had taken his degree out of Princeton. I took a course in microscopic mineralogy from him, and I enjoyed it so much that I went back and took another course in bull pipe [?] analysis. This is the analysis of minerals by using the old-fashioned bull pipe.

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I took another course in crystallography. This was in the days when crystallography didn't have the advantage of all the x-ray and other techniques there are today. This man had a tremendous influence on my thinking and on the approach to things. I once went to him and asked him, I thought I deserved an A in the course and he'd given me a B. And I said, "Doc, I really thought I deserved an A in that course. Why did you give me a B?" He said, "Well, I'll tell you, if I had given you a A, I would have had to give Joel Gray an A, and I wasn't about to give Joel Gray an A." [laughs]

Droessler: [laughs]

Calhoun: But another thing that I picked up in one of his classes, as I say, he was a mineralogist and he was studying crystal structure. His approach to crystal structure, this was in the '30s, pre-x-ray technique days, he was using acids and various types of solutions to develop an etch figure on the face of a crystal. You take the face of a crystal in a certain way and put a reagent on there that will eat into the mineral or whatever it is, and you develop a figure which will reflect the presence of atoms in a certain lattice. So he would put this reagent on the clean face of the crystal, which he would have all smoothed down, and he'd observe the way that figure showed up, and it would lead him to conclusions about the lattice structure, the structure of the mineral lattice.

And he had done a lot of this work, and he'd published a paper he thought was pretty good. The reviewers had come back and said, "Well, there's nothing to his work. All he's got are a couple of anomalies." And I can

remember him telling the story and he stood there shaking his finger at us and he said, “Fellas, I want to tell you one thing. An anomaly is nothing more than a fact standing on its head trying to attract attention.” [laughs]

Droessler: [laughs]

Calhoun: Well, this is the sort of thing you pick up from teachers. Basically, I fortunately had three of four that brought out some things like this. You don't remember the subject matter content of these guys. You remember their point of view on life. And that's really, when you get back to it, that's really the significant thing about UCAR/NCAR, when you come down to it. It's long-lasting contribution will not be so much in what it contributed to an understanding of the atmosphere as it will be in what it has represented as a vehicle or a mechanism within the total structure of this particular domain of science.

Droessler: That will be in large part modified by the quality of the people that UCAR/NCAR brings in to do the job—

Calhoun: Absolutely.

Droessler: —in the laboratory and to do the task of managing the corporation.

Calhoun: In fact, the management capabilities that have been developed can be quite important. For example, Bob White used his management experience here to go on and manage a similar organization, right?

Droessler: Yes, after he left UCAR as president.

Calhoun: After he left UCAR, right.

Droessler: He became president of Joy [?], and then went into the Academy of Engineering.

Calhoun: Cliff Marino [?] I bet will do the same thing. But when we got over this UCAR/NCAR difference or—what shall I say?

Droessler: Identity crisis.

Calhoun: The identity problem, and found out that NCAR represented an operational facility but UCAR represented a management structure and a management capability, then it became obvious, well, gee, if you can manage a big laboratory like this, you can manage some other things. And in fact, I think the history of UCAR has show and some steps in this direction, and that may be the way they'll move in the future.

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Droessler: Moving back again to John Calhoun, the person, were you married as a graduate student?

Calhoun: Yes.

Droessler: When did you get married?

Calhoun: I got married just prior to World War II, got married in 1941, got married while I was a worker in the research laboratory and a graduate student on the side. It took me nine years to get my Ph.D., Earl, because I was working full-time.

Droessler: You married a Pennsylvania girl?

Calhoun: Oh, yes, Scotch-Irish.

Droessler: What's her first name?

Calhoun: Ruth. She's a—

Droessler: And how many children do you have?

Calhoun: She was a Huston—H-u-s-t-o-n, not H-o-u-s-t-o-n, same as Sam Huston, and I was a Calhoun, both good Scotch-Irish names. She had a grandmother named Caldwell. Caldwell's a good name back in the Calhoun family. We have three daughters, all married, and I have four granddaughters, scattered all over the country, one in Boulder, Colorado, one in Maryville, Tennessee, and the other in Texarkana, Texas.

Droessler: John, it's been a real pleasure for me to come and visit with you.

Calhoun: I appreciate your coming. I don't know what good I've done you

Droessler: Well, your perspective on the development of UCAR/NCAR, I assure you, will be one of the important milestones and treasures among the archives and historical records.

Calhoun: I always knew I'd get in the archives some way. [laughs]

Droessler: And I've always thought of you as not only a wonderful colleague and good friend, but also a person—

Calhoun: Better turn that off, Earl.

Droessler: —who had some unusual skills in the management of institutions and organizations and I for one as a meteorologist for whom the UCAR/NCAR concept was first developed appreciated that and want to thank you for taking part of your lifetime and working with us at UCAR and NCAR.

Calhoun: Well, I appreciate those comments. I hadn't thought of it in quite that way. Basically, I was asked to do a job. I was appointed as institutional representative. I went to make the best representation I could of the institution and its point of view. In the process, of course, you have to express your own point of view, but I have never been one to sit back and see something go by and be done in the wrong way just because I didn't speak up. So I guess I developed a reputation early on for speaking out and sometimes speaking out in a rather loud voice. In fact, I have been told at times that I rudely interrupt when I shouldn't. But on the other hand, I recall some very interesting discussions. Dick Cassander and I used to hit it off every once in a while. I considered Dick a very good friend. His background was more the atmospheric sciences, mine was more the administration, so sometime we'd clash on issues, but it was always good fun.

Droessler: And both of you were very much shrinking violets—

Calhoun: I think Dick Cassander was just as willing to speak out as I was.

Droessler: —and understated fellows. [laughs] Again, thanks very much, John.

Calhoun: I appreciate it.

Droessler: This is Earl Droessler concluding the interview with Dr. John C. Calhoun. The interview was conducted at the Texas A&M University in College Station, Texas, on June 15th, Wednesday, 1988.

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END OF INTERVIEW