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TAPE RECORDED INTERVIEW PROJECT

Interview of Jerry D. Mahlman

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Interviewer: Robert Chervin

CHERVIN: This is the 9th of November 2005; this is an interview with Dr. Jerry Mahlman, and we are interviewing at the Foothills Lab of NCAR. I am the interviewer, Robert Chervin. I've known Jerry for at least three decades, and we often ran into each other at airports. We don't travel as much anymore for a variety of reasons. But the purpose of this interview is to go over Jerry's long, scientific history and find out how it began and how it evolved. First of all, can you make some comments on the various influences on you, either in family or school or home life that caused you to become involved in science in the first place?

MAHLMAN: I think my first influence was my mother, closely followed by my second oldest brother, who was a physics major in college and was a physics teacher in high school for many years. I have four siblings, all male, all college-educated, the oldest with a Bachelor of Science degree, the next two with master's degrees. I was the fourth, with a PhD., and my younger brother has a doctorate in education. And all of these academic degrees were created with no sources of funds from our parents, partly because my parents lived in a poor town, in a relatively poor time, just coming out of the Great Depression. My father was a brilliant but, in retrospect, psychologically damaged person. Later I felt that he was somewhat threatened by the success of his five boys, including myself. I had a steadfast, wise, and stable mother who had a two-year teaching degree from the University of Nebraska. I sometimes describe my childhood as very normal, but in retrospect, it wasn't very normal. I remember at the age of six or seven being asked whether I wanted to have an allowance for my money, or I wanted to go earn it myself. I chose to earn it myself, because that's what my older brothers were doing. I never regretted that, although today that seems almost like child abuse. But it wasn't, because I had a lot of support from a lot of people. I had a normal childhood, but it wasn't necessarily typical in any reasonable way, because I was working those jobs, making spending money for myself. I was a Cub Scout, all the way through the ranks. I was a Boy Scout. I achieved the rank of Eagle at

age 14, and for my town I was the first winner ever of the Boy Scouts' God and Country Award, so I was off to a great start. Between ages -- well, fifth through ninth grade, I was very interested in science, particularly geology and astronomy. I was fascinated by Nebraska's severe weather. (I had the experience of being conked out by a golf ball-sized hailstone, for example, when I was in junior high). I would read books on these subjects. When I was in the fifth grade, they didn't quite know what to do with me, and so my teacher told me to go into the other room and read the encyclopedias, and so I read the entire Compton's Encyclopedia. In the sixth grade I got the nickname of "The Walking Encyclopedia," because I'd read the whole thing and I actually knew answers to many science questions. So I had a very rich and varied youthful life. Sports were already a big part of that, but not nearly as much as they would become later. At age 15 my life changed radically with impending adolescence, in the sense that, thanks to the birth of rock and roll, my good Boy Scout ways had fallen by the wayside a bit. I was extremely athletic in junior high and high school. I played football and had some talent in basketball. In track, I was a school record-holding pole vaulter, and an 880-yard runner. I was on the boxing team, was in the Golden Gloves boxing tournament (not particularly successful there). I played baseball and softball in the summertime, sometimes softball in the adult leagues. I was a wizard at table tennis and I qualified for the Nebraska high school golf tournament as a freshman. I was into bowling as I got into adolescence. I got so I could shoot pool for money, and make money. Also, this was the time of the birth of rock and roll. I remember that my life took a radical shift there with the hormones of adolescence. Songs like "School Days" by Chuck Berry and "What'd I Say" by Ray Charles, "Heartbreak Hotel" by Elvis, "Tutti Frutti" by Little Richard -- my life had been transformed from a straight arrow to the volatile adolescent.

CHERVIN: And did that have any impact on your academic activities?

MAHLMAN: Yes, it did. I tied the all-time record for the least number of assignments taken home in our high school, as in zero. But at the same time, I was reading books on anthropology and astronomy. It was also a time of evolution of what I call my racial and ethnic diversity consciousness. I was a minority on my high school basketball team. The majority were Lakota Indians. These guys were my friends, and I began to realize what a really rotten life circumstance they'd been dealt, and still have. I had similar awareness about African-Americans, obviously a rarity in my town, but I had the privilege of working for an African-American plumber when I was in high school for a part-time job in the summertime after my full-time job, which I'll describe in a minute. There was one Jewish family in town, who I actually knew because I delivered newspapers to them. They didn't stay in my hometown of Crawford, Nebraska, more than a few years. In retrospect, I'm not terribly surprised.

One of my big breaks in life was that at age 15 I got a job as a janitor, working for a PhD. cultural anthropologist from the University of Chicago, who was at that

time the curator of the Fort Robinson Historical Museum just three miles from my hometown of Crawford. I was the janitor. I squeegeed the windows and mowed the grass and did all those kind of things, but I also, as I was cleaning the windows in the museum and so forth, I began to realize the incredible degraded state that the Native Americans had been put to in this country, and that status has not changed radically over the last four decades, five decades, even. And so I was privileged that I was given this job as a janitor, lawn mower, window scraper, and so forth, but then my boss started giving me jobs to help with preparation of the exhibits for the museum. Then I started to be involved in archeological digs within what is now Fort Robinson State Park, and I actually learned the science of field archaeology because of this. At that time, I realized that there was this famous Western author, Mari Sandoz, and so with my boss's encouragement, he had me reading her books. She was the one who wrote the book *Crazy Horse, The Strange Man of the Oglalas*, it was called. And I had read her *Cheyenne Autumn* and a book called *Old Jules*, who was her grandfather. Well, one day Mari Sandoz, this famous author, came to the Fort Robinson Museum. My boss came in my room and it just blew me away when he said, "Would you like to have tea with Mari Sandoz?"

CHERVIN: And you were how old at the time, 15?

MAHLMAN: No, I was about 19 at that time.

CHERVIN: OK.

MAHLMAN: My "advanced age" was because I had been working summers at the Museum for four years or something like that. So I said I'd be delighted to meet Mari Sandoz. I then started asking her questions about her books, like how did Crazy Horse get the name Curly? Was he actually not full-blood Lakota Sioux? And she said, "I don't know, but that was his nickname." I think that I am thus the youngest person on the earth who actually had an intellectual conversation with Mari Sandoz, most of my "competitors," of course, are dead because she died at age 82 decades ago. She was something like 75 when I got to talk with her, my first ever famous person. It was wonderful for my self-confidence, to say the least, that I could talk to this icon and not be totally intimidated. She was extraordinarily gracious and helped me to begin to realize that there is a bigger world out there for me.

CHERVIN: And as I figure it, you were in your second year of college --

MAHLMAN: Yeah.

CHERVIN: -- at that time?

MAHLMAN: Right. Taking physics, chemistry and math courses.

CHERVIN: Yes.

And did you live on campus, or did you live at home and commute the 20 miles or so to the college?

MAHLMAN: Hold it, that's the next part.

CHERVIN: OK, I'm sorry. (laughter)

MAHLMAN: We'll get there. (laughter) At Fort Robinson State Park there was also the so-called Trailside Geological Museum of the University of Nebraska. At that time, if they had offered me a job, I would have bolted from the historical side to the geology side. In other words, there was this physical scientist in me –

CHERVIN: Yes?

MAHLMAN: -- waiting to burst out. Actually, the Geological Museum never gave me an offer, which was fine, but the Historical Museum was a nice experience for me. I was able to study some geology on the side, which allowed me to widen my physical science side, beyond what I could learn in my physics and math classes.

CHERVIN: Was there a particular teacher in high school who had influence on you in terms of physics or earth science or math?

MAHLMAN: Actually, no.

CHERVIN: OK.

MAHLMAN: It's in a poor town, the school system was also poor –

CHERVIN: OK.

MAHLMAN: -- and so the typical thing in my school is that they would hire new people out of college. They would get their feet wet, and then they would be off to a better job. So there were always young teachers. We didn't know it at the time, but my school was breaking in rookies to pay lower salaries. And so I didn't really have a physics teacher, a chemistry teacher, or a math teacher that challenged me to be college-ready. Although I didn't necessarily feel the deprivation, later I realized there was some math and physics I had to catch up with.

CHERVIN: Were there any consequences?

MAHLMAN: Of course, part of it was I wasn't studying too hard, either. (laughter)

CHERVIN: That's because of the rock and roll influence?

MAHLMAN: Sure, I had other priorities.

CHERVIN: Was any –

MAHLMAN: Girls might have had something to do with it, too. (laughter)

CHERVIN: Was any discussion at home in terms of choice of colleges, or was it just assumed that you would go to the local state college?

MAHLMAN: It wasn't necessarily assumed, because, as I say, I was not really studying that hard, because I wasn't taking books home, but I did finish third in my class, which is not too bad a deal, I guess. That didn't raise any big scholarship offers, but there was an opportunity called the Regent's Scholarship at the University of Nebraska that you take a test for. I aced the test, and was offered the scholarship for $300.00 per semester for tuition.

CHERVIN: And what was the total cost of the tuition at the time?

MAHLMAN: $300.00 per semester.

CHERVIN: OK, so it was a full-tuition scholarship.

MAHLMAN: Yes, a full-tuition scholarship. But I realized I couldn't make it on that alone. So I realized that I couldn't handle that economically, because there was room and board to be paid for. Because of that limitation, I decided that I would go to nearby Chadron State College. And even that I couldn't afford. I was just getting even financially because of my summer museum job. My older brother worked his way through Chadron State College as a barber in the dormitory. He also was coaching the Little League baseball guys, so he said to them, "Come to my house. Jerry will give you a free haircut."

CHERVIN: Now did you have any skill or training as a barber?

MAHLMAN: No. None whatsoever.

CHERVIN: I see.

MAHLMAN: But my barber training was in my giving free haircuts to my brother's baseball kids. (laughter) So I opened my dormitory barber shop as a Chadron State freshman and cut hair as an unlicensed barber.

CHERVIN: Yes?

MAHLMAN: For four years in the same dormitory room.

CHERVIN: And when did the free haircuts transition to haircut for pay?

MAHLMAN: When I went to college, because I paid my free "barbering dues" the summer before my freshman college year. Ironically, my daughter is still cutting the hair of her husband and her two kids, so we joke about that. And I haven't had a "haircut" since 1969, by the way.

CHERVIN: A professional haircut?

MAHLMAN: Right. (laughter) Administered by another human being.

CHERVIN: Yes.

MAHLMAN: OK. So, but anyway, that was my lifeline, economically, to be able to have a college education.

CHERVIN: Did you advertise your services, or was it strictly word-of-mouth?

MAHLMAN: I couldn't advertise too much, because—

CHERVIN: Right.

MAHLMAN: —the licensed barbers will figure out who you are. The "self-employment" was a good thing because I had the advantage of knowing everybody through all the strata of college classes. As a freshman I was giving haircuts to the seniors, as well as everybody else, where as a senior, I was giving haircuts to freshmen. I thus probably knew more people on that campus than anybody, I would guess, for that reason.

CHERVIN: Did you have time for any varsity sports during college?

MAHLMAN: I did play on the golf team my sophomore year, but didn't stick with it because one could not then earn a varsity letter. I did some intramural sports. I decided that the world didn't need any six-foot basketball player people working the paint—I'm kind of an "in-the-paint" kind of player—and so I realized that I couldn't play varsity basketball. If I did, I would be playing point guard by the time I was a senior, and I didn't want to sit on the bench for three years. But, in the spring at the end of my sophomore year, I went walking out on the sports fields and there was the pole-vaulting pit and the crossbar, and I said, I think I'd like to try this. So in my socks and my jeans and no shirt, after four jumps, I broke my high school record, which had still stood for four years as the record for my high school, by about a foot. And I did it barefooted. I said, I'm going out for track next year, because I knew what the guys on the varsity were doing. I did earn a track letter there in my junior and senior years.

CHERVIN: Had there been an evolution in the technology of the poles?

MAHLMAN: That's another story. I wasn't going to go there (laughter), but maybe you would like to hear it. The new technology of the poles was a radical, and unavailable, thing for me. When I was a senior, we went to a track meet at Dickinson State in North Dakota. There was this guy from Northern University in South Dakota, and he just killed us in the pole vault competition. He had a fiberglass pole. The rest of us had the old, stiff aluminum poles, and so some of us said, "Could we try that?" And it turns out within about 20 minutes, I'd broken my all-time record by two feet. Two feet! It turned out that people who were really good old-style steel pole-vaulters were "handicapped" because they were extremely powerful in their upper body.

CHERVIN: Right, the upper-body strength is a factor.

MAHLMAN: Yes, and they were very fast. And so the "traditional" vaulter would go through too fast because they adapted well to the style. I wasn't quite strong and quick enough to compete with them, and it turned out that the frequency of the bending in the pole was perfect for my speed. Swinging up to go over the top was like I'd been launched from catapult, and in one jump I broke my personal record by two feet. That's a lot.

CHERVIN: Was it the case—

MAHLMAN: And that was the last track meet of my life.

CHERVIN: Was it the case, in terms of the fiberglass poles, that they were customized to your size and weight and speed?

MAHLMAN: Yeah, but we didn't know anything about that.

CHERVIN: But you just picked up the only fiberglass pole available.

MAHLMAN: The only pole, and all the rest of us have-nots had the old, clunky steel poles. I was in perfect resonance with the new pole, and so technology obviously is a factor in improving all kinds of things. But anyway, that was kind of a fun time of my life. Just recently my wife and I were in North Dakota and happened to be going through Dickinson and she said, "Can I find the field where this track meet was?" and I found it. This was last August.

CHERVIN: I assume they did not have a brass plaque there that—

MAHLMAN: (laughter)

CHERVIN: — made note of your performance?

MAHLMAN: Eminently forgettable, was my performance, that's for sure. So by the time I got to my senior college year, I was very, very visible at the college, apparently

because I was a big frog in a very small pond, so to speak. I was president of the senior class, I was president of Blue Key National Honor Fraternity, and I was the chosen orator for the so-called Ivy Day ceremony for undergraduates.

CHERVIN: What was the population of the school?

MAHLMAN: Well, it's now about 1,200. It was 600, something like that.

CHERVIN: So if the total school were 600, it sounds like about 150 per class year.

MAHLMAN: Yeah.

CHERVIN: OK, that was small.

MAHLMAN: Oh, yes, definitely a small college.

CHERVIN: And was it primarily a commuter school, so it served that part of the state, the northwest part of the state?

MAHLMAN: Well, let's be blunt, because once you go out of town, there's prairie.

CHERVIN: OK.

MAHLMAN: Or hills, so it wasn't conducive to staying there too much. And, of course, in my case, if I went home on weekends, I could get free food from my mom.

CHERVIN: Right.

MAHLMAN: So there was this incentive to do that.

CHERVIN: Right, right. I did that in the Ivy League, by the way, but I took a subway home.

MAHLMAN: So when I got home, I'd give my younger brother a haircut and I'd give my father a haircut, and this kind of helped pay for my room and board at home, so to speak.

It was very interesting, because I had a dear friend who was on the college basketball team. He was a black guy from Chicago, and we just hit it off immediately. And he was, I think, the first black, second or third black person to be a student on that campus.

Unfortunately, he had a white girlfriend.

CHERVIN: And was he, in fact recruited to play basketball at that school?

MAHLMAN: Yes. I met him first as a fellow basketball gym rat, just going over to the gym to play. He thought I was one of the varsity players and surprised when I said no. He said that I should try out for the varsity team. I said, "Look, I know what I can do and what I can't do. I'm not good enough to be on the varsity." First he disagreed with me but finally acknowledged that I was a bit short on the talent side.

But he had this white girlfriend that he'd come and talk to me about her. Later he said that he was getting crap from the administration. That was the second time in my life where I had a racial awakening.

CHERVIN: And --

MAHLMAN: It so happened that I was the Ivy Day orator at the end of the school year. Guess what I spoke about? Racial discrimination.

CHERVIN: And --

MAHLMAN: There were some people who were not very happy with me.

CHERVIN: And this was about 1960 or so?

MAHLMAN: 1962.

CHERVIN: OK, so that was in your senior year.

MAHLMAN: Yes. I was the graduating honored senior, so to speak, so I'd gone from a freshman barber to something significant. But, at that time my black friend was forced out, and in fact, I recently tried to Google him to see if he exists now, because he had long ago gone back to Chicago, a victim of something I could not stop.

I was also aware of racial discrimination when I was on the track team. There was a black friend of mine who was sitting with me on the bus going to the track meet, and we just started talking, and somebody said something derogatory, and I said, "How often do you get that kind of crap?" Somebody had made some crack that he was supposed to be too dumb to hear, that kind of thing. He said, "Jerry, just stop and listen." So I listened to the chatter on the bus and so forth, and I turned around to him and said, "Oh my God."

CHERVIN: And so he was more attuned to it than you were.

MAHLMAN: Absolutely.

CHERVIN: Yes.

MAHLMAN: He taught me how to tune in to "disguised" racial comments. Another black guy who was on the track team, a high jumper, is now a lawyer in Chadron, Nebraska, so he obviously survived this, but it was very, very bizarre. And very recently (2005) I went to a Chadron State University alumni gathering in Denver. The college had invited some of the people from the Denver area. While there, I saw this other man across the room where we were eating. I said to my wife, "I'm going to go talk to that guy. I think I know him." It turned out I was right. He was one of the Hispanic minority who went to Chadron State, an older member of the Huerta family. This was Pascal Huerta, the oldest of the Huertas. He was a senior when I was a freshman, and his younger brother was, guess who? Philip Huerta, haircut customer. And so we started talking about Chadron and so forth, and we got into what it was like. Our families were kind of interleaved in age during those Chadron years.

CHERVIN: Right, right, age-wise.

MAHLMAN: Age-wise, and so we two families knew each other.

CHERVIN: But did you have much social contact?

MAHLMAN: Well, I did, with Philip, the youngest Huerta. He was a haircut customer and he was only two years older than me, while Pascual is four years older than me and he was living off campus, married. We got talking about what it was like being Hispanic there, and he said, "Well, it was interesting going to an all-white school." I said, "What kind of discrimination did you experience?" He said, "Well, to my great surprise, none." He says, "I think it was more politically correct there to be a Mexican than to be an African-American," because there were lots of successful Mexicans in western Nebraska, including one of my first girlfriends. And so he said, "No, I was treated extremely well there." I said, "Well, everybody wasn't," so he said, "I don't need to know the story." (laughter) I said, "No, you don't."

But anyway, that kind of consciousness had burst out from me twice, one from my Lakota friends that I played basketball with all the time and one from the Mexicans I knew.

There were two Lakotas that I knew well. One whose name I've forgotten at the moment, the other was Leonard Littlefinger, a Lakota from Pine Ridge Indian Reservation. I cut his hair. We did dorm rat talk. Also a couple of the black guys I mentioned earlier were in the same dorm. They heard my roomie and me playing 50s blues on 45 rpm records.

CHERVIN: Right.

MAHLMAN: They said to me, "Did we go to the wrong school? (laughter) How'd you guys get to hear about this stuff?" I said, well, we had to figure out where rock and roll came from, and we learned that it was the blues, so we had started to doing that, and he said, "May we sit in?" And I said, "Absolutely." And so some of these guys from Chicago would come into my barbershop and say, "We're starved for real black music," so we would listen to blues together.

CHERVIN: And the barbershop was the historic social center.

MAHLMAN: Yeah, I guess so. But amazingly, being out in the boondocks with minimal ethnic diversity gave me many, many lessons about ethnic and racial diversity.

CHERVIN: Had you had any family travel experience to bigger cities, or—

MAHLMAN: We went to Denver a few times.

CHERVIN: OK.

MAHLMAN: By the way, I should say that I have a sister-in-law who's Filipino Hawaiian, full-blood.

CHERVIN: And where does she live?

MAHLMAN: Well, she now lives in Cleveland.

CHERVIN: OK.

MAHLMAN: Her family came from the Philippines to Hawaii. He father was a supervisor on a pineapple plantation, and at Chadron State, she met my brother when I was 12. I thought that was so cool, because she was really short and I was taller than she was. She fed me sukiyaki when I was 12…in Nebraska.

CHERVIN: And that was your first exposure to Asian cusine?

MAHLMAN: Yes, and of Asians—period. To my parents' great credit, they were extremely accepting of her. And she's still quite a brilliant, loving, and truly idiosyncratic person.

END OF TAPE 1, SIDE 1

Interview of Jerry D. Mahlman

TAPE 1, SIDE 2

CHERVIN: This is Side B of Tape 1, and it's Friday, the 9th of December 2005. We're continuing the interview of Jerry Mahlman, and on the previous side we went through high school and talked a lot about the social and economic aspects of college. I was just wondering at this point to what extent did the college prepare you, or direct you, toward the scientific pursuits that you wound up active in?

MAHLMAN: Well, I think the obvious answer is that I didn't know what my intellectual pursuits were, other than that I was very interested in the physical sciences, whether it was astronomy or geology, or physics itself. Interestingly enough, I did have a scholarship to the University of Nebraska, and that scholarship was based not upon my academic performance, which I finished third in my class, but on a standardized test. I got a scholarship offer from the University of Nebraska, but unfortunately, it paid only $300.00 per semester. That wasn't enough to keep me going, so I actually considered volunteering for the draft, as something that people from small, isolated towns do to make it outside. Instead, I—in sports lingo—"walked on" to Chadron State College near my hometown. I set up my dorm barbershop, cut hair for four years to help pay for my room, board. My summer income and savings was covered by my working at the Fort Robinson Museum of the Nebraska State Historical Society, which was a wonderful experience for me. During the summer, I was not just mowing the grass around the museum, but I was helping prepare new exhibits, changing old exhibits, painting and participating in some of the groundwork for the State Historical Society and for the Nebraska State Game and Parks Commission. It was in those last couple of years where I was in the summertime working on archeological digs, specifically to define the boundaries and the conditions of the legendary Red Cloud Indian Agency, which was one of the reasons that the Fort Robinson Military Reservation actually existed. So this, plus barbering, were my spotty sources of income. In college, it was clear what I "thought" I wanted to do, so I declared a double major in physics and math, and a minor in chemistry, and I stuck with that. To me these were just courses I was taking, because even then I was still reading books on astronomy and astrophysics and not understanding the math very well, and realizing that I had to work harder on my mathematical side to nurture that. The physics side was easier for me.

During my time as an undergraduate, I played on the golf team for just a year, and then the golf team disappeared, because the budget for the golf team went down with the decrease in the state budget for the college. But for my junior and senior years, I was on the college track team, as a pole-vaulter and then, on the side, throwing the javelin. I was a mediocre javelin thrower and a decent pole-vaulter. In my senior year there I was president of the senior class and also president of the Blue Key National Honorary Fraternity. I was also in a couple of honorary fraternities associated with math and with physics and I kept pursuing with my

minor in chemistry. That helped me serve me in my future academic "lives" more than I thought it would. It was positive for me that I hung in there, because I wasn't really enthusiastic about chemistry the way I was about physics.

When I was president of the senior class, and as president of Blue Key National Honorary Fraternity, I earned some speaking engagements.

CHERVIN: Was this your first public speaking experience before a diverse group, in terms of age and background?

MAHLMAN: Well, I actually had taken a very valuable speech course at Chadron. Also in my physics and chemistry courses we were asked to give scientific seminars, so to speak. That was good training for me. I also had a lot of experience as a tour guide when I was working at the Fort Robinson Historical Museum, and so I learned how to interact with people that were far older than I was. And also it was a great experience, because Fort Robinson is a historical area, including its 75-year use by the military. Because of that background, I would give tours to the blacksmith shop, to the saddle and harness shop, and to the wheelwright shop: All these things were part of the infrastructure for the military. When I was born, it was a remount station where horses were brought in to be trained for military use. Later this remount station died a slow death, because it was obviously irrelevant in modern warfare. But I had the experience of meeting a lot of the old soldiers who came back to Fort Robinson. I would take them to the blacksmith shop and they would often say, "I worked in this blacksmith shop as a soldier. You don't have your display right." So I would listen and take notes, because I realized I was smart enough to know what I didn't know. I would go back to my boss, Roger T. Grange, a PhD. anthropologist from the University of Chicago, and say, "We don't have this right. I've got the straight word from the soldier's mouth." It was a wonderful communication experience for me, because a lot of the "tourists" would say, "You gave us a great tour," and they'd try to offer me tips. I would say, "We don't take tips." They would say, "Well, just take it on the side." I would say, "No, we don't take it on the side, either." Leading guided tours was a wonderful experience, because it allowed me to interact with audiences that are clearly non-traditional. They're just whoever shows up that wants to go on the tour. And I was privileged to guide the tour to the site of the Red Cloud Indian Agency because I had helped to literally create the archaeological information about where the Indian Agency was and how it worked. Obviously I got a lot of good education in my summertime jobs.

When I was president of Blue Key, I was asked, as I said above, to give a talk at the baccalaureate service for Chadron State College graduating seniors and for the college as a whole. The topic I chose was racial and ethnic discrimination.

CHERVIN: OK, and this was 1962?

MAHLMAN: 1962, yes.

CHERVIN: So it was very early Civil Rights era.

MAHLMAN: Yes, the really major events happened in '63 and '64.

CHERVIN: Yes, yes.

MAHLMAN: But the reason I chose ethnic and racial discrimination was because of my experience with the treatment of the ethnic minority students and friends that I had, including my Lakota friends, my Hispanic friends and my black friends. It turned out my talk was quite unpopular with the faculty, but very popular with the students. I had no initial idea who would be angry with me and who would be thrilled at what I was saying. It turned out that the students were much more attentive and more attuned to the issue than were the faculty, because the faculty was old school. This was a small part of the start of a revolution that is still in process today.

CHERVIN: What about the parents of the students? Were they on the same side as the faculty?

MAHLMAN: I don't remember any of them afterwards stopping to yell at me.

CHERVIN: OK.

MAHLMAN: I could see people avoiding me, but not yelling at me. So, if it were a problem for them, they did not say. I don't necessarily believe they had the courage to speak up about it. They might have gone away and griped to the dean or whoever.

CHERVIN: Or to their kids.

MAHLMAN: Or to their kids, yes. So I emerged relatively unscathed from the hubris that I expressed by choosing that controversial topic and going with it. It was largely based upon my earlier experiences that I mentioned to you, and I never had any regrets about it. I'm now going to fast-forward a little bit to my subsequent interactions with the college.

CHERVIN: Did you by any chance keep a copy of that speech?

MAHLMAN: I don't know that it exists anywhere. If it does, it most likely is in the hands of Con Marshall, a classmate and for 40 years has been the publicist for the college. He has remained a dear friend over the decades. Maybe I should ask him.

CHERVIN: OK.

MAHLMAN: It might exist.

CHERVIN: But you did prepare it on paper.

MAHLMAN: Yes.

CHERVIN: Did you read it, or --

MAHLMAN: No. (laughter) I thought I was going to read it, but then I just started talking.

So the obvious point is that, by then, I wasn't intimidated at all at the thought of public speaking, even to large audiences.

CHERVIN: And the size of the audience was a few hundred?

MAHLMAN: Probably 500.

CHERVIN: OK.

MAHLMAN: Counting parents, siblings and relatives. This was mainly for the senior class. But, later, well after I'd left the college and gone on to graduate school, I spoke there by invitation a couple of times to the science and mathematics division of the college, and then I also received Chadron State's Distinguished Service Award in 1984. That happened to be the same year, by coincidence, that I became director of GFDL. That was more about real people getting into the real world, rather than their wonderful academic achievements-- I didn't want a sugary sort of tampered-down talk. I began by saying that this talk is for the students. I'm not speaking to the parents of the students, or even to the faculty, I'm speaking to the students. And I've --

CHERVIN: And was that also a speech at graduation?

MAHLMAN: It was at their graduation, a commencement speech. Later, in 2000, I received the second-ever honorary doctorate that they'd ever given out at Chadron State since they started in 1905. I learned later that they're now up to three.

CHERVIN: —the other two were? Number one and number three?

MAHLMAN: Number one was a physicist who had won the Nobel Prize. He was Val Fitch, a Physics Professor at Princeton, who grew up in Gordon, Nebraska. We became "Princeton Friends" when we recognized our common backgrounds, in Nebraska, and at Princeton. Number three was my math teacher there who later became the President of Wichita State University, and President of the University of Northern Arizona. Both of these people were at Chadron State before I was.

CHERVIN: OK.

MAHLMAN: I was very well-treated by the college after I left, to say the least, and particularly so when I came back for the honorary doctorate, which was a real thrill for me, I have to say, because it didn't matter that it was a small college. It's very different than receiving an honorary doctorate at Princeton; I'm still well aware of that. But on the other hand, there were only two of us at the time in a virtually 100-year college history.

CHERVIN: Were there any particular physics or math faculty who influenced you?

MAHLMAN: Two were terrific. My chemistry teachers were mediocre, marginally qualified, I think, in retrospect. A youngish physics teacher who just passed away about three months ago, was interested in me as a person. He did creative things as a physics teacher that never happened for me before and never happened since. He was teaching some of the lower-level physics courses, and he came up to me one day and said, "Jerry, I don't know how you get your answers, but somehow you leap to the conclusion and it's the right conclusion mathematically." He says, "I encourage you never to change, even though I still don't know what you're doing."

CHERVIN: In other words, he didn't ask you to put in all the steps.

MAHLMAN: Right.

CHERVIN: From A to Z.

MAHLMAN: Yeah. I just leaped over those and it didn't seem like anything particularly brilliant, but he said, "You know, I've never seen anybody that I taught that attacked problem solving the way you do." It's something that still keeps me in different venues later. People still ask, "How did you get there?" And sometimes my answer is, "I'm not sure." I also had a physics teacher who had a superb knowledge of atomic physics, and I learned about modern quantum mechanics. The physics that was most important to me later in my career, namely Newtonian physics, was really just about F = MA and moment of inertia, this kind of thing. I really didn't get a big background in what I call classical physics, like the kind we use in atmospheric and ocean sciences. My taking all the physics courses was really important for me, because it helped me learn how Mother Nature works. When I was reading books on astronomy, it all started to make sense because I understood gravitation. I remember being blown away by the relativistic physics, and how'd they get there! How did Einstein get there?

CHERVIN: He obviously went from A to Z.

MAHLMAN: He leaped.

CHERVIN: (laughter)

MAHLMAN: (laughter) Right. So I've had the fascination with what I'll call the physical sciences side, forever. I remember when I was in sixth grade, going way, way back, trying to figure out how far the sun was away from the earth. I was off by an order of magnitude, but I actually was able to —

CHERVIN: One order of magnitude is damn close.

MAHLMAN: Right. And so -- but it just seemed like, hey, you know, there's this thing, sun goes around, there's this and so forth, and I said, you know, it can't be that close, so it's got to be this, or got to be that—obviously I was totally ill-equipped to do a quantitative calculation.

CHERVIN: There is some order in the universe.

MAHLMAN: Yes. I trusted the fact that there was order. I also had a very strong attachment to nature, partly because I went all the way through the Boy Scouts to the rank of Eagle. I learned how to live in the woods, and I learned how to not get lost in the woods, unlike the people in Colorado seem to do every other Tuesday. That was also part of my empirical education, this outdoors part of me. That has lasted all the way from the time I was in fifth grade, to present. Even when I'm out looking for birds on a hike I ask: How did that rock formation get there? Why are these trees here on this side of the hill and they're not on the other side of the hill? It is celebrating the joy and fascination of being a physical scientist.

CHERVIN: Did you ever have any access to—let's call them expensive toys, like telescopes?

MAHLMAN: Yes, I had a neighbor across the street who didn't like kids, but loved his telescope, and he was thrilled that this—

CHERVIN: OK, and he tolerated you.

MAHLMAN: —one kid was interested in --

CHERVIN: —astronomy.

MAHLMAN: —yes. Thanks to him, I was looking not just at the moons of Jupiter, but the moons of Saturn in this humongous telescope (which now would be about a meter or so), right across the street from my house. This from a guy who didn't like kids, but loved his telescope. For those of us who were nerdy enough to want to come over and see what was going on, he was extremely welcoming and explained carefully how the telescope worked. That was seriously cool stuff for me. My three older brothers never had a clue, because they were into other things. My second oldest brother later became a physics teacher in high school, and he taught honors astronomy courses in the Cleveland area. My dedication to him is great because he was (now deceased) the only person on earth who was totally

interested in the details of my scientific research career. He died in 1986, but I still think of him often. In retrospect, it's fascinating how people from the same family can go through very highly divergent futures. Unpredictable ones, as well. This is not a criticism of my brothers, because they're all very intelligent people.

CHERVIN: Right, in their own ways.

MAHLMAN: In their own ways, yes. And I wouldn't try to change them, although usually I have the family advantage. I could talk physics with my late older brother the physics teacher. I can talk bookkeeping and balancing with my brother the accountant, and investment with my brother the business professor, and sports with my brother the football and basketball coach.

CHERVIN: Because of budget experiences.

MAHLMAN: Yes, right. My youngest brother, who has a doctorate in business, and so, I can talk business with him. Only my physics teacher brother could talk science with me. But I had a rich array of smart people near me, including my father and my mother. My "family" education was ongoing early and still is "going on."

CHERVIN: And what point was it clear that you were going on to a further education beyond a bachelor's?

MAHLMAN: It became obvious to me that I wasn't interested in volunteering for the draft. This was before Vietnam.

CHERVIN: OK, 1962 we're talking about.

MAHLMAN: Right. I didn't want to join the military then, but I'd taken a course at Chadron State for preparing teachers, and I thought—

CHERVIN: Would this be at the high school level?

MAHLMAN: Yes. And I thought: the one thing I've learned most from this physics course is that I did not want to teach high school. And if I don't want to teach high school, then I have to pursue my dream about science, because I never had many teachers who could say, "Jerry, go for it" to me. Chadron State was primarily a teaching institution, even though I got a Bachelor of Science degree. So, I had decided that even though I took a course in teaching as kind of a hedge, I wasn't interested in high school teaching, a real concern for me at the time, quite frankly. The experience helped me realize that I don't want to be teaching high school for the next 30 years. I started looking at graduate schools, and quite frankly, I didn't get many bites, but I did get offers from the University of Arkansas in physics, and the University of Wyoming in physics, until they told me I had to have taken a Latin course, at the University of Wyoming!

CHERVIN: And this is 1962.

MAHLMAN: My third offer was from Colorado State, where a new atmospheric sciences department was being formed, and was accepted as a research assistant to Professor Elmar Reiter in something called satellite and space meteorology, which was led by Elmar Reiter. After arriving there, I learned what he was actually thinking about doing was completely orthogonal to anything I was interested in, because what he really was trying to do was get access to some of the new satellite data for analysis. So that's how I got involved in the stratosphere later. Fortunately, I got a nice CSU graduate assistantship to work in atmospheric transport under Elmar Reiter's advice. My wife, Janet, and I got married very soon after I graduated from college, so she went down to Fort Collins with me to CSU. I was pretty unfit for making it at CSU at the start, because most of the professors had just been imported from University of Chicago.

CHERVIN: And so was that the first year of the atmospheric science department?

MAHLMAN: Yes. There was a reason why I was the featured speaker at the fortieth anniversary of the atmospheric sciences department, in 2002.

CHERVIN: Because you were student number one-ish?

MAHLMAN: Well, student number two-ish. Jim Rasmussen was really number one, but the 40th anniversary committee was trying to find the oldest student, if you will, who's had an eminent career. Jim's successful career was very different than mine, but most of his professional life has been in administration of science rather than in the craft of scientific research. So in 2002, I was the featured speaker at the celebration of the Fortieth Anniversary of the Atmospheric Science Department at Colorado State University.

CHERVIN: Did that department actually begin as a Department of Atmospheric Sciences, and are they a department of meteorology?

MAHLMAN: Yes. The Department was born out of the CSU College of Engineering and actually remains there. Sometimes that's neat and sometimes it's stupid, but --

CHERVIN: Well, I've heard the NYU engineering school has closed down.

MAHLMAN: Sure, I can see why. There is a certain kinship there, but most of the school's graduate students were in civil, electrical, or hydraulic engineering, thus not so compatible with the "new field" of atmospheric science.

CHERVIN: Right.

MAHLMAN: I still was very concerned that I wouldn't make it at Colorado State, because there I was short a couple of math courses, and I had to take them during the first summer I was there on my assistantship to bring myself up to speed.

CHERVIN: Did you have a teaching assistantship?

MAHLMAN: No, I was a Graduate Research Assistant.

CHERVIN: Research, OK.

MAHLMAN: Yes. I was a half-time research assistant in the summer, and quarter-time during the rest of the year, most of it covered by soft money. It still is that way, pretty much. But we managed to live on my assistantship.

CHERVIN: And who was the patron at the time?

MAHLMAN: The patron was the famous Herbert Riehl—

CHERVIN: And—

MAHLMAN: Of tropical meteorology fame.

CHERVIN: And the agency?

MAHLMAN: University of Chicago.

CHERVIN: OK.

MAHLMAN: Ferdinand Baer and Bill Gray were also from the University of Chicago.

CHERVIN: What government agency was the patron for the assistantship?

MAHLMAN: Well, that came out of Professor Elmar Reiter's grant, mostly from the Atomic Energy Commission at that time. Anyway, the good news was that I had some good money that would support my assistantship, and that was nice. I didn't have to write for grants and assistantships.

CHERVIN: Did you continue to cut hair there? Or did you—

MAHLMAN: Only mine.

CHERVIN: Or did you put the barbering behind you?

MAHLMAN: Well, it was interesting. When I got to Colorado State, I had been cutting my own hair for some time because of the fact that I was being chased by the licensed barbers in downtown Chadron, Nebraska.

CHERVIN: And they wouldn't cut your hair? Or you didn't trust them to cut your hair?

MAHLMAN: I didn't trust them to cut my hair because I was kind of an unknown factor, but they did figure out my name if I went in for a haircut. And so, I started cutting my own then. There was a time in graduate school where I said, "You know, I'm not going to cut my hair anymore." And so, I stopped for a while, and cut my own flat-top and the whole thing. That carried all the way through until my professional job. I will mention this later, but my first professional job was with at the U.S. Naval Postgraduate School where the price for the haircuts was something like 75 cents because there was a PX on the campus. So I couldn't afford not to go to the barber. (laughter)

CHERVIN: Oh, as faculty you had access to that?

MAHLMAN: Yes. And so, four or five times I went and got my hair cut. This was in 1969. After I had been there for a couple of years I thought, "Oh, I'm not going to go to the barber shop." I didn't like the haircuts, so I said, "I'm going back to doing it myself." And as of now, I have an unbroken streak from 1969 until present. I still cut my own hair. Ironically, my daughter cuts her husband's hair and her kids' hair without any education from me about how to do it. (laughter) So—

CHERVIN: It's in the gene pool?

MAHLMAN: Well, my other brother who was a coach for 44 years cut hair to get through college before I did. I inherited his well-used equipment as a go-to-college gift. So, literally, I haven't been in a barbershop since 1969! Sounds bizarre, I know.

At CSU there were maybe ten to twelve graduate students in our Department. Almost all of them had a better physical science background than I did.

CHERVIN: And at the time that you entered Colorado State, were you expecting to continue on for a PhD or a Masters and then—?

MAHLMAN: Well, I had no clue.

CHERVIN: OK.

MAHLMAN: I was wise enough to say to myself, "Let me just focus on the task at hand." We all know how that can work well. I almost flunked out the first quarter—it was a quarter system—because I got a C in the atmospheric dynamics course where I'd never seen anything remotely like it.

CHERVIN: So it was the mathematical aspects?

MAHLMAN: Yes, for Professor Ferdinand Baer's class, yet he's been a dear friend for the last 30 years. (laughter) But, he has often told the story that he almost flunked the most successful graduate student in the history of the Atmospheric Science Department.

CHERVIN: And he almost changed history.

MAHLMAN: Yes, my history at least.

CHERVIN: Yes.

MAHLMAN: (laughter) Thus, I was focusing on interviewing for jobs, quite frankly. Herbert Riehl of all people, the kind of bad-dude Germanic guy --

CHERVIN: He was the chair of the department?

MAHLMAN: Yes, and he gave me an A. I had to have at least a B average to go beyond the first quarter.

CHERVIN: OK, so the A and the C worked out for you.

MAHLMAN: Yes, I survived. But if Riehl had just let things be, I could have been "on the street!" I think he probably gave me an undeserved A to balance my undeserved C, because Professor Baer simply said, "Here's the dynamics book"; He never quite lectured, nor offered assistance. Anyway, I survived by the skin of my teeth, but it was pretty much clean sailing from there.

END OF TAPE 1, SIDE 2

Interview of Jerry D. Mahlman

TAPE 2, SIDE 1

CHERVIN: It's Friday, December the 9th , and we're "at" Colorado State University, figuratively. And we talked about the various interactions you had with the new department of atmospheric science, and you had planned to do a step-by-step progression through the graduate school experience. And you got the Masters in 1964, and at that point what propelled you on for the PhD?

MAHLMAN: Interesting question. I actually, at the time, had no particular expectations that I was qualified or capable to go on for a PhD. And at the time of my Masters degree, Elmar Reiter was my supervisor, and Herbert Riehl was on my committee. But Riehl was by far the strongest personality. And I recall at that time that Reiter and Riehl had very different research styles, even though they both came out of the Chicago school. My main supervisor was Elmar Reiter. But Riehl was not a passive member of my committee. In fact, during those days there was no such thing as computers to write text on, and at that time all Masters degree theses had to be written and typed cleanly with no corrections. My wife was doing the typing. Every time I'd give it to Professors Reiter and Riehl to review, Riehl would come back with scribbles all over it. We did maybe four or five iterations of my thesis, and meanwhile my wife was ready to kill him on sight. So one day, he had done it to me again. I said, "I'm bringing this in to you, Dr. Riehl, but what I think you're putting me through is a test of character, not intellect." He then broke out in a big grin and said, "You're absolutely right." And I looked him in the eye and said, "Well, thank you. I know where you're coming from." I realized then that he never could touch me again because he knew that I knew what kind of game he was playing with me, the graduate student, and perhaps others as well. Ironically, he became a great supporter of what I was doing under Elmar Reiter. He was infamous as a less than warm-and-fuzzy guy. But, in our relationship for those last couple years on the way to the PhD, he was absolutely terrific and highly supportive of what I was doing in my research. So as a person that wasn't the kind of person that had a large and long friends list, I really appreciated that he was there. I went and talked to him once and I said, "I'm frustrated as to whether I should become an observationalist or a theoretician." And in his very dramatic way he said, "Good. That's a stupid separation." And I said, "Thank you." I thus felt liberated to not be constrained by scientific methodology in my career as an atmospheric scientist. By that time, it was very clear to me that I really enjoyed the field.

Elmar Reiter was a great supervisor for me, and Riehl was a great champion, even though he was always careful to make it obvious that he wasn't working hard on my behalf. He was recognizing that I was growing up enough to empower myself within the process. He was a strong supporter of my PhD thesis, which was quite a long one, on dynamics and transport in the stratosphere. An observational study. So, I owe not just Reiter, but Riehl a great debt of gratitude. Also, I

always mentioned Bill Gray, who is going through difficult times now because of the passing of his wife and his being marginalized because of his strangely unsubstantiated attacks on global warming theory and reality. But, I have to say for the record that he was always wonderful to me. He always said I was his vote for the best graduate student in the history of the Department, but with the qualifier, "Except for the fact that you believe in the theory of global warming." I often said, "Bill, we can agree to disagree about that." These are thus the people who, in a sense, were quite influential in my scientific life.

Many of my CSU graduate student friends have had quite successful careers, especially Jim Rasmussen in NOAA, and Russell Elsberry at the Naval Postgraduate School. Both remain good friends of mine today. I look back at my Colorado State years with a lot of warmth and gratitude, and I realize how incredibly fortunate I was as I was near the time of receiving my PhD. To my utter astonishment, I had 11 job offers. A big part of my receiving these many offers was the long timescale reaction to the 1958 Sputnik phenomenon where the Russians were beating the US in the space race. Thus there was all kinds of money available for people to buy new PhDs. And after a lot of deliberations over many different sites—I particularly remember one, Huntsville, Alabama.

CHERVIN: OK, that was a NASA site?

MAHLMAN: It was a NASA site. But it was a private company. It was a contractor to NASA, but they made me a nice offer, a huge offer. I kept saying to them, "I don't think I can accept that offer." Their recruiting strategy was to keep offering me a progressively bigger salary. I remember the salary offer went from an $11,000 offer to a $23,000 offer, which if you convert to today's currency, was a pretty handsome deal. I turned it down because I realized that I didn't want to be a contractor gunslinger. Already, the relentless pursuit of research contracts and grants at Colorado State was, and is, an unfortunate barrier against producing seminal high quality science. When I talk to some CSU graduate students, I realize that there are things about Colorado State's atmospheric science department, because of this need for soft money, in a less-than-well endowed state institution, there are some problems with their educational system. In fact, the last time I was there was when I was the guest speaker for the 40th anniversary of the CSU atmospheric sciences department. I was there at the dinner that night after I gave my talk, when I was approached by one of the engineering school deans. She asked me, “What do you think about the curriculum?” And I said, "Quite frankly, I found it somewhat disquieting and in some places even disturbing because of the need for soft money." The curriculum is now driven by a faculty member who has a grant to do a particular research project, so he or she teaches that single point of interest. I did not see either a coherent or integrated curriculum at Colorado State University in 2002. That's when this big 40th celebration was underway. I told her this: "I don't mean to be critical to the university that I love, but I don't think the graduate students are getting the best education when each of the faculty members is teaching his or her own research

grant." That made me less than popular, I think. Nobody wrote back and said, "Oh, I see what you're saying here." The obvious point for me is that universities in Colorado are under severe financial stress.

CHERVIN: Is there an undergraduate program at Colorado State in Atmospheric Science?

MAHLMAN: There has never been an undergraduate program in atmospheric science at CSU, militantly so, I might add. The CSU people say that it's a far better scientific education if people have the proper background in physics, math, chemistry, so that these are the conceptual tools that they can bring to their becoming an expert in the subject area, which is vast and eclectic. And it ranges from six-hour weather forecasts to projections of human-caused climate warming out for many centuries. I therefore approve of the idea of not having an undergraduate curriculum, because most of the schools that have them, especially Penn State, are primarily focused on people who are going to go out and do online weather forecasting. It is creating a practitioner rather than a scientist/leader. I don't really know what the graduate students and the recent post-docs would say about the teaching curriculum in the Atmospheric Sciences department, and the quality of their graduate school education. But, I cannot help but present my concern about how important state universities in the state of Colorado are being essentially witlessly dismantled because of the financial starvation that's happening there. And it's not just CSU and CU, but also the Colorado satellite colleges that are affiliated with them, as well as the other state colleges in Colorado. However, as I look back, I see that I was given an absolutely wonderful opportunity at Colorado State. I support them financially for that reason, because I think that they're an important group of people. I also realized that I personally wasn't interested in a job after PhD that was consistent with this culture of go get soft money and then teach your soft-money project to your students. I saw that this approach is not an optimal education. I was fortunate, however, to receive eleven offers—at wildly different salary ranges and circumstances. After receiving my PhD, I accepted a position at the US Naval Postgraduate School in Monterey, California. A year later, my CSU colleague, Russel Elsberry asked me what Monterey was like. I told him all that I knew. He was hired roughly a year after I got there. Interestingly enough, he's still there, and still doing a great job, and likes it there. He's been very influential within the Navy itself, especially so on hurricane and weather forecasting improvements. We had a little CSU ghetto there at the Naval Postgraduate School, at least for awhile.

CHERVIN: Had you done any teaching in graduate school?

MAHLMAN: Yes. I taught an undergraduate CSU course in Introduction to Atmospheric Sciences for upper-division undergraduates. I did that for two years.

CHERVIN: Did you get any constructive feedback from the students? As a first-time instructor, how good you were?

MAHLMAN: I got really good vibes from the students. A number of them wrote and said, "Thanks—I've never had a course like yours." That isn't necessarily a compliment! But I enjoyed doing it, and I enjoyed the students themselves. The lack of the large age differential was not a handicap at CSU, and some of them had a very good sense of humor. It was a good, strong, helpful experience. It gave me ideas of what to teach and what not to teach. So even though I was still under a research fellowship at the time, I was teaching these two courses. It was pure pleasure. A lot of them were engineers, and a lot of people were from the Physics and Chemistry departments, so I was able to teach a pretty quantitatively-oriented course. It wasn't just introduction to meteorology; e.g., "this is a front," you know? That kind of thing. I felt quite privileged to be able to have that kind of experience, because I clearly needed it shortly afterward when I went to Monterey after receiving my PhD.

CHERVIN: As a graduate student, did you have an opportunity to attend any scientific conferences and actually present papers there?

MAHLMAN: That's an interesting question. In 1966, I was asked to go to a conference on the middle atmosphere at Texas Western, now UTEP, University of Texas, El Paso.

CHERVIN: And 1966 was an interesting time --

MAHLMAN: Because of the basketball team.

CHERVIN: Because of the basketball team.

MAHLMAN: Yeah. Texas Western. (laughter) Bobby Joe Hill, the point guard and the "politically incorrect" all-black starting lineup. I actually saw Texas Western play at CSU in the 1966 NCAA tournament, soon to be a national champion against all-white Kentucky.

Anyway, that was the first time where—well, actually, the second time where I actually gave a scientific paper. My advisor, Elmar Reiter, had me present a sponsored Atomic Energy Commission piece on my master's thesis, which was on the transport of mass and radioactivity from the stratosphere to the troposphere. It went well. I was befriended by another person who was equally a scientific rookie, as I was, a fellow by the name of Mike Wallace. We've been friends ever since. So yes, I did have some nice opportunities, and opportunities to give seminars within the Department about my thesis and what I had accomplished. It was a great experience, and I look back at it fondly, and with gratitude. Over the years, it's been nice to see people who attended CSU who will come up to me and say, "I was a student at CSU, and I'd like to introduce myself." I guess that kind

of makes me a semi-luminous old guy, (laughter), whether it's at the CSU table at the annual AMS convention or wherever. I look back at those CSU graduate student years very fondly. I think this interview can go rather quickly into my post-CSU three years, OK? That will put us at the doorstep of GFDL.

CHERVIN: How was the transition from student to faculty? And what was the geographical transition out of the Mountain time zone?

MAHLMAN: I will get onto that, but I want to backtrack a little bit here, because back in the summer of 1963, there was a major summer conference on atmospheric science hosted by the new CSU Atmospheric Sciences Department. There were people there like Chuck Leith and some of the grand old men of meteorology. I had the deep privilege of volunteering for the Grand Old Man of g eophysics, and squiring Sir Sydney Chapman around, and he was as old as I was young. He was an extraordinarily gracious and brilliant man, and I quickly volunteered to be his chauffeur so I could ask him scientific questions. One of the other speakers was Phil Thompson, who I actually understood quite well, because he's a superb lecturer.

CHERVIN: Was his textbook available at that time?

MAHLMAN: I don't think so. I think it was in process, but his lectures were all laid out very well. I had taken a course from him in numerical weather forecasting earlier that spring, and so here was Phil laying out the dynamics and the mathematics of weather forecasting. Brilliant! Just a *tour de force* masterpiece that I understood very well. Another eminent speaker was a fellow by the name of Joseph Smagorinsky.

CHERVIN: In '63?

MAHLMAN: Sixty-three, talking about his '62 famous paper, which totally blew me away. I hardly understood any of it, because it was all about this revolutionary new approach to mathematical modeling of the atmosphere rather than just using the quasi-geostrophic equations of weather forecasting.

CHERVIN: And as I recall the '62 paper, he was the sole author?

MAHLMAN: Right. And of course, that was a classic simulation, just two-level model, but a two-level model using the primitive equations! I realized then that this guy had blown me away, and I didn't understand conceptually what he was doing at all. I didn't understand his notation; I didn't understand anything else. (laughter) I was ill prepared.

CHERVIN: And that was because of the mathematical shortcomings --

MAHLMAN: The mathematical barrier. Yeah, my shortcomings, and perhaps --

CHERVIN: -- of [your undergraduate?] experience?

MAHLMAN: No, his pedagogical shortcomings.

CHERVIN: Yes, yes.

MAHLMAN: It was a wonderful experience, because all of a sudden, these all-stars were coming in to speak to us, and to argue with each other.

Ed Lorenz was there, too, by the way. It was kind of an atmospheric science all-star game, if you will. It was all quite inspirational to me, and a few years later, I was stunned to find out that hey, I now understood what Smagorinksy was talking about in 1963.

CHERVIN: And these eminent people were at the CSU symposium at the same time?

MAHLMAN: Oh, yes, and they were listening to each other's talks with great questions. My volunteer job of taking Sir Sydney Chapman to his daily swim gave me a great opportunity to know him. He couldn't have been a more gracious person. There was also one of the super grand old men, Harold Jeffries, I recall. He was one of the British legendary atmospheric general circulation scientists. He was in his mid-eighties or something like that. Anyway, that was a wonderful experience as well. It gave me a better sense of people out there who were lecturing that weren't necessarily my teachers. They were talking about their science with focus and passion. For me that was an extraordinary privilege. These extra-curricular lessons were quite valuable for my transition to the Naval Postgraduate School. Also, these experiences empowered Janet's and my decision for me to go there, sight unseen. We didn't really know how spectacular Monterey was, because they didn't ask me to go there for a face-to-face. They hired me directly.

CHERVIN: And so you signed up, you agreed to take the position without a "plant visit," so to speak?

MAHLMAN: Right. And ironically, I received my PhD, we left CSU in June, 1967, and drove with our two kids to Monterey; I was there for three days in a place that we had rented and got the news that my father had died. And so three days after I arrived in Monterey, I had to—

CHERVIN: You had to pack up?

MAHLMAN: —say, "I have to take some leave here." I quickly learned that George Haltiner, my department head, is one of the classiest, nice people you'll ever know. He said, "Don't worry about that. Go to your father's funeral." And so we all went back to Crawford, Nebraska, and paid our respects. We then drove back to Monterey and I went to work. I got there in the summertime, a time of a light teaching load. Interestingly enough, because of that, I was able to start research

there, and I began my relationship with T. Krishnamurti, because he was working on using the new approach of diagnostic balance equations for spatial scales with Rossby Number of order one. I began then to see the power of that kind of diagnostic approach to the theoretical calculation of vertical velocity challenge. At that time, I couldn't have done it by numerical modeling, because there was no such capability in their little IBM 1620 computer (laughter); it couldn't do much. But that new research opportunity allowed me to have a research relationship with a scientist of T. Krishnamurti's stature, and that was very positive. The Naval Postgraduate School was on a quarter system, so there was one quarter that's actually in the summertime. So by July or mid-July, I'd actually started teaching a course. I began then to realize that I had major power and a major handicap, because I suddenly now was teaching military officers whose median age was probably 34, 35, 36.

CHERVIN: And you were younger?

MAHLMAN: I was 27, but that wasn't my handicap. My handicap was that I looked like I was 20. I could see these Naval Officers looking me over and saying, "Here's Lieutenant Fuzz. We can kill this guy." Well, they quickly learned after three days that I wasn't that vulnerable. (laughter) And they quickly recognized I was the boss of the classroom and they treated me with great respect, even though I still looked like Lieutenant Fuzz.

CHERVIN: And what was the first course that you taught then?

MAHLMAN: Actually, it was Introduction to Meteorology. But I had old computer exercises available for them by then. I taught that course three years in multiple quarters. I also taught a course once a year on the general circulation of the atmosphere. That was the course I most loved to teach, of course. And, I worked with the Naval Officer students and helped supervise their thesis projects. I supervised probably five or six master's theses; there was no PhD program there at that time. But it was a wonderful experience, because I was teaching people who had lived very impressive experiences. They were really classy guys; Navy guys. It was at the time that Vietnam was intensifying. That was clearly an issue with them, and one with me. I remember being in the lab one day when the so-called "Bucher-Pueblo Incident" came up. The vicious criticisms they had were of Commander Bucher as being incompetent—even though the press treated him more nicely. My officers said, "Bucher screwed up, big time." They clearly didn't have a high tolerance for people who screw up. Let's put it that way. They were a tough crowd, a very tough crowd. But it was very interesting in Monterey, because my tenure was from 1967 to 1970, in which the anti-Vietnam revolution in the U.S. had begun in high intensity in middle California. We had war protesters at the gates almost constantly. And meanwhile, all we straight arrow civilians watched from inside.

For me, it was kind of a privilege to be in Monterey during those revolutionary times, and seeing the stark contrast between the button-down Navy officers and the hippies yelling anti-war slogans. I also remember picking up some of the hippie hitchhikers and helping them get to the Monterey Jazz Festival and other "cool spots." All of this was a very rich experience for me, and of course, Monterey was a spectacularly interesting and beautiful place, independent of the polarized politics "from the right" (Navy) and "from the left" (war protesters) .

CHERVIN: Where did you live at the time?

MAHLMAN: I lived in the town of Monterey in a nice rental house, and then we finally got enough money to purchase our own house. We ended up selling it a year later for reasons soon to be explained. But one of the atmospheric science challenges that started to be really interesting to me was the Manabe, Smagorinksy, and Holloway nine-level model. These were first principle's atmospheric simulations. This foundation is still the essence of all of the subsequent atmospheric general circulation and climate models. Thus, I began to get a sense of: "I think I'm getting the answer to my question as to what kind of an atmospheric scientist I should be?" I was still then working on observational/theoretical problems on non-linear balance equations, as it were, with Krishnamurti, doing some of the computer coding, model and scientific, and reading and diagnostics work. And I said, "My work is really interesting, but now suddenly, I'm reading the Manabe and Smagorinsky and Holloway papers of '65, '67, and then thinking, 'This is really interesting and important science."

Also the Navy's Fleet Numerical Weather Forecasting Center was at Monterey. At the time there was a huge, almost theological fight within the Navy concerning the merits of analog weather forecasting versus numerical forecasting. All of this was interesting because one of my Naval Postgraduate School students had been stationed at Fleet Numerical, a very impressive and intelligent guy named Pete Kessel.

END OF TAPE 2, SIDE 1

Interview of Jerry D. Mahlman

TAPE 2, SIDE 2

CHERVIN: Continuing the interview with Jerry Mahlman. This is side two of tape two. We were in the Monterey years, and you were talking about an interaction with a young scientist named Pete -- I didn't quite catch the last name.

MAHLMAN: Kessel. Pete Kessel was a student, but also a friend of mine, who was working with an atmospheric theoretician named Terry Williams. The two of them started to put together a primitive equation global weather forecasting model, which was, in a very un-Navy-like way, an underground activity, because Captain Wolff, the head of Fleet Numerical Weather Forecasting Center, was vitriolically opposed to any such things as fancy models. He wanted only analog models that had been recently discredited. And so it turned out that Pete and Terry put their model together, and of course, it was not admitted into the Navy's operational suite, but at the weather map board, the officers started making underground forecasts with the new model, and it just clobbered the old model that had been so vigorously marketed. Because of its improved performance, all the forecasters over at Fleet Numerical Weather Central were saying, "Why don't we just make this the operational model?" It essentially produced a big fight within the Navy hierarchy, and the good news is that science won, not bureaucracy. And so the credit goes to Professor Terry Williams and Commander Pete Kessel. To me this was an indicator of the difficulty of working with the Navy. I was a civilian, and I remember multiple times in which they wanted to place me somewhere in the Navy hierarchy. At that time, I was still an untenured assistant professor. I would be in meetings where there would be captains and admirals, and they would look at me, and at my youthful face, and say to themselves with the smirk: "Lieutenant J.G. at best." Often they wouldn't listen to me. And of course, I was just foolish enough to go ahead and keep arguing with them. For example (my interviewer here will smile at this one) they made an impassioned defense of analog modeling rather than digital computing. They would say: "Digital computing has no future. Analog will rule the universe." And in my somewhat unrespectful way, I said, "I have a very strong sense that you're completely dead wrong, and history will prove you so."

CHERVIN: What year was this?

MAHLMAN: 1969. That was a very interesting time, because this was the beginning of the ending of my trajectory within the Monterey years. Then, I had a wonderful experience, in that I had been doing work on the dynamics of transport in the stratosphere, and there was a global conference on the general circulation of the atmosphere in London in 1969. Interestingly enough, I've lost the book from that conference. But when I was in London, I gave a talk on the energetics of the breakdown of the Northern Hemisphere polar vortex in the stratosphere. But, what intrigued me the most about that conference was that Joe Smagorinsky was

there giving a talk, and Sukuro Manabe was there giving a talk on his groundbreaking 1967 paper and his new work. I got to know Manabe then, and Yoshio Kurihara, who has been a dear friend of mine ever since, and Abraham Oort, who also became a lifelong friend. The two of them particularly befriended me because in spite of being still a young person, 29 years old. I guess now I looked like I was 22. I was thus kind of ignored by the upper hierarchy of GFDL, but Oort and Kurihara kept inviting me to come with them "Well, you might as well come out and eat with us guys, because well, you're here all by yourself." It was a wonderful time, because I got to meet all these great people. I met Joe Smagorinsky's son Pete before I actually met Joe, because Joe was talking to everybody, while I was just this young scientist in the crowd. But I was carefully observing the GFDL people. That conference was destined to change my life, and I'll tell you how and why.

At that time, I was still enjoying myself thoroughly in Monterey. It's a spectacularly beautiful place. A still now well-known scientist, Dr. Russel Ellsberry and I were on a faculty team that scrimmaged the varsity in basketball. The Navy had a team in some -- junior college league or something like that. So our limited skills were enough to give the Navy team a tough game. But, I also enjoyed my golfing in Monterey, and being on a base bowling team, sitting with the Navy officers, and things like that. It was clearly a very, very nice time of my life. After I got back from the conference on the global circulation of the atmosphere, I was working on my research project. I had helped to repair Krishnamurty's diagnostic balance omega equation model, because I found out that when you scale that to Rossby number order 1.0, instead of 0.1 like most omega diagnostics equations, the internal physics were mechanistically and dynamically unstable. So Krishnamurty and I had to get together to repair our problem. But, it was a great time for me, because as I said, how do you do these diagnostic calculations right? Going back to that conference in London, Joe Smagorinsky wrote a special piece. At the very end of it, he wrote a great short paragraph called "On the Research Climate for Climate Research."

CHERVIN: You know, I remember that title.

MAHLMAN: Do you?

CHERVIN: I may have seen it in the proceedings.

MAHLMAN: I think so. It was in the conference proceedings, and it was in the book that followed. It was kind of his final serious research statement. He talked about how you needed to put teams together, very unlike the old paradigm of a single PI with graduate students, doing small research. I read that, and was amazed twenty-five years later. I said, "Now I remember why I was so impressed with Joe Smagorinsky!"

CHERVIN: Well, if I do the math correctly, I think I know the position you were in 25 years after the 1969 conference, as the new director of the lab.

MAHLMAN: Right. We will get there. But Monterey was interesting. The third year I was there, I was awarded the teacher of the year. That meant a lot to me because there were lots of good teachers, and most of my students -- well, virtually all of my students, except for the "Lieutenant J.G.'s," were noticeably older than I was. But I felt that I had the respect of the students. They were very tough. They were all officers; they had all paid their dues. And they were the ones I respected the most.

CHERVIN: And their typical age was about 30?

MAHLMAN: Well, I'd say median age was probably 33. If you got a bunch of new Lieutenant J.G's the average age would lower; if they left, the age would go up -- because I had full captains. Or full commanders in my classes, as well as the "Lieutenant Fuzz"es if you will. So anyway, interestingly enough, in the early summer of 1970, I was awarded academic tenure after three years there. I was very pleased with that, and I thought it was as good as it gets. A couple of months after I had received tenure, I got a letter from one Joseph Smagorinsky inviting me to come to GFDL to work on modeling the dynamics and chemistry of the stratosphere. I remember going home that night, and talking to my wife Janet about it. Our kids were both quite young, so that they weren't emotionally tied to a place or region. As we sat down, my wife said, "Well, I don't know what this New Jersey is like." I had actually been there only once in my life. To my surprise, she said, "Well, why not? It's a wonderful opportunity, and you obviously were impressed with these GFDL folks." Within a week or so after that, we made the decision, thus blowing off my tenure virtually before I was ever to reap its benefits. Some people have called me somewhere between over-idealistic or crazy. You know: why give up on a good deal in Monterey for...? But, my appointment at Princeton was only a conditional appointment. But I did have a safety net, in that the dean of the Naval Postgraduate School said, "If you go there, you have a trial appointment. I will offer you the job back. If you wish to come back, we have a place for you." He was extraordinarily generous, but I felt I was undeserving, because I was literally walking out on them.

CHERVIN: Now was this a civilian at the school, or was it a military man?

MAHLMAN: Yes, it was the Admiral who made the offer. The Admiral. Usually, there's only one of them there, and he's the equivalent to the college president.

CHERVIN: The dean? Oh, OK.

MAHLMAN: Not just the dean, because vice-admirals might be deans or something like that. They had a very strong role there. There was also a strong civilian faculty there at the same time. It still is. So, anyway that was an extraordinary gesture on

their part, and I was really grateful for it, because I had no real idea what life would be like once I walked into the real world at GFDL.

CHERVIN: Had you visited the lab before the offer (inaudible)?

MAHLMAN: No.

CHERVIN: So you did not know anything about the physical plant?

MAHLMAN: I didn't even know how to get to New Jersey. In fact, when I first got --

CHERVIN: You take Route 1.

MAHLMAN: I learned that later! Well, when I first was told about this offer, I asked a professor who was a Princeton alum, and I said, "What town in New Jersey is Princeton in?" (laughter) And then he said something like, "I think I hate you." (laughter) "How could you not know what town Princeton University is in?" The answer, of course, is: Princeton. Anyway, I didn't win any points with him for that. But we decided to accept GFDL's offer. We left Monterey, and said our reluctant farewells to wonderful people like George Haltiner, Russ Elsberry, and Maurice Danard, who is still an active weather scientist in the Canadian system. We sold our house, loaded up our two kids, and hopped in our car, and off we went. When we finally got to GFDL, we stayed in a hotel for a while until we closed on our house in Lawrenceville, New Jersey. From our arrival and beyond, we were treated extraordinarily well by Suki Manabe and his wife. They had us over for dinner a number of times, because Suki was my nominal supervisor. Actually, he was my true supervisor. Let's be straight here about that. The Manabes were the ones that recommended Lawrence Township, halfway between Princeton and Trenton, as a great place to live. They couldn't have been nicer to us. I thus got off to a good start at GFDL, but then it was followed by roughly the worst six months of my life. I felt like I was sort of a kid in a candy store, but I clearly felt that I had the potential to be totally blown out there. First it was Princeton University; but second was the now-famous GFDL run by the now-famous Smagorinsky, and the now-famous Manabe. I realized that in my interactions with Joe Smagorinsky, they were difficult and strained, at least from my perspective, because Joe was a very tough, confrontational sort of guy. I felt that after I had been at GFDL for awhile, he seemed to be critical of everybody except Norman Phillips and Jule Charney. To myself I said, "Well, if that's his standard, I think I'm in big trouble." (laughter) I actually began to get acid stomach problems, which I slowly realized were due to what I called "irreconcilable stress of can I make it here?" Even though I had the backup commitment to allow me to return to Monterey, that didn't relieve my personal stress.

CHERVIN: And you were 30 years old at the time.

MAHLMAN: I was now 30 years old. So suddenly, I'm not the fuzzy-cheeked kid anymore. And I thought, "Well, now, I've sort of danced through life without ever hitting brick walls, but suddenly it felt like a psychological brick wall." I felt that I didn't think I was ready for the big time. I actually considered very much going back to Monterey, because I knew that I would've been given a near-hero's welcome if I had returned there. But I soon realized that I first need to find out what's going on with me, and why is it that I'm feeling substantial gastrointestinal stress. I finally realized that I needed to face this problem head on, thanks to my wife, who has very good counseling skills. With her assistance, I declared that, internally, I cannot give a damn about what Smagorinsky thinks of me, because I'm not so sure he even knows that he thinks he's being critical of me. And therefore, I made my commitment to be me, whatever baggage that carries, whatever my strengths and weaknesses are. I thus committed to be "just me" from here on out. Interestingly enough, that simple commitment to validate myself was quickly followed by the cure of my stomach disorders. And so, I then thought, "I gotta be me," and I made the decision to inform the commanding Admiral of the Naval Postgraduate School that I'm not going to return, that I'm burning this bridge. The Admiral was very gracious, to my pleasant surprise.

CHERVIN: And was that at about the six-month mark, or...?

MAHLMAN: Yes, it was the middle of my six months there, in 1970-71. So it was in '71 where I made my declaration of personal accountability, rather than my perception of the GFDL Director's version of my accountability. Obviously, the reader can see later when I became GFDL Director that I had a very different style in how I dealt with people than Joe had. Yet he was extraordinarily successful, and I'm still a big fan of his. In 2005 I was granted the personal honor of writing Joe Smagorinsky's scientific obituary for the **Bulletin of the American Meteorological Society** (published in the June, 2006, issue)**.**

CHERVIN: In those early times, who were the other recent hires at the lab? Either just before you, or immediately after?

MAHLMAN: I helped create the position that enabled an atmospheric chemist, Dr. Hiram Levy II to come to GFDL. He was the first scientist to join my group. I quickly hired Walter Moxim, who was a super programmer. He is still at GFDL, super-programming. At that time, I began to work with some new graduate students, and by the end of the year, I was asked to teach a course in Introduction to the Atmospheric Sciences at Princeton. So anyway, once I had declared that: "I'm OK and I'm still OK," my angst about being at GFDL just vaporized, simply because I realized that I was getting my chance to set my own research group's goals, subject to Manabe and Smagorinsky's supervision. I then told Joe that I'm not going back to Monterey, even though I had seriously considered it, and he said, "I'm glad." With that commitment I was really introduced into the middle of the intellectual center of GFDL. Plus, I still had my early friends like Abraham Oort, Yoshi Kurihara, and Kikuro Miyakoda and many others. And, of course,

Suki Manabe became a treasured friend in many, many different dimensions, especially so, because he started out as my boss, and yet, he was extremely supportive of my independence. I was grateful that he didn't try to manipulate me or boss me. Indeed, he "willed" to me the pioneering challenge of constructing GFDL's three-dimensional modeling of tracer transport and interactive chemistry in the troposphere and the stratosphere. Suki was very supportive to me, and because Suki was very supportive, Joe Smagorinsky was very supportive. So, I got off to a pretty good start on this daunting modeling challenge. Building those complex mathematical models— of course, I don't need to tell Bob Chervin this— was a huge amount of really hard work.

CHERVIN: To what extent did you build a model from scratch as opposed to an extension of an existing model?

MAHLMAN: Well, we started with an extension of an existing GCM. Suki had prepared some model output history tapes, so I could use that as an "off-line" general circulation model that I could use to develop GFDL's atmospheric chemistry and tracer work. The good news was that I could understand what the improved chemical transport model provided very well; the bad news is that first model was a single realization of the atmosphere. Also, the model, of course, had its own pathologies. But we successfully wrote a number of pioneering papers on atmospheric chemistry and transport, not just for the stratosphere, but also the global troposphere and for regional air pollution. Meanwhile, I was still trying to think about how to address this problem correctly, because when you're modeling an atmospheric chemical that has no feedback, especially a passive tracer, the fluid dynamics become extremely challenging. And by the way, in 2006, it is still true today.

CHERVIN: And probably even beyond 2006.

MAHLMAN: Right. The nagging question that inspired me is, how inviscid, or lack thereof, is the real atmosphere, and can we model it? Actually I still have the various derivations and algorithms I constructed when I was deeply involved with building the models. Interestingly enough, in one of my retrospective notes was a derivation for a proposed three-dimensional fourth-order finite difference scheme that reduced a lot of the truncation errors attributable to three dimensional advection by a lot. In 2005, this fourth-order scheme was adopted by GFDL's new climate model. The reason was, of course, that they now were doing highly-parallel computing. The "traditional" spectral transform method soon became a huge liability because of the three-dimensional need for parallel algorithms across many processors, in three dimensions.

CHERVIN: Because these are local computations?

MAHLMAN: Yes, because you're getting a big jump in overall calculation speed out of parallelism, rather than just the speed that you can get off a local chip. I felt quite

honored by that GFDL choice, because once they adopted my scheme, they were able to get rid of the horrible error problems if you utilize, say, the particle and cell scheme, or the quasi semi-Lagrangian scheme. The remaining daunting problem is that there's almost always numerical dissipation that occurs when you are calculating a particle trajectory. So, how do you map that accurately? It turns out that GFDL tried out my variance-conserving 3-D advection scheme that worked out very well, and is now used in the GFDL climate model. Janus Eluszkiewicz and I, who I worked directly with as a colleague and a collaborator, were both sold on that scheme, and we published it in the *Journal of Atmospheric Sciences* in the year 2000. We pointed out that the traditional semi-Lagrangian advection scheme has a frightfully high level of implicit diffusion in three dimensions. If you're actually interested in conserving potential vorticity, potential enstrophy or tracer variances, you need this new kind of approach.

The older methods have an ultimate dissipation of quadratic quantities that's not at all like the real world. This new approach became part of my professional approach on the modeling side to things. I realized that I had by now paid my various dues over on the modeling side. I realized that that was where the pioneering work needed to be done to empower the, up to now, latent third-leg of science. This modeling challenge still needs to be addressed, particularly so by gifted mathematicians more insightful than I was.

I'll say it now, and maybe say it later. It was a total no-brainer that Joseph Smagorinsky should have been elected to the National Academy of Sciences. I personally think it's a huge embarrassment to the National Academy of Sciences that they didn't have the class to recognize the great magnitude of what he had accomplished. Many academicians knew very well that Joe was a prickly guy, and did make some enemies simply because he didn't suffer fools gladly. But without Joe Smagorinsky, there wouldn't have been a GFDL; without Joe Smagorinsky and GFDL, there wouldn't have been me and my current and past scientific trajectory. What Joe also brought to physical science was extremely high standards in research, but more importantly, he also had an extremely high level of scientific patience, because he knew how hard this kind of research is. Bob, you've had your own experiences with this high level of modeling, I know. I realized early on that there's something very special about what Joe accomplished. It's a fact that Joe pioneered the third leg of physical science. Yet the Academy didn't see that singular achievement worthy of election to membership to the Academy! I think that it is a major embarrassment that Joe was excluded. I'm willing to say that to anybody and everywhere, that this is a persistent embarrassment to the Academy, not to Joe Smagorinsky. Is it because he wasn't one of the good old boys that were patting each other on the back? Or was it because he was from the Weather Bureau? Or was it because his dream didn't fit into the university's single PI, and a couple of postdocs or graduate students' paradigm of science? Maybe that adds up to strike three for Joe ever being elected to the Academy, but if anybody ever asked me—which nobody ever

does—I'd say he now should be elected posthumously, because he just died two months ago.

CHERVIN: Well, it could well be that that 1969 publication from the London conference set the framework for the future.

MAHLMAN: I agree.

Joe's phenomenal accomplishment is clearly now of global significance. It wasn't just this little lab in New Jersey, followed by NCAR in Colorado. It was now globalized because the Global Conference was about establishing Joe's vision. At that time people were now saying, "Joe's new approach allows us to attack problems that we couldn't do before. It's revolutionary." Admittingly, at the time, I didn't see it as so blatantly revolutionary as I do now, interestingly enough. Thus, at the same time that I was still fighting a feeling of inferiority around Joe, my personal confidence kept growing and growing. Amazingly, over the years, Joe confessed to me that he found me intimidating, simply because he couldn't intimidate me. Obviously, this reveals an interesting psychological perspective about my own maturing. When I was being considered in 1983 as a candidate to succeed him as GFDL director, I went to Joe's office and asked him a question: "Joe, what do you think my biggest liabilities are if I should accept a decision to have me succeed you?" He was dumbfounded by the question, because I was asking him to expose my weaknesses rather than to extol my strengths. He finally said, "Well, I think maybe you're just too nice a guy." He laughed, and so did I.

CHERVIN: Had he seen you on the basketball court?

MAHLMAN: Actually, he was quite a sports fan and he knew I played a quality game. But you know, on his scale, he was right; maybe I was too nice a guy. But nobody's accused me lately of having a high wimp factor. That was a remarkable statement, coming from him. Later he began to realize that I was starting to do things, that I was accomplishing things he couldn't do because he *wasn't* too nice of a guy, or that he was regarded as a really prickly guy, and thus didn't have many warm and fuzzy buddies. But interestingly enough, over all that time, I'd sort of quietly declared my independence and I was now far from being subservient to what I think Joe Smagorinksy's opinion of me might be. So I worked for Suki in those early years till about 1975, putting all of these research challenges together. In 1975, Suki and Joe said, "Jerry, we'd like you to have your own research group." By that time, I'd started to work with Steve Fels and Dan Schwarzkopf -- I think you know them both -- and with Hiram (Chip) Levy. You'll notice my group is very dominantly Jewish. (laughter)

CHERVIN: Yes.

MAHLMAN: I hadn't thought of that before. (laughter)

CHERVIN: How did that happen?

MAHLMAN: I hadn't thought of that before, but it's literally true. And so...

CHERVIN: And so you were the minority in the group?

MAHLMAN: Yes, I was. But I never was thought about that way, to the best of my knowledge.

CHERVIN: They adopted you?

MAHLMAN: Not officially, of course, but it was fine for me. I made a specific effort to get Steve Fels and Dan Schwartzkopf into my group. So anyway, those are very good things that happened, and at that time, I was adding Russ Sinclair, Walter Moxim, Rick Hemlen, and John Wilson, who later on was using SKYHI to "model Mars." This happened because Suki and Joe had said, "Jerry, we want you to make a new stratospheric model." That was in 1975. I don't know how many years there were before NCAR had one, but that was the birthing of the GFDL "SKYHI" Model. I do remember fighting with both Joe and Suki about the creation of "SKYHI." I said emphatically that I had to have to have at least 40 vertical levels to create a quality model, but they panicked at the thought. I said to them: "Well, adequate vertical resolution is much more important in this problem, so why should we waste most of our computer power on much more expensive horizontal resolution?" Suki actually campaigned to Joe to let me go ahead and do it, even though the computing power was egregiously insufficient to do the kinds of things you need to do to model the stratosphere right. Ironically, in 2005, that's still true.

CHERVIN: And it'll probably be true in 2006. And 2015?

MAHLMAN: Maybe. Well, later on, Kevin Hamilton and I did learn that some important aspects of stratospheric dynamics are strongly influenced by horizontal resolution. But if you want to get the stratospheric semi-annual and quasi-biennial oscillation right, you want to get the gravity wave mean-flow interaction effect right.

CHERVIN: And then you need the vertical resolution?

MAHLMAN: Yes, you need excellent vertical resolution. And Steve and I were able to demonstrate that you need both, but the computational demands to do both are still challenging today. But, thanks to Russ Sinclair, Steve Fels, Kevin Hamilton, and Dan Schwarzkopf, we had a really powerful scientific team there. Steve and I were the very first to ask the question—what does doubled atmospheric concentrations of CO_2 do to the temperature of the stratosphere? We were able to show that, at the stratopause (roughly 50 km.), doubled CO_2 gets you a ten-degrees Celsius cooling—an outrageous number that has stood the test for 30

years! Even today, people ask me to explain why we expect to have such drastic cooling of the stratosphere due to added CO_2 in the troposphere and the stratosphere.

END OF TAPE 2, SIDE 2

Interview of Jerry D. Mahlman

TAPE 3, SIDE 1

CHERVIN: This is the 13th of December, and we've talked about the formation of Jerry's research group at GFDL, and his development of a new stratospheric model. But as I recall, historically, in the early seventies, there was something called the "Climatic Impact Assessment Program (CIAP)," sponsored by the Department of Transportation. Could you comment about your individual involvement in that?

MAHLMAN: Yes. I was deeply involved in CIAP of the Department of Transportation, which was an odd sponsor. But they were interested in the climatic impacts of supersonic aircraft first, so that it wasn't nearly an overarching theme as one might expect by looking at their acronym -- "Climate *Impacts* Assessment Program." Well, in retrospect, it is kind of nice to see that a number of people were actually talking about climate impacts in 1972. There were actually three major CIAP assemblies: volume one, volume two, and volume three. I have lots of contributions that are in those volumes that haven't been entered into the record here. It was a very bold thing to do for a department that was essentially transportation-oriented. I gave a couple of major talks there; I can't remember whether I gave keynote talks all three years, but I was there for all three years. I also remember deeply that when I gave my first talk, it was on modeling of the stratosphere and the environment in which supersonic transport aircraft were to have been put into. There was a person in my audience that got my special attention: a fellow by the name of Norman Phillips!—because this was in Cambridge, Mass. Joe Smagorinsky, a friend of Norm's, asked Norman Phillips how I did (Joe was the one that told me about this later), and Norm said to Joe, "Well, it was a great talk, but he appeared a bit defensive." And I thought, "Well, the famous Norman Phillips says I was a bit defensive; I wonder what he was really thinking about." I never quite found out. But maybe Norman, being the very insightful person that he was, realized that I still was classifying myself as essentially a rookie in 1975. I was clearly a rookie at GFDL then, and this was the very first showing of my stratospheric model. At the same time, there was now modeling going on at MIT; a fellow young post-PhD like me (and now long-time friend named Kevin Trenberth) helped put together the MIT quasi-geostrophic model that was part of his PhD dissertation. That's how I got to know Kevin -- because my model was an off-line tracer transport model that had less sexy vibes than Kevin's model did. But, Kevin's model really wasn't designed to work for the so-called "major CIAP problem." It was interesting to me that Norman Phillips was at least mildly critical, with this somewhat cryptic assessment, that I was a little defensive when the questions came out. I didn't recall that I was. But I do remember asking him later about that, and he said, "Well, you don't seem very defensive these days." (laughter)

CHERVIN: How much time had passed between these two conversations?

MAHLMAN: Oh, probably a year or so.

CHERVIN: Oh, OK.

MAHLMAN: One year later Norman Phillips visited GFDL, and we were just talking. Talking because he was now very interested in the stratosphere. Because he was Kevin's supervisor, because he had built a quasi-geostrophic model of the stratosphere, which wasn't really good for transport, because the global static stability was rigidly fixed. Overall it was an interesting experience working with CIAP, even though they were of dubious credentials because they were from the Department of Transportation. The atmospheric science and atmospheric chemistry communities actually converged and thus took CIAP seriously. If you look at the CIAP volumes stacked up, they're roughly six inches deep. That's a strong testimony to the willingness of the scientific community to break the old paradigm of single PI/single investigator, or single post-doc, or single graduate student. I thus think CIAP deserves a spot in science history, because what it did was to make it socially acceptable, and even powerful, for different groups to work together on a mega-problem. I'd like to say for the record, that right now in 2005, I haven't yet seen that the National Science Foundation is an entire entity that has mastered how to address the major problems. I still question that, because I just recently had the privilege of having reviewed a National Academies' committee that was reviewing the ATM division of NSF. I realized that in the review process where I was reviewing the reviewers, the other reviewers were seeing and saying some of the same things I was: that NSF is still struggling with the need to break out of the single PI paradigm. One can argue with credibility, of course, that NCAR itself has long since broken out of that narrow paradigm. But as a relatively new person at NCAR, I find myself sometimes asking the question, "Did NSF allow NCAR to be far more eclectic? And if so, was the NSF leadership mature enough to guide NCAR in the mission that it needs to do and has long needed to be done? Or is NCAR something that NSF is sort of tolerating as a renegade child that grew up and became irascible?" I don't know. It's a very interesting question for me, because I did see that CIAP broke that narrow paradigm; it didn't follow the NSF model. I would like to talk about this a little bit, because in my years at GFDL, I was involved in three of the assessments that are quadrennially done at NCAR. And each time, I saw some tension between the NSF people, the UCAR people, and the NCAR people. I still haven't seen yet that that problem has been solved.

CHERVIN: Well, is it clear that NSF is capable of supporting big science as opposed to the individual investigator and one graduate student or post-doc mold?

MAHLMAN: I don't really know. But it was very, very obvious to me in some of my experiences as a reviewer that the NSF headquarters staffers were feeling very defensive as they were evaluating the science of NCAR. At first, I thought, "Wait a minute: shouldn't the NCAR people be the uptight ones because they're being evaluated?" I thus saw a greater sense of confidence from the NCAR people than

from the NSF-ATM staffers. I think it's because the NCAR scientists had committed to go beyond what I call "classical NSF science" as a single PI kind of thing, with a single three- to six-year grant for graduate students or postdocs. I thus began to sense a certain cultural stress between NCAR and NSF, and with concern or uncertainty about the role of UCAR and how it brokers that somewhat dysfunctional relationship. It's awkward for me to be saying these things because I'm now a grateful NCAR employee. But nevertheless, I think the question is in, say, 1972 when CIAP was emerging: Does NSF have a coherent plan on how to move over toward bigger science on the way to big science? The most recent relevant report has been just released in late 2005—and I'm listed as a reviewer of it. But, it still doesn't seem to show a high recognition of NSF's needs to evolve into support for greater issues than just small PI, university-type science.

CHERVIN: Well, as I understand, in the past 20 years or so, they've made attempts along those lines in terms of the supercomputing centers --

MAHLMAN: Right. I agree.

CHERVIN: —and the science and technology centers—

MAHLMAN: Clearly.

CHERVIN: —which have a five- to ten-year life—

MAHLMAN: Right.

CHERVIN: —as well as even engineering research centers.

MAHLMAN: Right.

CHERVIN: As I recall, at one time, there were about five of the supercomputing centers, maybe 20 science and technology centers, and perhaps ten of the engineering centers. But I think all of those have had sunsets, and it isn't clear to me that any exist anymore.

MAHLMAN: Well, some of them were highly successful. C4 at San Diego was obviously very successful, thanks to the leadership of Professor V. Ramanathan. There's others that have been, I think, unsuccessful. I remember recently reading and reviewing an NSF center proposal for "NEON," which is a biological/land surface interaction venture. I gave it a very harsh review. The reason I did so is that they were talking about special observation sites for ecosystem studies and so forth, but none of the proposers were saying, "In order to create this special site, we also need to add stations that are monitoring the physical variables, such as temperature, water vapor, rainfall, wind, stream flow, health of marshes relative to precipitation events," and so forth. None of those were in the proposal! Thus I thought, what I'm reading in this proposal is an attempt to use the center's concept

to keep local NSF single PI-intensive research alive. And because of that, I gave it a fairly harsh review. Indeed, the proposal evaded putting it into a national or global framework, e.g., "What would be climate impacts on stressed ecosystems?" Instead it was more or less, "What proposal can we write that keeps this institute going, to make it safe so we can do the "small-think" NSF research?" I find that mindset to be disconcerting. It's almost like NSF has drifted in the direction of a self-serving bureaucracy rather than a mission-oriented, "we've got to solve these major kinds of mega-problems" agency. I've had many friends in NSF who heard my concerns in the past. I remember at GFDL, in about 1978, I was asked to write a proposal to the National Science Foundation on atmosphere-ocean interaction for Princeton University graduate students and postdocs. It turned out that it was rejected. The reason it was rejected is because they had one section of NSF that does atmospheres, and one section that does oceans, and—

CHERVIN: And they didn't talk to each other?

MAHLMAN: They didn't talk to each other. And so therefore, they said: "We can't administrate such a grant, because it's two different administrative divisions." There was the water one, and the air one, and ne'er the twain did meet. And so therefore, GFDL's effort to get more interdisciplinary, more climate-oriented things from its graduate students and post-docs was put on hold for roughly five years mostly because of that institutional mindset. As a person who currently gets a check from NSF, it sounds pretty ungrateful for me to say I don't think they quite have their act together. I do recall an event in the recent history, just before I left GFDL; it was one of those four-year reviews of the NCAR-NSF-UCAR relationship. The panel I was on was split up into various science divisions. It was extremely interesting, because I asked the question, "Who reviews the relationship between NSF headquarters and NCAR?" Surely it's not UCAR, because they have to be evaluated by everybody. A nice person at NSF who I've known for years sort of went ballistic on me and said, "Your job here as an external reviewer is NOT to review the Foundation." I replied, "Well, if the NSF/NCAR relationship is not succeeding or is dysfunctional, then we're going to evaluate it anyway, because sooner or later, we clearly have to have an organizational structure that allows important new things to be covered and initiated." I had witlessly exposed the Achilles heel coming from NSF, simply because their stance was: "We do not need to be reviewed." Our committee of visitors agreed unanimously in our report that we would say, "Yes it does, because it's not clear to us how the relationship filters from NSF headquarters through this mushy world to UCAR." That's perhaps a pejorative comment on my part. From my safe distance, it still looks like a pretty mushy world when I talk to UCAR people and ask them to explain exactly how the NCAR/UCAR relationship works. Some had said: "Well, you know what? I don't know if I know well enough, either." Clearly, if NCAR wants to achieve what it needs to achieve in climate science and climate impacts research, we need to have a rational and logical interactive relationship with our sponsors. I've been accused of saying similar things in my many years at NOAA, so a reader can calibrate my

statement. Because the question still looms: Is it our job to serve the hierarchy, or is the hierarchy's job to empower the people who are creating the products? To me, that's a no-brainer. I'll admit here that, at GFDL, my bias was to flatten the organization. There were people in the NOAA bureaucracy who found that very, very threatening, including some of my own senior scientists, because this was kind of a signature of my to-be discussed future life as director of GFDL; it's no secret that I was pushing an envelope out. It's very, very interesting that my attempt to flatten the organization was obviously fought at NOAA headquarters in Washington. My approach was fought by some of my senior scientists who felt more entitled than I had thought that they should be. I am somewhat guilty because I would go ask questions of their programmers or their junior scientists. Some of the senior scientists would get upset because I didn't go through hierarchal channels to confer with their supervisees. I would say, "If I thought you knew the answer to the question, I'd talk to you first. But quite frankly, I'm benefiting from your supervisee's competence in his/her specialty to help me answer this question." That wasn't always well received by the traditional hierarchichists, if you will. Obviously, it is now a long ways from CIAP, but it still does raise really interesting questions as to how do large organizations collaborate? I'm willing to assert right now that the recent GFDL-NCAR Earth Systems Modeling Framework (ESMF) for cooperation has been wonderfully empowered. The responsibility was turned over to the next generation of scientists through the Earth Systems Modeling Framework (ESMF). And right now, even today, I'm advocating for a system of ESMF-like cooperation, that works on climate impacts, say an Earth Systems Diagnostic Framework (ESDF). For every million lines of model code, you need roughly 10 million lines of diagnostic code. This is actually starting to get some advocates, and I'm currently speaking to people at GFDL, NCAR and elsewhere about this. I'm sorry for this fast-forward to the future, but this is one of my current passions. I'm glad I'm here at NCAR, because lots of people realize now that we need to do just that. We need to go from the NCAR/CGD preparation of the models to having a diagnostic subculture that could actually accept and interpret many petabytes of model data. The current question is, how do we set up an infrastructure that works to accomplish that? I've even gone over and talked to some of the NOAA people who are excellent diagnosticians and posed the question, "How do we put together a climate impacts infrastructure? Is it going to be answered by NCAR's ISSE here, where I now sit? Or is it going to be answered by a collaborative interface network throughout the US, Canada and well beyond?" I may be spending my last NCAR days on this topic simply because we need to flatten the organization enough to allow that to happen. I didn't know I was going to give a sermon. I may be guilty of a Steve Schneider-ish segue here. (laughter)

CHERVIN: Well, I was going to say, for instance, not to be a competitive event, but from the current vantage point, was CIAP a success in terms of your own individual scientific work?

MAHLMAN: Yes.

CHERVIN: And in terms of answering some questions to the community on the impacts of supersonic transport?

MAHLMAN: I think it was a success because it was, at least, mildly product-oriented. In other words, is it safe to fly these SSTs in the stratosphere? Will it fill it full of contaminants and water vapor? Yes, CIAP was a success because what it did do was introduce an empowering paradigm of people working together across the agencies to address problems of common interest. Permit me to fast-forward to what has happened more recently with the Earth Systems Modeling Framework, led by NCAR and GFDL. This now internationally cooperative network has gotten far less attention than CIAP ever did, because they're not seeking publicity; they're seeking infrastructures that allow major problems of the world to be solved. CIAP was definitely important in energizing the process: CIAP made it OK to interact with other scientists, to not implode into one's own organization, to do one's own little project. I do feel proud that I was involved with CIAP. I obviously participated years later on a number of other kinds of activities, such as my role in helping IPCC get started, for example. My colleagues and I spent much time on the International Ozone Assessments, and other mega-challenges, such as the World Climate Research Program.

CHERVIN: Was your participation in CIAP accepted or encouraged by the GFDL and the NOAA establishment, considering that another agency was involved?

MAHLMAN: Well, considering that Joe Smagorinsky was the ruler of GFDL, he was mostly unencumbered by any interventionists from Washington—probably because he intimidated most of them. Joe realized early on that if you're going to take environmental modeling seriously, particularly on the global scale, it takes a commitment of not only an entire organization, but multiple organizations. So you have to give CIAP a little gold star for initiating something globally important. CIAP was followed by the Department of Transportation's High Altitude Pollution Program. I served on its board of advisors for a few years. I even have an appreciation plaque on my wall from them. I was proud to be a part of that because we in the scientific community were giving resources to help them without necessarily sacrificing ourselves in the process. The Department of Transportation people also realized that they were a single-issue group. That people from other agencies made us more capable of focusing in and helping DOT solve some environmental problems through the participant modelers, but also the chemists, who became more and more important as the years passed. And, interestingly enough, in the upcoming assessment report number four of IPCC, which is still discussing trying to quantify the radiative forcing of, not necessarily ozone depletion because of flying in the lower stratosphere, but water vapor and aerosol pollution, and to what degree does that contribute to cooling part of the radiative forcing, plus a group of small processes. So to me, the DOT created a kind of founding fathers status. Probably most of the people in the Department of Transportation don't even think about anymore, but they were a real contributor to the evolutionary history of global-scale environmental science,

because they got the ozone issue in play early on. They didn't get it right the first time. Molina and Rowland told us later, of course, that we got that a bit wrong. So anyway, I think that DOT empowered other groups to start working together, and I think that's a positive point on the evolutionary scale.

CHERVIN: Well, in terms of approximately your first ten years at GFDL, who were the most memorable colleagues that you interacted with?

MAHLMAN: Well, my most memorable colleague is clearly Syukuro (Suki) Manabe. He's very smart and insightful, and delightfully idiosyncratic and delightfully creative. The two of us had a wonderful sort of masochist/masochist relationship in the sense that every paper he wrote, I would review, and every paper I wrote, he would review, because I was now head of a group, so that he was now my equal, so to speak, although he was still the justly famous "Suki" Manabe. (laughter) The interesting relationship is that no matter how hard I would work on a manuscript, he'd give it back to me, and there would be red ink all over the thing. Then I would review one of his manuscripts, and there would be red ink all over the thing. We would then get into deep talk about the other's manuscripts. GFDL had a tradition that preceded me that every manuscript that we wanted to send out for review to a major journal had at least two people in the building read and review it.

During my tenure as the director at GFDL, I "over" reviewed something like 1,200 manuscripts. There was a lot of burning midnight oil sessions there, because I felt that by doing so, it would encourage an interactive relationship between me and the submitter. That worked very, very well, because you would just say: "Well, Suki, I read your paper, and I have some problems here. Let's just sit down and chat for a while." Suki and I would do just that. Or sometimes he'd say, "Well, I'd like to think about your comments before we talk about them." It was a way to me of keeping really, really well-tuned in scientifically into what was going on in the science of GFDL, because essentially every manuscript that was written, I would also review it. It was expensive; a lot of 10 o'clocks to midnights spent reading. But Suki and I had that tradition well before I became director at GFDL. Joe always encouraged that, too, although he didn't necessarily review all the papers. He admitted that he had moved on beyond that stage, and he never said, "Well, Jerry, you should do this now that you have become director of GFDL." But I felt that reading all of GFDL's manuscripts was a way of me keeping up to date scientifically. That definitely worked very well for me, and I think for my colleagues, because a lot of them still call me on the phone and talk to me about a paper they're doing, and sometimes I call them asking for advice, just like I can go up to the Mesa Lab and ask people for advice. And I do. I talk to Jerry Meehl, Kevin Trenberth, or Jim Hurrell, and lots of other people up there. If I don't understand something, and I think they do, I take advantage of my access to be advised by people who know more things about certain subjects than I do. I thus consider that one of my greatest points of satisfaction over the 30

years at GFDL was this culture of interactive review of everything important we do there or did there.

CHERVIN: How did you begin to interact with Steve Fels, who, as I understand, was actually in Manabe's group?

MAHLMAN: Yes, he was, originally, Steve and I; we called ourselves "the unlikely soul brothers." Here is this smart, Jewish, short, unathletic guy from New York City, and me, just almost the opposite in everything other than that. He became my dearest friend. I was there with him often when he was dying of immunoblastic lymphoma. That was at a time after my 1990 heart attack, and both of us were questioning our mortality. Steve, in his usual blunt way, said, "I'll trade you odds." Then I said, "Touché." We worked very, very well together over the years because he was really good at some hard science I was fairly weak at, and he helped me be a lot better on the mathematical side. Indeed, I understand much more about the theory of radiative transfer because Steve was there. I just reveled in his creativity, but also his rigor. He was clearly mathematically more rigorous than I am. It was a joy to work with him. Indeed, we co-taught an atmospheric sciences course at Princeton for three years. His programmer, in all the radiative transfer calculations, was Dan Schwartzkopf, who was another part of the remaining part of the GFDL story for me, and, to my delight, he's still there at GFDL doing his highly respected radiative transfer modeling.

END OF TAPE 3, SIDE 1

Interview of Jerry D. Mahlman

TAPE 3, SIDE 2

CHERVIN: This is tape 3, side 2, the 20th of December, 2005. We're continuing the interview of Jerry Mahlman. You were talking about your "soul brother in science," Steve Fels. Could you comment a bit more on the interactions that you had with him over the years?

MAHLMAN: Well, Steve and I first became acquainted in the mid-1970s. He already had a strong career in physics, but decided that he would like to go into atmospheric sciences. He was very, very strong in the physics of things like radiative transfer, and mathematical descriptions or developments concerning virtually anything. He and I became not just great friends, but also valued research colleagues. We were working together on different aspects of putting together the first real stratospheric/atmospheric circulation model that was viable as a self-consistent piece of mathematical physics and fluid dynamics. Steve had an uncanny ability to capture the essence of problems and express them in mathematical form, and he inspired me to become better at that than I was before I met him. We worked together on a number of projects that were very successful, and our friendship continued to evolve. He slowly became my best friend.

We worked together on many, many different things, but by that time we had gotten involved in the ozone problem, particularly the cause of the "Antarctic ozone hole." We were all puzzled as to whether the cause of this Antarctic ozone hole was chemical or dynamical. We began to see that many people were claiming now that it's chemical, but no one had come up with a valid mechanism to justify the assertion that the cause for the Antarctic ozone hole was purely chemical. So Steve and I both agreed that we need to have an alternative hypothesis: namely, is it dynamical rather than chemical? We put together a so-called "Antarctic vortex fluid spin-up argument" that actually predicts a hole in the ozone layer, so to speak, which really says that, as spring begins, the vortex starts to tighten up, and it begins to produce rising vertical motion in the middle of the polar vortex. No one had talked about this possibility at all. And so, Steve put together a little kind of a "toy" mathematical model that essentially reproduced what we were theoretically projecting or hypothesizing. The interesting thing about that is that our hypothesis actually worked. In fact, I remember going to an atmospheric chemistry planning session in Boulder in 1985-86 about flying airplanes deep into the Southern Hemisphere to study the polar vortex. In that process, I suggested that we also have a dynamical theory that needs to be tested. I remember Bob Watson -- formerly science leader for the IPCC, and the ozone assessments reports, and also the climate assessment reports. But, Bob said, "This is a chemical expedition. We're flying airplanes in to look at the chemistry." And I said, "Bob, if you fly airplanes in to look at the dynamical structure of the vortex, all you'd need is at least one, and possibly two,

conservative tracers such as a carbon dioxide, or chlorofluorocarbon 11 or 12, or nitrous oxide." Those measurement systems actually got onto the plane, using their gas chromatograph measurement. The Fels-Mahlman hypothesis worked! That's the good news, it worked! But it was off quantitatively by a factor of six! Steve, being the hardcore physicist that he was, and me being the straight arrow that I am, we declared this result as a failure. We published the failure of our hypothesis, therefore leaving the question still open. But Steve and I found this to be very, very interesting, because there was a strong ideological trend toward "it's all chemistry" before we even had a chemical mechanism. I think it's due to the intellectual integrity and courage of Steve Fels that we were able to put all that together. When the measurements were actually analyzed by Professor Dennis Hartmann, who found that our results were in the data, but the dynamics alone were too weak to explain the magnitude of the observed phenomenon. We thus publicly declared quantitative failure. There were even some chemists who were giving us a bad time because we had failed. But on the other hand, at that time, there still was no successful quantitative theory for the ozone hole itself that was chemical. So I attribute that wonderful piece of what I call diagnostic, honest science that only Steve Fels probably could have put all his mathematical and fluid dynamical skills together. So, it was a real pleasure to work with him on that project. Even in the mid-seventies, Steve and I were working on what would happen if we doubled carbon dioxide. We found out that the stratosphere is a place of enormous cooling due to the effects of radiative infrared radiation leaving the stratosphere and into "outer space," so to speak.

Steve and I did lots of other things together, but it was in the late eighties in which he began to get sick with immunoblastic lymphoma cancer. That was a good time for me and a bad time for me, but I somehow feel honored that I was the last person that he ever talked to that wasn't from his own family. And his wife Margaret was strongly supportive of me coming to Steve's house to talk with him. And I remember Steve coming down the stairs, saying, "You know, I'm not going to make it. It's like trying to put out a forest fire with a fire hose. It just isn't going to work." I said, "Yeah, Steve. I know that's true." We bid our joint farewells, and that's the last we ever saw of each other. My years with Steve were a joyous and a poignant part of my life, simply because of the fact that we were so close, as soul brothers from different traditions.

CHERVIN: Yes. So we are obviously now well into the eighties, and as I recall, you became the second director of GFDL in 1984.

MAHLMAN: Correct.

CHERVIN: What sort of impact did that ascension have on your own science in terms of the obligations that you had, both within the lab and externally? And what sort of reaction did your former colleagues have with you as the bossman?

MAHLMAN: First, I would like to say, for the record, that I backed into the job. I didn't campaign for it. The tangible evidence that I wasn't campaigning for it is that I had been selected as chair of the search committee for the next director of GFDL.

CHERVIN: Now, this almost sounds like the piece of Cheney being chair of the search committee for the position of vice-president, and somehow, he was chosen.

MAHLMAN: (laughter) Well, I guess one could argue there are similarities there, but it was clear that I was not campaigning for the job. Perhaps Cheney wasn't, either, but it's hard for me to unravel what Cheney is really after anyway, so I'll just pass on that. But it was very, very clear in the GFDL Director search committee -- which is essentially the senior research scientists of GFDL -- we realized that at that time, in 1984, when Joe Smagorinsky had been gone from the position for almost a year and a half, that we needed to do something, but there was a big fear throughout GFDL -- particularly in the senior leadership -- that we might get a "bureaucrat from Washington" to be the next director of GFDL. That was regarded as being somewhere between "absolutely no way" or an "over our dead body" kind of reaction to that. We thus decided that we would select two people who are leading scientists, who are capable of leadership, and are as uncorrelated as possible. I was selected as one of those trial nominees, and the other was Dr. Isadoro Orlanski, who is a fluid dynamicist, mesoscale dynamicist, small-scale atmospheric physics kind of guy. Also he had a completely different attitude toward life and research than I had. He described himself properly as a Jewish-Argentinean-Hispanic-American. And so our anti-correlation with each other was virtually --

CHERVIN: The correlation was almost nil.

MAHLMAN: Virtually perfect. And so we laughed about that. And I lost, and became—

CHERVIN: Which means you got the job.

MAHLMAN: (laughter) Yes, I got the job! I honestly didn't campaign for it. I do remember giving a short talk after I had been selected, saying, "You all know me. You also know that my door has always been open, and it still will be, and that I will still seek counsel from you, and I'll seek support from you. But I won't necessarily have all the answers, and I won't necessarily make decisions that you will endorse or enjoy. But," I said, "my pledge to you is that I will do my very best to make sure that the future of GFDL is incontestably scientifically intense, as it always has been in our culture. I still intend to be part of that intense scientific culture as well, although I quite well recognize that a number of interesting things will change in my life." I said, "For example, I will still lead my research group for as long as I can, but I'll be spending less and less time doing actual research, other than as a mentor to my research group." I also said: "I do recognize that by making this decision, my personal odds for ever being

elected to the National Academy of Sciences has probably dropped by a factor of five or more." Further I said, "I accept that, because my job is to not be your boss, but to empower your research. I also recognize that there's a certain ethnic prejudice against federal employees being elected to the Academy, let alone lab directors and suspected administrators." I said, "You know me well enough that you will not see me as simply an administrator. You will see me as a leader and a person who is going to be seeing you on a daily basis, and my door will still be open most of the time. Some of you will take good advantage of that, and others will find it intimidating. I also will be coming to see you if you're not wandering in to see me about what's on your mind, I'll come in and ask you about what's on yours. I therefore hope to keep it simple. I hope to learn the skills of Washington, D.C. and budgeteering that I confess as my biggest weaknesses. Thus, consider me not as your boss, but as your servant." Then I ended with a tribute to Joe himself, because he had often counseled me beforehand. I asked him what he thought would be my biggest weakness, and Joe quickly said, "I suspect that you might be too nice of a guy for this job."

CHERVIN: I see. That's in comparison to the first director?

MAHLMAN: Yes.

CHERVIN: Himself.

MAHLMAN: (laughter) Right. Steve Fels, I think, shortly afterward says, "Well, Jerry, you don't have to worry about that anymore." (laughter)

CHERVIN: So you overcame your early inadequacy?

MAHLMAN: Definitely. But anyway, I treasure the fact that in those 16 years I was director of GFDL that I found to be a very meaningful time in my life. I also realized that it was an opportunity, not an obligation, that I got the job of being director of GFDL. And so in that sense, it was a very good time. I did count that I published roughly 35 papers since I became director of GFDL, depending on just what a "paper" is. Clearly, that would not have been possible had I not kept my research group, because the programmers were invaluable, and people like Steve and others that have now replaced Steve have also been invaluable. I realize that it was a privilege that I was able to be able to find time to become and stay a research scientist by mentality. I chose for a very good reason to leave the federal government in September of 2000, because I realized when I was first given the responsibility of being director of GFDL, that there's something that I had to kind of work through, and that was how do I maintain a relationship with the wonderful and smart people of GFDL? One of my major priorities was to flatten the organization as much as possible. Even the lowest computer operator knew me because I was often down in the computer room talking to them, and all the way upward through the hierarchy. That came at some cost for some of the senior scientists who were foreign-born: they essentially said to me, "Wait a

minute, this IS an aristocracy. You should validate that, rather than fighting it." In return, I said, "I'm sorry, but I refuse to do that, because if GFDL tries to work with senior scientists only, without programmers and support scientists, and people who can contribute in their own way, then this is not a GFDL that I think is appropriate." I also said at the time that I was given the job that I was aware that if you stick around too long, you will start to wear out your welcome. I said, "I think the natural time scale of a laboratory director of a place like GFDL is an order of one decade. And after that, time for somebody else." At the time I told the lab that I was intending to retire at the end of the fiscal year in 2000, late September, and I had already overstayed my welcome by six years, because by ten years as Director, everybody can see what your weaknesses are, and have long since forgotten what your strengths are. So it was time for me to move on or up. Joe Smagorinsky made a similar choice when he turned 60. I chose to kind of honor that Joe's message that people shouldn't hang on forever, but learn to let go, to learn to not necessarily use a job like this as a source of self-esteem where you can tell yourself that you're still at the top of your game. I realized I'm not necessarily at the top of my game, and in my current time here at NCAR, it's obvious that I'm not. I do look backward at my 16 years as a wonderful opportunity that I had to provide resources and science services to GFDL's staff.

I also would like to say that one of my major goals was to strike down these silly competitive barriers between NCAR and GFDL in climate modeling and climate dynamics, and in a number of other science research challenges. I said, "Over the years, I've learned that most of these people on the other side of the fence at NCAR have become good friends of mine." I'd say, "Why is it that we're fighting instead of uniting with each other?" I've thus been very, very pleased at the very end of my tenure as GFDL Director that the Earth Systems Modeling Framework (ESMF) grew out of some model coding developments that happened both at GFDL and NCAR, and actually invited other places like the Hadley Center, the Goddard Space Flight Center, Goddard Institute for Space Studies and the Canadian Climate Programme into an era of cooperation rather than an era of fierce competitive fighting with one another. I've been very, very pleased with the young scientists at NCAR and at GFDL who have essentially declared that old so-called rivalry to be obsolete. "Under the new cooperative modeling effort, we don't have time for rivalry; we've got work to do, and we have to work together to solve our problems." It's been a pure joy for me to see the GFDL people buying into that ethic at the same time that the NCAR people are as well. And they did something very important with Jim Hurrell's cooperation. He told me what was going on inside as he said, "Well, Jerry, it would be nice if you could be here, but we have this pact that we only invite systems modeling frameworks discussions people who have either directly contributed or have a direct stake in it. I said to Jim that seemed like a brilliant reason to not be inviting me to this, and I thus said, "I strongly support you." The only thing I'll say is now we have to do the same thing in climate impacts research, which is only beginning to be a gleam in some of our eyes.

I consider my whole era as GFDL Director to be a deep privilege, accompanied by lots of 60- and 70-hour weeks. Particularly, I continued to review all the GFDL manuscripts that were going out to the journals, usually from 10 o'clock to midnight. It was a privilege to be able to do that, because if I hadn't done it, I would have lost contact with the very thing that GFDL is all about, namely, excellent science in the service of humankind. And so that I feel very good about all of this, and pleased about it. I can now look back with warm gratitude and a sense of collective accomplishment. At least it was so on my terms, and I'm not saying about how others have evaluated me, but for me, it was very clear that this was a special time in my life with a special set of responsibilities.

CHERVIN: At this point, could you comment on the evolution of the laboratory in the time that you were the director in terms of the number of staff, the type of staff, computer acquisition, and the type and scope of the science that was done?

MAHLMAN: Well, during my era, it was very clear that we had to break down the ghettoes that defined each of the different research groups. We thus began to work much more across groups, because it was very clear that the major societal problems, whether it's ozone depletion or whether it's global warming, needed to be attacked on a larger scale, and not just across groups, but outside the building and outside New Jersey, and outside the United States. It was thus clear that we were in a different era. I was aware that I knew that the politics could always get tough enough that I could get fired. I accepted that as part of the deal because I realized that I had to fight for the integrity, quality, and quantity of our scientific output. I also had to deal with people in Washington, D.C., most of whom were good people; a few of them were political ideologues, well before the current Bush II administration.

I always felt good support from the Washington crowd. There were always fights for budgets because Congress always wants to take as much money away for their districts as they possibly can. For every big budget initiative you prepared, you would get extra money for your lab one out of four times, or so. I did realize that this was part of my job, to make sure that the financial resources were adequate for us. I was fortunate enough to have people in Washington that recognized what GFDL is doing is important, not just for NOAA, but for society itself. I felt fortunate that we were able to keep up with the demands for ever-increasing supercomputer power. During my tenure there, I was responsible for procurement of three different supercomputers. I thus felt very validated in the job.

What was interesting to me most recently is that, when I go back to visit GFDL, as a matter of habit, I always go in the back door. The first people I encounter are the computer systems people or the computer operators. There is this policy at NOAA labs that because of the security problems, thanks to Iraq and lord knows what else, everybody has to wear security badges. The first time I returned to GFDL during this time of tight security, the head of the computer operations group came out and said: "Because you rang the bell at the back door, we're

supposed to give you a security badge, but we, the computer operators, say you are not allowed to wear a badge when you come in here (laughter), because we know that this is all so kind of rinky-dink and irrelevant and stupid. We don't want our former Director to be wandering around with one of those dopey badges." The computer operators came out of the operations room and said, "Yes, we agree unanimously that you militantly do not wear a badge when you come in here as a visitor, because you are not a visitor to us!" That was very, very heartwarming, to say the least.

Even now, when I go back to GFDL, I always walk the halls to greet the people there. People are always wanting to talk science when I walk the halls, still. Mostly, it's about global warming these days, but it's also about ocean dynamics, what the people are doing there, and what's going on in the weather models. They want to share their research with me because they know that GFDL is a place where people are passionate about their science, and they want me to hear about their research—*now*. That was a big surprise. I still can't go 30 feet down the halls without running into somebody else who also wants to talk about what research they're doing. Their interest is now partly related to the suppression of climate warming science that's happening at GFDL right now. I now would like to back up a little bit if we can.

CHERVIN: Yes. Continuing on the evolution of the lab during your tenure as the director, as I recall your personal history, sports or athletic competition are an ongoing aspect to the life that you had. Did that impact any of your hiring practices at the lab? Did you ever try to hire a scientist who was also a power forward, for example?

MAHLMAN: Absolutely not. I have a clean conscience about that. (laughter) For example, my late soul brother Steve Fels was arguably one of the least athletic persons I've ever known in my life. Indeed, he "forgave" me for my athleticism, and I forgave him for his disdain for things athletic. So that's another reason why we were such good friends, because opposites really do attract, I guess. Back in the early days, when I got to GFDL in 1970 and early '71, there was an unusually large increase of new hires, many people who had masters degrees and were super programmers who were also quite good athletes.

CHERVIN: In what sort of sports?

MAHLMAN: Well, we started out in basketball mostly because it was winter when we started this tradition. By 1971, when we started basketball, our point guard was 6'5"; I was 6'1" in the center. (laughter) We played in Princeton intramural leagues a number of years. Sometimes, we would play against last year's freshman players who hadn't made the varsity. Those were sometimes long games because we were often outclassed. Later, we got into the B league, where often we played the off-season football players, who would muscle us around, but we were quite successful. In the summer, we also had a golf ladder match play

format where we would compete on a nearby golf course. Softball was a big thing, a very big thing. We actually played against teams like IBM and RCA, who were monstrously large and way outnumbered us. We of GFDL were roughly 100 people, but we would play IBM that had 2,500 people. But roughly once a year, we would upset these IBM's and RCA's that had 2,000 to 3,000 employees. We thus got a sort of street credibility after a while as this tiny little place that could compete with the local large corporations. For awhile, we played weekly touch football. In fact, that's where Dr. Isaac Held met his wife, playing touch football. Neither one of them are athletic, but they came out and played touch football anyway. When we realized our touch football episodes were averaging significant injury per game, we phased that out of our sports agenda.

END OF TAPE 3, SIDE 2

Interview of Jerry D. Mahlman

TAPE 4, SIDE 1

CHERVIN: It's still the 20th of December, 2005. Continuing the interview of Jerry Mahlman, and we were talking about basketball (laughter), softball, and volleyball as part of the GFDL Olympic training regimen.

MAHLMAN: Yes, thank you. I mentioned briefly that we also had this golf ladder where we'd go out and play competitive golf with each other about once a week. That was a subculture of maybe eight or ten people, but I was one of those people. There was a very active Frisbee game—at lunchtime. I realized that Frisbee was not a game that I was competent in, so I didn't play much, but I sometimes played just for the heck of it. I just noted above about our football game once a week, at which we had a major injury once a week that was dangerous to our bodies. I had been playing basketball, not just only on the GFDL team, but the leader of a Friday night basketball mixed game, just for the fun of playing. I did that for about ten years, just to get kids off the streets on Friday nights. Slowly I began to realize that my body was being punished for playing too much basketball. One of my ankles was basically out of commission for a year. When we played in the Princeton intramural leagues, many times the teams we were playing were mostly Princeton football players trying to keep in shape, or previous freshman who didn't make the varsity, but who would run us to death. We began to run out of talent after a while, and thus phased down. But somewhere in the early 1980's, we began to play volleyball. GFDL's head of the Computer Systems Group, Jim Welsh, was a person who played hardcore volleyball when he was younger, but his knees and hip were essentially gone. To his credit, he got us going on how to play "correct" volleyball, and when we got a few post-docs and graduate students coming to GFDL, we began to get critical mass of talent. I thus began to play volleyball fairly seriously somewhere around 1980. We'd go out once or twice a week at GFDL, and usually played two-on-two "beach" volleyball, although we found out that three-on-three was a better game. At the time I became the new at director of GFDL, the big buzz in the volleyball culture was will the director of GFDL lower himself to coming out and playing lunchtime volleyball? So at the very first game I showed up in my shorts and my funky t-shirt. And I continued to play with them until the time I "retired" from GFDL.

CHERVIN: OK, but no Director hat or...?

MAHLMAN: No. Gosh, no.

CHERVIN: Or armband, or something like --

MAHLMAN: No respect for the Director.

CHERVIN: No respect?

MAHLMAN: I didn't have to even say that. They knew.

CHERVIN: They knew?

MAHLMAN: They knew. Interestingly enough, since I left GFDL, when I returned for a visit, they knew I was coming back, but GFDL volleyball had kind of lost momentum. But when I came back to visit, there would be a new volleyball game. Later GFDL began to get new graduate students so now it's a viable activity again. GFDL volleyball for me was great, because by the time I was 46 and still playing basketball, my body was being assaulted in many, many ways. I learned that volleyball is two orders of magnitude safer than football or basketball. The other thing that's strongly entrenched at GFDL is lunchtime running. I did that for a long, long time. One of our legendary runners was a fellow by the name of Dr. Steven Lyons. Steve was a world-class 800-meter runner, as in running under one minute, 50 seconds. We at GFDL decided one day to have a GFDL one-mile race with Steve Lyons, although he had to run the mile, and we got to run segments.

CHERVIN: So, it was the most likely individual against a GFDL relay team?

MAHLMAN: Exactly! (laughter) Well, I was asked to be clean-up runner.

CHERVIN: The anchor leg?

MAHLMAN: The anchor leg, by the director of GFDL. But we beat him. We said to Steve, "Of course we beat you. You ran by yourself, and we had a relay team, for crying out loud." But Steve was mortified. He said, "I expected to kill you guys."

CHERVIN: Really?

MAHLMAN: Really..

CHERVIN: And you had to do the exchanges of the baton?

MAHLMAN: Yes, we had to do the exchanges. Essentially, there were a couple of us that ran 220s, but then two of us were asked to run a 440, and as Lab Director, I was asked to run a quarter -- the clean-up quarter.

CHERVIN: The anchor?

MAHLMAN: The anchor. The good news was that the other guys had a good enough lead (laughter) that Steve never caught me. But he was so proud of his ability to run that even though the deck had been stacked against him, he was still mortified that we had collectively beat him. And I said to him, "Steve, lighten up!" (laughter) "You could clobber any of us one-on-one at any distance you want to." So, anyway, Steve later left GFDL, and a couple of jobs later, ended up becoming

the hurricane expert at the Weather Channel, including some contemporary aspects about Hurricane Katrina and category five storms being part of global warming, or not, perhaps.. So Steve was an interesting part of the history of GFDL. We've had other people that stayed around GFDL a long time that are still playing various sports.

CHERVIN: Was there ever any consideration of putting in a pole-vaulting pit?

MAHLMAN: Well, now, that's very interesting.

CHERVIN: Why is that?

MAHLMAN: Once, when we were playing basketball over at Jasmine Gymnasium, there was an indoor track available, and one of GFDL's star athletes was a guy named Ted Terpstra. He was a 6'5" basketball player. And there was a pole-vaulting setup that we noticed after we had been playing basketball.

CHERVIN: Oh, indoor pole vaulting, yeah.

MAHLMAN: Yes, indoor pole vaulting! There was a fiberglass pole along with a crossbar and landing pits. And so I told Ted Terpstra, "I'm going to put the bar up here." I put it at something like nine feet. Maybe ten. (laughter) Anyway, for some reason, I put the bar too low. I sailed over the barrier and threw the pole back with such speed that it injured his hand. He said, "I will never, ever catch the pole for you again!" (laughter) So my pole-vaulting career at GFDL was over with one jump! (laughter) One jump. So, the GFDL sports thing remained interesting, because we have other people who are excellent road racers; Bill Stern, for example, who worked with Kikuro Miyakoda for eons. Bill still runs 10Ks routinely during the weekend. So it was interesting that the culture of getting out and having fun, basically, buttressed by some sport competition, was really good for lab morale. Recently, when I was back there visiting, they asked me to defend myself against the infamous collision that happened when we were playing softball—our opponent was RCA. I was playing centerfield, and they were up to bat. They had a guy on third; if they got a single, they win. If they don't, we win. I was ready, and the infielders were. So the guy hits a pop-fly, "Texas-leaguer." I said to myself, "You guys aren't going to get there," but I'm going off in a dead-sprint. I got there, and I got the ball in my glove, followed by a colossal collision that broke Sid Levetus's arm. John King, a computer operator, also had to go to the hospital that night with chest contusions. I had to --

CHERVIN: Did you hold on to the ball, though?

MAHLMAN: No.

CHERVIN: Uh-oh.

MAHLMAN: No. I had it in my hand, but the collision occurred—and I never knew where the ball landed. But anyway, I still get teased about that as the one who provided the momentum source that tried to vaporize (laughter) two of GFDL's valuable employees. And I don't think Sid Levetus ever quite forgave me for it.

CHERVIN: Did he leave the lab a short time after that?

MAHLMAN: (laughter) Well, he left it after that, but I recall it was a year later.

CHERVIN: (laughter) Yes!

MAHLMAN: But I do think it was because he wanted personal scientific autonomy, not necessarily (laughter) athletic revenge on the lab director. So it's kind of interesting how that is kind of tied into the history/folklore of GFDL. And obviously, there was a kind of a step-jump to my time from Smagorinksy's time, who had clearly never done an athletic thing in his life, by his own confession. But he was a good fan of GFDL sports, especially basketball.

CHERVIN: I seem to recall tales that he and his wife organized sports car rallies...

MAHLMAN: That's correct.

CHERVIN: And did they continue after he stepped down, or...?

MAHLMAN: It was discontinued well before Joe stepped down. The rally was a phenomenon of the seventies, mostly. They just kind of died a quiet death. But it was a cool thing, however, because it empowered a different subculture of the lab. Clearly, I wasn't the car rally-type person.

CHERVIN: As I understand the nature of the beast, it was sort of like a treasure hunt going on.

MAHLMAN: Yes. You had a set of cryptic instructions as to where to go. I recall my wife and I getting irreversibly lost somewhere in the wilds of Hunterdon County, New Jersey. The Rally was almost a cult thing. Of course, people called our GFDL athletic endeavors a cult thing, too. I guess you could say they were, in the sense that it was pretty much the same group of people wandering across all these different athletic activities. But the good news is it never interfered with our work, except, of course, Sid's short hospital stay with his broken arm. (laughter)

(break in audio)

CHERVIN: OK. This is still side 1 of tape 4. It is now Tuesday the 27th of December, 2005. We are continuing the interview with Jerry Mahlman. We have talked about the athletic component of day-to-day life at GFDL. But, there was also a very strong academic component with the connection to Princeton

University itself, with respect to the graduate students, the post-docs, and the visiting scientist program. Could you comment about these programs and your role in them?

MAHLMAN: Well, the history of the GFDL/Princeton connection starts with Joe Smagorinsky and his effort to move from Washington D.C. up to Princeton, empowered by strong advocacy on the part of Professor George Mellor, who wanted to have this kind of a program at Princeton University. It was originally called the Geophysical Fluid Dynamics Program. Later, it was called the Atmospheric and Oceanic Sciences Program, wisely so, because a decade or so later, it became very, very obvious that if one were to start over with this laboratory, you probably wouldn't call it the Geophysical Fluid Dynamics Laboratory. Probably it might have been something like "The Atmosphere and Ocean Modeling Laboratory." So we realized that GFDL will stay "GFDL" simply because it's now an acronym of the atmospheric, land, and oceans sciences language. Someday, perhaps it will be renamed into something that is more descriptive of what we actually do. GFDL it started out that way because, with the advent of modeling, we began to realize the potential for direct simulation of non-linear fluid dynamics problems, ocean or atmospheric, or even planetary. The name did stick, and thus became just another word in the language of science. It was not until well after I had started my tenure as director in 1984, when we split off the Geophysical Fluid Dynamics Program and renamed it as the Atmospheric and Oceanic Science Program, AOSP. That was a good move, I thought. That was kind of the background structure of what I lived through from my time arriving in 1970, to leaving in 2000.

I first started teaching one course a year, most of them with Steven Fels -- again, my soul brother who died too early. The two of us would teach one course a year together, and we would not be paid to do it. But it was a wonderful experience for the two of us because we were able to communicate with the undergraduates who were interested in becoming graduate students. A meaningful handful of those folks ended up becoming world-renowned scientists in our field because of their experience at Princeton with the GFDL scientists. One of the things that I was very impressed with when I first arrived in Princeton was the strength of GFDL's visiting scientist program with Princeton University, and also with the graduate student program. We had a number of Princeton graduate students who had their offices at the GFDL building out on the Forestal research campus of Princeton. And interestingly enough, we found that this worked wonderfully for our system. We wanted to have a very specific number of graduate students and a very specific number of post-docs, because we felt if the number were very small that they would be handicapped by having insufficient contact with their peer groups, particularly the other graduate students. But, we also felt that it was good to have as many visiting scientists as we have students, because the obvious implication was that we at GFDL were in the business of making scientists of the future, and so the Visiting Scientist program was our bread and butter to help create them. A significant number of our visiting scientists did get offered

appointments at GFDL that were permanent. From the time I became director of GFDL in 1984 until the time I "retired" in 2000, I was the head of the Visiting Scientists selection committee, which was a -- pardon the expression -- bipartisan coalition of Princeton University people participating in the Atmospheric and Oceanic Sciences Program, later to be expanded into the Princeton Environmental Institute, which is still a going concern. What we wanted for both our graduate student population and our Visiting Scientist Program is, to the best of our ability, create world-class scientists in our field and in our peer groups. That worked very, very well, because neither the post-docs nor the students dominated each other, and everybody attended the same seminars where there's students, visiting scientists, research scientists, support scientists, and even laboratory directors. I was somewhat infamous for probably asking too many questions at seminars, but I'm guilty of that here at NCAR, too, so it's a pathological thing with me. I really am curious about maybe too many things.

CHERVIN: Did you sit in the front row or --

MAHLMAN: No.

CHERVIN: -- in the back row, or...?

MAHLMAN: I sat in different places all the time. (laughter) I usually ate my lunch during the lunchtime seminars. We were able to continue this tradition of the critical, but empowering, conversational dynamic among the invited speakers, the faculty and the GFDL employees. It got to be that the lunchtime seminars were actually rated higher than our invited seminar program. The seminars went on for many decades, actually, and I think they are still going on -- simply because the lunchtime seminars allow such a free interchange of ideas, and the freedom to receive militantly dumb questions from the audience. I always tended to "lead with my chin" as the person who maybe asked the dumber of the questions just to make sure that everybody understands that this is not a hierarchy. GFDL is a meritocracy, from my perspective. And, if the Lab Director doesn't know what he's talking about, it's everyone else's obligation to point out where he's wrong.

CHERVIN: Right. Part of your job description was to ask naïve questions.

MAHLMAN: Well, not exactly part of the job description, but it's also part of the GFDL ethic, and that ethic was empowered also by the tradition of internal reviews of all manuscripts coming out of GFDL. Whether they're from GFDL scientists, the visiting scientists, or their colleagues, or their pre-thesis publications, all of them had at least two internal reviews, and as a matter of practice, I provided a third review. One of the reasons why I was able to maintain my professional activities without becoming obsolete is that I virtually read all of the 1,300 manuscripts in my 16 years at GFDL. And my wife said, "Yes, and it was caused by too many 10 pm to midnights." And I said to her, "But I needed to know what people were doing." The idea of vigorous internal review turned out to be very, very powerful,

because it's a completely different psychology when you're asking a peer to look at this manuscript you've just written, rather than sending a manuscript to a journal and saying, "OK, this is perfect; review it." It was much easier to come back to the scientist and say, ", I'm going to write down a few things about your manuscript, but let's talk first, and try to get the punchlines straightened out, rather than do a formal review."

CHERVIN: Right, right. And the talk was more important than the words in the margin?

MAHLMAN: Yes, absolutely. That was also an advantage to me, because it made sure that I wasn't losing it scientifically when I had to deal with Washington, D.C., bureaucracy quite a bit of the time. I felt a certain privilege that this approach was accepted, because it was very obvious that a lot of times, people don't want people messing with their manuscripts because they're feeling a little edgy about it. The easiest thing to do is sit down, and look the person in the eye and say, "Well, this is where I think you're off-track," or "You haven't communicated it in a way that I can read it." They would sometimes call me "the final filter" before we sent the manuscript off to a journal. But, because of that ethic, our submitted manuscript success rate was high, and the informality of internal review didn't leave people uptight. Well, maybe sometimes, much more often not.

CHERVIN: Was the same kind of review imposed upon a post-doc as well as on the senior scientist?

MAHLMAN: Yes. Indeed, a post-doc can review a senior scientist paper. If they're the one that's more qualified to do so, they indeed did. And so I claim that I have provided internal review for roughly 1,200 manuscripts in my 16 years as Director. And I considered it a privilege, not an obligation. But it certainly helped me be attentive, aware and sensitive to when things aren't going right, or when you've got something that's on the edge of being brilliant, but they didn't hit it out of the ballpark, so to speak. They then could get their act together before they sent the manuscript to a professional journal. And it was not mandatory. It was suggested and requested, but not mandatory. Each person got to choose their own intra-GFDL reviewers -- and of course, there was no such thing as an anonymous review. I think that was extremely powerful, because here I was, Director and lead author of certain publications. I would give my manuscripts to Suki Manabe, and he'd often clobber me. But we had this masochist/masochist relationship, because he'd send me his manuscript, then I would clobber him because his English was contorted enough that I wasn't getting his punchlines. It wasn't a matter of accent so much as a matter of how do you make this work in English?

CHERVIN: What were the approximate populations of the different groups? Of the regular staff, of the graduate students, post-docs, and the visiting scientists?

MAHLMAN: GFDL's post-docs and visiting scientists ranged from ten to 15 in each group, and it was somewhat oscillatory depending on the quality of the applicants, or the specialty of the applicant that we needed to have at GFDL to provide a disciplinary match for each of our Senior Scientists. I just considered a lot of these "visitors" to be a vital part of the culture of GFDL. I personally mentored graduate students and visiting scientists all the way to virtually the time I officially retired from GFDL in September of 2000.

CHERVIN: And could you comment on some of the more memorable people in each of the categories of the students, the visiting scientists, and post-docs?

MAHLMAN: I can comment briefly on that, because I intend, as part of the history of GFDL to write my view of past, present, and future of our postdocs and visiting scientists. I'm mining for those names myself, because I actually have to go back to GFDL, and see if they can pull the records out to get the entire list. I intend to highlight those students and postdocs who became famous. Many of them decided they didn't want to pursue this or that discipline. Frankly, the attrition rate for females, whether graduate students or visiting scientists, was visibly higher, percentage-wise, than for the men. This was a disappointment for me. But, in many cases, it was a graduate student woman or a visiting scientist working with me, like Gabriel Lau's wife, who realized that she wanted to leave atmospheric sciences because she didn't want to be in her husband, Gabriel Lau's way. And there were all kind of social dynamics that led me to say, "You get to choose your life path, but I don't have to tell you what your scientific life should be, because it's quite frankly none of my business. If you want to choose a scientific career, I'll gladly continue to work with you. If you want to go teach school somewhere, that's your decision; I respect it completely." There's not a single pattern here that works very well, although percentage-wise, the number of women students and postdocs were lower, and their percentage dropout rate was higher. I felt kind of deeply concerned about that, until one night, I was talking to my wife about this, because I was missing a young woman who was a postdoc of mine who went to NASA, and we had published a couple of good papers earlier. She decided that she didn't want the competition of the hard research world, but that she wanted to teach. One evening, I was talking to my wife about this. I said, "You know, I consider that I failed with her." Then my wife said, "Is she happy?" My answer: "Certainly." "Do you think she'll do a good job as a teacher." My reply: "Definitely." "Do you think she'll find meaning in it?" My reply: "Obviously." She said, "Well, then, what's your problem?" And I responded with, "Touché."

CHERVIN: Success has several measures?

MAHLMAN: Exactly. I often say to people: If you don't know what you really want to do for your life or your career, it's always a good thing to do to follow your passion or your heart. What do you want to do, independent of all these other extraneous forces, what do you really want to do? My wife helped teach me that I shouldn't necessarily stereotype all people into fitting into a neat scientific niche,

because quite frankly, none of the rest of us at GFDL were in a particularly neat niche, male, female, or whatever. So, that was a good lesson for me. It was a great lesson for me to volunteer to completely be involved with the graduate student program, including choosing of graduate students, and choosing the visiting scientists. It was always a democratic vote, partly because the financial source for the graduate students and postdocs all came from GFDL. I had direct responsibility for the quality of our choices. All the time, I would chair the Visiting Scientists' selection committee, and less frequently, I would chair the graduate students selection committee and the evaluation of the graduate applications, even though that was much closer to a university function rather than to a GFDL function. There was always a question of balance as to how you work that out: how you empower the students—male or female, how the postdocs have fared. Do you want somebody here for a year and then go back to their organization, or do you want somebody that you want to groom to stay here? A number of postdocs have stayed permanently at GFDL.

END OF TAPE 4, SIDE 1

Interview of Jerry D. Mahlman

TAPE 4, SIDE 2

CHERVIN: This is the B side of tape number four, and we're continuing the interview. It's still the 27th of December. How did your life change when you became the Director of the GFDL in terms of having to handle things that may have been a little bit distasteful in terms of budget requests? Overall, to being a spokesman for the lab, both for the general public and for the Washington D.C. community itself, including the people who provided the funding for GFDL, of course?

MAHLMAN: This is a very important ensemble of questions boiled down to two questions. I think the obvious point is that I had some adjusting to do, because I was "labor" turned over into management.

I quickly realized that GFDL did not want a director who was not of high scientific credibility. My becoming the Director produced some obvious perturbations in my life. The good news: I still had my research group. My research group members and I agreed -- that I would do my best to stay as head of the group, but I'd be spending less hard science time. In retrospect, that worked extraordinarily well for me. I published something like 36 papers thanks to the great assistance of my research group. I admit: some of them were sole-author papers by myself, but typically, I was empowered by my support staff, including some PhDs such as, for a while, Steve Fels, Kevin Hamilton, Chip Levy and Rick Hemler, Walter Moxim, and Russ Sinclair were all invaluable members of my research team. I realize how that if I hadn't had that marvelous support, my scientific skills would have degraded far more. I was the one in the group who was preparing mathematical derivations needed to set up various parts of my GFDL "SKYHI" model structure, parameterizations of physical processes, and what would lead to improvement of SKYHI. I was thus empowered to follow my "research heart," because these talented people still wanted me to be the head of our group. I haven't mentioned John Wilson, who ultimately took my SKYHI troposphere/stratosphere/mesosphere model and converted it into a Martian model. The last time I asked about it, it turned out that that SKYHI converted into a Martian model has recently been Earth's best model for simulating the atmosphere of Mars. I've had some fun about that with John, actually thinking about how first principles can give one a real understanding of how the Mars atmosphere actually works. These insights are still relevant with the Mars landers taking surface observations, but also the satellites orbiting Mars, giving us further information.

I did have to spend most of my time directing the lab, and there were two parts to that. The first was how I dealt with the individuals of the lab: populated by wonderful, patient, irascible, idiosyncratic, marching-to-their-own-drummer kinds of scientists and the terrific people who worked for them. That was the easy and fun part of my job, simply because these are people I know and respect greatly. I

was still able to bring in visiting scientists to work with me, people like Allan Plumb, Dave Andrews, and Flossie Shu, who were visiting scientists who came to work directly with me, even though I was GFDL's Director. That worked out quite nicely, I thought; certainly for me, and according to their testimony, well for them, too, so that they felt that they had not gotten a bad deal for being saddled with the Director.

One of the bigger challenges for me was managing GFDL's budget. Every year, the budget was seemingly in deficit in some way or another; or a budget crisis. We often had to go to Washington to make the case as to why we needed to either maintain our budget in harsh years, or to accelerate the budget in the (rarer) good years. The acceleration of the budget requests was related to the number of people we had, the attrition rate, the new hire rate, and what kind of new talent we needed to hire to bring to GFDL? I was very actively involved in that, like it or not. I found myself going to Washington, D.C. roughly monthly for one reason or another. Part of that was just dealing with OAR (Oceans and Atmospheric Research), the headquarters for the NOAA research labs. I thus had to learn how to deal with people who were totally boggled by GFDL. A lot of people in Washington found GFDL very intimidating, partly because Joe Smagorinsky had been very intimidating, and he didn't suffer fools gladly, in the old cliché. But I found that they were also intimidated by me. Even though they found me a nicer person, they were still intimidated by me because they knew that GFDL had this relentless pursuit of quality in terms of how it relates to producing products for NOAA and for the world.

I have to say that, on balance, my times in Washington were really quite positive. There were always times there in which I had tussles with various Washington, D.C., people, simply because they had agendas of various kinds. People often complained about why GFDL seemed to get more resources per capita than other labs do. My answer was, what we do necessitates that in terms of scientific quality, and also in terms of needed supercomputing capability. There were people in other labs that would say, "GFDL gets more resources than everybody else." But in terms of percentage increases, that wasn't even true. There was a bit of a budget fight all the time, but there were many great people in Washington who were big fans of GFDL, simply because they respected our integrity. And in fact, there was an entity called "NOAA CORPS," which is a quasi-military coastal guarding part of NOAA. The NOAA CORPS people just loved GFDL, which stunned me. I slowly learned that it was because GFDL had a clear sense of mission; we had quality and focus on the mission, and we had talented people, and they were highly impressed by how talented and dedicated our people were. They weren't threatened by GFDL. They were just elated to find out that these guys from GFDL actually have their act together. Some of those NOAA CORPS people around who will actually still come to GFDL if they are having a year-end review, or they just come because they're GFDL fans. So, of the directors of OAR, I think there were three during my tenure, maybe four. Most of them I found very easy to work with. One was completely irascible and threatened by

the intellectual dominance of GFDL, and he had his own personal hang-ups. But, overall, I don't really consider that a bad mix at all.

CHERVIN: Now was that the position that Rick Rosen is in at the present time?

MAHLMAN: Rick, the head of OAR.

CHERVIN: Yes.

MAHLMAN: Yes. And in my opinion, Rick was a perfect choice for that job, but he just got muscled out of it.

CHERVIN: OK, is that a sign that he *was* the perfect choice?

MAHLMAN: Well, yes. He was the perfect choice because he was a threat to the current Bush administration. You don't need to erase the tape.

CHERVIN: OK.

MAHLMAN: It shortly became obvious that he walked into a very difficult circumstance. He was close to the perfect choice for that job. He knows the science, he knows the people, he's a great and caring human being, and he's fair to the max. And he's getting jerked around by the Bush administration. I think it's disgracefully appalling, and yet, he's such a good guy, he doesn't even complain. I recently asked him point-blank, "Are you complaining publicly?" Rick said, "Well, I don't do that." So it's ironic to me that much of the political dysfunctionality that is happening inside NOAA, is now arguably the most contaminated organization from the top-down of any of the major agencies of the government. I can't speak for the Secret Service, the CIA and other "secret" agencies.

CHERVIN: Is that possibly as a consequence because NOAA is in the Department of Commerce?

MAHLMAN: I don't know if the current system has enough intelligence to recognize that NOAA is mostly a weather and climate services organization, within the semi-comatose Department of Commerce.

CHERVIN: Oh, OK. (chuckles)

MAHLMAN: I think it's because NOAA is too threatening to this Administration because it can supply crucial non-political information on the science of global warming. That is unpalatable to the Administration's ideologues. NOAA is now politically incorrect relative to the ideological hostility against global warming that I think now contaminates affected branches of the U.S. government. In fact, right now I'm working with some people outside the system to take on those in the

U.S. government who are censoring climate science research as I speak. We have all kinds of evidence of coercion of scientific testimony, and clamping down people's right to speak or to submit scientific papers that are ideologically inconsistent with the administration. So that's fast-forwarding to today, but a lot of the seeds for that were laid earlier. We'll now talk about other administrations that I've dealt with.

CHERVIN: When were you first asked to do a congressional testimony? Was that before you became Director of GFDL?

MAHLMAN: Well before. I'm sitting here looking at it, as we speak. This was at a Senate Hearing on March 1st, 1976, on ozone depletion. It was just two years after the famous Rowland-Molina paper on the role of chlorofluorocarbons, where their breakdown by ultraviolet light is producing a self-reinforcing chemical reaction that destroys ozone effectively. In 1976, I had gotten my new GFDL SKYHI model to a place where we actually were simulating ozone on a first principles basis, but it was still pretty primitive. I was asked, along with Chip Levy and Steve Fels, to come to a hearing on the Subcommittee on the Upper Atmosphere of the Committee on Aeronautic and Space Sciences. Joe Smagorinsky, Chip Levy, Steve Fels, and I all went to Washington to provide testimony. We proposed a piece we wrote on the global ozone depletion problem and prepared the testimony, which was stated not by the three of us scientists, but by Joe Smagorinsky. That was kind of interesting, that Joe was asked to present the oral testimony because he represented GFDL. I think he did a pretty good job of that. Interestingly enough, this particular hearing was before the discovery of the Antarctic ozone hole, almost a decade later. I still have this congressional record of questions and answers from all the members of that Aerospace and Space Sciences Senate committee. It was kind of interesting, because we were raw rookies who didn't know how to present Congressional testimony. My written testimony for Joe to present to the Committee was a bit wordy and a bit pedantic. Later, I learned to make such testimonies stark and really simple.

But, it turned out that we at GFDL were relatively minor players in the ozone controversy, because we were still building our GFDL SKYHI model that could go from the ground to the mesopause to get the chemistry right. It turned out that getting the chemistry right was much harder than we expected. Many of the reactions we needed to know hadn't yet been quantified in the laboratory. But anyway, that was my first encounter with Senators, and you could see that at the time of the hearing in 1976, the ozone hole hadn't been heard of yet, but it was thought that there was just going to be some degradation to the ozone in the stratosphere. It turned out that our presentation was a precursor to what was the real bomb later. That was good experience for us, and good experience with dealing with the fact that the Senators are asking questions -- not just hearing our testimony, but asking questions about how all this works. In a sense, that hearing was more important for me than it was for these Congressmen, because it gave me a perspective of, "OK! We are now science in the public arena, and because it's

science in the public arena, we have to get our acts together." Oddly the next one was 11 years later.

CHERVIN: I see. At which time, you were the Director of GFDL?

MAHLMAN: I was the Director. But, in looking back, I said that I was a guerilla in the ozone wars, but not necessarily a general or anything close to it, because we were still working on how do you make a model that simulates the stratosphere correctly, rather than working through what possible chemical reactions could explain the ozone hole.

CHERVIN: And so what was the occasion in about 1987 or so that you spoke in Congress, or actually to Congress?

MAHLMAN: It was before that, because of the National Academy of Sciences' "Charney Report" in 1979, global warming became a big deal. And a number of hearings were held with Syukuro Manabe speaking. I was asked to have Manabe come down and testify on what he knew on global warming, and he did. Unfortunately, my dear friend Suki Manabe was a disaster at congressional hearings, only partially because of his accent -- his Japanese accent. But it was more because he was far more verbal than any of the Senators who were asking him questions. Unfortunately, he would go on and on and on, making policy recommendations, and commenting upon all kinds of things. And somewhere in, I guess about in 1987 -- here it is, yes -- July 1987, one of the staffers of the Committee on Natural Resources called me and said, "Please do not send Dr. Manabe down to testify, because people have no idea what he's talking about. Plus he wanders away off-subject, and is offering policy recommendations, which we are definitely not asking him to do. Next time, would you come down?" So, if Suki had been a wonderful climate communicator, rather than a wonderful climate scientist, my career as the testifier at congressional hearings may never have happened, because if he'd been really skillful at it, they wouldn't have asked me to come in his place to testify about global warming!

CHERVIN: Well, on the other hand, maybe he was quite skillful and he didn't want to ever be asked again.

MAHLMAN: Yes! OK! His militant incompetence!

CHERVIN: Exactly. Dynamic incompetence.

MAHLMAN: (laughter) He was beyond dynamic incompetence. So anyway, global warming became a fairly public topic in the mid-eighties, even though it really began after the National Academy of Sciences Charney report in 1979 (based on pioneering GFDL and GISS models!). But, it was clearly evolving through the early to mid-eighties. You are not aware that in 1976, Steve Fels, Dave Schwarzkopf and I published a paper on what would a doubling of CO_2 do to the

temperature of the stratosphere? We found a ten-degree cooling at the stratopause, at 50 km. And that created a big buzz, actually. Ten degrees Celsius anywhere is a big deal.

CHERVIN: And especially ten degrees C as opposed to F.

MAHLMAN: Right. (laughter) Exactly.

CHERVIN: They're bigger degrees.

MAHLMAN: Sixteen F. Global warming had become a real thing, because other Academy committees were now being formed and so forth, and you've got the SMEC Report and the SKEP Report, and lots of other international reports. And then, in the late eighties, came the Jim Hansen-mid-U. S. drought announcement that he attributed to global warming. I disagreed with Jim on this and it turned out I was right. In July '87, I testified to the Subcommittee on Natural Resources of the House of Representatives. Here's one in February 1989. Committee on Appropriations, Foreign Operations focusing on global warming. Here's another one in '89 on the Committee on Science, Technology, and Space. This was a great one because of George Brown, a terrific and scientifically-trained congressman from California. Another one in '89 on Committee on Commerce, science and technology. And then here's one...

CHERVIN: Did you ever get any guidance from headquarters on what you should or shouldn't say?

MAHLMAN: The first "guidance" I ever received was from a staffer for the Administrator of NOAA, and that staffer worked very hard to get me to alter my testimony. That was in 1989. I said to him, "You do not tell me what to do for my testimony. Congress asked me to testify, not NOAA to testify." I said, "If you do not give me permission to proceed with this, I will submit my testimony as Jerry Mahlman, private citizen, from Lawrenceville, New Jersey."

CHERVIN: A citizen?

MAHLMAN: Yes. And they caved completely. It was kind of interesting, that I got away with sort of flaunting that. That hearing was my first coerced testimony ever. And to his great credit, John Knauss, the Administrator of NOAA, said, "Jerry, nobody tells you what to do. I have read your testimony, and I am proud to make your testimony be NOAA's testimony." So there were people who were the ideologues, because this was through the conservative Reagan years. But I would call it sort of a benign ideological tweaking of the system. It was much more recently that I saw more wicked stuff happening. So that was my experience with coerced testimony. It was my gambit to say, "Look, if you coerce my testimony, I will just testify as a private citizen." They always kind of ran for cover. Here's another case that was coerced testimony, in 1992. This was when

Senator Al Gore was inviting Jim Hansen, Steve Schneider, and me to testify. That was very interesting. Because it was Al Gore, I again said that I would testify as Jerry Mahlman from Lawrenceville, New Jersey. This time, I still didn't get any backing from the Reagan administration, but I got plenty of backing from Senator Al Gore. At the same time, there was a different form of testimony that Jim Hansen prepared over in NASA that was also coerced. In my case, I just fixed what they coerced, and gave my original testimony. Jim just said, "Well, my testimony was a bit flawed, so I'm not giving it." That was huge in the papers the next day. I think Jim and I were both in newspaper pictures that day. I remember sitting right next to Bob Schieffer before I went up to the witness stand. But Senator Al Gore was very much on the case. He wanted to know what was happening, and why is coercion of climate science going on. Interestingly, I just saw him at an Aspen conference in September, 2005, and we talked about that 1992 event. He brought it up, in fact. And so it was pretty terrific how really supportive he was of what Jim and I were doing at that time, as a matter of principal saying, "This is coerced testimony."

In retrospect, those look like small things compared to the modification and censorship of testimony that goes on now, and that it has to be politically approved. So, in terms of this kind of climate censorship, and in congressional hearings, we are now in what I would call the real Government Censorship Dark Ages. How that will play out, I don't know, but it's clear that the management of information, especially scientific information, from this administration is unprecedented in my experience of 33 years as an employee of the United States government. Why that has not become a major scandal is almost a major scandal in itself. During the George H. W. Bush years, some of this was going on, but it seemed to be at a local, more harmless ideological level that wasn't coming down from the Executive Office of the President. This time, it's very different. Still I don't know how all of this is going to work out. I really don't. I do have to say that I feel privileged to be a part of this process over the years, because it is the way that we in the scientific community actually communicate to the government of the United States. And the last coerced testimony that I've ever done was in 1993, again on global warming. I did one in '92 on the UN framework on climate change that was mildly tampered with. One in '93 on climate modeling was coereced, but was fairly benign. I think that was the one where Richard Lindzen and I were together on it. I continued providing testimony on through various years, in 1995, 1996, 1999. Most recently, I testified at one by Senators John McCain and Joe Lieberman, just a year and a half ago. So in that sense, it's kind of a nice chronology. The one I find the most interesting, is a set of questions from Representative George Brown from the Committee on Science in the House, where he was asking me to refute the testimony of one Patrick Michaels.

CHERVIN: And when was that?

MAHLMAN: This was 1996. And he has since died. But, Representative Brown was a major supporter, empowerer, of "science in the service of humankind." When he

saw this kind of coerced bogus science, he was tough, very very tough. The kind of questions he asked were honestly inspiring.

END OF TAPE 4, SIDE 2

INTERVIEW OF JERRY D. MAHLMAN

Tape 5, Side 1

CHERVIN: It's Tuesday, the 3rd of January, 2006. We're continuing the interview of Jerry Mahlman. This is Tape Five, side 1. After your official retirement from the Geophysical Fluid Dynamics Lab at Princeton on October 1, the year 2000, you remained associated with the university for a while. What were your primary interests and activities at that time?

MAHLMAN: I'd like to begin by starting earlier than that. It was December 1999 when I announced my intention to retire from GFDL, from NOAA, and from the federal government. It was a definite breaking of a deep and lasting tie. At that time, I had been Director of GFDL for 16 years. I said before that the natural timescale of a lab director's tenure is roughly a decade. By the time you hit that level, people take what you do well for granted, and ignore what you've accomplished otherwise. I realized it was now time for me to move on. Joe Smagorinsky was GFDL Director for 28 years, and I realized that this was not a goal that I intended to challenge, because I think that the evolution of science, and the evolution of labs, was happening on a much faster timescale than that.

I announced in December 1999 my intention to retire from GFDL My actual retirement date and retirement symposium were in September 2000. That was an amazing event for me, because there were something like 500 people came. And I'm not sure where all of them came from or how I happened to know all of them (laughter), but it was a wonderful experience. The people setting the symposium up were my hard core scientific colleagues, and they wanted to have a hard core science symposium. But they did ask me what I wanted, and I said I also would like to have a separate symposium titled, "Beyond the Science of Climate Change." That bothered them a bit, but we finally decided on a compromise. That compromise worked very, very well, because the first day of the "Mahlman Symposium," so to speak, was on my research on atmospheric dynamics and chemistry, plus "New Directions in Stratospheric Science." The speakers featured were all people who are world-famous in this sub-discipline: Kevin Hamilton, Allan Plumb, Michael MacIntyre, Jim Holton, Jim Anderson, and Susan Solomon. The session that was wildly out of character for GFDL was the second day. It was titled "Beyond the Science of Climate Change," and I had a different group of featured speakers. One was Suki Manabe, who was talking about the future of global warming, and how it might play out, and Dan Albritton from the NOAA Aeronomy Lab, a long-term friend of mine, a pioneer in the stratospheric ozone assessments with Robert Watson. He and Bob Watson were also pioneers in proposing to form the Intergovernmental Panel on Climate Change (IPCC). Dan talked about the broad implications of internationalizing ozone and climate assessments. Rosina Bierbaum, a good friend of mine who was the Office of Science and Technology Policy person at the White House, working directly with Al Gore. Tom Schilling of the University of Maryland, who just got a Nobel

Prize in economics for something that was more mathematical than economical on game theory space. He specifically talked on how difficult it is to think about the future. Economists are always concerned about the equivalent discount rate, and when one pushes far out in the future, it becomes an irrelevant tool after a while. Rob Sokolow from Princeton talked about the transition of the greenhouse-constrained economy of the future. Kai Lee, a dear friend of mine, talked about environmental challenges over the next ten years or so. He also talked about me and how I had transitioned from a hard, pure, climate and atmospheric modeler to someone who's actually gotten deeply involved in the National Academy of Sciences Board on Sustainable Development. So my softer side was being openly discussed and challenged here. Dale Jameson talked about the mega-ethics of global warming, and who pays. This is kind of interesting, because I'm still involved in research on that very subject. In fact, this very morning, I was writing on it to a person who wanted it to be characterized in a way that real people can understand better. So, my retirement symposium got me off to a really fast start after my retirement. The symposium was an incredible experience, an incredible high. I remember most fondly that my boss, David Evans, who was the head of ocean/atmospheric research at OAR was extremely frustrated because we were supposed to start the session, and I was not there, and the third speaker was not there either. He was a Professor by the name of Michael MacIntyre from Cambridge University who wanted to talk to me about hard-core geophysical fluid dynamics. We were over in a corner talking (laughter), and everybody says, "Where's Jerry Mahlman? Where's Michael MacIntyre?" Some GFDL people came out and found the two of us going at it full-blast, and thus ignoring my own symposium.

CHERVIN: In the same place, at the same time?

MAHLMAN: Indeed. So I got sort of street credibility (laughter) because of this at some level, even though the two of us we were just out in the hall talking. My retirement symposium was a wonderful experience, and vastly exceeded anything I'd ever even dreamed about people actually doing this kind of thing. The GFDL senior scientists gave me the honor of sitting close to the front so I could ask pointed and sometimes irrelevant questions of the speakers, as I usually do at seminars no matter where I am. That symposium was a really good send-off. Three weeks later, I officially retired from the United States government. Days later, I was invited by Princeton University to move in to Sayre Hall, which is just across the street from the GFDL building. I was working with Jorge Sarmiento and Rob Sokolow on the carbon economy problem, the $C0_2$ in the atmosphere problem, and what are the major scientific challenges. The two of them had a big grant from one of the oil companies that actually realized that global warming was a serious issue, and they had to get into it. So I was immediately quite busy, and I was also at lunchtime still playing the volleyball games, and, on the days there wasn't a volleyball game, I was running with the runners. But I realized it was a deadly thing to sustain because after the volleyball game, or when we were cooling down from our run, people would start griping about the new

administration or the new GFDL boss. By the way, the interim Director of GFDL was Gabriel Lau for about three-and-a-half months before my successor actually came in.

CHERVIN: I assume that convinced you that he didn't ever want that position on a full-time basis?

MAHLMAN: I think he was convinced before he started, and it only certified (laughter) what he was considering before. But Gabriel did a very good job in the transition. But when people would start complaining about what was happening in Washington, or what was happening with the new director, Ants Leetmaa. People would ask me: Why isn't he like you? I always said to them, "Well, that's one of the good things, that he's not like me." But I soon realized that it was time for me to think about moving on. By that time, it was something like April 2001. I went home one night after work at Princeton, and there was a piece of paper in the door of my house. I took it out of the door handle, and it said, "I am desperate for a house. I would like to buy your house." He left a phone number. So I went into the house, talked to my wife Janet, and said, "What do you think of this?" She said, "Well, this is great, because we're thinking about moving west to Colorado anyway." We "sold" our house to the person who dropped the note in our door on the same night, and finalized the no-realtor transaction in less than ten days.

CHERVIN: Because of family issues, or...?

MAHLMAN: Yes. It was very, very clear. When I was in Princeton, our nearest relatives were my daughter's family, from Chicago. When I moved to Colorado, my furthest relative was my daughter, from Chicago! (laughter)

CHERVIN: They're one and the same daughter?

MAHLMAN: They're one and the same, yes. We realized that we wanted to change our center of gravity, and the fact that we were both from the West helped us decide to move. Soon afterward, we moved out to Colorado, made a decision to live in Longmont for two reasons. One is that the house prices were half as expensive as Boulder's, and the other one was that my wife's mother was in Longmont. So it was an obvious decision. Approximately three days after I was finalizing the process of closing on our house in Longmont, I got a call from Tim Killeen, the Director of NCAR. Tim said, "Jerry, can you come over to my office? I'd love to talk to you." I said OK. It was mutually arranged that I see him at his office. Immediately I had this remarkable interview with Tim Killeen, who I knew at a sort of running into him at meetings kind of level, but never as a personal colleague or friend. To my astonishment, he was amazing. And I did not expect this welcome. He said, "Jerry, I know that you know much more about the science that NCAR actually does than I do. I know you know more than I do about climate than I do. I know you know more than I do about atmospheric chemistry. I know you know more than I do about mathematical modeling. And

I know you know more about mesoscale dynamics and observations." In response, I said, "That's true, but you know a lot about the upper atmosphere and the sun that I don't know." Tim then said, "I'd like you to be my senior science advisor." I was totally unprepared for that, so I gathered myself a bit, and I finally said, "Tim, I think this is not a good idea." He said, "Why is that?" And I said, "For reasons that I've been working for a decade to try to break down, there's been a sort of odd and ill-defined rivalry between the climate modeling people at NCAR and the climate modeling people at GFDL. I've tried to point out the irrelevance of that, because we're all now evolving more into a time of inter-laboratory mutual interaction and cooperation. But I said, "I would come bearing baggage if I came in as an advisor to the Climate and Global Dynamics division, for example, and less so in an Atmospheric Chemistry division." And so they ended up putting my office upstairs at the Atmospheric Chemistry division, but my main job was in the Advanced Study Program, ASP. It turned out that didn't work very well, because I only wanted a part-time appointment, and if I'm going to advise post-docs or graduate students, or visiting scientists, I need to be present more than just whenever I show up. So, I was given an appointment in the ASP -- Advanced Study Program -- an unusual appointment, because I didn't want fulltime. I requested a quarter-time appointment, and that if I were to exceed that once I got beyond the quarter-time appointment level, they would not have the funds to pay me. So that was the deal. So, in a sense, I was given the perfect job. And, like all other things that seem to be perfect, they're not necessarily perfect. I enjoyed thoroughly my affiliation with the Advanced Study Program.

A year later, I ended out making a request to join down at the Foothills Lab the Environmental and Societal Impacts Group, ESIG [now ISSE, the Institute for the Study of Society and the Environment]. I did that because of my affiliation with a number of National Research Council Academy committees. And, as part of that process, I realized that when you're talking about global warming, the hostility by the Bush administration against the science of global warming where he says, "Well, we're going to adapt; we're going to do this and everything else," I realized that this was patently false. At our National Research Council Committee to evaluate Bush's climate change science plan (CCSP), I began to say, "We need to empower the people who are working on the impacts of climate change." And, at that time, I realized that Bush's statement that he was going to focus his policies on adaptation to climate change was phony. It was phony in the sense that from that time of him uttering that until today, there's not been a dime of new money coming from the government to focus on impacts and adaptation to human-caused climate warming. Soon I recognized the patent falsehood of that statement. And I thought, "Well, if I'm going to take addressing the impacts of climate warming seriously, I need to be more closely affiliated with the people that I am defending. I already knew what they were doing down at ESIG is very, very important." ESIG has since changed into ISSE. But, I had a very good time up in the CGD area of NCAR, especially so, because many of my scientific colleagues were there, and they accepted that I was more affiliated with the Advanced Scientific Program rather than CGD itself. So that worked out extremely well, and I would

like to say that it worked out so well that some major things have happened -- not because of me, but because they were happening -- was that GFDL and NCAR started putting together the Earth Systems Modeling Framework (ESMF), and associated with that was GFDL's Flexible Modeling System. It was kind of a hardware and software under-story for more flexible, more adjustable, and more changeable models of our messy and complicated climate system. So, because of this new ESMF, there was an understanding that when they were going to have a diagnostic workshop that the only people who could be invited to the workshop were those who were contributing directly to the problem.

CHERVIN: And did that include you, or...?

MAHLMAN: Absolutely not.

CHERVIN: Oh, I see.

MAHLMAN: I said, "I think is a very good idea, because what you don't need is people who are protecting the past. What you need to do is find people who are willing to pioneer the future." That has been my credo for a long, long time, and that was the way I tried to run GFDL as much as possible: to see the future, plan for it, and be patient enough to make it happen. I felt very privileged to be able to be given that sort of access, that sort of freedom, because when I would return to GFDL to visit some of my old friends, they'd say, "Jerry, we're having this wonderful time with the climate modeling people at NCAR." And I would say, "I'm not surprised." And I'd go back to NCAR, and they'd say, "Jerry, we're having this wonderful time with these really smart, nice people at GFDL." And I'd say, "I'm not surprised. The only thing surprising is why it took so long." I said if part of that was my fault, I would take the blame, but I think that's not the case, because I was advocating for it long before the new NCAR-GFDL collaboration actually began to happen.

That gave me a very good start at NCAR, when I moved down to ESIG -- now ISSE, the Institute for the Study of Society and the Environment. It has been a very nice experience for me, because I'm talking with, and sometimes advising, people who know about lots of things that I don't know, but they also know that I know an amazing amount of things about how the climate system works.

Shortly after, I had become part of NCAR, I was invited along with Maurice Blackmon to attend a meeting in Tokyo on their Earth Simulator Computer System. That was a very nice experience. Suki Manabe had moved back to Tokyo, and I soon found that he was highly critical of the way their new computer system was working, and of the way they were managing their computer time. But Maurice and I and a scientist from Australia and other outsiders were beginning to realize that the Japanese didn't have a clue about how to put together a large modeling system. Each individual Japanese had his or her own little model, and it was thus like being in the middle of a university that had not yet

learned had the concept of why and how it is that people can work together on such a huge computer system.

CHERVIN: And about when was that?

MAHLMAN: January 2001.

CHERVIN: OK. So that was fairly early on.

MAHLMAN: Quite shortly after I retired from GFDL. But, it was a very nice experience. And of course, by that time, my good friend Suki Manabe was already back in Tokyo, and my good friend Yoshi Kurihara, the world's pioneer in the state-of-the-art hurricane model, was modeling tropical cyclone behavior there. So I got to reconnect with some of those GFDL retirees, and some of my other colleagues and friends from Japan as well. But, it was also frustrating, because they had a very, very large vector-based supercomputer, so I was taken over to see their Earth Simulation supercomputer. The head of that computer facility was a very gruff, very arrogant kind of man. He was offering me computer time on their simulator to be sent to GFDL. I told him that it was flagrant of conflict of interest for me to even think about that. And I said to him, "If you want to ask me about the future of pipeline versus commodity parallel processing computers, I'd be happy to talk about that. But, I am in no position to negotiate with you or anyone else." He was very upset with me, but I was truly not in a position to negotiate on behalf of GFDL or NCAR. He said: "Why did we invite people like you if we can't negotiate directly with you?" I said, "Well, because you need my advice. That's why. I am not here to negotiate deals." So that was a mixed experience, to say the least.

CHERVIN: But you did get to talk to any of the bench scientists there who were actually trying to accomplish something?

MAHLMAN: I did, and it was extremely discouraging, because each one of them was the boss of his or her own micro-specialty. Like the land surface biosphere guy, his contribution to the climate modeling was to study fungus on leaves.

CHERVIN: That's quite micro.

MAHLMAN: (laughter) This is nano-thinking! And also, at that time, Suki Manabe was very frustrated with them, because each person had to have his or her own model in order to make it in the Japanese system. Suki would keep saying loudly to them, "This doesn't make sense!" So the response of the Japanese bureaucracy was to be mad at Suki. Well, it turns out that the Australian guy, Maurice Blackmon and I, when we stood up to talk, all basically said the same thing: that you've got to put teams together that will actually work, or you're going to get clobbered by your international competition. Having a big, fast computer --

CHERVIN: Is not enough.

MAHLMAN: Exactly. It's hardly even a necessary condition if you don't have the code to work on it. But there was also a lot of discussion about highly parallel versus pipeline kinds of machines, and the implications of transferring from the vector pipe types of machines to the commodity processor massively parallel machines. Unfortunately, we have a revolution to work through in order to make commodity processor machines work very well. So, the Japan visit was a good experience for me.

Also, about in 2002, I was still up at NCAR's Mesa Lab, mostly affiliated with atmospheric chemistry people. It was a good experience because most of them I already knew, and they didn't seem to feel nearly as threatened by my presence as I suspected the CGD people felt, simply because in the Atmospheric Chemistry Divisioin, to them, I was myself; to CGD, I was the director of GFDL that was a rival to the CGD models. So, it was kind of nice to be part of the Atmospheric Chemistry Division, and I was very warmly received there. But, after my experience with our National Research Council Review of the Bush Administration's Climate Change Science Plan, I realized that I had been talking a lot about the need for more budgets for far better climate change monitoring systems, and also for climate impacts research. I said loudly that they're both desperately underfunded. Because of my strong stance, it was natural for me to come to the Environmental and Societal Impacts Group, ESIG. Obviously, I was a square peg in an ESIG round hole, in the sense that many of the professionals are sociologists, biologists, and all kinds of other eclectic people, who are exactly the kind of people that they need. But, the question is, what would ESIG need me for? It turned out that they need me for all kinds of things, because of regional climate change, how to negotiate climate impact efforts, etc.

In that same year of 2002, I spent two weeks in China, first at the Institute for Atmospheric Physics (IAP) in Beijing, and then at Nanjing University. The Chinese were very, very interested in the climate impacts problem. That was a real pleasure for me, because many of the people -- particularly in Beijing -- I knew very well. Most of the senior folks at IAP had done one-year stays at GFDL, and some of these were my dear friends. But, I discovered something remarkable there in Beijing that was very surprising to me. I found out that the highest-ranked people in China had essentially no access whatsoever to the political part of the process. It was sort of like the head of the National Science Foundation or the head of NASA doesn't have any contact with the Bush administration, for example. That is almost unthinkable when you think about it. I began to realize that the Chinese scientists' knowledge about having to take climate impacts seriously was completely stifled by the inability of the leading scientists in this field to actually do what needed to be done. My two weeks in China were an extremely interesting experience for me, particularly because I had a wonderful time with the graduate students, both in Beijing and Nanjing University. They were taking me out on the weekends and to show me things.

China was a wonderful experience, and it was wonderful to see China 20 years later from the time I was there before. It was like going to a different planet again.

In 2003, I spent a lot of time just working on mainly the science of climate change communication. Just this morning, I had three requests for a need to explain a certain aspect about global warming that the science writers needed to know. I've spent a lot of time doing that, just simply responding to their questions, giving invited talks, or giving them advice. For example, this year in 2006, I went down to Colorado Springs as one of the co-lecturers with Hank Brown, the president of CU. So I'm getting access in places, even though, at NCAR itself, I occupy what is objectively a pretty obscure position, in the sense that I am now working in a whole new area that isn't populated with talent, or funding, to the level that is needed. I also have the interesting privilege and/or sometimes problem of not being supervised by anybody at NCAR.

CHERVIN: And you're not supervising anybody else.

MAHLMAN: Nobody else, at least officially. Essentially, I have the run of the NCAR store. There are times when that turns out to be extremely valuable, like when Cindy Schmidt or Anatta want to have information about PR for this or that, or me to explain global warming to somebody. Once, Linda Mearns, Cindy Schmidt, Susan Moser and I were asked to meet with staffers from Senator Hagel of Nebraska and Senator Bingaman from New Mexico. I was explaining the science of global warming to a rookie, but PhD in physics, for Senator Hagel from Nebraska. Well, it turned out he knew a lot less about Nebraska than I do by several orders of magnitude, because he'd only been in the state once in his life. He was working for Senator Hagel out of Washington, D.C. For me that was kind of amusing. But the staffer wanted to know: What is the underlying physics of global warming? People in the audience were hesitating, but I said, "Well, I'll be happy to explain it to you."

CHERVIN: I assume he was able to understand --

MAHLMAN: He understood me, because I speak --

CHERVIN: -- the atomic physics and quantum mechanics of it?

MAHLMAN: Yes, on the infrared spectrum and the spectral lines, and opacity in certain parts of the infrared spectrum, where the ozone-destroying chlorofluorocarbons are important. So, he said, "It's totally convincing. What's the big problem?" (laughter) I said to him, "It's only a big problem if you don't understand physics and your mind is closed." There were a number of other times in which I've had nice access to Tim Killeen and his front office people. I have to say I'm quite grateful that Tim keeps reminding me that any time I have a concern or an issue that I want to talk about, let him know, and we'll put it on his calendar, or on the

clock depending on what's going on in his life at that time. So, I feel that I'm quite comfortable in my new life at NCAR, but I also realize that my power inside the institution is microscopic compared to that which I wielded in NOAA.

CHERVIN: But you're accepting of that.

MAHLMAN: Absolutely. It's part of the deal. It's nice not to be supervised by anybody or supervising anybody, but to recognize that I now have very different, but still important, scientific opportunities to serve society and environmental science.

END OF TAPE 5, SIDE 1

INTERVIEW OF JERRY D. MAHLMAN

TAPE 5, SIDE 2

CHERVIN: This is Side B of tape 5, continuing the interview of Jerry Mahlman. We've just spoken about the issue of communication on the science of climate change with the media, and with the general public. Now, to what extent is this an organized activity versus a haphazard thing when they call us and we respond?

MAHLMAN: Well, the media's communication with those scientists who are actually qualified to explain climate science has been spotty in the sense that we climate scientists participate in national assessments, and in international assessments and to specific requests. We respond often to reporters. I've been at two recent climate scientist communicators/climate science writers' workshops. One was at the University of Rhode Island in 2001, and the latest one was at the Lamont-Doherty Earth Observatory at Columbia University. These workshops were very interesting, because most of the scientists who came to the workshops had been involved in these kind of things before, but most of the writers were pretty much out of it. That was a little frustrating to some of us. Two of the science writers were actually very "with it," while the rest were not. So we climate scientists kept leaning on the very seasoned Andy Revkin to essentially make sense out of the confusion of the writers on the climate science side. We got a lot of angst from the writers about, "Well, how do we know how to write about these hard science kinds of things, and how can we tell the truth?" We would reply by saying, "Look: we can lay out the truth for you, but we can't babysit you completely. You have to write your own article." And they said, "Some of us writers need to talk to Andy Revkin, because Andy has worked with the scientists on this relationship for a long time." Andy was in the room, and that helped a lot. But it pretty much boiled down to yes, we writers are going to put up a blog to do most of the hard work, and so we can communicate through the blog. But, the writers pretty much agreed that we scientists were right, and that they'd call us when they need us; we don't call them even when we want to talk to them.

Ironically, from a historical perspective, we're in a very strange environment right now, and I think it's a paradigm shift. Whether it'll last, I don't know, but the paradigm shift is this: there is now pressure on scientists to be relevant in the global warming world. There's now a lot of reinventing of wheels going on, a lot of hustling of results outside the refereed literature, saying, "See, my institution or my college or university! We're now involved in global warming." A lot of them are using little toy models so they can say that. But, it's really quite frustrating to many of us, having them reinvent things that were published in 1990, for example. I got in quite a little warm exchange (laughter), on the edge of being a heated exchange with my favorite climate science writer, Andy Revkin.

CHERVIN: On global warming?

MAHLMAN: Yes, Andy Revkin. I told Andy that the paper that he was going to publish in the *New York Times* as a new big, big thing was not. I said to Andy, "Are you aware of a virtually identical paper published by Jim Kastings in 1990? Using the same toy model?" And Andy said, "No." So, then we had this discussion that there's lots of people who are now publicizing all known facts about global warming. You can see it here in the *Denver Post*. Whatever anybody feeds them, they write. If the story comes through the Associated Press, they will write the article, and almost in the same breath will say, "There's some scientists or many scientists who believe in the theory of global warming." So it's caveated to please nervous editors. The press/scientist relationship right now is fairly dysfunctional, but some people blame us because we don't communicate clearly enough. But, we have to have the channels to do so. Typically, most of this information has been out there for a long, long time, but the new generation of writers are essentially unqualified to write on climate science, so we go through the same song and dance most of the time. In fact, just this morning, I was looking at my mail and then responded to a writer who wants me to explain why people are so concerned. So, I write a whole mini-essay that will help a climate science writer get it right. But, I argue that it's still a fundamentally unequal relationship. When they call us, we respond. Typically, we don't call them because we don't need to advertise our new results, particularly if it's not new. We live in an interesting time right now. There's close to a feeding frenzy about global warming articles, because people want to say that my school, or my college, or my university is also relevant in the very public science of global warming. My answer is well, not very relevant, because you came in too little and too late. A lot of that problem has to do with the university system, which actually validates the single PI doing a small thing with a couple of graduate students, and maybe a post-doc.

CHERVIN: Well, the funding agencies are also a factor in that.

MAHLMAN: Well, and particularly in those university settings.

CHERVIN: Yes.

MAHLMAN: But at NOAA, much less so. The NSF still is facing this problem that they mostly have not dealt with well. I remember being at an NSF workshop way back in 1992 in which we participants made two huge recommendations for the NSF and the US Global Change Research Program. We said we're only going to offer two recommendations because they're both really, really big. One was we have to take global monitoring of the climate very seriously, and the other one was that we need to take climate impacts and climate adaptation research very, very seriously. That was the published, two-punchline summary of our workshop. Not a single thing has been done by NSF in the meantime because the problem is too big and too messy. So, NSF ran for cover, by saying without explanation, "We can't participate in the creation of a global climate monitoring system that actually has tight instrumental calibration over time." Of course, NASA never did. Nor was an infrastructure yet created to study climate impacts and adaptation research.

So, we've known this for now virtually 15 years since that crucial time, and essentially nothing's been done. Our institutions can thus be a source of scientific power and relevance, or they can be a detriment to it. I offer an example of where NASA has been a detriment in the sense that their satellites are never calibratable from one satellite to the other, with only very rare exceptions. Typically, those exceptions are altimeters that measure glacial ice, ocean ice and sea level. The rest of the stuff? Mostly garbage. A lot of the so-called "controversies" about trends of atmospheric temperature are from witlessly improperly calibrated satellites. In fact, the old NOAA series of satellites were, and are, in that category. So, the dysfunctionalities are not just in terms of how we communicate or to how the world responds to it, but mostly by how the institutions respond to it. The answer is, well, somewhat dysfunctionally, quite frankly. And now we've been piled onto, because over the last couple of years, there's been a very systematic attempt to Censor Climate Science, whether it's at EPA or NOAA or NASA where it is not politically friendly.

The problem is that global warming itself is not politically friendly, because it is inexorably, but slowly, accelerating to a place where, if we don't pay attention, we're going to be beyond the real danger zone -- which might be nearly a century from now, by the way -- and that's not very sexy. But global warming is our legacy for the future. Do we really care about our subsequent generations? Do we really care about whether our great-grandchildren will live in a very different world than we do because our generations didn't pay attention? Because we didn't commit to taking meaningful action? So, we live in very, very interesting times, and much of my scientific life has been focused on that for one reason or another. As mentioned above, I have profited enormously from two climate science/climate writers' workshops. One was at the University of Rhode Island, and one was at Columbia. I was also involved in a different workshop at Dartmouth on climate. The one at Dartmouth was on ethics of climate, and it was from a very philosophical point of view. You can imagine we didn't get very far, but the process was fascinating.

This year, 2005, was quite rewarding. I spent two July weeks in southeast Australia, working with CSIRO of Australia. It was quite interesting, because we had an outside review committee that was composed of four people that got shortened to three -- the Canadian climate scientist couldn't come. We had a regional air pollution person from the University of New Hampshire, a fisheries expert from Canada, and me to cover everything else. All the weather, all the atmospheric chemistry, all the climate, all the global mathematical models.

CHERVIN: And the ocean?

MAHLMAN: And the ocean. Which I thought was somewhat humorous, and I'm sure my oceanographer interviewer will also find humorous, in that I was the ocean expert in this whole Australian country and its vast surrounding oceans.

CHERVIN: In that group?

MAHLMAN: -- in that group, and in that country, for two weeks. But, I had a wonderful time, because I was just eclectic enough to be able to cut across to the regional air pollution concerns of theirs. There was another whole section that was taking random NASA satellite data to try to make some sense out of their data, and without NASA ever giving them their calibration of the satellites. I was able to say, "Look: you've been dealt a rotten hand here, because you're supposed to get all this cool stuff out of their satellites without having the requisite quantitative measures of the value of the satellite information." I then learned that they were still doing 1950s kind of boundary layer turbulence, classical kinds of vertical turbulence fluxes. They told me that these measurements were going to close the global carbon budget. I said, "No, it's not even close to closing the global carbon budget. In fact, to an excellent approximation, it's totally irrelevant to the carbon budget." This was kind of interesting, because we were starting to make people fairly nervous when they were defending these obsolete studies. But they also knew that I knew what I was talking about. After the official sessions, when we would go out to eat or take a break, they would keep asking me scientific questions. I was tough on them, but at the same time, I was very relevant for what they were doing.

That was at our first stop in Canberra, the national capital. After a couple of days we flew to Melbourne and Aspendale. That was very interesting, because suddenly, all the climate science people were there, the ocean science people were there. And they were a very knowledgeable group. They were trying their best for having their climate modeling able to compete internationally. I suggested to them that we had an Earth Systems Modeling Framework that was already being internationalized outside the US and Canada into UK and to Germany. I said to them: "You're not necessarily doing yourself a favor by refusing to participate in the Earth Systems Modeling Framework, because our problems are intrinsically global." That actually worked, particularly at dinner later, when the big boss wasn't sitting there. Then all of a sudden, they wanted to talk hard science, and --

CHERVIN: The younger scientists?

MAHLMAN: The younger scientists were very, very interested, because they hadn't encountered somebody like me who was A, old, and B, hip, in terms of how climate science works, how it relates to policy, how it is very useful to many "consumers" of climate science. Australia was a great trip for me, and a good trip for them, so to speak, to really get focused. Suddenly, they were asking many detailed questions. Clearly, they were very interested in climate. They have a current administration that is hostile on climate, but not as hostile as the Bush administration is on climate. So, one night over dinner and beers, we were discussing which government was more dysfunctional these days. We all agreed that Bush wins by a landslide of dysfunctionality. (laughter) We did have a good laugh over that. But to me, the obvious point is that Australia's kind of an

international pariah because of their sticking with the Bush administration on the denial of anything about global warming that is relevant or real. It was uncomfortable for them to admit that. It was a good time there. We had lots of discussions about atmospheric dynamics, atmospheric modeling, and monitoring of long-lived trace gases from Paul Fraser's group at Cape Grim. They were very focused on how really important these measurements are to understand the geochemistry of the atmosphere. Thus, I had a wonderful time in Melbourne as well.

Then we went off to Tasmania, where essentially all of the focus was on the ocean or marine science. That was very interesting to me, because they're actually talking about doing numerical weather forecasting of mesoscale eddies in the ocean. I found that effort to be very, very fascinating, because, in the Northern Hemisphere, I don't know if anybody's really taking that research frontier seriously. There's, of course, the question of how you initialize such an ocean eddy forecast when the data is between terrible and poor in terms of making it work. Do they have an observing system that could actually do that? Do the new Argo floats actually measure the ocean eddies so that they can properly do a state of the ocean "weather" forecast? I found that to be quite fascinating. Then they were asking me questions about how we would be able to take these ocean forecasting models all the way to the beach? I said, having worked with coastal ocean forecasting systems through Princeton's George Mellor and GFDL, that getting all the way to the beach with a model that's going out thousands of kilometers is really asking for big trouble, because the scales and the aspect ratio change radically as you approach the beach, and suddenly you're into breaking waves, nonlinear breakdowns, ebb currents and all kinds of challenging things. My advice to them was be very careful about trying to extrapolate onto the beach unless you want to work outward from a beach model itself, where you have sand and breaking waves, and a lot of these other icky things that are really hard to model. These discussions began to give me a sense of validation as to why I was there, because the other two foreign reviewers were just listening to what was going on. The Australian scientists really wanted to talk the science, and they knew I was ready to talk it with them. I thus felt a sense of privilege being there, because they knew that I knew what I was talking about. They knew that I was listening to what they wanted to do. They knew I recognized them for the capable, talented scientists that they are, whether it's ocean or atmosphere or climate modeling itself. We had a lot of discussions in Melbourne about the art and craft and science of doing climate model work.

CHERVIN: Was there any concern there and then about impacts?

MAHLMAN: Very much so. There, impacts were a big thing because they did have some measurements showing that the interior of Australia is warming faster than everywhere else on Earth. Of course, that was almost a no-brainer to me, that one would expect that. But the interior is not so affected so much about soil moisture,

because the interior, to a good approximation, doesn't have any soil moisture. (chuckles)

CHERVIN: Right. It's almost fixed at zero.

MAHLMAN: Almost fixed at zero, or episodic precipitation events. That was interesting to me, because they were realizing that they were looking at secular trends in temperatures there, and that they had an observational system that was good enough to actually determine that. When you get to the coasts, it gets a little trickier. Once you get out into the ocean, it gets much trickier, because we realize this "COWL" kind of thing -- cold ocean/warm land phenomenon -- and most all the global warming time series show that of course, all the heat capacity is the ocean to an excellent approximation. So anyway, that was a pretty valuable discussion. And then I went to Tasmania. I actually had the weekend off, and one of my good buddies, a GFDL candidate for one of the three smartest people I've ever known, Steve Griffies--I have Dickinson at one end, and Isaac Held at the other (laughter). I realize they're all out of my parameter range, let's put it that way.

CHERVIN: Right. Out or --

MAHLMAN: Yeah, yeah. (laughter)

CHERVIN: -- possibly even way out.

MAHLMAN: Yeah, yeah. My brother, the physics teacher, saw my high school tests that put me in the 99th percentile. He was eight years older than I am. He said to me, "But just remember that you could be in the bottom 1% of that 99th percentile," because the range goes on and on through multiple decades of 99.9, 99.99...

CHERVIN: It's a long tail.

MAHLMAN: Yes. It's a long tail. That experience told me how to be humble in the midst of genius, especially if I recognize that they do things that I don't understand. And, I would argue that I do things that they don't know, too. So anyway, it was a good time in Tasmania, because suddenly we were talking about ocean currents and changes in the currents, ocean variabilities from various kinds of data sets, and also fisheries. To myself I said, "Now this is where I'm totally incompetent." But, because I've spent life part of my life as a fisherman, and I'm interested in how Mother Nature works, I was able to ask questions and get away without looking like a complete idiot, which was good (for me at least). I gave a seminar there on the 4th of July, an invited seminar on "The Mega-ethics (or lack thereof?) of Human-caused Climate Warming." The experience was very interesting to me. I talked for about an hour. But most all the questions were from the senior scientists.

CHERVIN: And this was in Hobart?

MAHLMAN: Yes, Hobart. At the CSIRO facility. A beautiful place. And it was on the 4th of July. At the end, the hosts said, "Thank you, Dr. Mahlman. The questions are over." So, most of the audience left. But the post-docs and graduate students didn't leave. They just came over afterward and stood near me and said, "We have more questions to ask." They said, "How about if we go out in the hall?" (laughter) I said, "Fine, we'll go out in the hall." Meanwhile, all of the top scientists had already left, and all the people asking the big-wig questions had already left. So there we were, talking out in the hall, and then the janitor said, "I have to close the door." So then we went out in front of the building.

CHERVIN: Right. Outside?

MAHLMAN: Outside I still had like, 20 or 30 people asking questions, but they were all young scientists, graduate students, or post-docs. That was a wonderful experience, because it showed the intensity of their intellectual curiosity, and their willingness to engage -- at least as a group to engage; as individuals, maybe not. But it was a wonderful experience.

My genius friend Steve Griffeys had a good friend who is the CSIRO librarian in Hobart, and it turned out that he's an expert on Tasmanian birds. So, Steve and he offered to take me out to look at Tasmanian endemic birds:13 of them that only live in Tasmania and nowhere else on Earth. Early the next morning, I'm tramping around in the wilderness with this guy, and of course Steve Griffies didn't have a clue, but said, "I wonder what you birders are really up to!" One single day, I got to see 11 of the 13 Tasmanian endemics, and many of the bird species that fly in and out of Tasmania. I thus have this wonderful warm spot in my heart for Tasmania. From Tasmania we flew to Brisbane, and more on marine science, and more on fisheries. But again, I was learning and learning.

So, that was my Australia experience in 2005, and it was something to be treasured by me forever. In September of 2005, I was involved with the GFDL 50th anniversary celebration. And right now, I'm in the process of writing an overarching perspective essay on the achievements and culture of GFDL, and how it worked well, and maybe sometimes why it didn't work, so well. So, I'm involved in that right now, and the 50th Anniversary of GFDL symposium went very well. Part of the history of that celebration is that the ex-director of GFDL, me, blacked out for about ten seconds, and ended up in the Princeton hospital. But I won't bore anybody with that. It turned out to be a total false alarm. I kept telling people it was. I'd had a hemorrhagic stroke in 2004, and I said, "It's just a symptom of that, especially if I did not get enough oxygen. If I'm jogging, it tells me when to quit." But everybody was kind of in hyper-mode about my being rushed to Princeton Hospital. So, I missed the second day of the symposium when I was supposed to give a speech and overarching comments about the history of GFDL. As a result of that, I now have prepared the assignment of writing an

integrated perspective on GFDL's science and GFDL's history, which I have now completed. I think that I would like to say that we are on the cusp of a truly historical anomaly of government dysfunctionality. Those of us who have had connections with the federal agencies that are direct -- not like NCAR, which is safely isolated from these kind of things, but if the people at NCAR like Tom Wigley or Kevin Trenberth were saying the things that they're saying -- all of which are true, by the way -- that they would now have been subject to deep censorship if they were in NOAA or NASA or EPA.

CHERVIN: Now exactly where does that censorship come from? Is it from the administrators?

MAHLMAN: No. It comes from the political appointees. They are hired by whether or not they will toe the line to what the administration wants to do, to spin the story of global warming in such a way that they don't have to take any responsibility. But, what I just got involved with very recently is that there is an expatriate's very vocal "underground" that is dealing with all of the issues of censorship of climate science. There are now people that I am talking to in NOAA that are not supposed to be talking to me or anybody else. They talk to me because they know I won't blow their cover. So, it is extremely ugly right now, and it grieves me to be ending this interview with what I think is one of the greatest crises of environmental science at the US government in history. There's a number of government people who have left on purpose because of this. Some of them left noisily, resigning in protest. There's an underground that's actually forming outside of the government of not-so-underground people being quite vocal about what they see and what they know. I am one of those.

CHERVIN: And to what extent is the media aware of the censorship?

MAHLMAN: Well, some of the hardcore experienced climate writers know that it's going on, but there is this new phenomenon, and the more business-orientation of even the largest newspapers that are highly respected like the *Washington Post* and the *New York Times*, for example. I remember talking with Andy Revkin a couple of times recently, where we'd set up a story and I helped him write the science part, and then we iterated until we got it right. A couple of times I got an e-mail back a couple of hours later where Andy said, "My editor just canned it." And I thought, "Man! If you can can Andy Revkin, who else is vulnerable?"

CHERVIN: At the *New York Times*?

MAHLMAN: At the *New York Times*. What does that tell you about the state of affairs these days? This UCAR/AMS interview ends with my frustration with the blatant censorship and the witlessly shameless way that it's being done, that they think everybody's an idiot and doesn't see it. I think this "era of censorship" is an historical anomaly. Over the next ten years, how will this play out? Will it explode before Bush is out of office? I have never in my 33 years inside the

government, or my now five years at NCAR, have I seen anything remotely like this in terms of the manipulation of science for political reasons. In previous years, where I testified at congressional hearings, the Congressmen would just challenge you, and then they would disappear. Now, they proceed without apology. When the U.S. Delegate went to the recent Montreal conference, the Bush Adminsitration sent the most right-wing ideologue I think I've ever met in my life, Harlan Watson. He used to be a Congressional staffer.

My story will go on simply because I cannot just sit here quietly and watch the science that we know and worked so hard to do it right, get clobbered through censorship, for withdrawal of funds. I consider that for climate science right now, we're living in the deepest, most blatant of censorship of science that this country has ever seen.

END OF INTERVIEW

Final comment: I am deeply indebted and grateful to Diane Rabson, NCAR Archivist, and to Dr. Robert Chervin, retired NCAR Oceanographer, for their steadfast and invaluable contributions that made this interview a great success for me, and I hope, UCAR, AMS, and GFDL.

Jerry Mahlman, NCAR