William Kellogg: So, this is Will Kellogg, and I am talking to Roscoe Braham. We are here in New Orleans at the annual meeting of the American Meteorological Society. The date is 13 January, 1987. Roscoe, just to get this down, tell me where you come from, where were you born and where did you go to school, that sort of thing, just the beginning of (your vita?). [laughter]

Roscoe Braham: I was born in little town of Yates City, Illinois, in 1921. But the family moved to Xenia, Ohio in 1930 when I was in the fourth grade. So, my school years were spent in Xenia, Ohio for all practical purposes. We actually lived in a little town of Goes, which even at the time we moved there had mostly gone.

WK: [laughter]

RB: It was little, rural farming kind of community. From there to Ohio University for college. Essentially, I left home then when I went to school at Ohio University. Graduated from Ohio University in January [19]42. I actually finished school in three years and I didn't go back for the diploma.

WK: What did you major in?

RB: That was geology.

WK: In geology.

RB: It was during the 1941 academic year that World War II was heating up as far as our involvement. I had accepted a fellowship to Columbia in geology. But I received word from chairman of the local draft board in Xenia that they had been passing over my case for [laughter] some period of time until I finished my degree. That it probably would be smarter to look around and select a niche in the military rather than take the fellowship at Columbia for graduate studies in geology. So, I did that. I entered the Air Force because they offered opportunities in meteorology. Of the subjects offered, that seemed to be the one of greatest interest. So, I went to University of Chicago in their meteorology program and graduated in May of [19]43. Yes, I think it was May. May, [19]43.

WK: About a year after I did.

RB: About a year after you did, right. Stationed initially at Langley Field in Virginia. It was there that I became involved in the aircraft meteorology mix, primarily as a result of a busted forecast that caused some discomfort for a certain (Captain Davis?) and his crew that were on sea search. I remember I was on the midnight trek along with a fellow by the name of (Marvin Diamond?) who went back in the air weather service after the Korean, or at the time of Korean conflict, and spent most of his career then at (Chanute?) Field. Marvin and I were on the midnight trek and came time to brief a Captain Davis.

WK: Where were you stationed at this point?

RB: At Hampton, Virginia, Langley Field. The activity of the unit was sub patrol sea search. They were using B-18s with this very modern device called radar, which was highly classified. I told them of the existence of thunderstorms, but was not really appreciative of the role of the land breeze at night in focusing thunderstorms out over the Gulf Stream. Their sea search involved them in a contact. Of course, it was standard procedure to stay with a contact until you had been relieved by the succeeding airplane in order to follow the contact. Hopefully, it would surface where you could drop some of your little depth charges. Davis stayed out near to the limit of his gasoline and was unable to make it back. A part of that problem was the fact that there were these unanticipated thunderstorms. So, a few days later, the same crew was scheduled for a flight, and I was the weather officer.

WK: They did not make it back. They had to ditch, I suppose.

RB: They landed. They didn't ditch, they landed on the beach. Of course, the beach was filled with these runways. The entire East Coast beach was littered with little temporary landing strips where they had painted a stripe down the beach for some guidance to the landing airplanes. It was quite safe to land, but you couldn't take them off. You had to truck them back because they couldn't get up enough speed in the sand to take off. With sand it'd be quite hard actually, and so you could land. A few days later, the same crew was heading out for a flight and I went in to brief them. This time I had learned a little bit more about thunderstorms and Gulf Stream. When I mentioned that I anticipated that there could be some thunderstorms, Davis got up from his seat in the front of the room and went to the back and pulled a Mae West out of the rack. It was fairly obvious to me that that was my Mae West, because he was already wearing one. He intended that I would go with him. So, I did. I went out with him. It was an excellent flight. It was the beginning of an association that emphasized our very pitiful state of knowledge of meteorology, and particularly the mesoscale, thunderstorm scale meteorology and the likely value of the airplane and radars for investigating phenomena of that scale. Of course, that has been my career ever since. From Hampton, Langley Field, I was shipped to Kearns for overseas processing for the CBI. But didn't make the shipment because of an argument between the air weather service and the training command as to whether I would be a pilot weather officer or just a weather officer. Took my pilot training in the West Coast Command, ended up as a B-24 pilot and was sent to Santa Ana as the staff weather officer in the West Coast flying training command.

WK: You had your wings by then.

RB: I had them. That's right.

WK: Funny. You and I had the same career then starting out in meteorology and taking flight training.

RB: Then flight training, yes. So, I ended up with qualifications in one-, two-, and four-engine aircraft, but really spent the year of the war, the last year, in Santa Ana as staff weather officer. But there's relatively little weather activity other than the teaching in the Southwest Command. So, most of my time actually, was spent in accident investigation because I was housed with the safety officer for the command, (Major Gay?). He had more to do than he could get done, and I had less. So, I worked with him on accident investigations until they closed the West Coast

Command. It must have been September of [19]46. I was shipped to Randolph Field. On checking in at Randolph Field, I was advised that it probably wasn't worthwhile even checking in to the BOQ because they were releasing so many people. If I wanted out, I probably could get out. So, I spent a little while in the officer's club and was discharged, went in time for Thanksgiving on [19]46. Went to Chicago in order to see what was involved in going back to school and found that Horace Byers was in the middle stages of organizing the Thunderstorm Project. He encouraged me not to go back to school at that time, but rather to [laughter] go back and go into the Weather Bureau as an employee to work on the Thunderstorm Project. That I did. He appointed me in charge of the analysis section, and I participated both in the field activities and in the analysis, and then ended up as the acting official in charge of the Weather Bureau's involvement in the Thunderstorm Project.

WK: Where were you stationed with the Weather Bureau? Where did the Thunderstorm Project take place at that point?

RB: The field effort was carried out in Central Florida near Orlando, close to where Disney World is today, [laughter] or Disney Land – Disney World I guess it is – in summer of [19]46, and in Wilmington, Southwestern Ohio, in the summer of [19]47. During the intervening winters, and then the period from fall of [19]47 until May of [19]49, we were in Chicago on the campus of the university, although our quarters were actually in the top floor in the rotunda section of the Museum of Science and Industry.

WK: Then you were working with the Weather Bureau and doing the analysis of the data for them?

RB: That's right. The same material then was used for my Ph.D. thesis, which was on the water and energy budgets of thunderstorms. Stayed in Chicago, finished the Ph.D., all except for turning in the dissertation. I had the dissertation approved in preliminary form and Steve Reynolds and Jack Workman from Socorro, New Mexico, came by one day. We were talking about thunderstorms, and we were pretty sure we knew what the internal circulation of thunderstorms was like. As a result of studies that they had carried out, initially by some of your colleagues, Workman, Holzer, and (Pelzer?) in the Albuquerque area. They felt that they had a handle on the electrical activities of thunderstorms.

WK: Bob Holzer was big on the electrical part.

RB: That's right. So, why wouldn't it make sense for me to come to Socorro, New Mexico, and spend a year, which was probably all it would take, in order to put together these two fields of knowledge? Well, since that was the only offer that we had, [laughter] and it certainly seemed enjoyable.

WK: Was it a we already by that time?

RB: Excuse me?

WK: Was it a we? Were you married at this time?

RB: Oh, yes. Yes. We got married in [19]43, and we had two or three children by then. We got married near the finish of the cadet training at Chicago in March of [19]43. So, I went to Socorro, New Mexico. Our technique there was the experiment, which I still think was a well-conceived experiment. Was to lay out a field of gradient change sensors from which we could identify the location and polarity in cloud-to-cloud lightning strokes. From this, deduce the polarity of the main charge centers inside of thunderstorms. The so-called Workman-Reynolds Effect suggested – this was laboratory studies that Workman and Reynolds had carried out – suggested that if clouds could be seeded with ammonia, the dipole could be reversed, and we should be able to detect a reverse dipole from the surface network. So, it was a very simple experiment concept. We would use airplanes to seed thunderstorms with, or incipient thunderstorms with ammonia as they were to drift across our ground network of gradient change devices. We obtained B-17 from Mrs. Whedon of the Signal Corps with the help of (Helmut Reichmann?), where the airplane was instrumented by filling the bomb bay with huge ammonia canisters with a big plastic pipe that connected the canisters and ran back to the rear of the airplane and was dumped on the rear of the airplane.

WK: Roscoe, can you briefly say, what was the idea? What made them think, or you, think that ammonia would change the dipole of a thunderstorm?

RB: Yes. Well, the Workman-Reynolds process, or Workman-Reynolds Effect, which was discovered in the laboratory and certainly is a natural phenomenon, results from the fact that as ice freezes from the liquid water, most contaminants in the water are rejected from the ice structure. But contaminants that have a size and charge that is not too dissimilar from some of the ice, may be incorporated or rejected less effectively. As a result of that, on freezing water, the ice takes on a charge that differs from the water and these ions are selectively rejected. Most natural contaminants, the contaminants you find in natural rainwater, all would give the same polarity to the ice and to the water. If one assumes that the charging was generated by rhyming processes, such that the freezing ice then developed one polarity, and the liquid then was shed as small droplets, and it would contain selectively rejected ions. So, it would have an opposite polarity. If the graupel particles, by virtue of being larger, were falling downward, and the shed water droplets being smaller carried upward in the updraft, you would result in a net polarization to the thunder cloud.

WK: The charge separation mechanism.

RB: Of such a sign that the top of the cloud would be selectively positive and the lower charge center would be negative.

WK: It is the opposite.

RB: Well, just the opposite. Now, a few substances, but ammonia particularly because of the nature of the molecule, could be absorbed differently than the natural contaminants and just reverse the polarity of the ice and the water. So, that if natural thunderstorm charging resulted from the so-called freezing effect, the selective rejection of contaminants in the cloud water as the cloud water froze on rhyming – if that's how thunderstorm charges were separated, then this experiment, you see, offered the possibility of pinning that down with a single experiment because we could reverse the polarity. Of course, it was well known that thunderstorms of reverse polarity were just, I'll say, non-existent. Perhaps there was a little argument as to whether once in a while you might get one in the opposite polarity. So, the experiment was set up to release ammonia in incipient thunderstorms. We were following the clouds with some simple radars. The day of the experiment came along where it was all indications that the particular clouds were going to move across the network. We had about the right amount of time to get the airplane off. It had been a period of frequent thunderstorms, and looked as though it was going to continue. So, we signaled the airplane to take off for our monumental experiment that was going to prove thunderstorm electrification processes. The airplane was based at the little airport at Socorro, New Mexico, which is really a rather small airport for B-17s. But the B-17 is a very kind airplane. So, in taxing out for takeoff, the pilot accidentally got off the runway and got stuck.

WK: [laughter] Oh, no.

RB: That was the end of the experiment on that day.

WK: [laughter]

RB: We had to go to, I don't know whether it was Roswell, in order to get some jacks capable of lifting a B-17 and getting it back on the runway. That took several days. So, after, the airplane was put back on the runway and cleaned up, and we were all ready for a second try. The day came along. It was just a perfect day. This time the pilot stayed on the taxiway and went out into the cloud. The cloud was filled with ammonia, and there was copious lightning. The lightning was just exactly like what lightning from every other thunderstorm we had ever looked at. [laughter]

WK: [laughter]

RB: It was fairly clear that either we hadn't succeeded in injecting the material at the right place, the right time, or alternatively the Workman-Reynolds freezing effect was not really fundamentally important in how [laughter] nature makes thunderstorm charges. Unfortunately, our resources were expended and time was expended. So, we had only the single, good trial. But, nevertheless, we thought it was a good experiment because it proved that we were wrong and that maybe nature still had some secrets we didn't know.

WK: Just as a historical note, it is interesting that in that same part of the world, Bernie Vonnegut and Marx Brook are doing another thunderstorm reversal experiment with a different idea.

RB: Succeeded in turning one over, according to the recent reports. That's correct. That's absolutely right. So, that's quite interesting. The same institution, in fact, is involved at Socorro. So, School of Mines at that time, but now it's just New Mexico Tech. [laughter]

WK: New Mexico Tech, right.

RB: Well, by this time, which was now about 1951 or 1952, the Air Force was creating a laboratory at the University of Chicago for doing experimental work to serve as a complementary activity to RAND Corporation, which was involved in a lot of theoretical think tank kind of studies for the Air Force. The Air Force had several problems they wanted investigated, one of which was the claim that cloud seeding might be an effective weapon. Because in the fall of [19]46, Vince Schaefer had discovered that the dry ice released in the super-cooled stratified clouds would, in fact, convert the cloud to ice crystals. The super cool drops into ice crystals, and these perhaps would grow and aggregate to form snow. Of course, that opened a tremendous field of weather modification, a field that is still quite important in my view. But at that time, there were real questions as to whether it might be such an effective lever over and modifying what nature does and whether that it might in fact be a useful weapon of war. So, the military formed a large project known as the ACN Project, the Artificial Cloud Nucleation Project, distributed it to four different research groups with four different objectives. The University of Chicago Midway Laboratories, which was this new laboratory created by the Air Force for doing laboratory and field research. We were assigned the job of seeding convective clouds and thunderstorms because of the fact that we had just completed the Thunderstorm Project, and presumably was aware as anyone as to what thunderstorms might be like and how they might respond to seeding. So, I went back to Chicago and worked there on the ACN Project for a few years. We carried out our experiments in Puerto Rico in the wintertime and in Central United States in summertime. Our studies resulted in one of the early AMS monographs, I've forgotten the number, where our results were consolidated with those from Ferguson Hall who was responsible for the Weather Bureau effort, Jerry Spar from New York, who was responsible for a study on East Coast cyclogenesis, and I guess the three groups. There might have been a fourth one that escapes me at this moment. Well, the general conclusion reached by all of the ACN groups was that you could indeed cause substantial changes in cloud micro physical processes. But it appeared as though it was unlikely that any major change in precipitation patterns would be produced, at least with the simple approach that we were using.

WK: Were you seeding from aircraft, from the ground? How did you go about it?

RB: We seeded from aircraft.

WK: What did you use as a seeding agent in those days?

RB: We used dry ice and we used liquid water spray.

WK: This is before silver iodide.

RB: No, it wasn't before silver iodide. At this moment, I'm not entirely clear why we chose dry ice over silver iodide.

WK: Maybe it was cheaper.

RB: But we did not use silver iodide. Our seeding with water spray turned out to be the more important scientifically. Because as we now know, the coalescence mechanism is exceedingly powerful, and one way in which nature generates a lot of natural precipitation. Whether or not the coalescence process gets started depends upon some details of the cloud-based temperature and the aerosol that's being ingested into the cloud and the updraft speed. It's a rather delicate balance as to whether or not clouds do develop precipitation by coalescence. By inserting droplets that had already reached some fair size, the coalescence process could be started immediately. The effects were so obvious. Those results are still important foundation for some of our studies in coalescence. The studies with dry ice seeding however were inconclusive. It appeared that yes, indeed, we did increase the number of ice particles in clouds, and there was no evidence that we really were doing anything very important in the precipitation.

WK: Were the same experiments done in the Midwest where you were and also on the East Coast?

RB: The East Coast Jerry Spars' experiment was to release dry ice, I believe – but it might have been silver iodide – into the clouds along the East Coast and along Hatteras where cyclogenesis was thought to be probable, with the notion that the released latent heat of fusion in the cloud might be enough to trigger the development of East Coast cyclogenesis.

WK: Yes. Probably does.

RB: Well, in principle, it certainly does. Yes. It's question of magnitudes and they were not able to generate very convincing evidence that what they did was very important. So, after the studies with the ACN Project, there were a number of activities at the Midway Labs. Of course, some of them parallel with things that you were doing at RAND. The early director was Ed Teller and (Thophan Hognus?). I've forgotten the dates, but mid-[19]50s, the support for Midway Labs dwindled our report that cloud seeding did not appear to be an effective weapon. Resulted in the movement of our activity from the Midway Labs building, onto the campus of the university itself, into the department of meteorology, and substantially decreased support for that activity. The Air Force, however, continued to support our work. You see, during this Midway Labs period, we had an entire detachment of airplanes assigned to us of Air Force. (Major Ball?) was detachment commander, and we had something like a half a dozen B-17s that was assigned to us.

WK: Pretty big operation, was it not?

RB: Yes, it was.

WK: University of Chicago Air Force. [laughter]

RB: [laughter] You see, the Thunderstorm Project was really the first big science and the ACN Project was probably the second major field effort in meteorology in this country.

WK: Probably was.

RB: I feel that our group at Chicago, we certainly were involved heavily in those early days, and perhaps played some useful role in getting research in meteorology involving airplanes and radar on a par with the more theoretical studies that had been going on, of course. Well, it was during this period that Vince Schaefer and Irving Langmuir came to us and recruited our involvement in weather modification. We wanted to finish the work we were on. But because of the ACN seeding effort, we became thoroughly involved in weather modification and have stayed in that area, at least to some extent even to the present time. One of the early things of interest, there was a group in Arizona, a group of ranchers spearheaded by a former ambassador, Lou Douglas.

WK: Oh, I remember that. Yes.

RB: Who was the U.S. ambassador to England during the war. He's the chap that had the unfortunate fly-casting accident that resulted in a fishing fly in an eye and he lost one of his eyes. So, throughout the later years, he had to wear a patch on one eye.

WK: The original Hathaway shirt. [laughter]

RB: [laughter] Well, they raised some money that was to institute or to form a group involved in cloud seeding research at the University of Arizona.

WK: University of Chicago?

RB: At the University of Arizona.

WK: Of Arizona?

RB: Yes. Douglas came to Chicago early on, and wanted suggestions as to persons they might recruit to lead this new effort. We gave him a long list. He disappeared. He came back six months or so later, and said, "I have contacted every single individual. I have found no one who's willing to take on this job except one." He said, "I do have one individual who's willing to take it on, under the condition that he be allowed to staff it and set all of the research policies." Well, offhand that doesn't seem too much to ask of anyone that you're putting in charge of a new study effort. However, we quickly discovered that the individual was Irving Krick. It was Byers' belief, and I shared it, but I felt less strongly about it probably. It was Byers' belief that that would doom the new effort to failure because they would be unable to carry out research in a totally unbiased way. They had had already decided that cloud seeding did in fact, do many marvelous things. It would be an inroad of the private commercial weather-making activities into academe, and that that just would not be good.

WK: Well, I think even in those days, Krick's true colors were showing quite clearly.

RB: Oh, yes, indeed. Yes.

WK: It is interesting that I attended one of the meetings near Tucson at Douglas's place there. Krick was there. Were you there?

RB: Oh, yes, indeed. I organized the meeting.

WK: Yes, right. I remember Krick turning on the charm. [laughter]

RB: Well, we were somewhat at a disadvantage because we really didn't want the commercial weather activities to gain a recognized stronghold inside the university.

WK: Commercial weather modification activities.

RB: That's correct. I'm sorry. Yes, the weather mod activities. So, we had no other suggestions as to persons that might head the activity.

WK: [laughter]

RB: So, I agreed to take it on with a joint appointment between the University of Arizona and University of Chicago. I had an appointment as an associate professor of physics at the University of Arizona, and associate professor of meteorology at the University of Chicago. We proceeded to set up the institute. After about a year and a half, I realized that the thing was on sufficiently sound footing that I would either have to resign from Chicago or Arizona. It was unfair to continue a joint appointment and be a part-time director of the Institute at Arizona, is what it amounted to. Because of some medical issues that made it not really too desirable for me to stay in Arizona, I resigned. That must have been about [19]55. Then of course, in the process we had moved a substantial amount of equipment and Chicago people to Arizona. We gave the equipment to Arizona and the people were allowed to choose where they wanted to stay. Of course, Lou Battan, Jim McDonald, all the large-scale climate –

WK: Was Bill Sellers involved?

RB: There's another.

WK: Not Dick Kassander.

RB: Well, Kassander was involved, but Kassander wasn't really at the University of Chicago. We knew of Kassander and actually thought it would be good to get him to Chicago. We knew of him through Jim McDonald, Jim McDonald was in Chicago. Of course, McDonald and Kassander were together at Iowa. So, I resigned from Arizona and went back to Chicago and spent full time there next organizing the Project White Top which was a cloud seeding experiment in Missouri.

WK: I remember that.

RB: Project White Top became a very controversial project. All indications are that we in fact, decreased the rainfall very substantially.

WK: All eyes were on White Top by this time. It was kind of a [unintelligible], I think.

RB: Well, no one was really willing to accept that you could interfere detrimentally with nature. Somehow, we're optimists and we think that anything you do has to be for the good. In this case, I think the only reasonable interpretation is that we did indeed decrease the rainfall. That was very hard for the community to accept. Like the large-scale weather modifications in general, they are exceedingly difficult to prove because nature is so variable. Probably even today, there are some residual – what do I want to say – concerns, some disbelief that we did in fact decrease the rain.

WK: Maybe just disappointment.

RB: Well, clearly disappointment, yes. In fact, we've just finished the AMS monograph entitled, *Weather Modification; The Scientific Challenge*. I think it's one of the major scientific challenges that we have. There is a substantial amount of evidence that changes in cloud microphysics – which are very demonstrable. No one questions you can change cloud microphysics. The changes in cloud microphysics under certain circumstances can indeed alter weather systems in ways that are significant. We have not carried out cloud seeding research, weather modification research in the past several decades – well, that isn't true. There is some research going on, but the pace is slacking substantially. I think in fact, that it will come back because there's more and more evidence that anthropogenic weather effects are real.

WK: You mean the thing that you get down when from St. Louis or Chicago?

RB: That's correct. That's right. Or our agricultural tilling practices. That man is influencing weather in perhaps small but important ways.

WK: To say nothing of the greenhouse effect, which is a big one now. [laughter]

RB: To say nothing of the greenhouse effect, which may not turn out to be small according to predictions. So, I think that's a big field. Well, we have not said very much about the early days of NCAR or UCAR.

WK: Well, chronologically you are coming up to the point where you were involved in those early days of the academies research or study.

RB: That's right. My first direct contact with that area came in the summer. It must have been [19]57 or [19]58. I was in Florida working with working with Bob Simpson in hurricane studies. You see, we were carrying out seeding experiments in hurricanes. In fact, I carried out the first seeding experiment in hurricanes after the very famous Langmuir experiment of the late forties. I've forgotten the date when Langmuir released several hundred pounds, [laughter] probably of dry ice into a hurricane off the East Coast. The details of the release are obscure. In some of the reports, it almost sounds as though they released it in one big hunk.

WK: [laughter] Done much good.

RB: [laughter] That hurricane subsequently had a very erratic path and ended up making landfall in the Carolinas or Virginia and was probably the last of several incidents that led GE to conclude that cloud seeding research was just politically too dangerous. The liability issues were too high and forced GE to give up the Vince Schaefer-Langmuir type of research, which they had been doing. So, there was no further seeding of hurricanes for many years.

WK: Simpson.

RB: Bob Simpson came to the university and asked if we would undertake research in the microphysics of hurricanes and the seeding of hurricanes. Because he had a theory that seeding the rainbands outside the eyewall might enhance the vertical motions enough to siphon off the low-level inflow at a radius larger than the existent eyewall, and therefore reduce the speed of the winds.

WK: That was the hypothesis.

RB: That was a hypothesis. Our job was to release the ice nucleant. We chose silver iodide. In fact, we did indeed seed. I've forgotten the name of the storm at the moment. We seeded two storms.

WK: Is that [19]57, [19]58?

RB: It must have been around [19]57. By checking my diary and notes in Chicago, I could pin it down. The first experiment, (Chuck Jordan?) from Florida State was in the observing airplane, a navy constellation with the big radar down under the belly. That was giving us a continuous radar image of the hurricane and we were seeding the hurricane. The scopes were photographed, and the film was developed at Navy Jacksonville. The report from Chuck Jordan, both in the debriefing and subsequently, was that there was a major change in the eyewall structure of the hurricane in the time period, thirty minutes to a couple hours following our seeding. He was convinced that we had, in fact, made a major change in the hurricane. Interestingly, the film for that flight was lost in the processing lab at the Navy Jacksonville. We never did really quite understand just how or why. Then the following summer, we carried out another seeding experiment – again, by checking my notes, I could give you the names – in which, as far as I was concerned, there was relatively little evidence that we had done anything other than expend our silver iodide load. [laughter] Of course, the seeding experiments continued then for some time. But Chicago was not actively involved in them. Well, I was in Florida at a hurricane research group. We were north of Miami, Fort Lauderdale. There's an Air Force base there. In the summertime, I received a telephone call from Horace Byers who said that he wanted to know if I would take on a job as a staff person for a series of studies to be carried out to determine not the feasibility of a university corporation for atmospheric research, because that had already pretty much been decided. But rather to flesh out the concept to see what it might look like. So, I said yes.

WK: To be under the ages of the National Academy of Sciences.

RB: That's right. Actually, the mechanism was an NSF grant to MIT. Henry Houghton was the principal investigator of that grant. That grant provided funds for us to carry out a series of planning exercises. As it came down, Tom Malone was asked to chair that planning exercise, and he agreed. I was told he agreed providing [laughter] it would be based principally at Hartford, because he was the director of research for travelers at the time. So, that was acceptable to the committee. Bill Von Arx from Woods Hole and I joined Tom in a three-person group to carry out this planning phase. In a sense, Tom was representing the commercial application of meteorology, and Bill Von Arx was from a major government laboratory, and I was from the academic sector. The planning, of course, consisted of a series of meetings, workshops. My recollection is about seventeen or eighteen. Again, I could give you the details because I have all of my notebooks from that period.

WK: Must have been an exhausting process to have so many.

RB: Well, each workshop took a week. I'm sorry. The activities of a workshop took a week. The workshop itself was Tuesday, Wednesday, and Thursday. We held most of them in a mountain retreat that was owned by travelers. You took part in one of those.

WK: I remember the place.

RB: Monday was used for the arrival of the participants. Friday and Monday, we did our staff work and got ready [laughter] for the next one and that sort of thing. I commuted from Chicago to Hartford for that entire period. My wife and the kids would take me to Midway Airport about mid-afternoon on Friday. I'm sorry, mid-afternoon on Sunday. I would fly to Hartford and then spend the week in Hartford and return to Chicago Friday night.

WK: [laughter]

RB: Have Saturday with my family. Well, that went on. A few of the meetings were held at other places. We held a meeting at La Jolla. We held one at Yerkes, and there might have been another one held at a different location. But for the most part, they were held. The result was the *Blue Book*, which is history and mapping out of a laboratory that might serve as a focus for greatly enhanced research activity in the atmospheric sciences principally through this new laboratory that came to be known as NCAR with this.

WK: In the early days of the *Blue Book*, I get the *Blue Book* itself, there was quite a bit of emphasis on a big rotating tank facility. You remember that? Of course, you coming from the University of Chicago where Dave Fultz had such a rotating tank, did this influence this idea?

RB: Oh, I'm sure it did. I think these workshops involve the leaders of science. Each workshop was built around some science theme in the atmospheric and physical oceanographic sciences. I'm sure that the fact that you see the laboratory modeling of large-scale flows was in its heyday at that time and it clearly had played its role. Well, on the occasion of the NCAR twenty-fifth anniversary, I was leafing back through my notebooks from that period. Interestingly enough, some of the remarks which I have quotes around indicating that they are attempts to have verbatim quotation of remarks by scientists – many prominent, some have passed away – on their ideas as to what would become important in meteorology downstream. I remember one remark was that a certain well-known individual could see no earthly way in which airplanes and radars could ever contribute to the advancement of the atmospheric science. [laughter]

WK: This is in the late 1950s? Is it early or late?

RB: This is the 1950s. It was a well-known meteorologist. [laughter]

WK: Of course, we never did have a general circulation simulation lab at NCAR. I remember when I got there asking people, "Have we missed something?"

RB: There were two major problems.

WK: I got the message that probably we were not at that stage.

RB: Yes. I think that's right. There are two major problems in terms of simulating the atmospheric flow. Of course, the so-called dish-pan experiments went a long way in helping to straighten out the thinking about large scale flow. I should say it may be possible. But it didn't seem feasible to obtain a flow that really simulated the atmosphere in the sense of a spherical shell with inward directed gravity. Spherical shell, you can generate pretty easily. In fact, Dave Fultz has one in Chicago. But we could not easily get an inward directed force component that simulates gravity. Gravity plays such an enormous role [laughter] in the motions of the atmosphere. Of course, studies of that kind were exceedingly useful in elucidating principles of fluid flow. GFD in its more abstract meaning, been greatly advanced. The reason that use slowed down was the inability to make sufficiently detailed measurements inside of the flow channels without disturbing the flow. Even today, if you could measure density or make measurements from which you could calculate density distributed throughout the space of the fluid flow, you could carry out experiments that would be very valuable for further elucidation of basic principles of GFD. Now, whether or not the new laser techniques, the laser tomographic techniques, for pinpointing spots inside of the fluid and determining three dimensional motions and conceivably temperature, it might in fact revive the fluid type experiments. But in my view, they came to an end for the two reasons. As far as simulating the atmosphere, we couldn't simulate the spherical geometry with an inward directed gravity. As far as illuminating fundamental processes of fluid flow, we couldn't make the measurements internally in the fluid itself without disturbing the fluid.

WK: Do you not think too, the enormous advances in computers and the ability to model it theoretically replaced to some extent the dish-pan experiments?

RB: I think it did. But I think it's unfortunate because I think it illustrates a problem that permeates our science, and that is to think that you can generate new truths from computer models. I personally think that cannot be done. I think that the computer models in general, they'll only respond to what you put into them. The basic physics you put into them is all that'll ever be there.

WK: [laughter]

RB: They'll carry out enormously complicated and involved calculations using whatever code you put in. But if you have your initial physics wrong, you'll never find it out except by comparing the computer output with laboratory or field observations.

WK: I remember Dave Fultz using that as one of his big arguing points, that namely, we can test our theory with a real fluid.

RB: I think you have to do that. I think that physics is built on that principle, not without solid reason.

WK: [laughter]

RB: So, I think that I wouldn't deprecate the enormous advances made in theoretical meteorology, and the value of the computers is absolutely indispensable. But I do think that they may lull a person into some comfortable feeling that we're solving the problems.

WK: You have been in and out of NCAR ever since it was created. Are there some things that you are disappointed in that NCAR did not adopt from the *Blue Book*? We did not adopt the dish-pan laboratory. Are there any other things that you see in NCAR that you feel where we missed the boat?

RB: Well, I think there are some places where our original thoughts and even my thoughts still suggest that it turned out a little differently than we had hoped. But before, I think along those, I would say that I've been enormously pleased with NCAR. I think that, no, it was unquestionably the biggest thing that has happened in the atmospheric science in my lifetime and has allowed us to quantum leap forward in the advancement of our science. I have spent a lot of time working for NCAR, and I don't regret it because I think that NCAR is kind of the focal point of world meteorology. But there are a few places where our early concepts, and subsequent observations, suggest that we may do a little fine tuning. In our early days and throughout the workshops and the discussions that Tom and Bill and I had, we saw NCAR as involving a substantial number of long-term visitors. That the full-time staff would in fact be perhaps not more than two thirds of the total complement. I don't remember the exact numbers we used, but the thought was that NCAR needed a strong cadre of resident scientists that were world leaders in their own right. That there should be a major opportunity to allow world-class scientists from outside of NCAR to spend substantial periods of time working at NCAR, or to provide opportunity for NCAR scientists to spend substantial periods of time working with other peer scientists around the world. We saw that as good from the point of view of extending the good things about NCAR to worldwide, making NCAR accessible to worldwide science. Furthermore, as a buffer against short-term budget fluctuations, I have felt that we have tended to put too much emphasis on the long-term "tenured" appointments at NCAR. I really think that it would be healthier if there was an enhanced visitor policy. I think that within the divisions at NCAR, there are enough differences in the nature of the staffing regarding visitors and non-visitors to show the probable value of enhanced visitor program. I think over the years, HAO has had the strongest visitor program. Perhaps it's unfair to say that they are the most illustrious of the NCAR divisions, but clearly, they have illustrious worldwide reputation in their area. The other place where we need some adjustments, I think has to do in the field of education. Let me back up to get started in answering it. You see, in the early days throughout the [19]50s and [19]60s, we at Chicago fielded our own airplane and our own radars. Gradually came to the realization that the costs were escalating, the requirements for the science were becoming harder and harder to meet. The more you know about the subject, the more detailed, the more difficult the measurements needed to make further advances, the requirements on personnel in order to maintain the first-class sensing systems with the advancement of the instrumentation possibilities. These things meant that I was spending increasing fraction of my time paying for an airplane and for radars, which I couldn't use full time, even if I could maintain them at the forefront of the measurement capability and where the research itself demanded that the capability be. So, we reached the decision to go out of the airplane and radar business and give all of our equipment to NCAR. So, the university turned over to NCAR, all of the hardware associated with their field efforts. Some of it, the cloud physics instrumentation equipment, in fact, were the first instrumentation that NCAR had for airplane measurements in clouds.

WK: That is true. I remember that.

RB: In fact, an anonymous reviewer on a recent proposal that I submitted to NSF made the statement that it's true, but not generally realized that the University of Chicago is responsible for founding FOF. [laughter]

WK: [laughter]

RB: Well, at any rate, when we had our own airplane and our own radars, it was commonplace for the graduate student and undergraduate student to become involved in field efforts in a routine and extended way, to sit in the right-front seat of an airplane and fly through thunderstorms, to sit at the console of the radar and twiddle the knobs. Be involved in setting up this equipment in the field, insulation, fixing the equipment. Then analyzing the data and recognizing that some little problem that you didn't fix in the field comes home to haunt you, and you wish that your measurements were better. We have lost that. By and large, there is very much reduced opportunity for the graduate students and undergraduates in meteorology to have a hands-on field experience with equipment. Now, it is still true that you can do that at the University of Washington and at Wyoming. Those two universities have the major programs today that are feeding students into the instrumentation observational parts of meteorology. None of the rest of us have students anymore in those areas. I think that the reasons for abandoning our airplane and radar efforts we're sound. I don't regret that. But we haven't found a way to replace the value of having those in the training effort.

WK: Could NCAR include those students and give them the kind of training that you used to give them?

RB: Oh, they could indeed. I think it's merely a matter of setting up the mechanisms for this. I think for example, at the summer colloquium and workshops is a step in the right direction. But perhaps we need to think more about the subject matter of those. We often hold them in the area of computing. But the university capabilities in training the kids in those areas are far greater than it is in the observations and instrumentation sides of meteorology. So, perhaps our balance on the subject matter for our summer colloquium should be rethought. I think we need ways to pick up promising undergraduate and early-year graduate students and then train them in field projects. It's difficult to entrain them in somebody else's project. You see, when we took students into the field, they were working on a project which then became their own thesis subject. They were taking their own data as well as data that would be used by somebody else.

It's difficult to do that. Although students from institutions that go into the field with NCAR support, perhaps could be factored into a greater extent than we do. I think that some careful thinking would find ways to improve the opportunities for contact of students with technology.

WK: That is a very important point, Roscoe.

RB: Those are the two areas where, I think, I won't say I'm disappointed, but where some fine tuning of NCAR should be.

WK: In terms of being the long-term visitor, the senior visitors, and the training of students in instrumentation and field programs.

RB: Yes. I don't think that we'll ever, in our lifetimes, reach the point where we don't need to carry out field programs.

WK: Oh, I am sure that is true. [laughter] We have got an awfully big atmosphere to explore. Our tools get better and better. We can go further and further, but we will probably never see the end of attempts to get better observations.

RB: Yes. Now, where are you entrain the student is an important question. If we were to send students to NCAR for a full summer of direct participation, say with one of the field groups, it is not likely that they would spend it in a subject area directly related or intimately related to their thesis work. In which case, it ought to be done at a fairly early stage in their training. On the other hand, if one of my students could spend a period of involvement in the field with your boundary layer studies, even though it's not directly related to the data they would be using, it's the same subject area. Talking with the principal investigators and seeing equipment, using it would truly be useful to that student. In the general area of education and training, we've made NCAR a superb research institution. We've taken steps. There's a lot of things NCAR does that is directly related to education and training. It really is. It needs to stay that way or to be enhanced.

WK: Well, that is true. Of course, we do have students there. Particularly in the summertime, we have the postdoctorals. We have the graduate research assistants who we see through to their Ph.D., cooperative effort between some staff member at NCAR and the university. So, we do have these. What you are saying is we do not have enough of them, I guess.

RB: I guess that's my feeling, particularly in the case of instrumentation, the interface between developing and using high technology, and the interpretation of data analysis, and planning of experiments.

WK: Well, that is a good point. Roscoe, I would like to turn back to something which I went by a little fast and it intrigued me. After your early days of cloud seeding under this Air Force program, you mentioned that you were already pretty well convinced, at least in the Midwest, you were changing the large-scale systems. You were not increasing rainfall, and yet you went on for many years after that still trying to do this. So, apparently you still had hope even after those early experiments had shown rather negative results.

RB: The ACN experiments clearly showed that you could modify the microphysics of convective clouds. The water spray seeding carried out in the tropical queue indicated that you could substantially modify the precipitation processes. We were not able to demonstrate in the ACN studies in the Midwest that we altered the precipitation processes in the Midwestern queue.

WK: You mean increased or decreased the precipitation.

RB: Precipitation of the ground, right. We couldn't follow the seeding signature, if you wish, to that extent. There was no question, but you could alter the microphysics. You could increase the number of ice crystals and get them started growing. All of the theoretical understanding suggested that the coalescence process, which we knew we could modify in the tropics, and the ice crystal mechanism, which we knew we could initiate in the Midwestern clouds, the theoretical understanding said these two things are the beginning points of precipitation development. So, it seemed rather reasonable to continue experiments in that to further our knowledge. In addition to that, the literature, it was filled with claims of rather substantial changes in precipitation as a result of seeding. There was no existent observation or theoretical knowledge suggesting that it couldn't be done. None. So, we were intrigued with the possibility that we would go into the field, in what led to be White Top, and implement an experiment that would correct the deficiencies that were recognized in the commercial cloud seeding efforts. Provide measuring systems and detection systems that weren't flawed by the possibilities of unintentional bias or intentional bias [laughter] from the operators. Show to what extent things that some of the commercial cloud seeders were claiming to happen could in fact be demonstrated. So, we fired up the operation in Missouri. We put out a network, for those days a rather substantial network, of rain gauges for sensing precipitation. We arranged for the group at the University of Missouri to operate the rain gauges, collect all of the data, and reduce all of the data to sets of tabulated numbers, which they published before we at Chicago saw the data. They were denied access to the knowledge of which days we operated, whether we were even in the field operating.

WK: They were really well-designed experiments.

RB: We put a radar in to map the area of precipitation. Well, by coding the day on the radar film, there are no dates on the radar films, just a coded day. The code was known only to the radar operator and the statisticians at the University of Chicago. The radar operator was denied access to the knowledge of what the day was like, what we were operating, and we weren't operating. Those two streams were not hooked together until the analysis was finished. The data were made available to the public at the same time we had it in Chicago. One of the reasons Chicago had so much publicity was just that, that it could be analyzed by others outside as early as the experimenters got ahold of the data. In fact, the statistics group at Berkeley found this enormously rich data set and spent a lot of time in it.

WK: Jerzy Neyman.

RB: Jerzy Neyman and his group, Elizabeth Scott. Jerzy Neyman, in fact, preceded us in the publication with the conclusion that an impeccably designed – he uses the word impeccably. I don't know what impeccable experiment is.

WK: [laughter]

RB: "Experiment proves without a question that seeding decreases rainfall." Jerzy changed his mind on it years later because he found several things. He found that the rainfall in Kansas City, twelve hours before our seeding, had a very high association with the fact of our subsequent seeding.

WK: [laughter]

RB: So high in association that his understanding of chance statistical tests would force him to believe that there had to be a hidden connection between these.

WK: [laughter] Somehow the atmosphere anticipated your seeding.

RB: Something. Furthermore, the five hundred millibar temperatures at Columbia and Little Rock were systematically a few tenths of a degree different. Warmer, I think, on days that we seeded than it was on days we didn't seed. This was true in the 6:00 a.m. [unintelligible], when we then commenced our seeding at 10:00 a.m., three, four hours later. The significance of that difference, as he computed it, was several zeros so large that he was inclined to believe that there had to be collusion. So, he published a paper in science, which says in effect that he has reached the conclusion that we tampered with the data. Now, what he said was toned down considerably because he had some considerable difficulty getting the paper published. I first received a copy of the paper from science from AAAS wanting to know if I would review it. [laughter]

WK: Essentially like asking the fox to guard the chicken coop. [laughter]

RB: [laughter] Of course, I objected substantially, and a number of people were brought into the fray. But I think Jerzy never did accept the possibility that we didn't bias the data intentionally. Because we somehow had a notion that seeding wouldn't work and therefore, we could pick the days which we were going to seed. I don't know if he ever realized the extent of forecasting skill, which he attributed to us [laughter] in that. But it was only in about 1974 that we put into the literature a substantial series of articles that effectively pointed to what the problems were. You see in, I think it was [19]74, but could be wrong a year or two, but I think it was [19]74, the American Statistical Association asked me to give their major address at a meeting of their association on the subject of cloud seeding. I did, and I wrote what I thought was unbiased and dispassionate paper as I could.

WK: [laughter]

RB: I don't know if you know the publishing technique of the Journal of the American Statistical Association. They invite a paper, but they tell you in advance that it will be sent out for normal review, standard type of review. That the comments from the reviewers, assuming the editors decide yes to publish on the basis of the reviews, then the full comments of the reviewers, signed, the reviews are all signed, and a rebuttal from the author is published in one issue.

WK: That is interesting.

RB: So, my paper was sent to, I think it was ten or twelve reviewers. The reviewer's comments bumped some two to four times the length of my original paper, maybe more than that.

WK: [laughter]

RB: Even in this published form in the *Journal of the American Statistical Association*, there are about seventy-five printed pages discussing this issue. My original paper, the remarks by the reviewers all signed, and then my – it's not really a rebuttal – it's my comments response. I think it clearly sets forth the fact that the Berkeley group became victims of the multiplicity issue which is so common in our field. That we use theory of probability to try and make estimates of the likelihood that such and such would've happened under such and such circumstance, without recognizing that those calculations are very difficult to interpret. They're almost not applicable. That's an overstatement. In situations where you determine after the fact how you're going to carry out the analysis, what tests you're going to employ, and so forth. Only one set forth before the fact, "Can you even apply the method, the theories of probability for this purpose?" After the fact, there is no way of knowing what you have considered and rejected, every association that you've considered and rejected, even subconsciously. Obviously, if you pick the association where the evidence for that particular sample shows the high association you're going to – if you pick pairs of variables for which a restricted sample shows high association, every statistical test will grind out numbers indicating significance.

WK: If it is a short sample, you mean.

RB: Of any sample. If you carry out an experiment and you say you have a hundred seeded and a hundred not seeded events, now you could look at a lot of things. You could compare the seeded and the not seeded in an enormous number of variables. Only those variables selected before you ever carried out the experiment, and analyzed in the way that you indicated before you carried out the experiment, only to that pair or that test can you fully use the power of probability. Because you see, in this instance if you have seeded and not seeded days, well what attributes of the meteorology are you going to consider? You're going to consider rain, you're going to consider the amount, the timing, the distribution. You're going to consider wind directions. Rain represents energy released to the atmosphere. You're going to consider the thermal structure of the atmosphere. You're going to consider dynamic structure. You're going to consider the temperature at five hundred millibars before and after. Each and every one of those is a possible test. If you examine all of those tests and then choose one which happens to have a strong parent signal, either for or against your hypothesis, then it's clear that the probability of getting that, as you would compute it from normal probability theory, is totally wrong. You could take two things that have no association in a large, complex data set, such as things dealing with the atmosphere. We could just go back in the calendar and say, "Let's pretend that I seeded on all of the odd number days last summer. Now look to see what in the atmosphere had systematic differences between the even and odd number days." You'll find a lot in the mid…

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