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TAPE RECORDED INTERVIEW PROJECT

Interview of Roscoe R. Braham
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Interviewer: Stephen Cole

Cole: This is Steve Cole, freelance science writer, interviewing Dr. Roscoe Braham on Wednesday, June 19, 2002, in Washington, D.C. Dr. Braham is attending a National Academy of Sciences Weather Modification Panel meeting this week.

Well, the first big question was, I guess, what's your assessment of Langmuir's lasting contribution to meteorology, if there was a lasting contribution? Most significant...or the contribution to the science, or just impact on the field.

Braham: I would say his big contribution, his big impact on the field, would have had to be the work he did in weather modification, particularly in calling to the attention of the world the possibility that one might induce changes in the physical structure of clouds.

Cole: Right.

Braham: Now, most of that work that he did, in my opinion really--the work itself really didn't advance the physics very much.

Cole: OK, that's good to know.

Braham: One of the--one of his contributions, which has stood the test of time, is a calculation that he did with a lady by the name of Blodgett.

Cole: Katharine Blodgett.

Braham: And which they calculated collision efficiencies between drops of different sizes. And although the range of sizes he chose does not cover the size range ordinarily involved in natural raindrop growth, that material certainly set the stage for the collision--studies of the collision of raindrops with airplanes --

Cole: Right, right. Yes.

Braham: -- towers, big radio towers, and so forth.

Cole: Right.

Braham: And some of that work is still regarded as basic.

Cole: OK. That was the work they did...was that the Mount Washington work? I think they did some of that preliminary droplet sizes at Mount Washington.

Braham: Well, these were calculations carried out in the GE lab.

Cole: Oh, OK. They did some -- like before, like in '44 and '43...

Braham: Early days of the war, I would say.

Cole: They were doing some measurements on that.

Braham: Yes, right. Right.

Cole: OK. But the calculations--I understand.

Braham: Right.

Cole: I understand. Another big question about something we were talking about before. One of the interesting things for me has been, again, that Langmuir was an outsider to meteorology, devoted an extremely large amount of energy in the last decade of his life to this topic. This is asking you to speculate, but what -- why do you think he did that?

Braham: I guess I don't really know.

Cole: No, it was just a question.

Braham: He certainly enjoyed the publicity that resulted from the discoveries. And particularly this -- the 1947 seeding of a stratus cloud over western Massachusetts.

Cole: OK.

Braham: I think Vince Schaefer actually did that work.

Cole: OK. That was the original – original--

Braham: The original airplane seeding --

Cole: Yes, yes.

Braham: -- of stratus clouds. Of course, that attracted an enormous amount of attention, and I think Dr. Langmuir enjoyed that. And I think he sincerely believed that major changes in natural phenomenon of the scope of weather

might indeed be foreseeable. I think he believed it was closer at hand than has proven to be the case.

Cole: Yes, yes. True.

Braham: But I think his intentions were solid. I think he made some mistakes.

Cole: What were some of the mistakes you think that specifically he made?

Braham: I believe his forte was chemistry, and particularly surface chemistry. And I think he did not appreciate the vast range of scales in the atmosphere, and the vast range of conditions encountered in the atmosphere. He also was accustomed to the laboratory where you could have -- or you could repeat an experiment. And if, in the course of your experimentation, one of the trials goes wrong because some equipment wasn't quite what it ought to be -- maybe the voltage was a little too low or (inaudible) -- you could throw it out.

Cole: Yes, yes.

Braham: And you end up with a series of "identical" -- as identical as you can have and recognize.

Cole: Right, right.

Braham: But in the atmosphere there, you can never have an identical experiment. No two clouds are identical.

Cole: Yes, yes.

Braham: With all of the efforts one might go to in order to try and select comparable situations, there just are no two clouds alike, and so if you really think philosophically, you cannot repeat an experiment in the atmosphere.

Cole: Well, and that seemed to be the bane of the whole field.

Braham: Now --

Cole: To try to get that repeatedly.

Braham: And so the meteorologist has to be content with a sort of mixture of evidence. You would like physical cause-and-effect evidence to be as statistically reasonable as you could, and you would like the statistical evidence to be as reasonable as you can get. And these two _____ are different realms.

Cole: Yes, yes.

Braham: And it's very difficult.

Cole: Clearly. The original discovery, if you will, of the first seeding in western Massachusetts that Schaefer and Langmuir did in that fall--what was the initial reaction in the meteorological community when that came out? I believe they presented some results at an AMS meeting, or some meeting shortly thereafter. But how would you characterize how the community responded to that at first?

Braham: I'm not sure that I had my finger on the field as a whole at that time. My guess is that most meteorologists were probably amazed that you could write the GM emblem in the surface of a stratus cloud. And most, I think, were probably skeptical that the inferences drawn from that to the effect that you could cause wide ranging and highly significant changes...they were really skeptical of that.

Cole: OK, OK. When did you... I know that you were working in that period, '46-47, on Project Thunder --

Braham: Thunderstorm. That's correct, yes.

Cole: Thunderstorm. When did you first become aware of this field? Because it sounds like shortly thereafter you did get involved with some of the seeding activities.

Braham: I first met Dr. Langmuir in the summer of 1947 in Wilmington, Ohio. I was working on the Thunderstorm Project. The Thunderstorm Project had an advisory committee, and Langmuir was a member of that advisory committee.

Cole: Oh, really? OK, OK.

Braham: And in the summer of '47, Dr. Langmuir and Vince Schaefer came to Wilmington, Ohio to visit the project, and I think also to tell us these startling events that he had discovered.

Cole: OK. Right.

Braham: And the plans he had for a major thrust in that area.

Cole: Right, yes. He was--

Braham: And during that visit, he asked me and Lou Battan to quit the Weather Bureau and come to work on his project --

Cole: Oh, really?

Braham: -- which was obviously going to be far more important. Why worry about the thunderstorms when the prospect existed you might be able to eliminate them, for example?

Cole: OK.

Braham: And I confess that with a science background, I was impressed with the physics that might be involved in what he was doing. But both Lou and I felt we were committed to the Thunderstorm Project.

Cole: Right, right. Because that was the second year, I guess, of the--

Braham: That was the second field --

Cole: Field.

Braham: Field year. But we had another two years of work to do in the analysis of the data, after which I went to Socorro, New Mexico to work with Jack Workman and Steve Reynolds.

Braham: And again bumped into...

Cole: Yes, so he was the -- Langmuir and Schaefer around there.

Braham: They were... Langmuir...

Cole: Neck of the woods, as well.

Braham: Because at Socorro, we were conducting the seeding one day a week that --

Cole: Yes, the periodic seeding that they were...

Braham: -- was a part of his periodic seeding experiment.

Cole: So you were involved at that period at that period...

Braham: I was there in the same laboratory.

Cole: OK, OK.

Braham: I was not involved in lighting the burner or that sort of thing. But I was there in the same laboratory, and it was common knowledge what we were doing.

Cole: Yes, yes. No, I had a question about Workman coming up. About the Project Thunderstorm, then--so Langmuir and Schaefer's visit was informational? To collect information for themselves and to give...

Braham: Well, presumably. At least they told us about the work that they were doing.

Cole: That they were doing. OK. Did their visit have any impact on the program itself in terms of what you all did?

Braham: On the Thunderstorm Project?

Cole: Right.

Braham: Not that I'm aware of.

Cole: OK. Because one of the things I... I might have to check my notes to see where I saw that, but I was under the impression that during that summer, the Weather Bureau did some seeding experiments of its own. I think I've seen commentary from Francis Reichelderfer saying--if I have the time right, said, "Weather Bureau tests of seeding" [I think in that time frame] "showed no effect."

Braham: They...

Cole: --as early as that summer, and I didn't know whether that was as part of the field activity you all were doing, or some other--it may have been done completely separately.

Braham: I don't remember precisely the date. But roughly that same time, the Weather Bureau fielded a so-called cloud physics project.

Cole: OK, right, right. And that was--

Braham: Which involved--which was headed up by Ross Gunn, and involved seeding clouds in several different locations, but certainly in the Ohio -- in the Midwest.

Cole: Right, right.

Braham: They observed changes in clouds similar to that reported from seeding of the stratus. They also reported that you could open holes in clouds, but principally, during the phases of the cloud lifetime when the cloud was dissipating anyway.

Cole: Anyway. OK.

Braham: And they--don't remember exactly how they stated it, but the essence of the report was that it seemed unlikely --

Cole: Right.

Braham: -- that major changes in the weather would ensue from seeding.

Cole: I think the confusion I had was that those reports were reported as having done some seeding in the Midwest, and I know -- and then you all work in Ohio, and I thought, "Well, maybe..." OK. But that was a separate activity?

Braham: Yes.

Cole: I just wanted to be...

Braham: But the Weather Bureau also worked off the West Coast, off the Washington/Oregon Coast, and I think they also worked in Gulf.

Cole: OK.

Braham: And they may have worked off the East Coast.

Cole: Right. Yes, there were a couple of--bt that was separate from Thunderstorm. OK. What the... When... So just to be clear, Schaefer and Langmuir came, and they actually then were recruiting people, I guess --

Braham: Yes, they were --

Cole: -- to come work with them.

Braham: -- they were not reticent to increase their staff.

Cole: I guess they would accept --

Braham: They were excited about...

Cole: Oh, yes. And they would have just... I think in February of that year, they received the Project Cirrus go ahead from the...

Braham: That's about the time Project Cirrus was firing up.

Cole: Yes. It's getting to that. What was--again, at that point, when you were on Project Thunderstorm, you were working as a Weather Bureau employee?

Braham: We were employees of the Weather Bureau.

Cole: OK. What was the Weather Bureau's initial reaction? Or from your perspective, how did they react to the initial finding in that first year when everything was--

Braham: I really can't tell you.

Cole: OK.

Braham: We were employees of the Weather Bureau. We were stationed at the University of Chicago. Not in the mainstream of the Weather Bureau's activities at all, and so I can't say.

Cole: OK, yes. Aside from that interaction with Langmuir, what other--and I want to hold off on the Workman/New Mexico one for a minute. But what other kind of interactions did you have with him during the rest of his life?

Braham: Essentially none other than the periodic seeding.

Cole: Well, then let's come to that. That was in--I guess they were starting the periodic seeding--it sounds like late '49 and then early '50 was the--

Braham: I thought it was a little later.

Cole: OK. The notes I had was that he did a preliminary paper on it in November '50 in front of the National Academy?

Braham: November '50, yes.

Cole: 1950.

Braham: Right.

Cole: So it was... And that was several months...

Braham: No, I think it was perhaps the fall of '50.

Cole: OK. In that same...

Braham: I don't know when he may have discussed this possibility.

Cole: Yes. OK.

Braham: But...

Cole: Well, tell me, then--I guess from--so you were at Socorro?

Braham: At Socorro, right.

Cole: In that time period?

Braham: Right.

Cole: What were the interactions then with the seeding project? How was that conducted? I understand Workman's staff were doing the activity?

Braham: That's correct. That's right. I would say not very much attention was given to it. I think we were doing something that Workman had agreed to do with Langmuir.

Cole: Yes.

Braham: Until Langmuir began to talk about the heavy rain conditions in the Midwest as being -- having their origin in his periodic seeding.

Cole: Right, right.

Braham: And about the time that Kansas City was in flood, and it was wet throughout that part of the Midwest. And as I recall, Dr. Langmuir was not hesitant to indicate that this was a result of the periodic seeding.

Cole: Where he had pointed--.

Braham: The governor of New Mexico called Jack Workman up to Santa Fe on one particular occasion, and Steve and I went with him.

Cole: Steve who?

Braham: Steve Reynolds. And the essence of the governor's message was that we either had to quit periodic seeding, or we had to give up the state funding for the institution.

Cole: Oh, boy.

Braham: Because the state could not accept the liability inherent in Langmuir's statements that the floods in Kansas City were due to our seeding.

Cole: The seeding. Right, right. Yes.

Braham: It might have been his project, but we were doing the seeding.

Cole: Yes, you would be liable. That's right. (inaudible).

Braham: So as a result of that, the periodic seeding was stopped.

Cole: OK, OK. As a direct result of getting that word from the governor?

Braham: Well, that and following conversations. I'm sure Workman talked with Langmuir.

Cole: Sure.

Braham: I know that there was an AMS function meeting in New York, I believe. I'm a little hazy on this. And I know that Workman and Reynolds, and I went to Schenectady and talked with Dr. Langmuir. And that was--I guess that was the last time, maybe, I saw Dr. Langmuir.

Cole: And that was in this period of --

Braham: When we were...

Cole: -- '50 when it was...

Braham: We were trying to --

Cole: --seeding was still going on?

Braham: Right, right.

Cole: OK, OK. One of the things I had read -- and I believe it was in a history of New Mexico -- Workman's Institute, or--what's it called now? There was a story -- kind of a review book about kind of the development of his land and cloud seeding, which is one of the chapters on the work they did. It's described--and I just want to see if you have any insight on this. That at the point the periodic seeding was going on, and there were discussions, obviously, of what Langmuir and Workman and his staff, about what it meant... And when they got to the... When it became... When Langmuir said, "I'm... You know, I think you can see the signature in Ohio, and I'm going to go public with it," in a talk or paper, I don't know which, that at that point either Workman personally, or the group in Socorro said, "Well, we can't support that."

Braham: Yes.

Cole: And there was some kind of break at that point.

Braham: Yes.

Cole: What -- you were there or near there.

Braham: I was there.

Cole: What was it? What happened there?

Braham: I was not a party to the discussions between Langmuir and Workman, so I just can't comment.

Cole: OK. But after that --

Braham: I know that we did quit seeding.

Cole: OK. That was the thing that... I mean, what it made it sound like to me was that--whether personally with Lang -- Workman or not, but that claim of--that specific claim that cloud seeding has the impact in a specific locale was just one of those claims that many meteorologists just kind of said, "That's it. I just can't go along with it."

Braham: Oh, I think...

Cole: What was the reaction to that?

Braham: Well, I think very few traditional meteorologists accepted that at all.

Cole: OK.

Braham: They said, "It's just nonsense."

Cole: OK.

Braham: But I think it is true--I believe it's the case that the Weather Bureau, Dwight Klein, and some other person, undertook an examination of past weather records, searching for evidence of a seven day periodicity. And several were found, but none with quite the persistence --

Cole: Right, of that...

Braham: -- of that period following with -- during and following the seeding.

Cole: OK, OK.

Braham: And so it's one of those tantalizing things. I think the majority of meteorologists still would likely not give it very much credence.

Cole: Yes, that's what it sounded like. You mentioned -- and I don't want to talk you much longer for this. I know we only said a half an hour, and that's fine. This was from the tape from '87 [ed.: UCAR/NCAR Oral History Project #36] . You began some work with hurricanes in the late '50s with Robert Simpson.

Braham: With Simpson, that's right.

Cole: And I think that the statement on the tape was that that project was the first time since the Langmuir Project Cirrus, either in '47 or '48, that hurricanes have been seeded again.

Braham: I think -- that's my understanding.

Cole: And I'm sure it is. But --

Braham: That's my understanding.

Cole: How did that come about? That--I guess, well first, what was your activity with that seeding at that time?

Braham: As I recall, Dr. Robert Simpson was head of a hurricane research group in the Weather Bureau. A forerunner to the Hurricane --

Cole: Right, right.

Braham: -- to Stormfury.

Cole: Right.

Braham: They were based in an Air Force base. Lauderdale, I believe.

Cole: Florida.

Braham: In Florida.

Cole: Yes.

Braham: And Dr. Simpson, and perhaps others, had come up with the thought that if seeding enhanced the vertical motion in conductive clouds, and if you could seed the eye wall clouds of hurricanes, you might enhance the vertical motion, enlarge the eye, spread the energy of the storm out over a larger area, and reduce the intensity of it.

Cole: Right.

Braham: So he had to have somebody to seed the clouds. The Australians had developed airborne burners, units to burn a silver iodide-acetone mixture from an airplane. And they'd used it successfully in seeding some clouds in Australia. The University of Chicago--in fact, my lab in Chicago had a contract from the Weather Bureau to develop measuring instrumentation and cloud seeding equipment --

Cole: Oh.

Braham: -- to mount on the Air Force WD50's to seed hurricanes.

We essentially failed. We... The Australian burners could not cope with the heavy rate it was ingesting water in the hurricanes. And we had great difficulty --

Cole: Just in terms of making that--

Braham: -- sustaining a burner operation.

Cole: Oh, OK.

Braham: But we did...

Oh, I think -- check in there. We did attempt to seed on two hurricanes. And in hurricane Debbie, I believe it was, or Daisy--Daisy or Debbie.

Cole: OK. In that '57, '58 period.

Braham: That's right.

Cole: There'd be one.

Braham: We managed to get the burners to run quite a substantial period. Not really all of it in the eyewall like we wanted. But it wasn't a bad first try. The Navy had a "Connie" -- you're resurrecting history here. This was part... The Navy was part of this effort. They had a reconnaissance Connie--a Constellation--with radar mounted underneath, and they were flying surveillance, and it was

their radar scopes that were used to determine where in the storm the seeding plane would fly, and also to observe what happened. A professor from Florida State was on the Connie along with the Navy. (Trying to think of his name.) After the flight, it was reported by the crew of the Navy Constellation that substantial changes had been observed in the hurricane.

Cole: With the radar?

Braham: With the radar.

Cole: The radar, OK.

Braham: But for reasons that we had never understood, the radar film got lost in the processing lab -- the Navy processing lab at Jacksonville.

Cole: Nobody ever --

Braham: So...

Cole: Nobody except the crew ever --

Braham: That's correct. So I guess we were the second to try to seed a hurricane.

Cole: Oh, OK, OK.

Braham: I guess we failed, actually, in seeding it very much.

Cole: What are the...

Braham: And I doubt very much if anything happened.

Cole: One of the reasons I asked that was I've been looking into some of Langmuir's letters and papers, which are at the Library of Congress, and it seems that his interest, at least as stated in some of these letters, in the last--in '56, '57, I think he's in the last years of his life--was -- he stated, "My interest is now in hurricane modification." I don't know what he was doing, or if anything, but for some reason, he seemed to, by the end of that period, focus on that. I think it was after the Orville Committee -- the President's Committee had started to have some -- what Langmuir viewed as positive findings _____ about hurricanes. But...

Braham: Well, he had one or more expeditions into the tropics. I think he had worked in Honduras.

Cole: He did. Or seeding early.

Braham: Or seeding...

Cole: Probably --

Braham: Towering convective clouds.

Cole: Yes, in the early period.

Braham: And thought that he was responsible for explosive growth in those clouds, yes.

Cole: Yes.

Braham: When we were on the ACN Project flying out of Puerto Rico, our objective was to initiate the precipitation process, which is really different from whether you cause explosive growth. Our objective was to initiate precipitation through water drop seeding, and we took Dr. Langmuir on a flight, and let him select the clouds --

Cole: Oh, really?

Braham: -- we were going to work on.

Cole: Yes, OK. As part of that--that was the--that was on the other tape that you were describing the ACN Project--?

Braham: Every one of the clouds he selected... Well, our objective was to seed clouds which did not have precipitation, but were very likely to develop precipitation. And then the question was does a water drop seeding actually enhance precipitation.

Cole: Right.

Braham: This, you see, follows directly from his paper where he calculated that seeding with water drops would enhance precipitation.

Cole: Right. The coalescing--

Braham: And Bowen carried out an experiment –Taffy Bowen in Australia--carried out an experiment and reported that this, in fact, would increase precipitation. What we were--that was the background to the work we were doing in Puerto Rico. But when we gave Dr. Langmuir the job of selecting the clouds for experiment, every one of them had long since developed precipitation.

Cole: Oh, before the seeding.

Braham: And they were big clouds.

Cole: Oh, OK.

Braham: So we got nothing at all out of the flight which he directed. But...

Cole: And this was in the mid-50s, I guess? This would have been... and this ACN Project was in the mid to early '50s?

Braham: A little earlier than that.

Cole: Was it? OK. OK. And how did his involvement come about? It was just he thought, "We're doing this field campaign…"

[BREAK IN AUDIO]

Braham: Well, this project was an Air Force, Navy, Weather Bureau, government project, and he was on the advisory board.

Cole: OK, he was on -- oh, for that, as well.

Braham: Particularly with his interest and enthusiasm.

Cole: Sure, sure. OK.

Braham: He was on all of the (inaudible).

Cole: That's what, again, amazes me. He was just everywhere, and involved with

Braham: Oh, he was an _____. There can be no doubt that he was an _____.

Cole: Yes. Well, I've taken up the time for this time. Thank you. Thank you, Dr. Braham. End of the first interview - go to the next side of the tape

END OF TAPE 1, SIDE 1

Interview of Roscoe Braham

TAPE 1, SIDE 2

Cole: Part two of the interview with Roscoe Braham by Steve Cole in Washington, DC. Part two is on Friday, the 21st of June 2002.

OK. Langmuir: a little bit more on Langmuir. In 1948, he had a paper come out --we mentioned this a little bit last time -- where he presented his chain reaction theory of water droplets and the role... His theory in warm clouds coalescence happen in a certain manner. In reading your history of rain formation, it was clear that there was other people doing work, and other theories were in existence at that time on the collision coalescence process. Could you give me a sense of the context Langmuir's paper came into? He clearly was coming out from outside the meteorological community, but at that time it sounds... Again, we're referring to your chapter on the history of rain formation. It sounded like the acceptance of the collision process as a legitimate process was still developing.

Braham: That's correct. We go back to Bergeron in '35, I believe it is, and for a scientific hypothesis as to how rain forms, and the Bergeron Process involved the growth of ice crystals in a cloud, which then might melt as they fell through the melting level and reach the ground as rain. At the IUGG meeting in which Bergeron gave his paper, there were present in the audience meteorologists from tropical regions who indicated--perhaps not a disbelief in Bergeron's hypothesis, but a statement that it couldn't be the only way in which rain was formed. Those objections, however, seemed not to enter these discussions very much, and maybe because Bergeron was a pretty aggressive salesman, and in many ways like Langmuir.

Cole: Really?

Braham: It became rather the accepted dogma, that rain forms through the ice process that originates at cold temperatures, and ice crystals grow. So the whole idea that precipitation would form through a non-ice process was kind of novel.

Cole: And this was in the mid-30s?

Braham: Right. And even in the late --

Cole: Continuous through...

Braham: Even the time of the war.

Cole: OK, OK. I wanted--

Braham: In this country, anyway. It may not be true for other countries, but in this country. The textbooks used in the training of meteorologists in the wartime

periods contain virtually -- give virtually no discussion of a coalescence process, and give the impression that the only way rain can form is through an ice crystal process. And in fact, clear up to the time of our Project White Top, there were segments of our profession that held the Bergeron mechanism as the only important mechanism.

Even at the end of World War II, the coalescence mechanism was not widely recognized, although those that were--meteorologists in the tropics were obviously keenly aware of the fact that a lot of rains formed from clouds whose tops were warmer than freezing. But most of that knowledge didn't get widespread, and it was only, in fact, after the war that a couple of conscious efforts were made to solicit information of that kind.

Cole: Oh, yes. You mentioned that in the other chapter. OK.

Braham: We designed Project White Top with the purpose of showing that glaciogenic seeding of convective clouds in the Midwest -- summertime Midwest--would produce rain. And obviously the thought was through the Bergeron mechanism. We also had airplanes fly through those clouds, on seeded clouds and non-seeded -- on seeding days and non-seeding days. And quickly learned that the clouds were heavily loaded with ice that formed naturally at temperatures warmer than the threshold of activity of our silver iodide. So it immediately put the basic foundation of our experiment at risk.

Cole: OK.

Braham: Now, those results are presented in a preliminary way at a scientific conference in Australia, and that must have been '60 or '61. I've forgotten exactly when that conference was. And I thought this was quite a revolutionary finding, that the convective clouds of summertime Missouri --

Cole: Yes.

Braham: -- southern Missouri had ice, all the ice needed for efficient rain production at a temperature of minus five to minus ten. And I was disappointed because after I made the presentation, there was virtually no discussion. And then I came to the conclusion that it was because no one believed it.

Cole: Right, yes.

Braham: They --

Cole: So at that time, you would have needed a lower temperature...

Braham: That's correct.

Cole: That was the assumption--

Braham: No one believed it. A few years later... I think it probably was '64, there was an IUGG meeting in Berkeley, California, and I was asked to give a paper, and I gave the same paper with updated data, because now I had more than one summers of observations, all of which were the same.

Cole: OK, yes.

Braham: This time, however, I added the wrinkle that we had to be careful in science that we didn't become captive to authority. We overthrew authoritarianism early in science, and we were on the verge of falling into the same trap, because our excuse is that science is now so complex that it's not possible for us to know everything, and check all, you know, statements and that sort of thing.

Cole: Yes.

Braham: And there's some truth to that. But the basic tenet -- the basic foundation of science is that we check and recheck.

Cole: Right, right. Right, yes.

Braham: And so I said, "You don't have to believe this result. Just go out and look." And about six months later, I had a letter from Australia saying, "Yes, we looked, and the ice is there at warm temperatures."

Cole: OK.

Braham: Then I since have come to the realization that the cloud physics community itself was dominated by individuals -- by the cloud seeding activity, you see. And so the people in the room didn't want to know.

Cole: OK. Because they assumed we already know that. We know how this works.

Braham: They assumed that there's very few active ice nuclei at temperatures of minus five and minus ten.

Cole: OK.

Braham: And therefore, there's something to be gained by adding the dry ice.

Cole: Oh, OK.

Braham: Well, it has since turned out, of course, that the pure coalescence mechanism isn't the answer either. What now we realize happens in a vast majority of the clouds -- at least in the middle latitudes, and in the tropics for those clouds that extend up above zero Celsius--you get initial formation of drizzle sized droplets. The exact mechanism is not really clear. It involves some

coalescence, but it may involve large or particularly favorable nuclei. It may _____ a stochastic collection process. And these drizzle sized droplets then freeze, and the main growth is through the drizzle sized droplets colliding with the cloud drops, and arriving --

Cole: Frozen.

Braham: -- and arriving in the little graupel particles. And it has been shown that that process is much faster than either the coalescence process by itself or the ice crystal process by itself.

Cole: So that's a kind of combined...

Braham: So that's kind of a hybrid mechanism.

Cole: Process.

Braham: And that appears to be the mechanism of formation in most clouds.

Now, Langmuir. You mentioned yesterday the Mount Washington research -- Langmuir.

Cole: Yes.

Braham: That's correct. But, you know, Langmuir seemed not to read extensively in sciences beyond his own specialty. He calculated the rate of growth of drops by condensation and didn't get the right answer because he neglected the interaction between the drop and its environment. He has a paper -- so called "Time of Rise" paper for -- which is a calculation of the condensation in a rising--

Cole: OK. Oh, yes.

Braham: And he invented and rediscovered some of these principles in spite of the fact that at MIT, only a few miles away was a group that was well-versed in the physics of the atmosphere up to the extent it was, you know, known at that time. And he was not real anxious, apparently, to make use of this knowledge which was just next door.

Cole: Yes. So this chain reaction theory that, again, was one of his early theories and he made a lot of there sound -- have similarities to _____ that only warm clouds --

Braham: Right, right.

Cole: -- coalescescence. But was it... Was it kind of another case of rediscovering -- getting close to the existing theory that--

Braham: It was an interesting idea which probably is a marginal view. But we carried out experiments in the Caribbean to -- we went to the Caribbean in an effort to verify that chain reaction.

Cole: This would be the ACN --

Braham: That was the ACN Project.

Cole: (inaudible) in Puerto Rico.

Braham: That's right. In Puerto Rico.

Cole: When was that? Was the _____I think in early '50s but...

Braham: '50s. I don't recall. I have to think for a moment -- the date. But I can get it for you.

Cole: That's fine. It must be in the papers that came out.

Braham: Oh, yes. There's... That's...

Cole: I can... I can find it.

Braham: And what we found was that the cloud lifetime is an essential part of the story. The clouds -- and particularly the cumulus cloud, it builds upward with an updraft and then decays. All the time it's in the building phase, water is being converted from vapor to liquid form. And the clouds that we flew in the Puerto Rico -- over the Caribbean, did not last long enough to carry the droplets that would release a cloud base up to the top to do an official scavenging job on the way down.

Cole: Oh, yes. OK.

Braham: Cloud didn't last long enough. So instead we released water spray near the top of the cloud, and studied the growth of the droplets on the way down.

Cole: --half the process--

Braham: They verified without question that coalescence takes place. Clouds into which we released the water at the top developed radar echoes, precipitation, earlier than those in which we had not released the material.

Cole: Right. OK.

Braham: But cloud modeling subsequently showed that it was probable that those clouds receded and delivered less rain to the ground because we triggered the coalescing process a little earlier than nature would have triggered it. So this whole business of weather modification involves complex mixtures of the dynamics of the cloud system and the microphysics that goes on. And these

things interact.

Cole: Yes, but the tradeoffs sound like... You know, there's no free lunch in getting all the water you want. A couple other. These are very quick, I hope.

Braham: Yes.

Cole: Just dealing with Langmuir, then we head onto some other things. But in looking through his activities and whatnot, it's clear he gave several of his key papers at AMS meetings. Did you ever witness any of these? When he was giving papers at AMS meetings?

Braham: No.

Cole: I actually kind of thought it was odd that he was talking at AMS meetings. I don't imagine he was a longtime member. But it's, you know--

Braham: I do not remember ever hearing--I know that he gave a number of papers at the AMS, particularly at the New York annual meetings of the AMS.

Cole: Yes. Near where he was, yes.

Braham: And I assume that I heard one of them, anyway.

Cole: Well, that's OK. Why don't we just...

Braham: I don't specifically remember hearing any of them.

Cole: You talked about the time in... I think it was also after one of the New York AMS functions where -- it was at the period where you visited Schenectady and talked to Langmuir with Workman.

Braham: Because Workman had --

Cole: It sounded like it was at the point --

Braham: -- the difficult task of telling him we weren't going to seed anymore.

Cole: Tell me a little bit about that meeting, or what you can remember of it. What was Langmuir's reaction or how did that go? I know it's been--

Braham: I don't have a vivid reaction -- recollection of that.

Cole: OK.

Braham: I know that we... Meeting took place, I know I was there, but I don't remember his reaction or--

Cole: OK. But the purpose of the meeting was to give him the --

Braham: Oh, no doubt.

Cole: -- bad news? OK. General question about Langmuir as a person, I guess. Based on your interactions with him, how would you describe him as a--his personality, his -- just as a person.

Braham: In the interactions I had with him would have mainly been in Socorro when I was there and he made frequent visits to Socorro. And he is certainly very-- I found him very personable, easy to be around. I remember being in the room when he discovered that he had a detached retina in one of his eyes and he certainly became more concerned about that than he did the science of clouds.

Cole: Yes.

Braham: But I don't have a--I don't have a really good...

Cole: OK. Oh, I remember that. I was looking through his papers.

Braham: Yes.

Cole: It was--he talks about that time, and what happened, and he had to leave and go, I guess, back to New York and have an operation on it. But... At the time...

Braham: He seemed prone to--

Cole: Yes?

Braham: He seemed one that was prone to make back of the envelope calculations, and he was very capable of doing this, you know, and just--

Cole: Yes. It was clear he was very proud of his theoretical skills.

Braham: Right, right.

Cole: In many things I've read, it was a source of pride, I think.

Braham: But they apparently--he had the laboratory and the chemist background.

Cole: Yes.

Braham: And never did come to grips with the variability of the real atmosphere, and our inability to duplicate--

Cole: Too complex. In the period there when -- after '47 when they did the first seeding until -- I think he passed away in the late '50s. There's obviously a lot of controversy, a lot of debate on this. Langmuir making _____ claims. In that period, who were... Who would you say were some of the

more vocal critics of what he was proposing?

Braham: Presumably--I guess it would have to be the US Weather Bureau scientists. And because of his attacks on the US Weather Bureau--I've heard him say things like, "Don't worry about forecasting the clouds. It's easier to make the weather you want and change it then it is to forecast it." I don't know how you would know that you had changed it if you couldn't forecast it, but nevertheless.

Cole: I could see how that wouldn't set well with forecasters.

Braham: He took the Weather Bureau to task, and they responded. And I guess that's the principal place.

Cole: OK. Was that a variety of scientists? I know Reichelderfer was the head of it at that point. I never know if he was involved in the -- key spokesman or how that would have worked --was he kind of the spokesman for the Weather Bureau or did it...?

Braham: Well, again, I don't have a real clear notion of this. Harry Wexler was in charge of the physical science and Ross Gunn another--both of them very capable, Ross a physicist, and Harry Wexler a meteorologist.

Cole: OK. Both with the Weather Bureau?

Braham: They were Weather Bureau employees at that time. Much of the Weather Bureau's rejoinders had to do with how likely is it that the observed event happened through seeding or would have occurred naturally. This brought up the subject of statistics and some of the statisticians in the Weather Bureau did a lot of work in that time as to how often large scale weather patterns developed. It's cycling pattern similar --

Cole: Oh, for the periodicity --

Braham: -- to the periodicity experiments.

Cole: It's a huge problem to look at. Were there--and again, in the same period with these people, and Langmuir being very aggressive and I imagine the critics being very vocal, too. Do you recall -- were there any major or kind of seminal events or encounters where this controversy really, in that period, _____ into you in meetings or anything like that? Any kind of anecdote?

Braham: I don't have a personal recollection of that. But the annual meetings of the AMS were certainly focal points for that discussion. You see, it's interesting that Jack Workman reversed his field. Jack Workman, after this--after having gone through this period, Jack Workman gave a paper in El Paso -- at a scientific meeting in El Paso--in which he developed the argument that if you

succeeded in doing what you claimed to do in glaciogenic seeding, it would actually decrease the precipitation.

Cole: So successful seeding would give the opposite result of what you wanted.

Braham: That's right, yes.

Cole: So it's not successful.

Braham: And he pursued that argument on physical grounds. And at the moment, I'm not -- I don't recall exactly what they were.

Cole: Yes, OK.

Braham: But it was a reasonable physics. Oh, yes, I recall. If you seed the cumulus cloud, and turn the upper part, the supercooled part to ice, you release heat aloft, stabilize the cloud, and reduce its updraft potential, and it's the updraft that converts the liquid water -- the water vapor—into liquid.

Cole: Puts kind of a damper on it.

Braham: So it--

Cole: Oh, OK. No, that... I think I have heard that later -- I think maybe it was in the '60s or something he -- _____ after the '50s he did that. But was he... In the early days of this when he was working in Socorro with Langmuir, was he pretty much -- was he open to it? Encouraged about the possibilities of seeding, or...? How --

Braham: Was Langmuir?

Cole: Workman, Workman.

Braham: Oh, yes. Jack Workman was... He was a proponent of seeding, I would say.

Cole: Oh, really? OK.

Braham: He certainly... He and Steve Reynolds certainly did a fair amount of research. And I think he felt that yes, there's something to this. And the physics of it, it's rather straightforward. You can convert supercooled liquid cloud drops into ice particles, and they will grow by diffusion, but the early work left out all the consideration about the rates at which these things take place, and that the whole thing is embedded in a cloud which itself has a life cycle.

Cole: Right, right. A dynamic system. But, OK. Just quickly, a couple of the other key players at that time. And, you know, you may have something to say about them, you may not. But Vince Schaefer.

Braham: Vince--

Cole: You mentioned the other day that you met with him a couple times with Langmuir.

Braham: That's correct.

Cole: But...

Braham: Vince was a meticulous observer. Vince could see nature as it really was. If he reported that, "I have seen thus and such," you can be reasonably sure that what he said was real. Vince, however, didn't -- made mistakes in assigning the cause of--how did those -- his observations, the things he observed, how did it get that way?

Cole: Right.

Braham: And he, like Langmuir, was less prone to think about the dynamics of a system, and the variability of natural systems. But Vince was a keen observer.

Cole: Bernard Vonnegut. He was also _____ I guess with some of the...

Braham: Bernie Vonnegut. I know -- I knew Bernie, but I guess I don't have anything particular.

Cole: I guess he... What I read... After the initial GE work, he was working with Workman, I think, on some of the electric--he was doing more electrical cloud research later--

Braham: Well, later, yes. Later he became concerned with cloud electrification --

Cole: Yes, yes.

Braham: -- and developed a couple of novel theories about electrification. But at this time, I believe Bernie was working on glass. The amorphous substance glass at the GE labs. He, too, I believe had a strong chemistry background.

Cole: Yes, yes.

Braham: And when Schaefer demonstrated the ability of dry ice, and particularly silver iodide to nucleate the ice phase, the story is that Bernie recognized that nucleation may have been an epitaxial growth of ice on the silver iodide because of similar lattice structures, and went to the published tables, Wyckoff's Tables.

Cole: Right, and found--

Braham: And selected substances which he said would be good ice nuclei. And, in

fact, I think Bernie probably discovered that silver iodide -- or predicted that silver iodide --

Cole: Yes, he's credited with that, true. But it seems that both men in the early '50s loved GE and really were not involved as closely as Langmuir anymore with the weather modification activity after that.

Wallace Howell seems like--I mean, I've seen both what I would say is somebody who took a scientific approach to the activity, but was also a commercial seeder, and was well-known for saving New York City in 1950 from drought.

Braham: In meteorology, Wally Howell is known for the calculation he did in his PhD thesis. Wally Howell did a hand calculator-type calculation of the growth of particles by collision and their mutual interaction. A calculation that must have taken days with a hand calculator, which takes seconds with a modern computer. But did a very fine job, and paved the way for that computer approach.

Cole: Really? OK.

Braham: It was a lead and seminal paper in the development of the computer -- use of the computer in calculating the growth of precipitation particles.

Cole: OK. Which must have then evolved into the modeling activity, which -- with the mathematics, you can do computer models.

Braham: Oh, yes.

Cole: But at this period --

Braham: But later, then, he did go into business, into the cloud seeding. He had a company -- well, maybe more than one, and was in the New York area, and then later was in Colorado or California.

Cole: But I notice he kept--a lot of these people would -- kept publishing in the meteorological literature different things on that.

Braham: That's right.

Cole: So he's an interesting character in that he puts in – what looks to me like both camps. He wasn't just a commercial seeder. He was--

Braham: When I first published a note, I think, in the Bulletin of the AMS concerning the development of ice at rather warm temperatures in these clouds in Missouri, Wally was the only one who sent me a letter questioning and asking whether or not this might be attributed to some other things, which he suggested. So he was a good scientist.

Cole: OK.

Braham: He was a good scientist.

Cole: So it sounded like through his career that he maintained that objectivity.

Braham: That's right.

Cole: Going through, even though he was doing some commercial seeding. OK. Last person to mention. I know you've had a lot of experience with him, but just in terms of this controversy with Langmuir. Horace Byers, I think... One of the things I've read was that he was extremely critical of Langmuir.

Braham: I wrote the Academy memoirs article on Byers.

Cole: That's what I read. I think this is where that first came up.

Braham: And could get you a copy if you don't have it. Yes.

Cole: Actually, it was online. I got it through the Academy.

Braham: I suppose, still, yes.

Cole: Yes.

Braham: What can I tell you about Horace Byers?

Cole: Well, in terms--what was his primary criticism of Langmuir's ideas and what was...

Braham: His...

Cole: Or are there just too many to--

Braham: Well, we would just recommend that they read Byers' article in Hess.

Cole: Yes, OK.

Braham: Or I think Byers wrote an article in the Academy memoirs on Langmuir.

Cole: Oh, that's right. That should -- I should look for that, yes. OK, I'll take a look at that.

Braham: But I think Byers felt that Langmuir's major mistake, his shortcoming, was that he didn't understand the variability of natural clouds.

Cole: Right.

Braham: And was inclined to attribute a particular happening in a cloud to something

he knew about, perhaps cloud seeding, whereas the same event was easily viewable at the same time over here in another cloud.

Cole: Too focused, OK. Shifting. OK. So much for Irving Langmuir.

University of Arizona. The Institute of Atmospheric Physics. The previous tape [ed.: 1987 UCAR/NCAR] talked about how that came to be, Lou Douglas and ranchers in Arizona. It sounded like wanted -- they came to the University and said, "We would like to start -- help fund a department looking at cloud seeding."

Braham: I don't know that... You said Wally?

Cole: No, I'm sorry. Lou Douglas.

Braham: Lou Douglas, yes.

Cole: (inaudible) Lou Douglas.

Braham: Lou Douglas.

Cole: When did that... Well, I can always find out later when that happened. But what was the motivation there besides Douglas and/or this group of ranchers-

Braham: I think they wanted--they felt that cloud seeding would increase the rainfall on the ranches of Arizona.

Cole: OK, so they had--

Braham: And they... He had secured commitments from some of the ranchers to contribute fund, which would be used. He clearly recognized some of the controversial aspects surrounding weather modification and I think believed that if it were -- the activity were couched inside the University, and do everything possible to get a respectable team, that it would lend credibility to all the effort and--so he came. Lou Douglas came to Chicago and asked for names of candidates of people that might take the job in Arizona. And apparently he--we gave him several names, and he apparently talked to them, and they all turned him down. And in kind of a desperation move, Byers arranged for me to go to Arizona on a joint appointment. Lou Douglas had told us that there was one individual that had agreed to take the job provided he would have total control on the activity. And that individual was a prominent cloud seeder. Happened to be also to be Byers' brother-in-law.

Cole: Byers' brother-in-law. This was Krick. Lou's -- Byers brother-in-law. Oh, that--must have made some interesting--oh, well. Oh, goodness.

Braham: Byers felt that it would not be in the best interests of the University or the science, so to get -- to break that deadlock, I went to Arizona on a part-time

appointment.

Cole: Now, how did you feel about taking that appointment, which must have been a fairly--I mean, it was controversial topic and--

Braham: I guess I looked forward to it. We had previously been in Socorro, New Mexico. We liked the Southwest. We thought the Southwest was a good place. I guess I thought that this might be an opportunity.

Cole: OK. So you looked forward to it. OK. Was there... At that time, were some of these ranchers--and I know in the in '40sAs early as '47 and '48, there was starting to be commercial seeding in Arizona. I think probably more in Phoenix than --but at that point when you went out there, was there active seeding going on or was this too early? The fact that you were--

Braham: I don't remember. I really can't answer the question.

Cole: OK, that's fine.

Braham: I know that the original thought was that the University of Chicago team -- the entire team moved out there for the first summer --

Cole: Oh, OK.

Braham: -- with the Air Force airplanes that we were using. But the Air Force... These were World War II-type airplanes. Hard to maintain.

Cole: Yes.

Braham: The Air Force did not want to continue that relationship, and there were some other reasons. I think an amendment to the Air Force appropriation made it more difficult for them to fund research that was not a direct mission-oriented task. So we felt that--we had no choice. We couldn't continue the airplane. But Lou Douglas, with his influence at high levels in the administration, secured from the administration--now, this is material which I think -- it's never before been discussed. He arranged for a directive from someone upstairs that would require Hanscom Air Force Base -- Air Force GRD -- to furnish us an airplane.

Cole: Oh, I see. Hanscom is in Massachusetts?

Braham: That's in Massachusetts, just outside of Boston.

Cole: _____. And they did? They did it?

Braham: Well, no, they didn't. But the Air Force had--the Boston Air Force Cambridge group had supported our work all the way along. I had some good friends at Hanscom. Some of them -- a couple of them still alive. And

it became apparent to me that to go that route would be to alienate the people we'd worked with, and probably we would have nothing but troubles in getting the airplane serviced and maintained, and so forth. So although we developed complete specifications as to the instrumentation of the airplane, the airplane was to be a B-50. Not an ideal airplane at all for this work, but eventually I turned it down.

Cole: And you were afraid that taking that plane would have alienated which group?

Braham: The meteorology group at, the group that we had worked with. The work group that had funded the work we had carried out up to this point. And, of course, there is still yet another player, and that is that NSF had just come into being, and was funding meteorological research, and although there was a hiatus for a couple of years or so, we picked up funding from NSF to continue.

Cole: Was the Institute -- was the activity in that... I think you had said you were there just for a year or two?

Braham: Well, I was there on a joint appointment --

Cole: _____, right. Right.

Braham: -- for the whole period, and that meant actually physically present only six months a year.

Cole: Oh, OK.

Braham: So --

Cole: Was the activity of creating this institute... Was it like an academic department, or was it just a research group that did specific research?

Braham: It was a research group and then--it was a research oriented group, but rather quickly became an academic function -- an academic department.

Cole: Now, most of the activity you were involved you when you were involved with that was --

Braham: Was research.

Cole: -- research and field work? Or research and field work plus analysis? I mean, the whole nine yards.

Braham: Research and field work. One of the... Lou Battan, who had been prominent -- was just a right-hand man at Chicago. Lou and I jointly did a lot of that work out in Chicago. Lou went to Arizona and --

Cole: And you're doing that together.

Braham: -- was a principal investigator for a cloud seeding project --

Cole: Right, right. Yes.

Braham: -- there in Arizona.

Cole: Just quickly. I know I'm running long here. Did--I guess--so Krick was interested in doing this. Did not get the appointment. Was he upset about this? Or did you ever know or even knew about it?

Braham: I guess I don't know.

Cole: That --

Braham: Oh, I think he probably knew about it, but I guess I have no knowledge on that.

Cole: I guess what was Douglas' view of--eell, first, did you have--is there a sense of what kind of funding -- how much funding was there to start this project? I just thought of that. What sense of the scale?

Braham: It would help if I could go look at my records.

Cole: OK. That's _____in records. That's fine. What's your sense of Douglas' view of how -- did this meet his expectations? Did it do what he wanted --

Braham: I think --

Cole: -- it to do?

Braham: I guess I don't know. Maybe I would guess that it probably didn't. But you should talk with--you should talk with the gentleman that became the principal leader of--

END OF INTERVIEW