

**American Meteorological Society
University Corporation for Atmospheric Research**

TAPE RECORDED INTERVIEW PROJECT

Interview of Joanne Simpson

September 6, 1989

Interviewer: Margaret LeMone

LeMone: To start out with, when and where were you born?

Simpson: I was born in Boston, Massachusetts, on the 23rd of March 1923.

LeMone: When did you first get interested in science?

Simpson: I was interested in math at a very early age in school. I don't think I was really interested in science until I went to University of Chicago. In school, I was interested in biological sciences. The physical sciences were not taught very well. We learned what we needed to pass the College Board Examinations, mostly about pulleys and levers and other boring things like that.

LeMone: Was there anybody during your childhood that particularly got you interested in mathematics, or did this come later on?

Simpson: It was probably my teachers at the Buckingham School. I think my favorite teacher was a math teacher. My father was good at math and also an outdoor person and very interested in nature, the oceans, atmosphere, hiking, and things like that.

LeMone: I should probably ask you, who were your parents and what did they do? Just for background.

Simpson: This relates to what we were discussing earlier about motivation. My parents were both journalists; my father had been to law school also, but mostly he was a journalist. He started out as a reporter on the Boston Herald; he rose to be the editor-in-chief; and then when Governor Saltonstall went to Washington as a Senator, he went along with him as Executive Secretary. My mother was forced to give up her journalism career when I was born for which she never forgave me, I guess. I came from a very unhappy family situation. There were many times my mother stated, "If it weren't for you children, I would go back and earn my living. Of course, I can't anymore." and so on.

I made up my mind, I think by the time I was ten years old, that there was absolutely no way I was going to get myself into that sort of situation. No matter what happened, I was going to put myself in the position to make my own living, and to provide for whatever children I might have without having to depend on anybody else.

LeMone: When you went to school as a child, you said that you went to a private school. Was this a boarding school?

Simpson: It was a day school within walking distance of where I lived in Cambridge, Massachusetts. Sending me to the Buckingham School was a gift my parents gave me for which I am the most grateful. Because not only was the school providing a warm, loving atmosphere, but intellectual stimulation. Most of the children that went with me, went essentially from first grade all the way through to college. Many of us were close, and we had very good teachers, most of whom had advanced degrees. It was regarded as an interesting thing to read books and to study and to know things. This was greatly in contrast to the Cambridge and Boston public schools in those days, where people were regarded as square if they were interested in studying.

LeMone: Was this a co-educational school or a girls' school?

Simpson: At the time I went there it was co-educational up to fifth grade, and then it was a totally girls' school from sixth grade up to college. When my daughter went there a generation later, they combined with a boys' school. The boys' school was the one my brother had attended, actually. In my perception anyway, the boys' school was not at the same scholastic level as the girls' school, and I don't think the combination turned out as well as I had hoped. I don't think my daughter got as good an education as I did. The classes were much larger, and the boys' school sort of swamped the girls' school. None of our old traditions were really left. It was still a high quality education, but not the very unique education that I got.

LeMone: Thinking back on it, did you think it was beneficial to go to an all-girls school during your teenage years?

Simpson: I suppose there were some drawbacks to it. When I got to the University of Chicago where it was co-educational, the classes seemed overwhelmingly large; and, also, I was somewhat afraid of boys, although I had grown up with a boy as a playmate. But what I think I got in terms of the education, the friendships, and the mothering by the teachers more than made up for that, because I adapted within a year or so to the co-educational environment at Chicago.

LeMone: What made you pick out Chicago?

Simpson: It was partly a rebellion from the "Eastern Seaboard Syndrome" where young ladies of prep schools went to one of the seven-sisters girls' colleges. My grandmother and my mother and all my aunts had gone to Radcliffe, and I wanted to get away from that environment. Also, there was a positive factor. I was in a dentist's office and read an

article about Robert Hutchins and about the four core courses at the University of Chicago. I was excited to learn that Hutchins believed in having his most distinguished professors teaching the undergraduates. I was impressed that they made their courses so interesting that there was no compulsory attendance. I found that indeed to be so. The Physical Sciences course and the Astronomy and Astrophysics taught was, I think, one of the main things that turned me on to science again, after having been turned off to it in high school physics.

LeMone: When you went to Chicago, did you have any idea that you would go into mathematics or physics?

Simpson: Absolutely none whatsoever. I was probably more oriented toward political and social sciences.

LeMone: What made you get interested in meteorology?

Simpson: It was partly through being a private pilot and belonging to a flying club. I was always interested in clouds and weather. And the involvement was partly an accident of World War II. There happened to be the World War II meteorology training programs going on at the University of Chicago. Rossby had just come there to form the Institute of Meteorology. I wanted to leave college and enlist in the military and my parents (who had split up by that time) neither one of them individually wanted me to do that. By getting involved in the training program in meteorology, I was able to finish college and at the same time contribute to the war effort by staying on and teaching the Aviation Cadets.

LeMone: Did this experience make you interested in going to do graduate work then?

Simpson: Not for sure, no. This came later, after I was married to Victor Starr and after I had my son David. I was just finishing my Master's Degree, the war was ending, and I was becoming aware of obstacles to women doing anything in any profession. At that time, I looked into a number of options, keeping in mind that whatever I did, I wanted to be financially self-supporting. I was quite interested in going to medical school at that time, but I found: a) that there were no scholarships for women, and b) nobody would lend me any money to go. I didn't want to borrow for my education either and thereby start out with a big handicap. So, after having looked into a lot of different fields, I finally decided (I assessed the situation myself at the time) that the advantage that I had by already having the training in meteorology with a Master's Degree, I would have a better chance of getting into a self-supporting position to go on with meteorology, to try to get a job of some sort, and go on for a Ph.D. This seemed more feasible than starting all over with no financial backing in any other field. In fact, it wasn't until 1947, which I will tell you about later, when a big transition in my life and interest occurred, and I really became seriously committed and devoted to meteorology. In most of the other interviews I have had, I think this point has been overlooked. It has appeared that, through my great devotion to science, I stuck through all the obstacles. This is actually a rather blurry

picture of the real situation. Because I didn't really get the real devotion to meteorology until Herbert Riehl's course in tropical meteorology in 1947.

LeMone: So you are already a graduate student who decided to go into meteorology somewhat objectively, but that this really made it catch fire.

Simpson: That is exactly a correct assessment of the situation. I remember that during the first couple of weeks of that course, he (Riehl) was going over the first results of the Wyman/Woodcock Field Expedition to the Caribbean, which was done mainly for Navy purposes to understand the behavior of smoke screens. Then on the basis of serendipity after they got there, they (Wyman and Woodcock) found that the Benard Cell Convection didn't occur as they had expected. They turned the purposes of the program to making observations inside and outside of trade-wind cumulus clouds. And afterwards Henry Stommel, the great oceanographer, deduced from these observations that some kind of entrainment or dilution by the outside air was occurring. The whole picture that was painted by their report, as presented by Herbert Riehl and by his presentation of the work that Al Woodcock did on the soaring of herring gulls to document the vertical motions over the boundary layer, all of a sudden acted like a light bulb in the funny papers. It just hit me that this is what I wanted to do.

LeMone: That's not only when you really decided that you loved meteorology but you wanted to work on cumulus clouds.

Simpson: That was when I decided that I really wanted to work on cumulus clouds. At that time I had practically zero self-confidence, so I didn't have any idea that I would be able to do anything with it. But I was lucky enough due to the rather coincidental fact that my mother was a personal friend of Bernhard Haurwitz. He had a project at Woods Hole in the summer that was carrying on with the analysis of the Wyman and Woodcock data. Through him, I got a chance to have a job there in the summer where I started on the project of examining relative motion between clouds and the environment. I had thoughts about the very first beginnings of some kind of a model of how cumulus clouds behaved. The latter turned out to be a major part of my Ph.D. thesis. That was just a terrifically happy accident that really then gave me some fairly concrete hope that I might be able to do something.

LeMone: So you basically commuted to Woods Hole in the summer and came back to Chicago?

Simpson: Yes. Because of the nepotism rule at the University of Chicago, the day that Victor and I got married, was the day I got fired from my job there, which was being sort of a part-time lab instructor and part-time research assistant. I was left totally without a job. So I decided I would write to every single college and university in Chicago and its suburbs after getting their catalogs, suggesting that I could teach a meteorology course. I got no answers to many of them, except a couple of turndowns, and finally when I had almost given up, a phone call from the Chairman of the Physics Department of Illinois Tech. ITT was (and is) an engineering school partway downtown, he said, "Well, could you possibly teach physics?" I said, "Yes." Even though I had absolutely no confidence

that I could. He said, "If you come down and help me out, we've got all these veterans returning on the GI Bill; we've got a much heavier load in teaching than we can handle. I'd give you a part-time job as a Physics Instructor and, if you want to, then you can teach a meteorology course. And if we get enough enrollment in it, we will keep on doing it." That was what my academic-year job was. I started out as a part-time instructor with Illinois Tech with the grand salary of \$1,100 a year, and that wasn't even enough to pay babysitters. So I had to borrow money from my mother. Then in the summer of 1947, I got invited to go down to Woods Hole by Bernhard Haurwitz for a couple of weeks. Then starting in 1948, I got to go regularly as part of his project in the summer. Then in the winter, in the academic year, I came back to Chicago and became a full-time instructor and finally Assistant Professor. All this time, I was bootlegging my graduate work. It was sort of catch-as-catch-can, but the great thing about Illinois Tech was that faculty members could take courses for free. So, after I had taken the minimum requirement of graduate courses at the University of Chicago, all of the graduate courses I took were either in Physics or Engineering at Illinois Tech. This was the greatest thing that ever happened to me - to be forced: a) to take these courses, and b) to be forced to teach them. Before I left Illinois Tech, I was teaching all the undergraduate courses, including advanced mechanics and electrodynamics. This was an education that was absolutely priceless.

LeMone: So it turned out to be a blessing in disguise.

Simpson: It turned out to be a blessing in disguise. It turned out to be much more beneficial to what I was able to do later, than if some fairy godmother had paid my way through graduate school.

LeMone: So upon graduation, you ended up eventually, I guess, at Woods Hole. Was it right away?

Simpson: What happened was, that I had originally started doing my Ph.D. with Rossby, who had been my Master's supervisor. But firstly, he was very unencouraging because of the woman situation, and he didn't think there was any future at all for women in meteorology. Then I was trying to do the kind of dynamics that he and Victor and George Platzman and the others were doing. I wasn't very good at it, and I wasn't very interested in what I was able to do. But then, after Herbert Riehl's course and after working at Woods Hole, I had the idea to work on tropical convection, which is just the time that Rossby was leaving to go back to Sweden. So I asked Herbert Riehl if he would be willing to be my thesis advisor. He said, "Well, I really don't know anymore about that topic than I talked about in those two weeks in my tropical course, but it's interesting stuff, and I'll give it a try." And that began a collaboration, which is active even today.

That was the way it evolved. I finished my Ph.D. in 1949 still teaching all the while at Illinois Tech. I stayed on at ITT until 1951, when we moved to Woods Hole permanently. David was born in 1945, Steven was born in 1950, and Chicago was becoming less and less of an attractive environment for my little kids. Woods Hole, and the possible opportunity to do field program research was just so exciting, that even though the salary

that I started to work at was actually less than what I was getting at Illinois Tech, it was the opportunity to do the work.

Columbus Iselin, who was the director of Woods Hole at that time, was very observationally oriented and very field oriented and supported Al Woodcock's work on the oceanographic ships. There weren't any aircraft programs right away as there was only a light aircraft available for local flights. I'd hoped that perhaps it would be possible to do another one.

LeMone: So did Columbus Iselin have any influence on you personally or on others?

Simpson: He had an enormous influence on me personally because I think without him being there, I wouldn't have had an opportunity to have a job there. I think without his support, we never would have gotten another PBY aircraft to do field experiments that we did in the early 50's on cumulus clouds in the tropics. And even after we got the aircraft, no woman could go on the oceanographic ships. They tried to apply it to the airplane also, and Columbus Iselin and the people in the Office of Naval Research, to whom I am eternally grateful, said, "No Joanne, no airplane." The guys hated to see it happen and gave me a rough time. I went along on the first aircraft flights as Senior Scientist.

LeMone: When did this occur?

Simpson: We got the airplane (PBY-6A) through the Office of Naval Research on bailment from the Navy. We got it in about 1951, I think. It took about a year to calibrate it, put strain gages all over it, and found out it had rigid wings and, therefore, one could mount an accelerometer at the center of gravity. And, if you knew all of the characteristics of the aircraft, you could figure out its sinking speed and, therefore, calculate the vertical air motions. We also put back the same instrumentation that the Wyman/Woodcock group had in the original studies of cumulus clouds off Puerto Rico. And what we decided to do was to go down and continue those with their instrumentation that measure temperature, humidity and pressure, but also to add accelerometers and instruments to measure vertical motions and to add very crude instrumentation to try to measure liquid water. The first field program we made to Puerto Rico was in June 1952.

LeMone: The field program from which you wrote your paper about the structure of the trade-wind boundary layer?

Simpson: That was the field program from which I wrote the paper, which appeared in the *Journal of Meteorology* in 1954 on something about trade-wind cumulus clouds. It was basically a first attempt to make a one-dimensional cumulus model. People didn't even know in those days that the motions in cumulus clouds were driven by buoyancy. In fact, Herbert Riehl didn't believe it for many years. He had some ideas and some observations he had made in the South Pacific, in which I believe the thermometers had gotten wet, because the clouds appeared to be negatively buoyant. The clouds, he thought then, were somehow driven by the larger circulation against negative buoyancy. Then, after taking

great precautions with the kinds of housing we put on the instruments, we were able to convince ourselves, and some other people, that the clouds in their active phases did indeed have both some temperature and moisture excess so they are positively buoyant in growing stages.

LeMone: That's truly remarkable, particularly considering that a lot of instrumentation still gets wet in cumulus clouds. So you managed to keep the instruments thoroughly dry, I guess.

Simpson: Well, there was the advantage of having such a slow, slow airplane. The airplane flew at 55 or 60 miles an hour if you wanted to slow it down. And we climbed it very, very slowly and we stayed away from precipitating clouds because the aircraft could only, with a great struggle, get up to 7,500 feet. And I wanted to make sections through one cloud at the same time as getting pictures of it so we could reconstruct the whole vertical profile of the cloud and rather than haphazardly making runs here and there through different towers so that you didn't know if it was the same one you penetrated before or not. The housing for the instruments had originally been built at MIT, and we had improved them so that until a thermometer got very wet, you could get the temperature pretty well. Then when the dry bulb temperature went below the wet bulb in an updraft, we then assumed that we had saturated air.

LeMone: So the one in Woodcock expeditions where they have negatively buoyant clouds, they must have had a faster airplane.

Simpson: They had exactly the same kind of airplane. They had positively buoyant clouds, but the clouds were just very much less buoyant than the parcel model predicted. This is what led to a lot of controversy, because this is how Henry Stommel postulated entrainment in small clouds. In 1 kilometer ascent, there was calculated about as much air was entrained from the side of the cloud as was already in the ascending mass flux, and a lot of people didn't accept this result at the time. It is ironic and interesting to note that a paper came out in **JAS** within the last few months, showing that it is now possible to use ozone as a conservative tracer in clouds. This paper shows (very interestingly to me) that if you follow a single cloud in updraft, that indeed most of the air coming into the updraft is coming from the upshear side as was exactly what we postulated then. And only when you get to the downdraft part of the cloud, did they find, when they were using both the carefully sealed thermometers and using ozone as the conservative property, that there was a significant amount of air coming from a higher level.

LeMone: That at the time was a very revolutionary idea and still is.

Simpson: It was a very revolutionary idea. In fact, the two other most active groups working actively on cumulus clouds, the Thunderstorm Project, which Byers and Braham, Chester Newton, Lou Battan, and a number of other people we all know were involved, and then there was a group in Great Britain at Imperial College who were doing time-lapse pictures and using gliders as much as they could. They just said that the reason you are getting these measurements is that powered aircraft are simply no good to make cloud

measurements. The aircraft affects the air, thermometers get wet. They were, in fact, postulating at that particular time, that the undiluted ascent of bubbles were the building blocks of cumulus clouds.

LeMone: Was it about this time that you also started thinking about merger clouds?

Simpson: That was many, many, many long years later. It wasn't until we were doing the Florida project in 1960, that we felt that we had gotten far enough along to look at cloud systems rather than individual clouds. I rather stuck carefully in the days to clouds that were as much isolated as possible. Not because I felt that these were typical clouds, but because you could make a clean-cut measurement on them. It wasn't until we got into the radar studies associated with the Florida experiment that we seriously started looking at cloud mergers and cloud systems¹.

LeMone: We will probably come back to that later. When did you first think about what people call now the "Hot Tower" idea?

Simpson: That's something I want to talk to Herbert Riehl about when I talk to him to see if his memory of this is the same as mine is. My recollection of it is when he and I got involved in the hurricane project (which Bob Simpson is talking to Ed Zipser about) there were things that we found were illogical to explain in the eye walls of hurricanes, unless we postulated that a certain substantial fraction of high θ_e boundary layer air got all the way up to the hurricane outflow air. I think it was almost at the same time, because this was in the late 50's that Herbie and I were following up earlier work that we had done on trade-wind boundary layer. We were trying to do heat balances in the trade winds and in the equatorial trough and do energy budgets, but we also found that we couldn't put the budgets together. We couldn't explain how the energy was getting to higher elevations if it was just a gradual ascent in the Hadley cell. The vertical motion had to be confined to the very small restricted areas of hot towers so that the high θ_e would get all the way up. That just wouldn't work out otherwise. We found that a suspicious conclusion at the time, but because we had been working on hurricanes, we said why not. In hurricanes it seems perfectly clear to us, for the source of the outflow air to be the lower boundary layer. We asked ourselves if there are these hot towers that are pumping up undiluted air, how many would we have to have around the tropics to do this? And it turned out to not be such an unreasonable number².

LeMone: You have been working on clouds for many years. Before we leave the subject of pure cloud research, do you have anything that you would like to add? Maybe something that happened in a field program?

¹ Correction after the interview was transcribed. Peggy was right and my memory was in error. I did indeed see cumulus mergers of very small trade cumuli in the Woods Hole time-lapse movies. I actually saw evidence of merger reported in the trade cumulus paper in J.Met. in 1954 and Riehl and I wrote more on it in connection with small, non-precipitating Pacific trade cumuli in our 1964 book. J.S.

² In the September 9, 1989, interview with Riehl, he correctly recalled that the "hot tower" hypothesis was evolved in connection with our equatorial trough work in 1956/57, while the application to hurricanes came a little later (1958-1961). J.S.

Simpson: I have been with clouds ever since the first tropical course in 1947. I have been working on aspects of clouds ever since. And it was sort of like building a structure. You want to start with as simple problems as possible. And I guess my greatest dream as a graduate scientist was maybe someday I would be able to make a model of a cloud that's either an analytic equation or a computer model. It soon became clear that it probably couldn't be analytical because of a tremendous amount of nonlinearity. Some kind of a numerical integration was needed where I could make a model of a cloud. If I had ever dreamed of what people are doing today in terms of cloud models, I wouldn't believe it could have happened.

LeMone: How did you get involved in weather modification research?

Simpson: That's entirely due to what we have been discussing. To me the modification part of it was only to learn more about how clouds worked. I did not care then whether the modification worked to the practical advantage that the operational weather modifiers were interested. Except that if it did, that would be a good way to get more support for the project. And, also, one likes to do things that are beneficial to humanity, if possible. And if it did turn out that clouds could be modified in such a way as to make more rain where it was needed, or something else that would be useful, that was fine, but that wasn't the reason I was interested in it. At the time when seeding flares with silver iodide became available, it just happened that I'd gotten to the first one-dimensional model, which we were still solving by means of a slide-rule. We were gradually getting on a computer when I found out that these silver iodide flares existed, I said, "Suppose you could freeze all the liquid water in the clouds just above the freezing level? What would it do to the buoyancy? And would it cause them to grow on above the trade inversion or the dry layer that usually stop their growth?" So I just stuck that into the model in the crudest way possible, that froze all the liquid water suddenly, and I found that, in a number of cases of the clouds that I had been studying, in particular environmental soundings, that there was just enough of an inversion dry layer so that the little shot in the arm of latent heat release at that particular time like 1-2 degrees, was enough to cause a difference in the model cloud between a cloud that would cease its growth at 5 or 6 kilometers and one that grew all the way up to the 13 or 14 kilometers in the high troposphere.

LeMone: I guess there is some experimental data to support this?

Simpson: Not at the time. What happened was, that at that time I was a consultant to the Hurricane Project and to Project STORMFURY, which is a whole other long story, and I persuaded them, in order to learn how to coordinate their aircraft and to get the flares where they wanted them to and so on, to designate a certain number of days (that there weren't hurricanes in the vicinity of Puerto Rico) to undertake a cloud project. So the first year that we did that, in 1963, since I was just experimenting with my model, I had no intention or idea of doing anything randomized. I just said we will pick some clouds and seed clouds in this vicinity and leave others be, and compare how they behave. And it was fortunate, because the very first day that we did this happened to be a day that the soundings were exactly right for a very spectacular seeding effect. Two or three clouds

that we had seeded that day just grew explosively; and, of course, the Navy guys were excited out of their minds and said maybe we've got something here that works. So they were willing to carry on the experiment for five or six days, I guess.

LeMone: How long did it take before you started getting model results that looked promising until the time you were actually able to go out and test them with it?

Simpson: I think it was at UCLA (I was there 1960 through 1964) at that time. A graduate student and I were working on the cloud model. We got the model results first and then we went out and did the first experiment the summer of that same year. Then we wrote an article about it and then a furor just broke loose. I was just totally unaware of the level of emotion and the level of hostility that was directed against anything that had to do with weather modification. In fact, I wasn't even interested in modification, I was just interested in making an experiment.

But at that time a great huff was made and a lot of statisticians stuck their noses in the act. (At this time in 1964, I believe that I was leaving UCLA and joining the Weather Bureau.) Yes, that was in 1964. They said it was required. Tremendous pressure was put on to repeat this experiment on a randomized basis. The instructions were to open envelopes selecting which clouds should be seeded and which clouds shouldn't be seeded, without the knowledge of any scientists as to which decision had been made. Glen Brier got involved in the project at that time. He's a very reasonable meteorological statistician. He knew how to do block randomization so that you can get a two-to-one seeding ratio. But the Navy said, "We aren't going up there and fly around and do nothing, we're going to seed clouds or we aren't going to do it." Finally, Glen Brier persuaded them that, if you want to have credibility in what you are doing, you have to do a randomized experiment.

Consequently, in the 1965 experiment, we had something like 23 or more individual clouds that had about a two-to-one ratio of seeded to control. It was very clear, statistically significant result that the seeded clouds grew several kilometers (on the average) higher than the unseeded ones. I was very happy with that result because I felt that it showed that there was some merit to the cloud model, and it was fun to do the experiment. At that point, Lou Battan said "So you showed you can make a cumulus cloud grow higher, so what?"

Also, a lot of factors converged at the same time. It was the time that Bill Woodley came to work for me; also, the Weather Bureau was just expanding to become NOAA then. We got the idea that really since these clouds grew so much bigger and so much taller and they lasted much longer, it was quite likely that they rained more. So we planned to move this whole experiment to south Florida, where we have radar and a rain gage network. The idea was to carry on this same type of experiment with individual clouds and see if, in fact, they do rain more on a carefully randomized basis. Because, if so, it might be that affecting the dynamics of the clouds by seeding (this is a different physical seeding hypothesis than the one that people had been working on previously), it might be a way to increase rainfall. Also, again, it might be a way to learn a great deal about the rain

processes and the interaction between the rain processes and the dynamics of the clouds. We were beginning to have computers available at that time where one might do more sophisticated microphysics and might even think of going to a two-dimensional model. It was at that time, I think, that Bill Cotton and Roger Pielke both came to join our group to work on the modeling aspects and the field experiments. So we were becoming a group of people, rather than just one person and a graduate student working.

LeMone: So it sounds like you started a major weather modification project, but at the same time you were doing some very fundamental research that started it. The results are still cited very often today. Did you ever manage to get statistically significant results in the way of rainfall?

Simpson: In the single-cloud experiment, results were convincingly statistically significant. When we dealt with pairs of clouds, the experiment took two or three seasons to get a large enough sample. The results were very clear, and I think were generally accepted in the community. The seeded clouds rained about a factor of two or more than the unseeded clouds. Not because the rain rates were any higher, but because the clouds lasted longer and had a larger area. A pretty undeniable result. Where the whole thing got into trouble was when it was, in retrospect, prematurely taken into a more practical and more statistically straight-jacketed project to do this kind of seeding for making cumulus mergers over a whole area. It was at that time that I began to feel that this area-wide study of mergers was taking the work in a direction I did not want to go. That is, I did not want to become an applied weather modifier because I didn't feel sufficiently competent or know enough about the interactions between clouds to do that. It was a very serious dilemma because the management was saying, yes, we want you to make this into something practical. However, we can only give you enough resources to do it for a limited time, and it had better come out with a positive result, or else. That was the time I decided, for that reason and several others, to leave NOAA and go to the University of Virginia. The seeding experimental indeed led to a large number of very valuable papers, in fact, per dollar, about twice as many papers as GATE after all these years. The area experiment (FACE) came out inconclusive, having a totally inadequate sample in terms of the natural variability in cloud systems. This was, unfortunately, a major factor in the demise of weather modification, which has greatly slowed down the studies of cloud physics.

LeMone: Yes, that is anticipating my next question. What are your feelings about the demise of weather modification?

Simpson: Weather modification was undertaken, in many cases, with too many claims, with an underestimation of the enormous natural variability of the system, and with impatience on the part of the management to get a positive result in a short period of time. As a result, inconclusive results came out, and people became disillusioned and said this won't work, it's no good, the theories are wrong, and we will quit. Whereas Israel (a region with greater need for water and no homogeneous scientific community and had a management which was desperate for the rainfall), they gave the scientists something like

10 or 15 years or more to do an experiment. They had a situation with large storms creating the rain so that natural variability could be taken into account by control areas in which rainfall was highly correlated with seeded areas, so that the problem wasn't as enormously complex. Of all the weather modification experiments, which had been carried out, the one in Israel is accepted in the scientific community as the sound experiment.

END OF TAPE 1, SIDE 1

Interview of Joanne Simpson

TAPE 1, SIDE 2

LeMone: We were talking about the “demise” of weather modification in the field of meteorology. Are there any further thoughts about that you would like to explain?

Simpson: There are several thoughts about that. Incidentally, Bill Cotton is writing a book called *The Rise and Fall of Weather Modification*, and you and I discussed it a bit. It is a very sociological scientific phenomenon. My feeling is, that a lot of what was done was in the sense premature, in terms of our understanding of how clouds and cloud groups work. The pressure of the increasing population and increasing water shortages will lead the scientific and public community back at some time in the future to doing this kind of thing again on a much sounder basis. I think we went on the wrong track when we got so straight-jacketed by statisticians who try to imitate agricultural experiments. Meteorology is not like agriculture. You can't have two identical fields of identical plants and different fertilizers. Doing things on the double blind basis makes it so the meteorologists couldn't use their knowledge and experience. There were a lot of mistakes made that can be corrected. Important post-analysis was done on the Florida clouds by Abe Gagin and his students. They also worked on other cloud seeding projects in Texas. In particular, confirming my perception and their perception that a large portion of the hypothesis that was used in those old days was correct. The problem was to see the signal against the natural variability, the power of which was underestimated.

This rise and fall of weather modification is part of a more general phenomenon. We have had not just the rise and fall of weather modification. In our field I have noticed, since I first got in it almost 50 years ago, we have had fashionable subjects which have been fashionable for a while, maybe something like the order of ten years or so, which have entrained a large fraction of the resources and people in the field. Then, at the end of a time, something else becomes a fashionable subject. If you want to get money and resources, then you have to work in that fashionable field. There was a period when we were told, that if you want to be a tropical meteorologist, GATE is the only show in town, and the funding agencies won't support you to do any smaller project of your own. Get involved in GATE or else. Now climate is the fashionable topic, and everybody and his cat are concerned about climate change and greenhouse effect and climate models. These have been carried, in my opinion, to ridiculous extremes. Both you and I, Peggy, were talking about how did this phenomenon compare to all of the fields, I don't really know. My feeling is, that our field perhaps being smaller and with fewer people in it and fewer resources available, there is less inertia in it than in many other scientific fields, and therefore, it's probably easier to torque the whole field into a fad than it is in other fields. I suspect there are fads in other fields.

LeMone: How did you get involved in the study of hurricanes? You said earlier you were a consultant for STORMFURY.

Simpson: I got involved in the study of hurricanes. Again, this is sort of related to what we were talking about concerning fads. We had that rash of successive hurricanes devastating the East Coast in the 1950s, and as a consequence, there was the start of the National Hurricane Research Project (which Bob Simpson is talking to Ed Zipser about in this interview because he [Bob] was the one who started that project). I heard that there were a lot of resources and activity. I was particularly interested because there were aircraft equipped to make observations in a far more advanced way than we had been able to find support to do at Woods Hole. So I thought, well gee, I'd better get into this, too. Hurricanes are, after all, systems of tropical clouds. Systems of tropical clouds that somehow get together and run wild. Why did they happen in that way? I convinced Bob in 1955 or 1956 that the work that we were doing on clouds at Woods Hole was an important part in learning how clouds formed systems and how they behaved when they organized into systems, and by this time we were involved in not just how clouds grew, but how they impacted their environment and each other. I started reading up on hurricanes and then I got an idea from the things that I'd read about it, how the cloud eyewall might to a considerable extent interact with the dynamics of the larger scale processes that were going on, how inside the eyewall these would lead to sinking motion, drying, and the kinds of soundings that were observed in the eye of the system. I did a paper on the hurricane eye and how it was sustained. At that time I didn't know about any interest in modifying or moderating hurricanes.

I was just involved as a consultant to the Hurricane Project. To learn about hurricanes and how they worked, I went down a number of times to Palm Beach when they had the Project. The hurricane flights there were made by military planes, and, of course, no women could go. A couple of years after that they took along a woman reporter, but they didn't take women scientists, which made me very mad. But we were able to work on the data. Where the Seeding Project (later STORMFURY) came along, apparently, although nobody let this out, was done by the University of Chicago group under the leadership of Roscoe Braham. They were involved in putting silver iodide burners on the DC-6 aircraft but were never able to make them work. All of a sudden a guy turned up from Naval Ordnance out in China Lake who had invented silver iodide flares. This immediately hit Bob as a great idea. He had a theory of how to seed hurricanes to reduce their wind force. He and Herbie had discovered, in a number of hurricanes, that there seemed to be one "chimney cloud" in the eyewall that was doing most of the vertical transport. That led to a chain of reasoning on how, if you could see the eyewall, that it was possible that the eyewall would die out and re-form at a greater distance from the center of the storm, therefore reducing the intensity of the maximum wind.

I really got involved in this as from the point of view of an antagonist rather than a protagonist. I thought that this was a rather speculative idea, and I expressed rather strong doubts in view of the huge amounts of energy that the hurricane released. It didn't seem likely that making a fairly major change in the eyewall would have any effect. Reichelderfer also had both interest and doubts, and as a consultant, Herb Riehl thought similarly. That is, he didn't think it was a completely sounds hypothesis either. Not necessarily unreasonable, but highly speculative. So we were both invited to be

consultants on Project STORMFURY. After I had gotten involved in doing the single cloud seeding experiment, and seeing some of the data that they had obtained from Hurricane Esther in 1961, I thought there was a very faint chance that there might be something in the STORMFURY hypothesis, and we kept on working on it and testing various parts of it that you might be able to improve. And if you weren't able to improve it, you probably would gain enough new information about storms to make the effort worthwhile, since it was a pretty inexpensive project. Also at that time, Stan Rosenthal was making models of hurricanes; he was just beginning to be able to try out what effect the hypothetical seeding would be in the model. It struck me that the seeding would be an interesting way to improve models and learn more about hurricanes. Although I was skeptical all the way through whether one could produce any significant modification, it was a long shot worth trying because the damage from a hurricane is approximately proportional to the square of the wind. So if there is anything you can do to reduce the maximum winds even slightly, it is worth a try.

Looking back on it, I still have the same attitude about it, and I am sorry that the whole project died out. It is very difficult to work at a political interface of science. The reason that I got out of it was because there was so much hassle and so much unpleasantness, that it got impossible to do any work. More time was spent dealing with the politics and putting out the fires than in trying to learn about hurricanes. I think that's why a lot of people have been driven out of weather modification. It will be interesting to see how it comes out in Roscoe Braham's interview. I think that he just found that the heat of the politics was so terrific that he wasn't going to get any science done if he kept on in that direction. He was going to go do something else, which is what I decided to do.

LeMone: One of the interesting things that seems to be emerging in the science now at least is that changes in eye structure do have a strong influence on hurricane evolution. Maybe a piece of this hypothesis is going to be at least partially vindicated.

Simpson: I think that is quite likely, and I think it is certainly worth keeping in mind in further work on hurricanes. I think people should be placing their emphasis on weather modification as atmospheric experiments, and I've said so all along, throughout the whole thing. Seeding of the tropical and semi-tropical cumulus clouds for dynamic effects and mergers, I think that that was an idea that was virtually certain to pay off. At least I would bet considerable money on it, whereas the hurricane modification effort is a very long shot. Since the stakes are high, it is worth trying.

LeMone: Going on to more general things now. You've worked at a government laboratory, at NASA, the Weather Bureau, UCLA, the University of Virginia, all different institutions. What sort of environment was the most fruitful for advancing your scientific goals?

Simpson: The two environments that I have found most useful were near the beginning of my work and the past decade. Namely, Woods Hole in the great days of The Woods Hole Oceanographic Institution, and NASA Goddard Space Flight Center. The last ten years have been in a very productive environment with brilliant young people to work with. I

had hoped that would be so at two universities. Illinois Tech was a great environment because I learned physics there, and I got my start. I had hoped that at UCLA and the University of Virginia things would have turned out more productively than they did. At UCLA, I was very happy; the people were very nice. There were very distinguished people there; there were also very good students. But the way the department was set up there, with the philosophy of the department originally developed by Bjerknes, was that the whole field of meteorology should be covered by the department, and there should be one faculty member in each area. There should be one radiation specialist, one boundary layer person, one general circulation model person, one person involved in convection or tropical meteorology, and so on. And, as a result, there were never any other than pleasant social interactions between the faculty members. The faculty members who were active research people were Krishnamurti, Yale Mintz, and myself. We each built up our own group of graduate students and one or two post-docs from time to time.

Occasionally, some of us had sort of had interesting interchanges scientifically, but people's fields were so remote from each other. And also in Los Angeles, the living conditions made people physically far away from each other. I felt that it was not a sterile environment, but it was not a highly challenging, exciting environment. I wanted to get back into doing field programs and getting out and measuring clouds. And I could do that better by going into the Weather Bureau, which later became NOAA.

Then the University of Virginia was a noble experiment in trying to do more interdisciplinary work. I went in 1974 to the Department of Environmental Sciences, which, if it had the right support from the administration and the right cast of characters in the department, might have been successful. But between the time I had accepted a position there and went there, the management had changed, the Dean of the Graduate School had changed, the dean that hired me was replaced by a Dean who was a failed physicist who thought all of meteorology was for the birds. The department had a group of people who were all rather prima donna-type people who resented each other rather than being happy about working with each other. And when we talk about prejudice against women, I think I found that probably was more intense there than any place I had been. There were, in fact, a few remarks made, well, if you weren't a woman, you wouldn't have been hired for this Chair. I was on the Dean's Advisory Council, and we were talking about hiring Joan Feynman, who is an excellent geophysicist. Well, the dean said, "The government has forced us to take x% women in our science department and you are it." And that ended the discussion about hiring Feynman. In fact, I really felt sufficiently unproductive there that I was actively looking for another position. At that time, Dave Atlas was starting his Laboratory at NASA, and I called him up and asked him, "Are you interested in convection and things like that?" He said, "Of course, when can you come?" I left the University of Virginia on leave of absence because I don't believe in burning bridges. It was clear after the first year that Goddard was a very exciting environment and exciting work. Except for getting paid a lot more money at the University and having children in college, there was no temptation whatever to go back.

LeMone: When you went to NASA, were you able to hire some of your own people and build a group?

Simpson: Yes. When I went to NASA, I guess it was just about the second year Dave had been there in charge of the Goddard Laboratory for Atmospheric Sciences, and he wanted to build up a severe storms and convection branch. He'd been looking for a head of it. So it just worked out perfectly. There were a couple of people already there. A couple of very good people. But I had the opportunity to hire five or six people. I went around and looked for them at various universities and got them just as they were getting their Ph.D.s or after short periods of being post docs. We had a program, where we brought in a number of people as post docs. They were so good, not all of them, but some of them were so good, and fitted in so well, that they became a part of the whole group. Tao, for example, is one of them. He got his Ph.D. with S.T. Soong and came to us as a National Research Council post doc, and he productively used up the two years of being a post doc. And since he wasn't a citizen, we kept him on in another kind of post doc for several years. He became a citizen, and just about a year ago in 1988, he became a permanent member of the group.

LeMone: I guess Dan Keyser and Louis Uccellini were already there?

Simpson: Louis Uccellini was already there. I immediately spotted him for tremendously brilliant, exciting young scientist. In fact, I have learned a huge amount from working with him. It has been a fantastically mutually beneficial relationship. Dan Keyser, Louis and I picked out; I think he was just finishing his Ph.D. at Penn State. He said, "Well, I really am a university-type person, but I would like to come and work at your place and do some exciting work and publish some papers and work on the kind of thing you're working on for a few years, and eventually I'll move on to the university." And that's just what he did; it worked out very nicely.

LeMone: You've been an active member of the AMS since at least 1947 when you wrote an article on *Opportunities in Meteorology*. How do you feel the AMS has changed in the last 40 years or so?

Simpson: The AMS has changed in a large number of ways. I first came into it as a student member and as a very junior member. It wasn't really until I was elected to the Council for the first time in the early 1970s that I began to see how the AMS worked. I think over the years it's grown considerably. It has now about 10,000 members. I think in the last several years it has been changing considerably, because many of its members and leaders are getting to believe that we ought to be more active in science education. (There had been resistance to that in previous years.) Also, we are beginning to get a consensus, I think, in the Executive Committee and the Council that Atmospheric Sciences aren't getting adequate support compared to other sciences and other Earth sciences. We'd better be finding partners and strengthening our presence in Washington to work on that. We were a very conservative Society when I first came on to the Council, a very conservative fiscal policy, and a sort of, I think, from my perception, an ingrown sort of attitude where the Society was concerned who was a Fellow and internal matters. It also had some things like accreditation and licensing programs, which were developed in the '60s. For example, the Certified Meteorologist and Radio/Television Seals. In that way it interfaced with the outside world. By and large, it was pretty much concerned with its

own affairs. It has always been an outstanding scientific and professional Society putting out excellent journals and holding excellent meetings. But it is growing more, now that younger people are getting involved, now that the World War II, more conservative generation is a continually smaller fraction of the governing body of the Society. The view is prevailing that we have to become more actively involved in the world-at-large, particularly since science education in the country seems to be so endangered. Meteorology can teach people to make measurements and look at the sky and measure the rain, which is a marvelous way to teach people basic physics and basic mathematics, and also it is a very good way to attract people into all sciences. This is not a unanimous view. I think right now our Executive Committee is fairly polarized on this. Gradually, the majority opinion is going to be that we have to take an active role in education, and we have to take an active role in finding partnerships with other groups to see that the Earth and Atmospheric Sciences are, relatively speaking, better funded and better supported.

LeMone: I know that the Board on Women and Minorities from its inception has always considered education as being an important tool. Of course, there it is in context of attracting more women and minorities into the field, but in so doing making many efforts, like the Science Fair Project, you end up attracting many more. How do you feel the role of women has changed in the Society in the last number of years?

Simpson: I think the role of women in the Society has changed from exactly zero to one of pretty equal opportunity. I think there is no activity or position in the Society that women aren't participating freely. And I think, within the Society, there is virtually no negativity or prejudice whatsoever. Of course, this year there's the President (J.S.), and we always have at least one or two women on the Council. In fact, when I was STAC Commissioner, I picked people just on basis of their merit and activity for the Committees, and there were always higher fractions of women on the Committees than there were in the membership-at-large. There was a higher percentage of women who were Fellows than the percentage of women members-at-large. Probably, women have to be better than the competition to be there, which is still true.

LeMone: There's probably, on the average, a larger commitment on the part of women meteorologists. You've been a positive influence on many meteorologists, including myself. You've recruited several into the field, one I think of immediately is Mike Garstang. Are there any others?

Simpson: I think my very first graduate student at Illinois Tech is probably a good example. When I was at Illinois Tech, I was working at Woods Hole in the summers. I started getting interested in natural laboratories that produced clouds, such as flat, heated islands, and we had both a theoretical and small observational program studying clouds forming over the islands in the vicinity of Woods Hole. The first grant I ever got from the Office of Naval Research was for \$5,000, which was enough to pay for one graduate student. My department chairman was very proud of me because very few people in the Physics Department at Illinois Tech had research grants. I think two or three out of a faculty of fourteen. And he says, "Okay, I'll take three semester hours off your teaching load, and,

furthermore, I'll help you find a good graduate student. I will look around among the incoming crop of physics graduate students and will find a good one with a fine record and who might be interested in working with you." After a few days he came back to me and said there's this guy I want you to interview. He needs the support very badly, and it seems like he would be very interested in working on the kinds of problems you are working on, and his name is Melvin Stern. So, this guy came around, he went to high school at Cooper Union in New York on some special honors program and was very, very good at math and physics. He didn't know the first thing about meteorology, but he said he was willing to learn. He started working on the heated island problem with me for his master's thesis. He had a really neat idea after reading Scorer's work on the flow over mountains of making an equivalent mountain to the heated island and that is what he did for his master's thesis. In those days we didn't have computers; we only had slide-rules. Everything had to be done analytically, and this enabled us to do a neat, analytic solution of the equations for the flow over a heated island depending on the wind speed, the lapse rate, and the surface temperature. Anyway, that was his master's thesis. He had just about finished that when summer came. I had a little money left in the grant, and I got a little bit of help from Columbus Iselin, because I wanted him to come to Woods Hole for the summer and get exposed to what heated islands and clouds really looked like. So he did. He did more work doing that and got very interested in the work that Henry Stommel was doing with salt fingers in the ocean, and he also got tremendously interested in ocean and ocean-atmosphere interaction problems. Then he got drafted into the Army and was gone for a while. But he came back and did his Ph.D. at the Woods Hole-MIT consortium. He did this on a problem in oceanography, and became a very well known oceanographer. He was at the University of Rhode Island up until the last couple of years. He now has an endowed chair at FSU.

LeMone: How about Mike Garstang?

Simpson: Mike Garstang always wanted to be a meteorologist. I first met him, during the 1956 expedition with the Woods Hole PBY aircraft down to the Caribbean. Most of the places we went to the Weather Stations down there, there would be people sleeping, and they allowed you to look at the weather maps, and that's about it. We'd been exploring a group of clouds that day, and it looked like what Herbert Riehl had described as the classic easterly wave out there. So I said that before we went into Trinidad to land, let's send a message saying it looks like there's an easterly wave out here, and we'll come into the Weather Station and discuss it. We got in there about five minutes to 6:00 pm. Garstang said later that he was supposed to go off duty at 5:00. It was only just an accident that he was there. He said, "So you are those crazy jerks that said there was a Riehl-like easterly wave out there. That's not what goes on here at all." Then he started drawing for us the weather maps using a different concept of tropical wave which is somewhat similar to Clarence Palmer's concept of waves in the tropical Pacific. I was very interested. But here was a guy in the British Colonial Met office down here who was studying these things himself and getting rides on Caribair Airlines whenever he could, and going out and photographing clouds and checking up on his ideas. There were three or four of us from Woods Hole. He (Garstang) said, "Why don't you come over to my house. I would like to show you what I'm doing for my master's thesis." So we said,

“Sure, we’d like to.” It was so refreshing to find anyone down there who was interested in anything. We went over to his house, and what he was doing for his master’s thesis was the study of the effect of the Island of Trinidad on the airflow and on the clouds. So I told him what we were doing at Woods Hole, and he was extremely excited, and this went on for several days. It didn’t all happen at once. We were there for about a week. I said I might be able to figure out a way to get him an opportunity to come up there and work with us for a while if he was at all interested. He just about jumped out of his skin; he was extremely interested. So when we went back to Woods Hole, I went to Columbus Iselin and explained that he was a South African citizen at that time and had gotten his degree in Geography at the University of Natal, I think. He was doing the island effect as a master’s also at the University of Natal, in Geography, which is where they had their work in meteorology. He came up to Woods Hole, and we were very impressed with him, and he was impressed with Woods Hole, and so because of getting his undergraduate degree in Geography with no calculus and no physics, he wanted to get a Ph.D. in meteorology. I talked to my friends down at FSU, which had a fine Tropical Meteorology Department; they said if the guy is willing to go back and get the fundamentals and start over as an undergraduate and learn these things, from your recommendation, we’ll be glad to take him on. So I persuaded Columbus Iselin to find some funds to pay for a scholarship for him. I forget exactly where the funds came from, because when he went to take the courses, he wasn’t doing any research, so it wasn’t anything that ONR could support. But he went all the way back and began virtually over again as an undergraduate. At that time I think Ed Zipser was a graduate student at FSU, because Ed Zipser was always sort of a role model down there. Garstang finished his courses in a remarkable time; he got the Woods Hole ship CRAWFORD to go out and study the waves that he talked to us about on the first day. And he also studied as just sort of a by-product of that, the diurnal cycle of cloudiness over the oceans. He did his Ph.D. thesis on the results of the CRAWFORD Field Program in something like 18 months after he had started the Field Program, which is some kind of a record.

LeMone: He was very determined. Do you have interests outside of meteorology, which have enriched your life as a scientist?

Simpson: Yes, I certainly have had interests which have enriched my life as a scientist, in flying small airplanes, in sailing, and to some extent, and lesser so, in skiing, but particularly in sailing. Sailing is a wonderful way to get exposed to the realities of the atmosphere and how much you really don’t know. One of the things that I admire in retrospect about Rossby that, although everybody thinks that Rossby was a theoretician, he actually also believed in being an observer and being a naturalist. He had a strong requirement, or at least recommendation, that everybody who was going to get a Ph.D. in meteorology ought to be either a private airplane pilot or glider pilot or a sailor. I wish people still had that kind of idea, because I think too many graduate students now are getting totally involved in models without exposing themselves to observations or data or the real atmosphere.

LeMone: This brings us to the next question. Your work always seems to combine modeling, theory, and observations. Was this just a natural evolution? I suppose it was.

Simpson: It was a natural evolution, and I can remember very clearly when it first happened because I was at Woods Hole working on the Wyman/Woodcock data. I took the tropical data that existed in those days and I was trying, with the encouragement of Henry Stommel and a few others, to develop a model of how tropical cumulus worked. I remember one day we were sitting there sort of talking at the blackboard and beating our heads around. You know we can't go any farther in this until we get some more observations. Why don't we see if the Navy still has any of those PBY aircraft and maybe we can not only put back the instruments we had in the Wyman expedition, but also make measurements of a few more things, particularly to get vertical velocities and liquid water. We had just been flying very small aircraft up to that point over the heated island on just a sort of hiring basis, and I remember thinking, if we get involved in getting an airplane, this is going to really eat up a huge fraction of my life. We sat around, Andy Bunker, Henry Stommel, and myself, in particular, saying, "Do we really want to do this, are we willing to commit all the time to undertaking all instrumentation of the aircraft, and installing the instruments and using screwdrivers, flight tests, calibration tests, and so on?" We finally decided that we had to; there really wasn't any choice about it, that we were not going to get any farther understanding the physics of clouds with making models of clouds without making further observations and taking what we had learned from previous observations and models. We tried to ask well-focused questions, not only repeating the same kinds of observations, but also adding other variables to these. We went into that quite consciously, realizing it was going to eat up a big part of our lives. It was with a certain degree of ambivalence.

LeMone: How do you feel about the balance today between modeling and observations?

Simpson: I think it's very important to try to do both of these things interactively—models and observations or theory and observations. There were only a few meteorologists willing. This is a hard way, because you have to understand how instruments work as well as how to do things with the computer or with theory. And it takes much longer to write a paper, and you're subject, of course, to criticism from several communities rather than perhaps just one, which I think is a minor point, but other people don't. There are very few meteorologists today who attempt this combination. I can think of some of them; Kerry Emanuel is one of the few theoreticians who actually goes out and makes observations. And people like Roger Pielke and Bill Cotton, whom I picked to work with me in part because they were people that were interested in doing theory and observations and modeling and observations together. I think many of the younger generation today are too carried away by models and too deceived to believe that the models are reality. They are so carried away by models, especially their own models, which have specific simplifying assumptions and are usually "tuned" to specific situations, that they have the tendency to say, "This is the way clouds are, and this is the way mesoscale circulations are." Many of these people really would be far more profound contributors to the field if they worked with data, if they made more sensitivity tests with their models, depending on how things vary in the real atmosphere, and if they exposed their models to tests under different kinds of situations in the real atmosphere.

LeMone: That's been the exciting thing about being able to communicate with Tao, who works with you, because he's always so intent about what really happens in the atmosphere.

During your career, you've moved around a lot. You mentioned, perhaps not in this interview, that Rossby was always commenting it would help to not necessarily physically move around, but from task to task. Did you consider this a benefit, or was this largely a matter of choice?

Simpson: I remember that about Rossby. I remember at the time that he first said it, I remember violently disagreeing with him. He used to say about six years or so in any one job or at any one university was enough. You needed the stimulations of new challenges and moving on to a different challenge at probably a different place. However, a lot of the moving I did was not done because of any reason connected with doing better science or broader science, it was simply the circumstance of being a woman married to a husband who had a job in a particular place. So it would not have been my intention to do that. More recently, I've had more freedom to do things like that, and I do think now that it is probably a good idea at least to undertake different kinds of work and to be very broad in the areas of work. One of the reasons I have had such an exciting time at NASA is because I have learned about radiation and remote sensing, and this has been a wonderful experience. If I had not gone to NASA, I would not have to undertake this broad learning experience. If you move and put yourself in the position in order to learn the next thing you need to do, you have to learn another discipline or another tool. I used to think in terms of tools, that I wanted to acquire a new tool every so often. I remember thinking I'd better spend a couple of months learning Fourier analysis. That was during the heated island research. Later, in moving to NASA, I've had to learn a lot of very different things about instruments from the kind I learned about in doing the research aircraft—space instruments and remote instruments. And radiation has been extremely valuable because the radiative properties of clouds are not only in most ways equally important, are interactive with dynamic and microphysical properties. Now we're getting to a point in cloud science where we are beginning to put cloud radiative models and cloud dynamic models together. Similarly, a generation ago we were beginning to bring cloud dynamic models and cloud microphysical models together. Again, if I had ever dreamed back in the Woods Hole days that someday we would be able to measure cloud properties from space, I would have said that's Buck Rogers' stuff; I don't believe it. If I could get an airplane that can get into the top of a cloud, that's my farthest dream.

LeMone: Just out of curiosity, when you were doing your budget of the boundary layer along the stream line in the Pacific, was radiation in this?

Simpson: The radiation was in there, but we took things like Julius London's figures and assumed radiation was constant. This is very interesting, because just recently Alan Betts and some guy named Ridgeway, attacked that same problem allowing the radiation to vary and interact back with the cloud systems. I had wished at the time we were doing that work, and when Herb and I were doing the sequel of it where we were actually making a model of why the trade wind clouds are stable and why the trade wind inversion

is stable, we were assuming that radiation in each layer was not feeding back with how the cloud systems were changing. I knew that if we put radiation into the loop that we would probably get very exciting and important results. But we had no way of doing it at the time. So that work of Alan Betts has been extremely exciting to me because that's what we wished we could do back then. That's one of the great things about atmospheric research. In the next generation, things we had to make assumptions about, people are now able to formulate.

LeMone: That certainly is very exciting. I've even seen that paper by Betts and Ridgeway.

Since we are near the end of the interview, I wanted to ask you some questions about the effect of being a woman on your career. Do you have any general comments about that?

Simpson: Yes, I think I do have some comments. This has been gone over and gone over, so I will leave out the comments that I've made previously, not that they aren't relevant, but here I will try to look at the whole situation in perspective. I would say that being a woman always has an effect on a person, probably in any profession, and certainly in our profession. It has a different effect at different stages of your life and your career, because in my very earliest life, I was impressed, as I said in the beginning, of how helpless women were if they regarded themselves, or were in fact economically stuck in impossible marriages, and I made up my mind I wasn't going to have that happen to me. That didn't motivate me towards meteorology, I could have just as well been a lawyer or something else. But I certainly was looking ahead and seeing how life affected women and how I could cope with it. Then, of course, during the years of being married to husbands who were Ph.D.s, who had careers, I always had to make the compromise in the early years, usually going where the husband had his best opportunity and more recently, with Bob, we've traded off. We've said this is your turn or this is my turn, whatever, and it's worked out pretty well. It's always a problem; there is always one person who's having perhaps to take less than an ideal position or a less than desirable one.

Then there is also a question of children. I thought when my children were young, particularly my second son, Steven, who grew up in Woods Hole, it was very hard to get anything like reliable and intelligent babysitters. I felt he got pushed from pillar to post, and he was not too well dealt with by babysitters. It's only been in the last decade or so, now that my children are grown, that we are having such wonderful relationships as adults, that they are saying how happy they are that their mother is a professional person in science. All my children in a way are involved in science. My eldest son is a mathematician, but he is doing right now applied math to fluid flow problems. Steven is an artist, but he's using computers to do choreography. He has a background and interest in science. Karen's a science teacher. They all tell me now that they are glad I did it. The wave of horrible babysitters has sort of faded out of their minds. Apparently, it didn't do any irretrievable damage. They seem to have pretty happy memories of their childhoods.

Again, the fact of being a woman has hit me again in being the President of AMS. I have always felt that I've been carrying a big burden for other women, because if I mess up, then the chances for other women to get the same kind of job are going to be diminished.

I fear that people may say, “So you can’t trust a woman in a job like that.” In a way, I’ve probably been running harder and working harder being President of AMS than many other people have done because I wanted to be the best possible AMS President. Not that I can be the best one, but at least I must do the best job I can, so that nobody can have the opportunity to say, “Well, a woman isn’t suitable to that job—she can’t run a meeting properly, she can’t be a hard-nosed manager.” Or whatever.

LeMone: The pressure to work twice as hard is still there?

Simpson: It’s still there.

LeMone: Have some things gotten better now?

Simpson: Oh, yes, a lot of things. By the time you get to be 65 years old you realize you can be an eccentric old lady as Bertrand Russell said, you can be a licensed lunatic. In many ways I’m the same. I am an eccentric old lady, and I enjoy it. There has been that effect. However, there is another problem, but this may be due more to me than being a woman, and that is I have a big hang-up about what would I ever do if I retired, because I have worked so hard and with such enthusiasm all my life, and it’s been more than a full-time job. I’ve never done anything else at all seriously such as build furniture or done domestic things or other things that people do as hobbies. There is really nothing else I can do. A lot of people move into a different area when they retire, or they go out and play golf, or go out fishing. I don’t have any interest in doing any of those things. My greatest wish would be to be like Grady Norton, who died of a heart attack while forecasting a hurricane; or like my early hero, Rossby, who keeled over and died in the middle of giving a seminar. I don’t like the idea of when I won’t be a meteorologist anymore. It’s just inconceivable to me.

LeMone: I hope you get your wish, I’m sure you will.

Simpson: I hope it’s not a morbid wish.

LeMone: Not that wish, but to be able to do it as long as you want to. After so many other obstacles, you’re used to it.

What about for women in general today? It is probably much easier now.

Simpson: I think that up through graduate school and beginning jobs, I don’t see any negativity, except in a few rare enclaves of male chauvinism - such as MIT - that we talked about earlier. The University of Virginia is a male enclave. I think women can warn other women to stay away from these. Sometimes I think there’s actually too much so-called affirmative action. For example, the Secretary of Commerce in this Bush administration was told he had to pick a woman for two out of the three Presidential appointments in NOAA. Well, that’s ridiculous. I would like to see women appointed at high-level positions because they were the best candidate, not because they were the best women.

LeMone: I guess there's a potential for that to create a backlash. It makes it harder for those women who come after.

Simpson: I felt at the University of Virginia that there definitely was a backlash. My life there went sufficiently badly to have that be a big factor in leaving. I think women have to work harder than the competition, better than the competition, and be more dedicated. But I think if they use their sense and now there is a sufficient network of other women that are around (it's beginning to be a sufficient network) to, if possible, avoid the enclaves of male chauvinism. I don't think the problems regarding being a woman are overwhelmingly great.

LeMone: Do you have any other advice? I always remember one marvelous piece of advice that I've always followed, and that is, wherever you are, take a paper, take something to read, take some calculations.

Simpson: I still follow that advice myself. When I had to sit in the stupid dentist's office the other day, I had your paper on vertical velocities in tropical oceanic clouds³. I've been carrying that paper around. It's the only way I get to read papers with all the job hassles I have now.

LeMone: Any other advice?

Simpson: I still don't see women should be so adverse, as so many professional women are, about learning how to do things like type. Because I got my start in summer jobs as a secretary. When there came to be word processors, I learned how to use one in half an hour. Now, if I retired, I could survive without a secretary. Whereas poor Dave Atlas, who is sort of retired and living on money from his research, is always screaming and yelling and trying to get somebody else's secretary. I could manage perfectly well without one now that they have Macintoshes and word processors.

LeMone: Actually, they are ever so much easier.

Simpson: And they are ever so much easier than trying to type on a typewriter. The paper I wrote in the Bulletin on TRMM, I did myself from start to finish. The secretary never touched it. It gave me a feeling of independence.

LeMone: If they are sick, you can finish it.

Simpson: Not a very profound piece of advice. A more profound piece of advice is to learn how to use PC's because it will open up a whole new world.

LeMone: I think that concludes the interview, unless you have any other things that you would like to bring up that I might have neglected.

³ Jorgensen, P.P., and M.A. LeMone, 1989: Vertical velocity characteristics of oceanic convection. *J. Atmos. Sci.* **46**, 621-640.

Simpson: No, I think I would only like to conclude by saying that I was very fortunate to get into meteorology at the time that I did. When Rossby and his students at the University of Chicago were doing the most fantastically exciting work, that I could at least be a spectator, if not a real participant in it - a field that has been tremendously fun because it's had so many exciting frontiers since then. Looking it all over, I had problems, but who hasn't, life is full of problems. It has been a fantastically rewarding field to be involved in. And I hope other people, men and women, will find it equally exciting to be involved in.

LeMone: I just want to ask you one more question that sounds kind of fun. We were talking about the fact that the only time we have to read papers is in the dentist's office or the doctor's office or whatever. There is an interesting question here, that again involves the evolution of meteorology and so on. What journal do you read regularly?

Simpson: There's such a fantastic proliferation of journals that would not be very popular among my AMS colleagues in saying that I think we have almost too many journals in the field now. I simply don't have the time to read them all. I don't have space in my office to put them all. *JAS* is the only one I subscribe to on a regular basis. I try to look through the titles of the papers and the abstracts of ones that look interesting. Whether they are near what I'm working on or not. And then for what papers I'm going to take around to the doctor's office and the dentist's office, many are by the young guys working with me. In fact, that's where I heard about your paper. Danny Rosenfeld came in and said, "You were worrying about warm rain with regard to TRMM retrievals? Read this paper by Peggy LeMone; it has a lot of valuable insight and observations about that, but other exciting things about clouds in it, different environments." So he ran off and made a Xerox for me. Most of the papers that I carry around, either somebody gives me a copy and says you ought to read this, or I hear that I should read it. Of course, I hear people tell me a hundred times for every one that I actually read. I have to be selective because of the time crunch. I admire people very much, like Roscoe Braham, who still subscribes to every AMS journal that there is and tries to read through at least the abstracts of all the papers. But that's a different kind of personality.

LeMone: This is Peggy LeMone interviewing Joanne Simpson at NCAR on September 6, 1989.

END OF INTERVIEW