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Tape Recorded Interview Project

Interview of David Atlas
30 September 1987

Interviewer: Robert Serafin

Serafin: Good afternoon. I am here with David Atlas, former professor, mentor and close friend of mine. We are at the National Center for Atmospheric Research in Boulder, Colorado and the date is the 30th of September 1987.

Our purpose in getting together here is to conduct a biographical interview of Dave for the archives of the American Meteorological Society. I might mention that we plan to provide both videotape records and audiotape records of the interview. We have also in accordance with AMS instructions had some photographs taken of the two of us together earlier this afternoon.

Before we get into the interview I would like to say a few words about the organization of our interview this afternoon. First we'll try to follow Dave's life and career in chronological sequence beginning with his youth and family background and experiences, his undergraduate education with some particular emphasis on science and then his experiences during the World War II years, which I think were very exciting ones for the field of radar meteorology; a field in which Dave has excelled and a topic with which we will undoubtedly spend a good deal of time this afternoon. After the World War II days were his career days at the Air Force Cambridge Research Laboratories, University of Chicago, NCAR and NASA.

I would perhaps like to return to this particular sequence of organizations, a group of organizations with whom Dave has been associated through the years and make some comments on my own in closing later on. After getting through the chronological biographical background we'll talk in general and try to fill in some of the gaps. Dave and I have tried to prepare for this by putting together some thoughts and some important questions, some interesting questions that we think; the kinds of things that might not be found in scientific literature or in archives elsewhere.

With that introduction let me ask Dave to begin to tell us something about his early days as a youth, where he was born, something about his family background and early educational experiences.

Atlas: Thanks Bob. That was a very professional introduction. You make a very good interviewer.

Serafin: We will see about that.

Atlas: In any case about the early part of my life: I was the third of three children. I have an older brother who is now 69 and my sister is four years older than I. My parents were born in—my mother was born in Russia. My father was born in Poland. My father was a very energetic hard working guy who was a very delightful man who finished high school in this country going to school at night while working. My mother only finished, I don't think she finished grade school, I think she started to work at age 12. We came from a poor family but rich in many other ways.

I graduated from high school in January of 1941 just at the end, toward the end of the Depression just before World War II at a time when the family was just gaining some financial stability. Up until that time, of course, my family could not afford to send my brother or sister to college. In fact in those days they were not inspired to go. On the other hand, economically I had the privilege of going on to college and also I guess my parents took a great deal of pride in my achievements in early grade school and so they encouraged me. So, I was the only one who did get into college.

I could not go without helping to support the family. Everybody really contributed to the family and so I had to work part-time. I worked part-time both before I went to college and during my college years at the New York Public Library at the princely sum of $11.00 a week full time. But it was never less than a great experience. The people there were interesting. I worked in the map room and in the patent department at the New York Public Library while going on to CCNY [City College of New York, now CUNY].

Serafin: Dave, I have just a couple comments to make and I don't know whether it is fate or what it is that brings people together in life but it seems to me that our backgrounds are remarkably similar at least to some degree. My parents were not immigrants but their parents were. I expect much of our ethnic backgrounds in the big city environment were very similar. It turns out that my grandparents also came from Poland. But beyond that we both played the accordion and neither of us succeeded apparently.

Atlas: Yes, I forgot to mention that. It's one of the things though I did with great dedication as I do with almost everything else.

Serafin: I can't say that I was terribly dedicated but my parents thought it was something I should do. I tried reasonably hard for many years and then finally gave it up. Tell us something about your university days.

Atlas: I think the College of the City of New York, CCNY, was really a tremendous college and university really in those days. It's now called the CUNY. I started out with the intention of studying electrical engineering but only got through my first, well a couple of years before I entered the Army. I think I was most inspired into science in general by Professor Zamanski who was the famous thermodynamicist and he wrote this lovely book, an elementary physics text and I just have been terribly fascinated by physics and science in general. So, he was really the one individual that got me clearly in the direction of science.

Serafin: I hadn't thought about that name in years but I suspect I use that textbook as well as an early undergraduate.

Dave, sometime at about the time the second world war was starting you got involved with the Air Force. Can you tell us something about that phase of your life and career?

Atlas: It was about the fall of 1942 the war had started December…we got into the war that is in December 1941. I was not yet 18 but I turned 18 the following May and it was clear that I was going to be drafted sooner or later. I wanted very much to pursue my education and to do something useful in the military. I noticed an announcement in the New York Times concerning the search for students who had some university education because they had run out of graduate students, that is people with bachelors degrees, so they were looking for those of us who had some background and wouldn't have to start from scratch. So, they started this pre-meteorology program, which then led into the advanced program.

I applied for that in the fall of 1942 and was rejected. But, I couldn't understand why with my qualifications I should be rejected so I resubmitted my application and this time this mysterious bureaucracy accepted me so I got into meteorology training. It started on March 8, 1943 at NYU with six months of pre-meteorology and nine months of aviation cadets leading to a commission in June of 1944.

Serafin: Dave, those days, your early days anyway, with the Air Force you were shortly thereafter assigned to NYU. Is that so for some specific courses in pre-meteorology? It was during that period as an aviation cadet that you met Lou Battan. Would you like to tell us a little bit about those experiences?

Atlas: As a matter of fact Lou and I met on the subway on the elevated train just as we were approaching Burnside Avenue station reporting for duty at NYU on March 8th 1943. He noticed my name painted on my little suitcase and matched it with the name on his orders. So, on the way walking toward the campus we decided we would try to room together. So, we met and we did room together for a year and one half or so and that started our long-term friendship, which lasted until his unfortunate death last year. It started both of our careers in meteorology.

Serafin: After that phase you began to, or at least you were introduced to radar meteorology somehow, is that not so? When did that occur and how did that occur?

Atlas: When we finished, before we finished meteorology school something was happening. We didn't know anything about radar but later subsequently we learned that people like Joe Fletcher had been meteorologists and had been assigned to radiation laboratory at MIT and they quickly realized the tremendous value of radar for first for wind finding for tracking balloons in all weather and subsequently for storm detection. As a natural fact Joe Fletcher is presently the assistant administrator for research in NOAA. As a young captain he convinced the commander of the Air Weather Service to select and train 100 meteorologists in radar and we all took examinations not knowing what they were for but in basic elementary electronics and radio theory. It turned out that both Lou and I passed the exam and went up to some sort of school. We studied at Harvard and subsequently at MIT. But for the first five months at Harvard we didn't have any idea of what we were studying. We knew we were studying electronics and radio theory and timing circuits and things of that kind but it wasn't until we moved over to MIT five months later that it all would put together under lock and key and then we recognized that we were studying radar, which was all terribly exciting.

Serafin: At about 1945 I guess it was you graduated from school in radar meteorology. At that time did you receive some sort of degree?

Atlas: In actual fact it was merely a certificate of the coursework at the radar school out in Harvard MIT. It was really very intensive so it was equivalent to at least a master's degree in electronics if not more.

Serafin: When did you receive your undergraduate degree?

Atlas: I didn't mention it actually in the notes that I sent you but I actually received my undergraduate degree from NYU in February '46. I simply aggregated all the credits that I had accumulated at CCNY, at NYU in meteorology and in the Harvard MIT experience. I asked NYU to give me

a bachelor's degree and that's when I actually received the degree in February '46.

Serafin: After that, sometime in 1945 or about that time, you were assigned to the all weather flying division at Wright Field and this is, I assume is, where you really got into the use of weather radar both for research and applications. Tell us about those early days.

Atlas: I was really very fortunate to be assigned to Wright Field. The war was just about coming to an end; in fact the war in Europe ended a month after I got to Wright Field. It was in that period just after radar school when I went back to Siegert, New Jersey to the weather equipment training school, I guess it was called, wets or wits and it was at that time I remember that I was walking from my home to the school waiting to be reassigned and I learned of President Roosevelt's death in I think it was April of 1945. It made a tremendous impact on me and I remember breaking down and crying. But I arrived out at Wright Field in early May I think it was in 1945 and all we had were a few hot pilots, Colonel Philpot who was the commander of the all weather flying division. We had a B-17 equipped with an APS 15 radar, APS 15, 3 cm radar and nothing else.

A few months later however we were moved down to Clinton County Air Force base, which was a sub base of Wright Field and then the group grew substantially. We equipped a couple of B-25's with APQ 13 radars and some basic meteorological instruments and some terribly crude recording instruments. All our meteorological observations were photographs of instruments, which had to be read frame by frame after the fact. It was exceedingly tedious to do but we did some elementary research in aircraft storm studies. Of course it was just subsequent to that that I started my studies of the quantitative estimation of rainfall by radar using a ground base APQ 13 radar 3 cm system.

Serafin: You noted that Joe Fletcher was your boss about that time and you also became involved in the original thunderstorm experiment.

Atlas: Yes. That was really a very exciting period for a youngster. You remember at this time now I was 21 years old and Joe became my boss in I think it was the early fall of 1945. One of his jobs was to act as the liaison between the Air Weather Service and the Weather Bureau and the academic community and the aviation community, which recognized the importance of understanding the hazards of severe storms. So, this was the beginning of the concept of the thunderstorm project. I was Joe's assistant, he was major Fletcher and I was second Lieutenant Atlas. In fact if it weren't for Joe I probably would never have made first Lieutenant. There was a landmark meeting such as you have repeatedly

now to planning big projects in those days in the fall of 1945. Joe and I flew up to Chicago for a meeting at the University of Chicago where all the big names in meteorology were present for this historic meeting to plan the thunderstorm project. Of course Horace Beyer's was already designated the director of the project but Henry Houghton was there and Henry Harrison and Alan Bemis and Ross Gund. Everybody who was anybody in the field was there. The project took off and grew like _____ it was really amazing how quickly it was planned and conducted.

Serafin: That I found interesting here in your notes that the project was apparently organized very rapidly. We don't seem to be able to do that these days. Can you speculate on why that may be so?

Atlas: I think that those were unusual circumstances. First of all we were all keyed up right after the war. We had all these people, the staff, the pilots, the equipment and strong motivation. We recognized here was these fantastic tool radar, which could see into these storms and the aircraft were equipped. We had magnificent radar facilities down at Orlando with the Air Force facilities and similar facilities were built up at the all weather flying division in Ohio so they were all ready. Perhaps it was our ignorance about the complexity of thunderstorms that made our design simplified. We thought we could really carry it off and it was carried off. I shouldn't say we because I was really always on the outside of the project. I was assigned, I was working at Clinton County Air Force base and that was the place, which the headquarters of the thunderstorm project in the second year in 1947. But you've got to remember that the project was carried out, the first year of the project was carried out, at Pine Castle, Florida in the summer of 1946 within less than nine months after some of the major planning conferences. It was really quite an astounding thing to do.

Serafin: And resulted in papers and monograph and documentation that is still used as a fundamental reference today more than 40 years later.

Atlas: Yes and a lot of the things that we are discovering today like microbursts can be found in the data of the thunderstorm project.

Serafin: At one time we knew that the Earth was round and then we began to think it was flat for quite a while, mankind did anyway.

Atlas: We tend to rediscover things in different ways and give them different names.

Serafin: One of the things you were doing in those days was your work on isoecho contouring, which of course has proved to be a highly successful and useful tool for both research and perhaps even more so in commercial

aviation. Tell us a little bit about that. What led you to conceive of the concept?

Atlas: That was really one of my first very exciting experiences in terms of discovery actually there was one I wanted to get back to if we have the time later. I started to work on measuring rainfall quantitatively by radar and it was clear that the outline of the echo was a contour, an isoplast, of constant power equal to the minimum detectable power at that particular gain setting. So we started our first measurements were simply to reduce the gain of the receiver on successive rotations and their vying buildup.

Serafin: It's interesting, let me interject here, I just returned from Taiwan where the people in Taiwan have recently installed a modern Doppler radar at their major airport but at one of their universities, I believe it's the Central Taiwan University, they have an incoherent weather radar and they are using step gains to calibrate reflectivity factors in their precipitating systems there.

Atlas: When you don't have modern data processing facilities you've got to resort to imaginative things like that although at this stage we consider it primitive. At any case the point is we did map quantitatively in step gain the reflectivities or the echo powers, which then could be converted to reflectivity. But it was clear it was taking us a long time to do this so I was trying to devise a scheme of getting all the gains simultaneously. I remember sitting at the bar, not that I was a big drinker but it was one of the few placed where bachelors could congregate in this little town of 5,000 people in Wilmington, Ohio. I remember Afsari Schwiller our B-25 pilot and I were sitting there at the bar drinking and I was thinking there must be a way of doing this contour mapping quickly and automatically and it just occurred to me that if instead of using one receiver with step gain, stepping the gain of one receiver, if I could use two receivers with different gains and butt one against the other. So, we simply butt the one receiver against the other and put a hole in the core representing another contour in the hole of the first. It was so simple that we actually implemented it the very next day by using two receivers. So, that was a very exciting period to see how this could be done and of course it was one of the first things, one of the exciting things that led to my first patent.

Serafin: You eventually patented that and made a little money off of it I think. We don't need to go into that but I think RCA was one of the people who bought your patent or the rights to it. You mention that this was actually your second patent disclosure and I wondered what the first one was.

Atlas: Actually the first one was a very complex cumbersome scheme to correct for attenuation at x-band, 3 cm radar attenuating going through rain. So, It was clear that if one could measure the echo power of the front side of the

storm and had some means of amplifying the next increment of range by the attenuation that one would hopefully come correct for the attenuation. We subsequently realized that it was taught by Walter Hitchfeld and Borden that this could lead to very serous errors.

Serafin: Erroneous results. That has been a problem of course facing the community for quite a long time. I have heard recently that the French believed that with two antennas looking at storms from different aspects that they can in fact create the equivalent of a radar tomographic image and accommodate for the attenuation regardless of what the mechanism is that leads to that attenuation whether it is water, ice or mixed base. So, this is very exciting particularly for some of the airborne applications where shorter wavelength systems must be used. It takes a long time to solve problems sometimes.

Atlas: I hate to say that I thought of that really but it did occur to me that if I could obviously because if you looked at the storm from the other side the backside would be different. So, it did occur that we should utilize two radars. But there is something that is missing in this isoecho contour mapping so that related to the thunderstorm project. Here I urged, when the thunderstorm project came down to Clinton County I urged Horace Beyer's, I can only call him Horace in retrospect because we subsequently became good friends despite the disparity in our age. He was not convinced that the radar should be modified and It disappointed me greatly and in retrospect it disappointed everybody else because in none of the observations of the thunderstorm project do we see any internal structure. Nevertheless I had a great respect for Horace.

Serafin: Dave, it is obvious that you are still a young person and yet were very creative. To what do you attribute that capability?

Atlas: I think it was to a great extent I was fortuitous; I was very lucky in the first place to have been trained in two fields, which mesh so magnificently meteorology and radar. So, it was a natural blend and a very exciting one. I suppose of course being so brand new of a field it was ready for all sorts of discoveries. There were few other people in the field and moreover every time you turned on the radar there was something new, something novel to see so that it was a very exciting time. Of course I was motivated strongly by family conditions to go out and earn a living. I had been sort of stimulated toward science in the first place by my early college days and then just tremendously excited by everything that we utilized in those days. Of course I would have to attribute part of it to the fact that this was a very heavy experience for a young lieutenant to be thrown into the mist of these great scientists whose names I had read in the literature and who have had that experience in September of 1945 of taking virtually the first radar observations of a hurricane. All of these things combined; so there

was strong motivation, a tremendously exquisite excitement of discovery and invention and of course there was this ego satisfaction the idea of being recognized for such achievements.

Serafin: There was also the incredible opportunity, I think the educational experience is a good one and perhaps we can get back to that later, but the opportunity was truly a unique one at the time. I wonder if one could point today to similar opportunities in the atmospheric sciences or in meteorology.

Atlas: Those opportunities such as were provided by radar were and subsequently by satellites and computers were really rare; these were big events, the tremendous technological advances, which made a whole new realm of research activity possible. So, that kind of opportunity perhaps doesn't exist but there are new kinds of opportunities now because now we have all the tools at our command. We have the satellites and they are not simply subjective tools we can make very quantitative observations with many of our remote sensors from satellites. We have the radars and now they have become very sophisticated and soon they will be more heavily distributed. We have these powerful computers and data processing things. So, I think the future is equally exiting if not more so in many respects to go on to blend these tools and to make further advances.

Serafin: I think you've answered one of the questions I had planned to ask you later. After the days during the thunderstorm project and Dayton you moved onto the Air Force Cambridge research labs. What was it that motivated you to go there or was it an assignment over which you had no control?

Atlas: Actually it was a combination of things. The main motivation was that I had met Lucille on New Years Eve of 1948 and just about the same time the geophysical research division or directory was being formed at Washington laboratories as part of what later became the Air Force Cambridge research laboratories. So, I wanted to move East and Lucille and I got married in September and subsequently although we had started to look for an apartment in New Jersey at Boston laboratories adjacent to Ford monolith before we did that the lab was moved up to Boston to Cambridge as part of the Air Force Cambridge research labs. So, it was an interesting combination of circumstances, which got us up there. Interestingly enough our first director was Al Trokowsky who was a captain and following him Joe Fletcher became the director. He was now a full colonel and he became the director of AFCRL.

Serafin: Who was your immediate boss in those days, immediate supervisor at AFCRL?

Atlas: My immediate boss was a fellow by the name of Nathan Gerson. He had an electromagnetic propagation laboratory so they were dealing mainly with ionosphereic problems so radar fitted into that for that reason but it was really an inappropriate assignment and subsequently we joined Pete Wyckoff who was later at…

Serafin: He was at IIT. That's where I was.

Atlas: And subsequently at NSF in the atmospheric sciences program. That was I guess the cloud physics laboratory at which I was a part. Then we started with a vertically pointing 1 & 1/4 cm radar. In fact we did a great deal with that radar in terms of looking at precipitation growth, which was my dissertation at MIT.

Serafin: It was about that time that you got involved in your graduate work as well. Was that not so? Tell us about that.

Atlas: Here I was doubly trained in radar, in electronics and radar and meteorology. It was clear to me that the real excitement in terms of discovery was going to be in meteorology. So, I entered my graduate work in the department of meteorology at MIT working full time but going to school part time three hours a week for seven years until I got my doctorate.

Serafin: You met and mention a number of people with whom you interacted during the days at AFCRL. Roger Lameet was one, Keith Browning was another, people from Japan such as _____ and _____ and Frank Ludlow. You made a comment here, which I found interesting you said that no one I know, and this is a quote, "No one I know compared to Frank Ludlow as an observer and naturalist." Perhaps you could expand on that a little bit.

Atlas: That was actually about 11 years later after I joined the AFCRL GRD in 1948 and took a, I got an NSF senior post doctoral grant and did a years sabbatical at Imperial College in London. The idea was originally Frank and I were thinking about doing an experiment in northern Italy to study thunderstorms but that fell through for a variety of reasons some of which I can't remember and we decided to do the experiments in England, which is possibly the worst place in the world to study thunderstorms. Nevertheless we were lucky and some important things were done.

Frank who never had a bachelors degree and went directly from, without a bachelors degree, he got a doctorate simply by submitting all of his papers to Imperial College and was awarded a doctors degree. He had a tremendously, almost a magical insight into what was going on in the

atmosphere. He could look at a cloud and tell you what was going on in the cloud or the storm. Of course if you go back to some of his work in the compendium I think there is a beautiful article on the nature of fall streaks, of cirrus fall streaks. If you go through his bibliography you will find a beautifully written and pictured both visually and schematically storm structures, what the flow field in the storm must need to give you its geometric patterns and to cause its growth. Just look at his article, I think its Meteorological Monograph #5, which is a masterpiece and of course his book **Storms and Clouds**, which was finally published after his death in about the early 80's is a magnificent book, which anybody interested in that subject should read.

Serafin: How was it you came across Roger Lameet? I have the greatest respect for Roger and I believe that you could be attributed with having brought him from France to the U.S.

Atlas: You have to go back a little. Actually it was Stewart Marshall who started the stormy weather group at McGill University and we it was really Marshall who really stimulated me early on because we, the Air Force Cambridge research labs at GRD, actually supported the work at McGill University for starting in 1949 and pursued that for 15 years. Stewart had invited Lameet and I think it was at the end of his visit to McGill University that I got him to come down to Blue Hill where we had our CPS 9 in the Blue Hill observatory in Boston. He came down for a visit and subsequently we communicated and I finally convinced him to join our group, which he did in 1961. Of course that association gave birth to a whole host of things. Lameet had and still has tremendous capabilities across the board. He is the, in terms of technology of radar and the ability to utilize the radar in a sensible way to study the atmosphere, no question about that.

Serafin: Dave, I have two more questions about your days at the AFCRL. You were there almost 20 years I guess, from about 1948-1966.

Atlas: 18.

Serafin: One was for the record when and where did you actually get your PhD. Was that an MIT degree?

Atlas: Yes. Both masters in '51 and PhD in '55.

Serafin: Secondly what was your position when you left the AFCRL?

Atlas: During that entire 18 years I was chief of the weather radar branch whose members included people that you well know and who were important colleagues of mine through the years including Ralph Donaldson who is

still associated with it. Ken Glover who was head of the branch was a buck sergeant. Graham Armstrong a technician. Roland Dusche, Ray Wexell was associated with us as a collaborative contractor, Keith Browning joined us for four years after his PhD and did fantastic studies of thunderstorms both at AFCRL and he did his first work on the _____ storm in England during that year that I spent there and I must not forget Ed Kessler who worked with us for 7 or 8 years in the early 50's. Then a series of visitors who were like Nyto, Tachechi Nyto from Japan who collaborated with me on a number of things they were clear air work. Vernon Plank on the angels and which subsequently led to the work at Wollup's island.

Serafin: I think and I'm sure you would agree, most people would agree that those were very productive years for the AFCRL and you noted here some of the key elements that led to that productivity. Perhaps you could expand on those and identify them for us.

Atlas: In looking back when you add it up I really was quite astounded…(tape end)…to be harvested. The field was ready to be harvested but we did a tremendous amount on precipitation growth, synoptic studies, hurricane rain bands, lightning studies, clear air studies and the whole 20 years of angel work culminating in the work at Wollup's Island and the proof that we were in fact seeing clear out.

The ingredients are difficult to really identify them but I think I can pick out perhaps four, which I thought were important. The first one is a chicken and egg problem. We had tremendous institutional support partly because I think we had four sided leaders and partly because we had quickly generated a track record for discoveries. So, they gave us tremendous support and flexibility. We had the freedom to follow our hot ideas and the things that were discovered or observed serendipitously. Then of course we were greatly stimulated by our interactions with people like Stewart Marshall and Walter Hitchfeld at McGill University. We exchanged visits a great deal; I was up at McGill University at least 4-5 times a year and they came down to visit and then there were others who stimulated us. There were catalytic interactions with people like _____, like Browning and Ed Kessler and of course Ray Wexler and a few others that I hope I don't miss the important ones. That was terribly important.

Then I think did something also which is a key to creative work and that is that there was a mix certainly in the observational sciences there was a good mix of technology and science. We seemed to oscillate from a technological advance to using that for understanding the atmosphere and once we used it we recognized the limitations of the technology and we got a new idea for advancing the technology and this way we stepped up one step at a time and it was a very effective way to go. I think it is a

lesson for others certainly in the observational science but they're analogs in other elements. I think that finally there was a willingness and excitement to take risks and no penalty, at least that we could perceive for failure. There was almost no failure because if we failed in going after one target there was usually another.

Serafin: Every observation resulted in a new paper in those days.

Dave, you had a good deal going, a really nice situation at AFCRL yet you chose to leave for the University of Chicago in 1966. Why was it that you left AFCRL?

Atlas: There were a combination of factors but I think that I had reached in 1966 I had attained a certain level of distinction in the field. I had been receiving tempting offers from a number of universities and I had some offers from industry. Then there was a group that broke off from AFCRL the GRD, Geophysical Research Directorate and formed the Geophysical Corporation of America or GCA. Then a few people started to leave. Browning left just in early '66. Lameet had left a little earlier and Kessler had left a little earlier. Once those things happened there tends to be a snowballing effect and of course when I finally got an offer from the University of Chicago which was my image of a great university and when they rolled out the red carpet it made it so attractive for me. It was one of the…at about that time the universities were also becoming competitive economically so they could offer me a decent salary. The combination of factors got me to the university and of course I think perhaps I had this image of what a professor, perhaps a distorted image, of what a professor is and of course the distinction of being a professor and the roles I had in the professors that preceded tempted me on.

Serafin: Tell me about the days in Chicago. You came to an entirely different environment and began to set some directions for the department there.

Atlas: That was a very exciting period. It was a period of which is really had some grave doubts in the first place and I remember having before I accepted the job at the University of Chicago I insisted that I see President Meedle, George Meedle, who insisted on our first meeting that I call him George. In fact one of the nice parts about Chicago the university was that everybody regardless of his position insisted on being called by their first name. In any case I went to see him and I said look I have tremendous ambitions to build a major observatory here and he said go to it, not realizing that I would have to really go out and get the funds.

Serafin: He realized that but you didn't perhaps.

Atlas: I think they put a great deal more faith in me then I had in myself. In any case we were successful in 1967 in getting something like $650,000 to build what later became known as the Chill Doppler radar and has been upgraded since and is still a very powerful system. I was scared because I had not been in a classroom for many years and here I had to learn the basics, relearn the basics, to teach the students, which was a terribly important experience for me. It was a very good learning experience for me as any teaching experience is. But then in order to make it go we didn't have any electrical engineering at Chicago so we formed then, as you well know, the joint laboratory with IIT on laboratory for atmospheric probing of which you are a member. So, we had good electrical engineering talent. I brought over Ramish _____ from India as a post doc and probably one of the best moves I ever made, a tremendously powerful scientist and versatile. But we didn't wait as you well know it was a long hard grind to develop the Chill, I say the CP2 because CP2 was almost…

Serafin: Patterned after it.

Atlas: Patterned after it, the CP2 with NCAR. In any case at the same time I think there was a resourcefulness on our part perhaps in the name of my natural impatience. The first year, for example, we went to visit work with Berkamyer at Wisconsin on troposcatter from turbulent layers and because I had just come from this tremendous experience at Wollup's Island of seeing turbulent layers on radar of these terribly powerful radars at Wollup's Island. We were able to make an interesting match there so there was a very important insight from that work with Berkamyer. Then following in 1969 the collaboration with Rifter on using the high resolution FMCW radar at San Diego and then the subsequent year bringing Metcalf and Strockman. Strockman was a post doc from Germany and Metcalf was one of my first PhD students. We went back out there to really investigate the structure of these layers, which was really an eye opener and of course I think was probably one of the things that gave us definitive insights into what was going on in these layers. Of course when we went and penetrated them with a Buffalo having…that was another interesting combination of technology because for the first time as a result of the work by Jim Telford and from DRI, the University of Nevada, and Don Linchow they developed the air motion sensing system and so we could for the first time measure the three dimensional flow field and _____ in the atmosphere. So, that led to Metcalf's dissertation.

Of course there were a number of other opportunities, which particularly in the Doppler realm, which were interesting. For example I think the students all had very interesting experiences since we didn't have a very powerful Doppler radar as a scanning system. We sent Ian Harris to work

with Browning in England. In fact Browning sent us a data package, which was on film, which _____, _____ and I analyzed in Chicago. So, there were a host of opportunities while we were waiting for this radar to gestate and to grow we took advantage of all of these interesting opportunities.

Serafin: Dave, I was your first graduate student I think, although my degree is not from the University of Chicago, you were my PhD advisor on Atmospheric Turbulence Measurements with Radar. But during this period and I mentioned this to you earlier, I feel that there was a great deal of excitement in the field of radar meteorology. At this time the field was perhaps 20 years old and what we were seeing happen at about this time were several things coming together that were allowing us to begin to exploit the potential of Doppler radar, the full potential of Doppler radar for research. You might want to comment briefly on that.

Atlas: I think back to those days. Lameet had come; he had really conceived a great deal of the early methodology of Doppler in using the pseudo-Doppler techniques in France. When he came to AFCRL we were fortunate enough to take advantage of the availability of the porcupine Seaban radar, which was available at Lincoln Laboratory. So, he and I did the first work on the velocity _____ display at AFCRL.

We immediately anticipated a wide realm of research activities with Doppler radar and of course that is when we started to build Doppler processing equipment. You were one of the key designers of the equipment. Although we had a lot of vision there was something missing at that time and that was the technology of data processing. Data processing and display systems I guess whereas we were only able to measure with either one gate or at least a few gates. In order to display the full range of information we had to get all the gates really working simultaneously and that became part of a lowly, I suppose just about the time in the early 70's when I got to NCAR. Nevertheless it was again in looking back it is quite remarkable to think of what we achieved at Chicago and again what has been achieved by the students who came out of that period who are now leaders of the field in their own right; yourself and Wit Carbone and Peter Hildebrand and John McCarthy and Jim Metcalf and Art Jamieson. These are all the leaders, well not all of them but a good fraction of them.

Serafin: This is a prominent group in the field today. One could ask why is it or what is it about that experience that produced so many leaders in the field now two decades later? Time does fly.

Atlas: It is difficult to speculate. I don't think there is any clear cut recipe but there are a few things that I have wrote down and I have thought about this

because we're in troubled times right now with respect of educating observational people particularly are concerned about radar meteorology. I think that some of the things that produce these successful people, very creative people who are these leaders now are first of all they had a thorough grounding in both theory and instrumentation. Secondly I think that Ramish _____ and I complemented one another very nicely because I was intuitive and he was theoretical, really established the firm theoretical basis for my intuitive insights. Thirdly I think that the interactions with the electrical engineers were really important, you and Les Peach and Ken Hay at IIT.

Then I think that every student that we had had some observational experience, field experience, either by going out in the early days with Metcalf and I went out to San Diego we went up to work with the troposcatter system that Wisconsin mentioned that Ian Harris going to England and so forth. Every one of them and later on of course Rit Carbone and Jamieson worked with Henry with the hail program. Then there was also a sense of excitement I think about radar meteorology from a combination of things. They were already making their own discoveries and I was able to stimulate and inspire them from recounting my own experiences in the field. I think that that brought some livelihood and vitality to the activity. Then of course I think the university itself, the department itself, gave these students a thorough grounding in the disciplines themselves in cloud physics, Roscoe Graham and Ed Fujita and _____ meteorology and the others in fluid dynamics and the turbulence theory as well and convection. So, they knew about the technology, they knew about what the tools could do and they understood many of the problems to which they could be applied. I'm sorry to go on at this length but I guess you sense the feeling of excitement that I feel recounting.

Serafin: In being a part of that. I'm not certain that we were aware of it at the time but I think that we were doing some basic research and development that has led to 20 years or more of a whole next generation of research with meteorological radar, the Doppler realm particularly and to new operational radar systems being deployed in the United States and all over the world. I think we can look forward with satisfaction not only on the research results but as well on the lives and property savings that are going to be obtained in the future. It's wonderful. I don't know that many people have that opportunity in their careers.

Atlas: I think it is terribly satisfying and rewarding to take that kind of satisfaction because we can see not only that the diffusion of knowledge but the people that worked with us, our foreign visitors for example and the students have gone to a variety of places. The ideas have diffused and the technology has diffused through the field. Of course we can't take all

of the credit but it is surprising how much really emanated from that small group.

Serafin: Dave, those were good years and I know gratifying to you and rewarding to you and to me however you left the University of Chicago after about six years in 1972 and came to NCAR. What was it that led you to come to NCAR at that time? By the way previously you had stated that you have grave doubts that NCAR would ever achieve the excellence or the prominence that was expected of it as a world-class institution.

Atlas: I remember when I was getting offers from Macademia and Walt Roberts and Phil Thompson asked me to come to NCAR to look around and see whether or not I would be interested in joining NCAR. I guess that was only a year after it was established. I came here and what was NCAR was located on the University of Colorado campus in the armory. They had very little facilities and a lot of ambitious plans and here I was coming from AFCRL where I had vast resources at my command plus all the resources of the Air Force if I needed them and really almost unlimited funding in those days at least in the later years. I looked at this organization and it didn't seem very solid although I had a great respect for Walt Roberts. I figured a bird in the hand was worth two in the bush and I never anticipated that NCAR would get so far, as far as it did. But later on I learned my lesson although the six years that I spent in Chicago were an exceedingly valuable and irreplaceable part of my career and a learning experience.

The fact is that in order to sustain the kind of level of program, which we were going, we were conducting at the university, which had about at its maximum about 18 people. There were 6 or 7 students, 2 or 3 post docs and several engineers at IIT technicians. It was a third of a million dollars per year and to bring that in we had to have five contracts bits and pieces incrementing from I don't know how many agencies so it was a really horrendous job. Teachers, scientists, salesmen, researchers and it was almost impossible to sustain and I think that is an important thing that perhaps we can come back to later because this is the same experience that others have had in other places around the country; in building and sustaining a, what I would call a, cutting edge research activity in academia in observational meteorology in general and radar meteorology in particular.

So, here was NCAR now it had some facilities and it looked like a natural place for me to come and to utilize those research tools without the burdens of being a perennial salesman.

Serafin: You were hired by Walt Roberts and John Firor and what was it they expected you to do on coming to NCAR?

Atlas: My expectation at first was that we would establish a first class program in radar meteorology but at just about that time, don't you remember this was early 1972 and NCAR was being very severely criticized. I think you have to go back and remember that was the time of the joint evaluation committee the JEC report came in December of '72 and NCAR was being criticized by the academic community for a loose management, excessive freedom, not being required to subject your right proposals and peer review, doing things which could be done at universities, not supporting, not providing the kind of resources to the universities which they demanded and I think a lot of this was motivated by jealousy. Of course at that time remember, about that time there was a sudden decrease in support to universities and so they started to look at NCAR as a competitor and I think that was unfortunate and I am happy to see that NCAR survived that period although there were certain changes in NCAR's operations at that time.

Serafin: You came and you took over the Facilities Division and changed its name to the Atmospheric Technology Division. I want to know whether there is anything to the rumor that you suggested that it be called the Advanced Technology Laboratory for Atmospheric Sciences with the obvious acronym ATLAS.

Atlas: I was not responsible for that. The one man around here who is most responsible for interesting acronyms I think is Vin Lowley who came up with all sorts of interesting acronyms like GHOST for the Global Horizontal Sounding Technique or something like that. He thought it would be cute if he named the laboratory Advanced Technology Laboratory for Atmospheric Science.

Serafin: Ah, it was Lowley.

Atlas: So please don't give me credit for that.

Serafin: Ok, I'll do that.

Atlas: But it was clear that because of the criticism…

Serafin: Credit or accuse you of that.

Atlas: It was fun. Because the Facilities Division at that time was criticized very severely for not serving the community as well as it should; not being responsive entirely to their needs or providing high quality observations either from aircraft or from other observing systems that something had to be done. At about this time Walt Roberts and John asked me to take over the Technology Division or what was then the Facilities Division.

Serafin: This began a new era for ATD. You hired a bunch of new people one of them was I. I remember when you left Chicago, maybe you don't remember it, you said that you would have a job for me in about a year at NCAR and I didn't take that very seriously but it did come to pass and it probably has been one of the better things that has happened to me career wise and for that I thank you. You might tell us a little bit about the reorganization that took place and the change in emphasis in ADT under your leadership.

Atlas: I think that part of the changes were really obvious but it clearly had to be more scientifically based and responsive and in order to do this it was…well partly we have to go back to a little bit of history and that is the fact that NCAR was built up very precipitously. It was manned very precipitously and people were hired to fulfill all of these functions and some of them, the choices, were not necessarily well made although I think that Walt did a magnificent job in building the institution. There were, in the Facilities Division there were a number of people who were available simply because they had recently retired from the military services and that had reached high levels and were good high well-known managers. But at the same time they were perhaps past their peak in terms of motivation and imagination. I can't say this for sure but at least they were not doing the things that the community demanded.

Serafin: I think I would put it slightly differently. They weren't trained or skilled in the kinds of activities that were necessary to move NCAR into a new direction. The example that I always give is when I came to FOF. FOF was about 60% devoted to setting up tents and providing housing for people who were off on eclipse expeditions. That was a useful and important service for the scientific community but it certainly was not characteristic of an atmospheric technology division, which should be at a place like NCAR at the cutting edge of the technology.

Atlas: Yes, well you were among the first that I looked to. We wanted to bring in people who were creative, imaginative and leaders in engineering and science and meteorology. One of the things that we had to do was bring in a little research capability to make sure that we didn't build instruments that were not capable of doing what they were alleged to do or advertised to do. I remember how badly the aviation facility was criticized because the instruments didn't perform according to specification or it was so difficult to process the data. So, I remember that one of the things that we did was to bring Don Lenchow in from what was I forget what the division was subsequently AAP. He was a turbulence researcher who also knew about instrumentation so he really helped vitalize that and put it into new directions. The same thing was true in FOF where you yourself and others started to do research and interact with the community.

Serafin: I guess I would like to comment on that briefly. You knew Jim Deardorf very well as I do and recently he is now at Oregon State University. I had the opportunity to visit there and gave a seminar and Jim attended that. Jim has been gone from NCAR for ten years or so. My seminars' topic was the use of modern Doppler radar for the detection and study of the initiation of conductive storms in the clear air before there are any clouds or any hydrometeors presents. Afterwards Jim came up to me and said he was really delighted to see that some of this technology looked like it was some sort of a marvelous invention looking for a purpose, at least that was his perception of it and I think maybe perception of many scientists at NCAR at that time. It was really being used now effectively in the research community and elsewhere. I feel that the reason that that has happened through the years, and if one looks and tracks your career, can always see at each institution with which you have been involved this inter-disciplinary approach to research and development. Scientific emphasis and technological emphasis are there in virtually every step of your career. Would you like to comment a bit on that?

Atlas: I think it's a rather natural process. It was certainly unplanned but it was natural in this sense; you have a tool and you suddenly use that tool to observe the atmosphere and if you have any curiosity at all about what that means you want to understand that phenomenon that you are observing. So, you set up a little research project to go further and you bring other tools to there. Then you find that you need better tools, higher resolution more quantitative tools to do the job. So, you go back to the drawing board and you think well you know what the tools can do and you know their limitations now so you want to go to the next step. It's this natural progression throughout. For example, throughout astronomy of building bigger and higher resolution telescopes, radio telescopes. So, I think it was natural but I think partly it's due to the intrinsic curiosity and this feeling that science or research is like a detective mystery; trying to unravel a who done it. It is an important process and I think it paid off when we started to do a little bit of research with an ATD. It frightens me the scientific components of NCAR a little.

Serafin: Indeed it did. In fact I remember John Firor once commenting on this that I guess this used to be called management committee meetings in those days but the philosophy of research scientists being in the atmospheric technology division was questioned and John responded I think very diplomatically. He said he didn't care where research was done at NCAR as long as it was good research. That helped me at that time, a relative newcomer into NCAR was really being challenged by many of these very well known and famous scientists here.

Atlas: Before you go on I'd just like to interject to you that I was, well I don't want to use the word amazed because in retrospect it isn't amazing but when you get up and speak so eloquently about meteorology and so knowledge fully about it I am somewhat surprised. The fact is that you picked up so much meteorology by your interactions with the community that you gained tremendous respect in the community and as a result ATD and NCAR have as well.

Serafin: Well it's a formula that works and perhaps it has not always been planned but it is now planned at NCAR and I think it's one that we never want to deviate from.

After getting ATD started Dave, you were asked to take over the directorship of the National Hail Research Experiment after Bill Swinbank had passed away.

Atlas: That's both an interesting and partly sad partly painful experience but in retrospect a very positive one. Bill Swinbank and I were very good friends even before he joined NCAR. He was one of those who participated in some of those very landmark conferences on turbulence and scatter in the troposphere and particularly in the boundary layer. But Bill was a very sweet and gentle person. I loved him dearly. NHRE, the National Hail Research Experiment as you well know was a high-pressure project. It unfortunately had a dual mission, an applied mission of seeding clouds and determining whether or not we could suppress hail and a research mission to understand them. It was clearly under great pressure because the Russians were claiming tremendous successes in hail suppression there. In South Africa a commercial seater was claiming great success and in fact throughout weather modification you heard only successes. Nobody seemed to fail. If they failed the first time they would search and search for some physically plausible reason to explain away their failure and they could almost always come up with a plausible reason. But in any case it was under great pressure and I think that unfortunately Bill did not give vent to his frustrations and I believe that some of the pressures under which he labored, as director of NHRE may have been responsible in part for his sudden death at Christmastime of 1973. Because of the urgency of NHRE and the unavailability of anybody else on the scene John Firor asked me to take over the hail program.

Serafin: Those were troubled and turbulent years I think and yet _____ the National Hail Research Experiment I feel contributed dramatically to the entire field of weather modification and meteorology. Tell us about that.

Atlas: Let's face it when I came into to NHRE after it was already into its third year of seeding. In fact I think that year when I joined the project I felt that so much of the data remained to be analyzed, that we had better stop

and give ourselves a breather to analyze the data and to understand what it was and what we were doing and what we were doing wrong. So, we did stand down for a year but at about the same time I took over I invited Keith Browning to come here as chief scientist for a year of NHRE and with his tremendous insight he really learned from _____. He has a fantastic intuitive insight into the workings of storms and in analyzing some of the data.

In that early period of my tenure he and Bradford quickly came up with the idea that it was entirely possible that the seeding of hailstorms, of convective storms particularly supercells with an overabundance of liquid water, could in fact increase hail rather then decrease. That was a politically hot potato but NCAR agreed to publish it after my exam and the potential legal consequences because we were seeding up in areas where there were a lot of farms.

In any case I'm rambling here a little but there was a great deal of superb research going on with Foote and Fankhauser in terms of the inflow field relative to the storm, the work with the T-28 and penetrating the storms and analyzing the microphysics, the work with the sailplane, Charlie Night's work on microphysics when they used the sailplane and on and on. So, there was a vast accumulation of solid knowledge and there was a great deal of skepticism about whether or not we could do anything about suppressing hail but we were under the gun. We were under the gun because of these other claims of success elsewhere and so if in our case the statistics, which were very limited, sparse we're neither here nor there they weren't showing any response. As a consequence, which the community of weather modifiers who were believers they were religious believers; they said we must be doing something wrong.

So, this program which was under the management of the NSF research for applied national needs ran, convened an advisor panel, a review panel to look at what we were doing wrong. One review panel said there is new management D. Atlas had just come in and we were going to put it right. This advisory panel was headed by Bernard Habre who came from industry and he was management oriented. That was not the problem obviously but that was not what some of the program managers in NSF wanted.

Serafin: A lot of it was the way the clouds were being seeding was one of the problems.

Atlas: That's right. There were a lot of manipulations behind the scenes, tremendous political pressures and emotional pressures. That was not the kind of recommendation they wanted. They wanted a recommendation to break it up into a seeding program, which they wanted to handle after a

commercial operator and a research program. That simply was not possible because the same people had to participate in both activities. In any case because the tremendous…(tape end).

Serafin: Both are think are functioning effectively again and Dave and I will have to watch the videotape to be sure we don't overrun.

Dave, we were talking about NHRE.

Atlas: Yes. I was just saying that I was about to say that we were under tremendous pressure because there were thinly veiled threads that if we didn't correct ours ways that the program would be cut off. Of course the idea of dividing it up into a commercial seeding program and a research program was an affluent to me because I felt that as I said before every one of the modification programs particularly the commercial ones always reported successes. I thought it was important that an unbiased credible institution such as NCAR should do the work. On the other hand I felt that we had to go back to the first principle because we simply did not understand the fundamental nature of these storms. We did not understand that it was entirely possible, which I later wrote about in Science Magazine in January '77 that it was entirely possible that the Russians could have been successful in modifying hail although I subsequently found out they didn't believe there was a reason not to believe it and it was entirely possible that the seeding in Colorado storms would not be effected because of the fundamental differences in the physics particularly the cold-based clouds in Colorado and the warm-based clouds in the Soviet Union.

Serafin: In my view I think that this is a very important basic finding as the way I understand it anyway. Prior to NHRE it wasn't understood that the hydrometeor formation process in cold-based clouds at least in the high plains begins with an ice process.

Atlas: Oh yes.

Serafin: There are no large super cool droplets in these clouds.

Atlas: That's right. What do the Russians call the large drop zone?

Serafin: Yes.

Atlas: Which Sulock believes he spoke about. But there was an interesting coming together at that same time because I felt that it was entirely possible that this was the case. Because I was on the PhD committee of Lorne Nelson at the University of Chicago, I suggested that he use this to model this. He was doing a microphysical modeling of precipitation

formation and hail. His dissertation showed that this would in fact result. Depending upon the temperature of the cloud bases and the nature of the embryos form in which hail formed that the responses to seeding in his model would be different. All this came together and I thought particularly with the same time that Browning and Foote warned us that we could be increasing hail, gave us pause and we felt that we should go back to first principles. But because of the pressures and claims of success of others we were obliged to go on. Since I felt that I couldn't proceed in this fashion, I resigned.

Serafin: It was not long after that the National Hail Research Experiment ceased to be.

Atlas: Yes, that's right. I felt a sense of vindication there although by that time I had made the decision to leave NCAR and go back east. I think NHRE was an important turning point for all of weather modification because it recognized; first of all it didn't make absurd claims of success. Secondly it recognized that they didn't know enough about clouds to make them, to control them as black boxes. Thirdly that there were paradoxes that storms in different regions could respond differently and you couldn't transfer the concepts and physics from one reason to another. As a result much of weather modification altered their cause.

Serafin: I know that that was a difficult time for you personally and a difficult time for those who were participating in the NHRE experiment. In retrospect I think the community as a whole recognizes the contribution of this program to meteorology and to weather modification. It was an intellectually honest program, one that was willing to come forward with negative results, as they were perceived to be and one which has placed the entire field of weather modification on a more intellectually honest foundation. So, I think the program did something for the weather community as a whole.

Atlas: Yes. In retrospect of all of those negative experiences it was a very painful one as I said. But in retrospect it was really a very important part of my entire career. It was a learning experience and I learned about some of the problems of managing a big program under very severe pressures.

Serafin: Dave, after NHRE I know you had some time at NCAR to write some papers and I don't think we should dwell on that but I know you did have an opportunity to get back into some research, some independent research. But shortly thereafter you went to NASA. Once again there was an organizational challenge facing you.

Atlas: Yes. I stayed on at NCAR for a little over a year after I resigned as senior scientist and that was a very productive one but it was also a year of

decompression. I went from running an organization with about 75 people and 150 in the summertime to being a lonely senior scientist up here on the fifth floor in the advanced study program without any support whatsoever. But we did have some post docs around and I had a backlog of research and so it turned it out to be valuable.

At the same time we had family problems at home that is on the east coast. Lucille was interested in getting back to care for her mother. Our children were mostly in the east. At the same time Bill Norenberg, who is now deceased, with director of applications at Goddard Space Flight Center and Bob Cooper the director of the center both of whom were really tremendously enthusiastic gung-ho types came out and made me an offer I couldn't refuse to invite me to take over as associate director of applications for meteorology and essentially to build up a new laboratory. There was an existing activity going on, a modest activity and Bob Cooper gave me essentially complete _____ in hiring and building up a new laboratory. I won't go into the gory details of doing that.

It was an exciting time too. It was the beginning of '77 just when the Carter administration was taking over so there were new people coming in. We had essentially four years in which to hire and to build up a laboratory. Using my University of Chicago experience and the NCAR experience we recognized that the selection of people was crucial to the success of the organization and so we pursued that with utmost care and I guess a combination of luck and care we recruited some very good people. There were something like ten stars some of who really didn't show their stardom until later on but there were some supernovas if you like. In a lab of a 100 people, plus there were about 100-150 contractors depending upon the time, that's about all you need because they become the centers of intellectual activity, the centers of gravity or centers of mass if you like for research. They can motivate and stimulate and excite a whole array of people. We were very lucky in doing that and we also in that period I decided that I would not make the hiring decisions unilaterally. That was very valuable because we shared the experience and each of the branch heads had a different perception and if we did make a mistake they shared the blame as well as the credit. But we were also fortunate.

It was difficult to say that we planned it ahead of time. There was a lot of fortuitous aggregation of activities, which were mutually interactive and catalytic. For example, we brought down the global modeling and simulation activity from GISS as part of the laboratory. That was the context in which we could A) incorporate remote sounding from space, temperature soundings first and develop the whole not only the rationale for use in utilizing space technique but the actual methodology for inverting the sounding and incorporating them into numerical models. Then there was a group of remote sensors who finally had a rationale and

a modus _____. They knew they suddenly perceived the meteorology to which their tools would be applied. Of course there were oceanographers and radiation transfer people, severe storm people like Joanne Simpson and Yale Minz came from UCLA having retired from UCLA and he was like reborn. All of these things came together in a somewhat fortuitous manner and the lad became rather distinguished in just 3-4 years. Although some of the stars have gone onto other places it still remains a very credible laboratory.

Serafin: NASA of course was a different kind of an organization with a different kind of management. I know you haven't always agreed with NASA's management style or principles. Would you care to comment on that at this time?

Atlas: I think there are two things; I don't know if you are alluding to both. First of all until 1977 when the new administration came in Washington NASA operated their disciplinary programs by establishing lead centers and Goddard was the lead center for meteorology. There was a lead center for environmental quality at Langley and I forget the other centers at the present. There was a very small headquarter staff headed by Marv Tepper. But then when the new management came in they decided to pull all the managers into headquarters and now you had a growing group of program managers, some of who really were not qualified to really establish programs. Some of them not realizing their full limitations started to direct and manage from above while others who were appreciative of their limitations and of the fact that scientific programs should be grassroots programs. They were the ones who counted upon us to develop programs. There was this conflict between the management handed from above and the management at Goddard and I must say that I spoke out rather forcefully and perhaps sometimes too candidly about it and was not entirely well received by those who wanted to manage tightly. There was a natural conflict because some of the managers actually superceded, or what is the word when you in bridge? Preempted?

Serafin: Preempted, yes.

Atlas: Preempted the management responsibilities of the laboratory manager.

Serafin: Dave, perhaps now it's time for us to start to pull it all together. Let me make a couple of introductory comments. Perhaps the most significant one, maybe it's the only one I wanted to mention, is that one in examining your career sees a chain of successes dating back to your early research and development days as a young man to the establishment, not only of an astounding and remarkable research record but of a number of centers and institutions and organizations that continue to leave their mark as centers of excellence in the atmospheric sciences. These are the AFCRL of radar

meteorology branch, the work you did at the University of Chicago and what has spawned from that, the influence you had on NCAR in the facilities divisions, the results of the NHRE project and then lastly the development of another center of excellence at NASA an institution not necessarily known for its scientific excellence. This is a truly remarkable accomplishment and I know you should be proud of it. I think that it's widely recognized in the many awards you've received and tokens of recognition I think are testimony to that.

But if we try to synthesize now, ask some general questions. Perhaps we might talk first about major accomplishments and we have a few more minutes I think on that videotape yet. Major accomplishments, scientifically and perhaps organizationally in the past 40 years or so. These are personal accomplishments. What few would you consider to be most significant?

Atlas: You had warned me that you would ask me a question like that and I wanted to be reasonably compact here. I should refer to some of the notes that I made on this. I think that perhaps the greatest satisfaction, it's difficult to categorize them in terms of priority of satisfactions there all important but I think first of all it's really been tremendously gratifying to have had a reasonably profound and broad impact upon the development of radar meteorology from its early days to the present. Of course I take considerable pride in that. Secondly I really take great satisfaction from the people with who have gone onto great accomplishments themselves; yourself for one, all the students that were associated with us in Chicago, _____ who I mentioned earlier as one of the most versatile scientists, the influence perhaps that I had on Browning and Kessler and Lameet and the influence that they had on me. I already mentioned the catalytic interactions, which I've had with people. The associations I had with Stewart Marshall, with Frank Ludlow, with Ray Wexler, with Ralph Donaldson and the foreign students they have gone out and been all around the world so that is tremendously satisfying.

Then you see the history of the trail of beautiful facilities, which we helped to develop. There is a trail, which I've left behind, which is interesting. The facilities, which were started at AFCRL some of which have disappeared but some of which have grown from those started there. The Wollup's Island facility is still thriving and now it's being rebuilt and many of the CP2 here and the things that you gone onto develop. So, that's a big satisfaction. But mostly I think of the people I've been associated with; I didn't mention Karl Obrick who was a longtime colleague and now a young associate _____ and Goddard and more recently my colleagues in Japan with whom I'm working on the space born radar techniques and those at JPO. It's really a tremendous pleasure.

Serafin: You didn't mention Lou Battan. I know that you and Lou went to school together but probably didn't work closely together throughout your careers yet I know interacted frequently.

Atlas: Actually an actual fact; we only coauthored I think one paper but that was a really phenomenal experience with respect to the nature of scattering from hail that you may remember. That was a mutually catalytic activity again. I use that word over and over again but it is descriptive of what goes on, serendipity and interactive catalytic work. We had gone to England in 1959 to study storms and while waiting for storms to occur in this climate we just said what can we do, we had three radars at different wavelengths so I suggested to Frank that we look at the scatter from hailstorms, which were not well known. We were astounded by their large magnitude. I knew that Ben Herman and Lou Battan had programmed mean scattering equations on their IBM 704 at Arizona and I wrote him I said, "I think you should see if you could replicate that." So, they did the calculations and I have a tape, which is Lou's recounting of this event, which he recorded for me just three weeks before he died. He told of his side of it. They did the theory and we did the experiment and they matched point by point with all the little detail _____ in the mean scattering curve. We were both astounded on both sides of the Atlantic so that was a very important experience. We published separately but subsequently we did a joint paper with _____ and _____ from Clarkston and Herman and Battan from Arizona and I think myself and perhaps one other from the AFCRL.

Serafin: And what else? I wanted you to mention a little bit about…

Atlas: Lou had a more pervasive influence on me. He was a role model for me in many ways although he was only a year older he was not as explosive a character, as emotional a character as I. He thought things out very carefully in advance. He was my sounding board for virtually every career move that I made. I must say that he was almost always right. Of course he was my best man at our wedding, my roommate during those 15 months at NYU and a fast friend through all the years.

Serafin: A fast friend in many of us through the years. He has had a profound influence on meteorology.

Dave, we are going to turn the tape recorder off for a moment here to change tapes…(tape end).

END OF INTERVIEW