American Meteorological Society
University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview of Chester Newton

March 13, 1990

Interviewer: James Fankhauser and Melvyn Shapiro

Fankhauser: This is an interview with Dr. Chester W. Newton. The date is March 13, 1990. The interview is being conducted at the National Oceanic and Atmospheric Administration Research Laboratories in Boulder, Colorado in the office of Dr. Melvyn Shapiro, who will serve as my fellow interviewer. My name is James Fankhauser and I'm a scientist at the National Center for Atmospheric Research. This is Tape 1, Side 1, and let's begin.

You were born in Los Angeles, is this where you received your early education?

Newton: Well, I left, my family left Los Angeles when I was about two years old or something like that, and I don't really like to claim Los Angeles in my past. I spent about a third of my early days in southeastern Alabama, about a third in Berkeley, California, and various other California towns. And about a third in Arizona in Phoenix. And my education, such as it is, was spread among those places with Phoenix being the last one for high school and junior college.

Fankhauser: What was your father's profession?

Newton: My father was a salesman and on and off he was affiliated in a wholesale grocery business with his brothers, a family affair. He had periods of trying to be a gold miner and various other things.

Fankhauser: Did you have brothers and sisters?

Newton: I had one brother who died about fifteen years ago, if I have the date right.

FANKHAUSER: Were there any secondary school teachers or other professionals who had a particularly strong influence in your life?

NEWTON: There was, I mainly recall one math teacher who I had in Phoenix Junior College, now Phoenix College. His name was Robert J. [Hamily?], and he was the man who gave me the most confidence in myself and tried to get me to go on to a career in mathematics, and to get a scholarship, and I don't know whether it's good or bad that I didn't take him up on that, but he was an inspirational person.

SHAPIRO: Did you have interest in meteorology during your early years?

NEWTON: I didn't even find out about meteorology until I actually got into it. I don't recall that I had any special interest, and I really didn't know anything about it, I didn't know that the field existed.

FANKHAUSER: I see by your dossier that contrary to the experience of most of your contemporaries you were apparently exposed to meteorology before the military service.

NEWTON: Well, I was in fact. A friend of my mother's saw an ad, a small ad, in the Phoenix paper. This is when I was emerging from junior college in which the Weather Bureau was advertising, or the Civil Service was advertising for people to apply for a position as a weather observer. And I followed that up, took the examination, and was appointed as a weather observer junior grade in the Weather Bureau.

FANKHAUSER: And then did you get drafted? Or what?

NEWTON: Well, no, I was not drafted. I spent two or three years as a weather observer and that was a position in which I was extremely competent, perhaps the most competent thing I ever did. I was working along, I attempted to enlist in the Air Corps, I had been interested in aviation since I was a kid, and I wanted originally to be a pilot. But I was turned down, the main reason being that I was very skinny and I was underweight. I tried eating bananas before the examination but that didn't work. They asked me if I'd eaten a lot of bananas. And anyway my subterfuge came through. After that I had tried to enlist in the Navy as a ______________________________ and I was denied that because I didn't have a college degree of all things. Then I don't recall how it came about, but somehow I wound up being appointed as an aviation cadet in the Air Corps. There I had the same disability, I just had two years of college, but it was waived, and my being underweight was also waived. So then I went to the University of Chicago as an aviation cadet.

FANKHAUSER: You were a student and faculty member at Chicago during its heyday. Were you conscious that you were associated with the avant garde of meteorology at that time?

NEWTON: I don't know whether I recall being conscious of the avant garde, but it certainly was hard, I appreciated that more in later years than I did at the time. I was conscious mainly, that I was in an exceedingly exciting place, and that a great deal was going on, exploring areas that were previously totally unknown in the field. Or almost totally, I might comment on that later on if you ask me something about my interest in the history of meteorology.

FANKHAUSER: Well, the faculty at that time included Rossby, Petterssen, and Byers. Can you recall some of these associations?

NEWTON: And also you might mention Dave Fultz and George Platzman and who have we left out, I don't know. Well, I certainly recall those associations. Well, Rossby was the one that steered me around for many years of my life, including the fact that he ultimately led me to Palmén. Palmén was, during my student days at Chicago, was a visiting professor there, and he came regularly every year, and he's the one who taught me synoptic meteorology, and I continued a lifetime association with him.

Besides regular staff members, of course, Rossby was a great organizer and a great human being. And he invited people to come there, and among those were Palmén and Bergeron, [Deeberg?], and a host of other people? These people brought a great infusion of different ideas into the department. And in fact it was a very small department consisting of a half a dozen people and not much more than a half a dozen graduate students. It was greatly enriched by these visitors. That was something that Rossby carried on later at the University of Stockholm, which was mainly I think a matter of having visitors from different countries come together.

FANKHAUSER: So there was a lot of daily interaction between the faculty and the students?

NEWTON: Yes, I think it may be partly the matter of small size that fosters that kind of thing. In fact everybody was interacting all the time, and there was all kinds of excitement going on. There were actually two periods that I spent at Chicago and one was in the late 40's. We had foreign visitors, we had foreign students, among them what are now called lifelong friends. ___________________and (inaudible-phone ringing). But one of those people that I didn't mention of course was George Cressman who was a very important person in the department and went on to do other important things. The atmosphere in the department was remarkable. You asked about interaction among the people and it just an extremely thorough interaction on a very informal basis. This went on not only scientifically, but it was very important to all of us that the human aspects of life was something that Rossby and Palmén and people like that fostered, insisted on us taking part. We couldn't just be students, but we had to be students of life. Another character at Chicago was Erwin Biel, who took us around to nightclubs. He took us to view all the statues that had been erected in Chicago and all kinds of things like that. So they attempted to infuse in us a feeling for life as well as work and drudgery. But the work wasn't drudgery because it was just so exciting at that time.

SHAPIRO: The sounding network was just coming on wasn't it? The upper air sounding network?

NEWTON: That's right, the global, or the northern hemisphere sounding network was just reaching the point at that time. And of course communications were an important part of getting the information to us, and publication of data by the Weather Bureau, so that on a daily basis, for the first time, it was possible to analyze weather maps on a hemispheric basis. And I had the pleasure of doing this under George Cressman. One of us would do the upper air and the other one would do the surface maps on a hemispheric basis. There were daily map discussions which was an essential part of what was going on at the department. And you didn't just drift into the map discussion if you felt like it. It was scheduled and Rossby insisted that everybody, all the staff and all the students, should come and participate in the map discussion. Of course it was led, typically, by Cressman giving a discussion of what the situation looked like. Pointed to the maps and his interpretation of it. Then followed by a general discussion led mainly by Rossby and Palmén. And sometimes there were fairly, these were not routine matters, but they were vigorous and sometimes rather hot discussions about interpretations of what was going on.

FANKHAUSER: Was Petterssen there at the same time as the other two?

NEWTON: No, Petterssen was not there at that time, but my association with him came after I went off to Stockholm and came back to Chicago.

FANKHAUSER: How did your visit to Stockholm come about?

NEWTON: Well, I got my degree, my PhD, I think it was in 1953 or thereabouts.

SHAPIRO: '51.

NEWTON: '51. And got off kind of by the skin of my teeth because Rossby had invited me to Chicago. One of the things I mentioned earlier about sort of the social aspects, and this brings me to how I got my degree. There was a restaurant, a famous gourmet restaurant in Brussels called l'Epaule de Mouton, where at the time, if you wanted a potato you could buy it for a dollar in addition to whatever you ordered, it would be $5.00 now. But Palmén had confided to me that after I got my PhD that it had been in fact awarded in l'Epaule de Mouton. These guys were great on eating wonderful food, drinking wonderful wine, finishing with a cognac and a cigar. And that's where the real serious business got done, and I think it was decided between Rossby and Palmén, and Byers, that the time had come when I could get my degree. Anyway, Rossby invited me to Stockholm to sort of be a participant in continuing the work that he had started at Chicago himself. Not working directly with Rossby, but planning to contribute to the general--

FANKHAUSER: So there was an interim there where Rossby left Chicago, went back to Sweden, before he came back to MIT.

NEWTON: He didn't go back to MIT, he stayed in Sweden. He invited my wife, Harriet Rodabush Newton, and myself to come there as part of a team. I might mention that both the University of Chicago work on the general circulation, and they continued to work along those lines and leading into numerical prediction and all those things, was sponsored by the Office of Naval Research, which in a way was a predecessor of the National Science Foundation, and that sponsorship was very important of course, because the whole works couldn't have gone on without that. That's not just a formal acknowledgement, but it's telling how things were. And that connection, I remember how it was you got money. You went and talked to Jim Hughes at the Office of Naval Research. When I went to talk to him, I remember I said, would you like me to write quarterly reports or annual reports what. And he said, "I don't want either of those, give me publications." And that's the way things were, and he decided you got the money.

FANKHAUSER: Back in Chicago, what was your research topic for your PhD degree?

NEWTON: My research topic for the PhD was shear lines. Rossby was very excited about shear lines, which are lines in the upper troposphere, and bottom of the stratosphere, where currents flow side by side from the south and from the north. In this very sharp discontinuity. This contrast was in the form of a wave. Rossby was very excited about shear lines, which well, he was the great man of waves, of course. But he liked new things, and during the particular years that I was doing my PhD work, or graduate work, there was an extraordinary number of shear lines. The atmosphere kept breaking down into these things Rossby called cracks in the atmosphere. Discontinuities between currents. I think Rossby talked me into doing this topic, I might mention also that when you did a thesis in those days or when you were a PhD student, in my own case I was never sure who my sponsor was. These days you have to have a thesis committee and you have to have a sponsor who shepherds you through the whole thing. Well it was clear it was Palmén and Rossby, but it was never formalized. But anyway, I worked on shear lines, through Rossby's inspiration and with Palmen's guiding hand. The papers that I wrote pretty well died. And there haven't been as many shear lines in the entire time sense as there were during that year that I worked on shear lines. Did I answer that question?

Shapiro: Yes.

Newton: I might mention something that Rossby said to me later, at Stockholm. I spent a lot of time working on thunderstorms and he took me aside, and sort of mentioned to me on one occasion. He said, "Why do you want to work on all these small things like thunderstorms? You should work on the big things in the atmosphere. Those are the important things, the large scale circulation."

Fankhauser: You had the squall line paper, your first squall line paper coincides with the awarding of your degree. So you were working on these things at the same time.

NEWTON: Well largely I got my first squall line paper which was published in 1950, came as a result of my association with the Thunderstorm Project, which was run by Horace Byers and Roscoe Braham, and by Lou Battan, who was my direct supervisor, poor fellow. I got interested in working in squall lines while I was working on the Thunderstorm Project. They had file drawers of data there that were necessary to do analyses, and it really started out good. I started because I because interested in them. I was not told to do it. And in fact I made myself unpopular by working on them rather than whatever I was assigned to do.

Fankhauser: Assigned in the context of the Thunderstorm Project?

NEWTON: Yes, the Thunderstorm Project, I came to realize later, did have a job to do. You had to analyze divergence, and you had to analyze vorticity, and you had to look at the radar data and so on. And I didn't fit very well into that scheme. I took part of my time and did what I felt like doing. And that's, anyway, as a result the work there had not anything to do with my student work at Chicago where I worked on squall lines.

SHAPIRO: Do you have any recollections about some of your fellow students in the Chicago environment?

NEWTON: Well, a number of names I can recollect off hand. I mentioned earlier about Yeh and Shei the fellow student of mine and getting their PhD's at the time. There was Noel Lesseur who has always been a distinguished person in topical meteorology since those days. At that time he was involved with Riehl in a project creating the monograph on jet streams, and monograph on forecasting in middle latitudes which was very important for the development.

Shapiro: So Herbert Riehl was in Chicago?

NEWTON: Oh yes, I failed to mention Herbie. Of course he was a great influence on me at that time and later. And of course he had a great many distinguished students, particularly in tropical meteorology. Another one of my student associates was Ken ___________________ who wrote a couple of very fine papers with Palmén at the same time that I was working with Palmén. Dorothy Bradbury was a presence in everybody's lives at that time, including yours, I think, Jim, who was a very wonderful person to work with. And Jim Carson was an associate of mine. These people all went in different directions. I'm sure there are names that I have not mentioned, but I think it has to be said that we got so much inspiration at Chicago that a large proportion of the students that went there went on to positions of distinction in various ways.

Reverting to Yeh and Shei, the Chinese students who were there because of the war, because they had stayed after the war, they both went back to China and we had the pleasure, Harriet and I, thirty years after we were students together, if my arithmetic is right. We saw them first again in 1979, that wasn't thirty years, but anyway, Yeh at that time had become the director of the Institute of Atmospheric Physics at Academia Sinica. And a very important figure. An organization that was doing important research. The Academia Sinica is a working organization. And Shei had become director, or the head of the department of Geophysics at Peking University. So those are people who went on to exert perhaps the two leading influences on Chinese science. And there are other essences of that among students who passed through Chicago. There was some kind of spirit there that infused these people and what they did later in life.

SHAPIRO: You spent some time in Woods hole. How did you make out that association?

NEWTON: Well, Woods Hole was kind of a holding orbit between returning from the Institute at Stockholm to a job back in Chicago in 1953, I think it was. Rossby had kept his irons in the fire at Woods Hole, and he was the one who saw to it that I had the pleasure of spending a very good working summer at Woods Hole.

Fankhauser: Is that when you formulated your interest in the Gulf Stream?

NEWTON: Yes, that's right. At the time I had started some work on the process of frontogenesis before I left Stockholm. And at Woods Hole I got my first look at meteorology [oceanography?]. Of course, Woods Hole is the place where they go off and sail across the Gulf Stream and make all kinds of depth measurements -- I mean soundings. And I became interested in the Gulf Stream as an analogy to the jet stream in the atmosphere. There I got to some of the important and interesting characters in oceanography. Fred Fuglester who was an important, a very able observationalist. Henry Stommel was there. Columbus O'D, O'Driscoll I think it was. Iselin, an intrepid sailor and director of the Institution was there, so I got a look at all of those legendary characters.

SHAPIRO: Your work with Palmén on frontal structures coincides with Reed and Sanders at MIT. Was there any communication between the two groups while this was going on?

NEWTON: No, I think it was quite remarkable that there was none at all. There was one occasion I remember when Dick Reed and I talked about fronts during my summer at Woods Hole. But I think that was the only time. I think this was most regrettable of course, because we could have learned something from each other. Reed and Sanders happened to write a paper about middle tropospheric frontogenesis processes at the same time I was writing the paper about middle tropospheric frontogenesis myself. But there really was no connection between the two.

Shapiro: But before the frontogenesis diagnostic studies that you did, you were also involved with Palmén in such problems as the mean structure of the polar frontal aloft, the 1948 paper. Could you say a few words of how you became involved in that problem with Palmén?

NEWTON: If I remember, I think that was the subject of my thesis that was assigned to me by Palmén. What is consisted of, well in those days there were some considerable uncertainty about just what the jet stream and the frontal structure looked like, and the paper in 1948 was an attempt to clarify that by compositing information from, I think it was a dozen cases, to get a more thorough picture of what the fronts and jet stream looked like than you could from observations at a particular time. Anyway, that was my assignment for a thesis and it was the topic that I presented at my first AMS annual meeting in whatever year that was. 1948, I think it was.

When you look back at a paper from that time, and of course that was about 40 years ago I guess, you may wonder about the primitive nature of these things. One of the things that was kind of nice was a main research fund in those days as a graduate student in those days, was that you did everything from start to finish yourself. You sat down with a roll of teletype paper and plotted your own cross-sections and maps and analyzed everything and hoped you would either have an idea or your professor would. It was primitive by present day standards, and this method of compositing that I mentioned may seem like a primitive way of going about things, but you have to realize that in those days there were no rawinsonde observations. There were radiosondes and there were pilot balloons and a few places that were radio tracked balloons. Observations did not go very high. Observations of wind, and observations were far apart, and if you made a cross section you had intervals of 500 kilometers or something like that. It was a vastly different thing from analyzing the present situation with closely spaced observations, research aircraft, and all those things that define everything very precisely, leading to the sort of things that you do now, Mel Shapiro. You learn more in one flight than as a student I can learn for a year doing research in those days.

Shapiro: When I was a student in the class taught by ___________ Le Seure, it was an historical class on the evolution of frontal concepts. And as students we were struck by, let's say the simplicity of the Reed-Sanders paper, which looked like something one could have done in a matter of weeks, in comparison to the complexity and the thoroughness of your paper on frontogenesis that came out a year later. One of the questions we had, which perhaps you could answer at this time was, the Reed-Sanders was fronotogenesis, your paper was entitled, *Fronotogenesis as a Three-Dimensional Process,* but when you showed your diagnostics and schematics you dealt with frontolysis. You worked, you had diagnostics on the exit delta region of the front rather than the entrance portion as was previously treated by Reed and Sanders. Can you comment on what motivated you to write a paper on fronotogenesis and do the diagnostics on frontolysis?

Newton: Well, I don't think I have a straightforward answer to that. But I think I had used that particular case, or maybe it was something that I analyzed for Palmén. It was, the case that I analyzed was a very straightforward case where there was a diffluent zone downstream from the frontal air, and I regarded at that time, the process of three-dimensional frontogenesis as the opposite of the process of frontolysis. And I worked on the frontal end, the frontal-- ___________________ end of the jet stream. I think that it's kind of a geographical thing, that you get a lot of these situations over North America where during the formation of an upper level trough, which is partly conditioned by what we now know as injections of potential vorticity. These things tend to occur, a lot over the central and eastern part of the United States and it's a geographical circumstance more than anything else that they had looked at frontolysis. I also was interested in this end of, what essentially is the exit region of the front and jet stream, because that's where some of the other processes like cyclogenesis and so on take place.

Shapiro: In that same paper you had diagrams from which it would have been very easy to do potential vorticity calculations. Have you had any thoughts in regards to potential vorticity features, maybe the tropopause at that time, or was it just coincidence that the figures were there and the calculations weren't made?

Newton: I was somewhat aware of it, but I didn't -- I had no appreciation at all of the importance of what would later become the importance of the potential vorticity perspective. That's where Reed and Sanders had me beat. My analyses were more thorough than theirs, but they were sort of mechanistic, but Reed and Sanders were the ones in that 1953 paper of theirs, that proposed or observed the high potential vorticty along isentropic surfaces and fronts, and suggested that this had to come out of the stratosphere. And that was a great perception which has gone on to great things later on. I didn't have that perception, I don't think. I don't think I knew the difference between potential vorticity at the level surface and potential vorticity on an isentropic surface which is a very important thing of course.

Shapiro: Prior to the Reed and Sanders and later Reed & Danielson, Kleinschmidt had done extensive work on stratospheric potential vorticity clouds and their interaction with the troposphere. Do you feel that Reed and collaborators had an awareness of the extensive work of Kleinschmidt at the time that he became involved in this problem? They don't refer to it in their papers.

Newton: I have no reason to believe that they did. Because it happened that when I was in Stockholm in the early 50's, just before these frontogenesis papers were written, Kleinschmidt was there, and he was talking about this concept. So I think this development went on probably independently. I don't recall that people, even our group in Stockholm, paid a whole lot of attention to Kleinschmidt's paper which I thought was kind of a nice thing and it got published. And it took its place in the collection of literature, and really it only came back to life with the importance that it deserved ten or twenty years later when it came into full recognition. In the meantime of course Kleinschmidt died so he never enjoyed the fruits of his success.

Shapiro: But Kleinschmidt was indirectly resurrected by Phil Thompson who gave me the Kleinschmidt articles in German, and they were translated at NCAR and later became very important components of Rainer Bleck's numerical studies. So Rainer really brought Kleinschmidt forward in the American journals and it all came around through Phil Thompson, which is something we'll probably discuss shortly when we get into your NCAR days.

FANKHAUSER: What is the chronology of the Palmén-Newton volume? How did you get started?

NEWTON: Well, we got started because Jacques von Liegen, who was the editor of the Academic Press international geophysics series, thought it would be a good idea if Palmén would write a book on what was later to be called Atmospheric Circulation Systems. So he issued the invitation to Palmén, and Palmén came to me and said, "I would like for you to work on this book with me, because I need you to clarify my European English." But I found out there was more to it than that; of course there was a great deal of work, and what Palmén was truly, really trying to do in this process of inviting me to write the book with him was to give me a little continuing education that I badly needed. And I learned a great deal from him during the process of writing the book.

It actually went on over a period of years, after von Liegen issued the invitation and I think it took us eight years working on and off with Palmén coming here, and me going either to Stockholm or Helsinki before it was finished.

(multiple conversations; inaudible)

NEWTON: Even Kansas City, and the Weather Bureau was generous enough to let me have time to do that sort of thing. At the end we sent it to von Liegen and he told the publishers to go ahead and publish it, no reviews or anything of that sort, so it was nice avoidance of the formalities of having things reviewed for publication.

FANKHAUSER: You're revising it now aren't you?

NEWTON: I've been revising it for an equally long period, and there are figures that are up on my wall, but there have been many delays.

FANKHAUSER: Were there any other interactions in Chicago that we ought to discuss?

NEWTON: I should mention that my second association with Chicago was after I came back from Stockholm and the Woods Hole visit in 1953, and at that time I went to work for Sverre Petterssen and his weather forecasting and analysis group. And this was another one of the great experiences of my life. Petterssen was a man who was very exacting and very interesting, and sometimes I enjoyed him and sometimes I did not enjoy vice versa. But I learned a great deal with him. It was really Petterssen-- I learned from Palmén the structure of the atmosphere and how fronts fit into the structure of cyclones for example, but I learned from Paterson how cyclones actually work. And I was fortunate to be there in those days when he was going through his experiments with Gordon Dunn and other people –

END OF TAPE 1, SIDE 1

Interview of Chester Newton

TAPE 1, SIDE 2

NEWTON: -- as I was saying, it was exciting to be there, there was kind of a marriage between university meteorology and practical meteorology in an operational setting when Petterssen was carrying on these investigations, and a nice flow of information back and forth between the practical guys and the theoretical guys. The idea of course, and this is another important influence in my life, I never had anything to do with Reginald Cockcroft Sutcliffe except for meeting him at a cocktail party with Petterssen or somebody else. But he was an indirect, important influence, he was one of the guys that I admired most. And as Petterssen acknowledged, Sutcliffe was one of the people chiefly responsible for his own thoughts on vorticity advection, and cyclone development, and so on. Sutcliffe primarily had a lot of the principals that were later found to be important both in that connection, and for example in connection with the circulation during frontogenesis.

FANKHAUSER: What was the motivating factor in your leaving the University of Chicago and joining the National Severe Storms Project?

NEWTON: Well, I think there was a combination of circumstances. First of all, I didn't get promoted to Associate Professor at Chicago, and this happened to not take place at precisely the same time that Bob Simpson invited me to go to Kansas City to be the chief scientist of the National Severe Storms Project. Basically, of course it was because I had found an exciting opportunity. I had for a long time had a great interest in squall lines and thunderstorms.

FANKHAUSER: What do you remember about the field research programs that were conducted in Oklahoma each spring?

NEWTON: What I can remember looking back, was that I didn't know very much about how to steer airplanes around, or direct them to secure research information, which is something that you are very adept in, Jim Fankhauser, and you have done a great deal with. There was an exciting prospect which brought major things for thunderstorms, on how to measure things with airplanes. There were limitations such as, for example, they didn't like flying their propeller driven airplanes through hazardous parts of thunderstorms. DC-6's were they?

FANKHAUSER: Yes.

NEWTON: There was, I remember a couple of -- I think I went flying with Jim Cook about three times and he scared me to death three times out of the three. He was a man who flew his A, was it?

FANKHAUSER: B-24 - no, no –

NEWTON: Whatever airplane, twin engine tag plane.

FANKHAUSER: He had an A-20.

NEWTON: A-20. Jim was the one who did like to go through, he liked exciting experiences and he managed to find the parts of the storm, he had an instinct for this, where you could both get measurements and have some fun, and have the life scared out of you at the same time. And I did have some experiences of that kind. In addition to the research flight facility airplanes and Jim Cook's airplane, there was a program using supersonic speed aircraft to penetrate thunderstorms, and those got some very, very good measurements of updrafts and downdrafts. The sad part of that was that after the expenditure of what I suppose much have been a couple of million dollars on these programs, they couldn't find $20,000 to process the data. And that's something that happily, that kind of situation is improved. Nowadays when funding includes processing and working on the data rather than just taking it.

The other aircraft at that time -- there were some successful measurements. Airborne radar was primitive. It didn't do us a whole lot of good, I didn't think, or think now. The Doppler wind systems and navigation systems didn't always work properly. The humidity systems nobody ever learned to calibrate, so by and large, there was a whole lot of data taken that might have been useful, but the systems were not ready for what we were trying to do at the time I didn't think.

FANKHAUSER: Did you have a good relationship with Clayton _____________?

NEWTON: I had a pretty good working relationship with him, but I never wanted to put words in his mouth, but I think he regarded me as an interloper in the project, and perhaps we weren't a close as we might have been. But maybe I just felt that way. Maybe I felt that I was being an interloper coming into a project that had some practical aspects to it.

FANKHAUSER: Did you know at the time you left Chicago for NSSP that NCAR was being formed?

NEWTON: I don't think I did know that at the time. Although I recall visiting Phil Thompson here, I think it was in Boulder sometime around that time; maybe I knew something about it.

FANKHAUSER: Did you think you would eventually join NCAR when you left Chicago?

NEWTON: At the time I left Chicago I had no idea about joining NCAR. It was Phil Thompson who called me up at Kansas City and invited me to come here, of course in cooperation with Walter Orr Roberts, who was the director of the budding NCAR at that time. So I have Walt and Phil to thank for being here.

FANKHAUSER: How much say did you have in formulating the makeup of the early synoptic group here at NCAR?

NEWTON: The answer to that is that I had absolutely nothing to do with it. I came here and found Harry van Loon and Henry van de Boogaard, and a group that was called synoptic meteorology at that time. And for some reason I was put in charge of it. But this was all set up by Walt and Phil working together before I came here. Later on, the formation of the group was more or less accidental, although I believe I had something to do with Jim Fankhauser joining it at one time because of our wonderful association at NSSP. The people that were put into the group at various time came from our leaders like Will Kellogg, who had the wisdom to invite Ed Zipser, for example. And I forgot to mention that Paul Julian was already here because he had been a member of the High Altitude Observatory which preceded NCAR. Rol Madden was invited here as an assistant to Ed Zipser and has gone on to very considerable fame. Working his way up, getting his PhD during the process. Other people who came to the group partly by accident, Mel Shapiro being one of those, Rainer Bleck, in transition from one place to another. What I have to say about the group is that it's remarkable for the distinction of its people, and as its alleged leader, I was remarkable for not having much to do with the excellence of these people. But they had the reputation of being a part of a renegade group of people who worked on what they chose to work on rather than being a tightly planned program.

FANKHAUSER: How did NCAR come to be involved in the National Hail Research Experiment?

NEWTON: Well, I don't think I can give a very accurate answer to that question, but my recollection was it was -- it came about largely and it got its support largely as a result of the alleged success of the Russian experimenters, ________________ and the like who claimed that they could totally abolish hail, and therefore save enormous amounts of crop and crop damage, and hail damage. The National Academy of Sciences made a recommendation, I believe, and the prime movers in getting things going, and infusing enthusiasm into the project were people like Verner Suomi, and Earl Droessler, who were great enthusiasts. And of course a lot of other people were involved in it. And NCAR I believe was asked to take on the National Hail Research Experiment, which had a great deal of planning behind it, in various meetings over a period of years.

FANKHAUSER: Were you sympathetic with the objectives as they were laid down?

NEWTON: Well, at the time I was hardnosed about not polluting science with practical things. And I also didn't have any great faith in weather modification. I never could at that time, and I cannot at the present time, imagine the possibility of modifying a storm that has a cubic kilometer of air per second flowing into its updraft, as is the case with a major storm. It still boggles my imagination, and in fact, it was found as a result of -- one of the things that was found out by the hail research experiment, was that in fact it was a few great storms, whoppers, that accounted for most of the statistics of what was done, and so the whole thing was inconclusive partly for that reason. But I think it was these impossible storms that made the whole undertaking impractical. However, I have to say that the NHRE, as Bill Swinbank liked to called it formally, had two objectives. And one of them was to business of establishing whether cloud seeding would be effective, or some other method of modification. And the other was a scientific objective. And the real good thing that came out of it was on the scientific side, and I think that in spite of its failure as being an experiment for doing away with hail, what came out of it scientifically was very great, under the leadership of both Bill Swinbank, and later on Dave Atlas. And not Bill, of course, at the end.

FANKHAUSER: What was the background on the transfer of the Monthly Weather Review from the Weather Bureau to the AMS as you recall it?

NEWTON: Well, this transition took place because the AMS was approached by the ESSA director at the time, who I think was Bob White. And ESSA was trying to shed responsibilities that cost money, that were not directly, were not directed towards the heart of their public charters. The Monthly Weather Review was one of those things that they felt should be more suitably done by a scientific society than by the government organization. So in what was close to its hundredth year of existence, the Monthly Weather Review was transferred to the AMS, and I had the pleasure of being its first editor, assisted by Harriet Newton, who made things actually run in the editing of that journal.

FANKHAUSER: Who solicited you? Or were you solicited?

NEWTON: I was solicited by, well, Ken Spengler was largely instrumental in my being solicited, and I cannot recall who the publications commissioner was at that time, but of course it would have had to go through the publications commissioner.

FANKHAUSER: Did you find the effort satisfying?

NEWTON: I found the effort more satisfying, and I think if I look back on all the things that I've done, it's probably the most satisfying thing that I have done. It involved quite a large amount of work. During a three year period we took this journal, which was wrapping at the time, when we inherited the Monthly Weather Review, I think there were five manuscripts on hand. Hardly enough to fill one issue. So it was difficult to fill a minimal size issue at that time. And we had some satisfaction to bringing it up to a respectable sized journal with very respectable content. We had wonderful cooperation all the way through with reviewers, no great fights with for authors, people were very cooperative along the way. And although it was a lot of work, it was at the same time a lot of satisfaction.

FANKHAUSER: NCAR was very supportive in this effort weren't they?

NEWTON: NCAR was very supportive. NCAR has a policy of supporting such things, and I think that's extremely important. I have known other editors who had be very tight with their time because they were constrained by their employing organizations. NCAR's objective, of course, includes the obligation of knowledge, and that's considered a proper part of it to take on the responsible tasks in such things as editing, book writing, and so on. They have been very generous to me in all respects.

FANKHAUSER: You've had a lot of association with the American Meteorological Society, and you've been quite active. You received the editor's award, and of course you were one of the past presidents of the organization. Are there any other recollections that you have in your association with the AMS?

NEWTON: Well, I have to say that the AMS has really been a major part of my life. During the editing period and serving on its committees. And I was president for one year, and before that I was president-elect, and after that I was past-president and spent a couple of years on the council. There was a lot of time and effort involved. During all this time my teacher was Ken Spengler, who guided me in the ways of the world, and we all have to be grateful to Ken for being the person who has run the AMS actively over a long period of years, decades, and has had a rather small organization. And Ken of course, has outlasted untold numbers of presidents, and he has been the guiding spirit along with Evelyn Mazur, and other people who have kept the organization going, knowing personally all the aspects of it.

My own thing I enjoyed most during this year was going to China. Right after the end of the Cultural Revolution, the AMS had established, under the leadership of Dick Reed, five or six years before, had established contact with the Chinese meteorological society. And it was a wonderful experience, first of all to visit the country and seeing the enthusiasm that was evident after the Cultural Revolution was over. And joining my Chinese colleagues Yeh and Shei that I mentioned before, who were the co-presidents of the Chinese meteorological society. So there we were, three University of Chicago students together in our delegation a number of other meteorology University of Chicago graduates, who were ex-presidents of the AMS, so we had a meeting of the Chicago club in Peking and other places--Beijing. And it was a revelation to see that in spite of all we read in newspapers, that the Chinese science had managed to struggle through all the domestic turmoil, and was actually still going on in spite of everything.

FANKHAUSER: What do you think were your most significant published contributions?

NEWTON: Well, the ones that I think were significant were the squall line paper published in 1950 that we mentioned earlier. And the one on the middle tropospheric frontogenesis in 1954. I wrote a good paper on lee cyclogenesis, cyclogenesis in the Rocky Mountains in 1956 that I think was good. One about the inertial oscillations in the jet stream written in 1959 and some that followed up on that. Then, in 1959, I co-authored a paper with Harriet Newton on the mechanism of thunderstorms and squall lines, and another one I am fond of is one of them written in 1964 on the movements of thunderstorms with James Christian Fankhauser. Those are the papers that I consider significant, and things that have been written since then have been largely derivative from those papers. Of course, the papers with Palmén on jet streams and frontal structures are closest to my heart, and best of these was the one on the three-dimensional motions and polar outbreaks, written in 1959, which I still think is a fine paper, and had his inspiration of course. And then, of course, the atmospheric circulations systems book in 1969. I published some papers with Anna Tragazon who, based on her numerical experiments in 1984, which I think are very good papers, but they are so complex I find it difficult to even read them myself, so I don't think they will have a whole lot of influence on me. Not the way things go in the field. But I have private knowledge that they're good papers. I have learned from this experience, which is the only one I've ever had in dealing with numerical experiments, being too lazy to do it myself, I learned the great value of these experiments for the interpretation of what's going on in the atmosphere. They have to be true to the equation of motion, right or wrong, and so they deliver consistent results and results that are tedious but very instructive to interpret.

FANKHAUSER: Your early work about non-hydrostatic pressure distributions around thunderstorms has been resurrected recently by some of my cohorts in the MMM division, and they tend to represent the situation as a new one. How do you regard this contribution?

NEWTON: I consider that I more or less would get it on its way, but they did it right.

FANKHAUSER: How do you spend your time these days?

NEWTON: Right at this current moment, I have spent a large part of the last couple of years has been involved in a memorial volume for Professor Eric Palmén, and one of the things that came out of the Palmén Memorial Symposium in Helsinki in August and September -- a week of August and September in 1988 was a Palmén memorial volume consisting of the twelve lead papers that were presented at the conference. Mel Shapiro is one of the contributors, and I wrote something on Palmén himself. That has occupied a lot of my efforts during the past year or two to put that together. And we expect it to be published in late summer of 1990 by the AMS. It's in the process now. Otherwise, I am now with that task drawing to a close, I'm faced with the prospect of getting back to revising the book with Palmén without his assistance and he has sent me no guidance from heaven.

Regrettably. But I hope that I can still remember enough of his spirit to do a good job of it.

FANKHAUSER: Is there anything else you would have done different? If you had an opportunity.

NEWTON: Well, I do have some regrets. And so I'll mention I think three of these. One is that I put far too much time into rehashing subjects that I worked on earlier rather than broadening into new ventures. One of the greatest wastes in this regard was in writing review papers. Although I had a conviction that this needs to be done because of the great burden of literature, and the fact that everybody doesn't know everything that goes into these papers. For me, I've put too much effort into this, and that could have been put into new and perhaps useful things.

FANKHAUSER: It may have been a waste for you, but it wasn't a waste for the science.

NEWTON: Well, that may be, but when you write a review paper, and then you write another review paper on the same subject the next year, it's not very productive. And you get stuck into a groove.

Another thing is that I wish that I had educated myself more in theory and numerical experiments. In my earlier years, and up until fairly recently, a decade or two ago, I was too defensive of the purity of synoptic meteorology, and the foolishness of this view was demonstrated by the fact that synoptic meteorology has become so nicely integrated with numerical experiments and theory, to the benefit of these branches that were formally separate. A long time ago I had the opportunity to branch out into a different field, namely to join the gang at the Institute for Advanced Study, and assisted them in interpreting the numerical experiments. And I sometimes wonder what might have happened if I had gone in that direction instead of the one that I've gone.

Perhaps my biggest regret is that I worked too much alone. Looking back on my publications, I myself found it surprising how few of them were co-authored with Palmén as a notable exception. And too late I have come to realize how much working in isolation denies one the cross-fertilization of ideas that you need to keep your thoughts going. I simply realized that too late.

FANKHAUSER: Well, if you had to do it over again, would you pick meteorology as a career?

NEWTON: I certainly would. I didn't pick it in the first place, but it is a science that I have enjoyed being a participant in all the way along. I think I came into meteorology at a particularly exciting time. I mentioned earlier the excitement of being in Chicago and Stockholm and so on, when in effect the general circulation of the atmosphere, although people had been thinking about it for a couple of hundred years, was being invented by the people in meteorology at that time. So it was exciting. I enjoyed those years, and I came to a point really where I really thought we had found out just about what there was to know about the main features of the atmosphere. Just like the head of the patent division who resigned in the early part of the century because he thought everything had been patented already. I thought the same thing about meteorology. Anyway, this was an exciting period that I lived through, but things have in fact gone on after I thought everything had been found out. And the modern tools of measurement, the development of new theories and so on, have, I guess, made the field as exciting now as it was for me. But I really consider myself fortunate to have been there during the developing days of the main ideas, or the firming up of the main ideas of general circulation. Having worked with exciting and inspiring people like Palmén, and Rossby, and Petterssen. Having been exposed to a great many leaders in the field. Charney of course I didn't mention. I had a close personal association with Charney, but I never talked science with him. But he was an influence in my life along with a lot of other people. It's been a glorious period in the development of meteorology, and I think that people like you and Mel can take credit for doing things, in areas that have pushed it along and made it exciting. I have watched both of your efforts with great interest for a long period of time, and in turn have been inspired by these things that went beyond any thoughts that I ever had. I didn't mean to get personal in that respect, but my point is to make the observation that the field is not dead, or dying, and it's still exciting and becoming more exciting all the time. And I had no idea that the beginning, or halfway along, the field of meteorology would develop to the point of knowledge anywhere near where it is now. And as there are many young people waiting in the wings who are prepared to carry on.

END OF INTERVIEW