Recorded interview with **Horace R. Byers** recorded at the Annual Meeting of the American Meteorological Society in Anaheim, California, on 7 February 1990. The interviewer is Roscoe R. Braham, Jr., and we will begin our discussions by allowing Dr. Byers to recall some of the early days in the development of meteorology and his career in meteorology. Why don't you go ahead sir.

HB: If I may, I would like to start in 1925 when I was a student at the Univ. of California in Berkeley where I took a beginning meteorology course in the Geography Department there. My teacher was Richard J. Russell, who was a physical geographer trained in geology but quite conversant with the literature on meteorology (which was mainly German and Norwegian literature) so that the course was taught in a manner rather different from the way in which it was considered in the Weather Bureau at that time. I remember my amazement when I found out or explored the San Francisco office of the Weather Bureau that the official in charge and head forecaster for the Pacific states were completely unaware of the German and Norwegian literature which had appeared at that time including, of course, Bjerknes and Sölberg later on.

However, John B. Leighly, who later on taught the course, was on leave that year. Leighly is well known, I believe, or was at the time well known, in meteorology, a climatologist who published a great deal. Later on he became one of the instructors in the Army Air Corps meteorology program at Grand Rapids, Michigan, then at Chanute Field. To illustrate the advances at U.C. Berkeley compared with other universities and the Weather Bureau in those days, we were instructed about the El Niño current on the west coast of South America. At that time, it was not too well known just how the current originated. There was a lot of discussion in South America but we were assured that the wild stories in Peru about the alligators coming down with the current from Ecuador were false. El Niño, as you may know, was important because the cold current which was normal there, outside of El Niño episodes, supported a great fish and sea-bird population and, particularly, on the rocks along the coast the droppings of the guani birds formed over thousands of years in thick layers were scraped off the rocks and exported as fertilizer. During El Nino the fish and birds disappeared and the fishing and guano fertilizer business in Peru suffered greatly. Anyway, I might skip ahead to 1944 when I was in Peru doing some consulting for Pan American Grace. There I met a German refugee physicist who was working for the Pacific Guano Fertilizer Co. and he said that his evidence indicated that the El Niño current came from waters farther out in the ocean. I think that's important to say at this time. His name was Schmidt; I'm sorry I can't remember his first name. I think it's important to give Schmidt this credit, although he never published it so far as I know, for discovering that the El Niño current came from the ocean farther out. Now going back to the 1920's again, at the end of that decade, Sir Gilbert T. Walker published his famous paper on "The Southern Oscillation," which was based, as you know, essentially on his records of the pressure difference between Tahiti and Port Darwin. Normally the pressure in Tahiti was higher than at Port Darwin as the trade winds would require. But there were episodes in which the reverse was true. So, now jumping to the present day, the El Niño-Southern Oscillation is a great thing for oceanographers and meteorologists to have "discovered".

When I was finishing my undergraduate work at Berkeley, I had the important-sounding position as meteorological observer at the University of California and the geography department published over my name monthly, a *Bulletin* in the University of California series of observations which I took. It was an official cooperative station of the U.S. Weather Bureau with fairly complete equipment: mercurial barometer, barograph, thermograph/hygrograph, temperature shelter with psychrometer, maximum/minimum temperature, rain gage on the roof of Wheeler Hall, and an anemometer wind vane on the top of South Hall of the UC Campus. It was an interesting coincidence that this *Bulletin* was mailed to a number of places including the Port of Oakland. The Port of Oakland had jurisdiction over the Oakland Airport.

In 1928, I received a call from the Chief Engineer of the Port of Oakland telling me that there was a man by the name of Carl Gustav Rossby, a very congenial Swede, who was representing the Daniel Guggenheim Fund for the Promotion of Aeronautics. He was looking for a meteorological assistant. This chief engineer had seen my name as the meteorological observer in the *Bulletin* which we put out each month. I accepted the position and Rossby and I operated the model airway of the Daniel Guggenheim Fund for the Promotion of Aeronautics on the airway between San Francisco, Oakland and Los Angeles operating out of the Oakland Airport and Vail Field, between Whittier and Los Angeles. The organization consisted of a network, more or less on the idea of the region network which resulted in J. Bjerknes and Sölberg's paper on "Meteorological Conditions for Formation of Rain" and the other one on "Polar Front Theory and the Life Cycle of Cyclones" all published in the *Geofiche Publikationer*, which, apparently, no one in the United States had seen or read, as far as I knew, although it was assigned reading in my sophomore meteorology course. That's one of the things that I was amazed about, that nobody in the Weather Bureau, which at that time dominated meteorology, (there was no full university meteorology course in the U.S.) was aware of these epic-making publications, although Rossby had come to the Weather Bureau in 1926 on an American-Scandinavian fellowship and he wrote a paper jointly with R. H. Weightman, one of the forecasters in the Washington Office of the Weather Bureau, the title, I believe, was "Application of polar front theory to series of American weather maps."

The network of weather stations in California was organized by the Daniel Guggenheim Fund as a "model airway" involving also an organization call Western Air Express, flying Fokker F10's between Oakland and Vail Field. We collected surface observations every 90 minutes during the morning hours and on into the afternoon. I handled the collection and other work at the Oakland Airport and George M. French of the U.S. Weather Bureau was engaged to take it on at the southern end. There was something like 30 stations between Los Angeles and Oakland, scattered along the coast and through the San Joaquin Valley and on down to San Diego, with daily pilot-balloon winds at Oakland, Hollister, Lebev/Sandberg and Vail Field. The airline operated on the direct route nonstop, following approximately the San Andreas fault line and across the ridge to L.A. They flew at 10,000 feet nearly all the way which was also somewhat.of a record. After the first year of operation, the system was handed over by the Daniel Guggenheim Fund to the U.S. Weather Bureau. It was operated under the jurisdiction of the Weather Bureau and Delbert M. Little was put in charge of it, stationed at the Oakland Airport. So my boss changed from Rossby to Delbert Little. He was later the Chief of the Aerological Division of the Weather Bureau in Washington, succeeding Willis R. Gregg in that position, somewhat later of course.

The Daniel Guggenheim Fund had established an aeronautical engineering course at MIT and had put up the Daniel Guggenheim Aeronautical Engineering Building and MIT, in cooperation with Secretary E. P. Warner of the Navy, had agreed to have a course in meteorology in that course on aeronautical engineering, and Rossby was selected in 1928 as the man to be the head of meteorology with the rank of Associate Professor. I recall that he was 30 years old at the time. Then Hurd C. Willett, who had been working in the Weather Bureau under W. J. Humphreys, became Rossby's Assistant Professor. They set up a laboratory there with all the collection by radio from the Navy which sent out the signals in word code, and weather maps were drawn by Willett and Rossby there. That was 1928. I continued working until 1929, then the Guggenheim Fund also established a graduate fellowship in meteorology and I received a telegram at Oakland advising me that I was to be awarded that fellowship and asking me if I would like to come to MIT as a graduate student in meteorology. I accepted and moved across the continent to MIT. I was preceded there as a graduate student by a group of Navy officers, I think most of them were in the grade of lieutenant junior grade, who studied there for a year in '28 and '29. I was there in September of 1929. Meanwhile, I had joined the American Meteorological Society and I entered the graduate course.

It might be of historical interest, I think, that all this was done particularly without the real cooperation of the Weather Bureau. They were a little annoyed perhaps to see that the Navy was becoming prominent in the field of meteorology, ending more or less the Weather Bureau monopoly, the Signal Corps, etc. A civilian also was in that first group by the name of C. L. Pekeris. Pekeris, I haven't been in communication with him recently, but he became a very well known geophysicist on the theoretical side ending up working in Israel.

I think it would be worthwhile for me to say something about the state of meteorology in the United States at that time. As I already inferred, the principal scholarly work in meteorology was published in Germany and Norway. If you are interested in a personal thing: During my last year at UC-Berkeley, Professor Leighly persuaded me to write a paper on the summer sea fogs of the California coast. Practically nothing had been done about that except by a Norwegian oceanographer by the name of Werenskiold, who used the ideas of V.W. Ekman (who I believe was a Swede); and everybody knows about the Ekman spiral. Werenskiold made some studies of the ocean temperatures in the California current, which obviously had an application to the meteorology of the California stratus and fog. I wrote this paper, it must have been some 30 or so pages, and had it published as a *Bulletin of the University of California-Berkeley* in their *Bulletin* series. As far as I know, this was probably the first time that the University of California Press published a monograph by an undergraduate student. That was my first publication in meteorology.

To get away from the personal side and back to the situation as it existed at that time, the only area in meteorology in which the American scientists could claim any kind of leadership was in the field of radiation. The Smithsonian Institution had this observatory at Mt. Wilson and Pine Mountain in southern California and they had been taking measurements of solar radiation for a long time. I would say, beginning perhaps in the early 1920's. C. G. Abbott, who was then director of the Smithsonian Institution, was very much interested when he went through the literature and made observations concerning sunspots. The Weather Bureau was making observations, at the surface, of the total solar and sky radiation using the pyranometers which had been developed, I think, in Germany and also by the Smithsonian. In the *Monthly Weather Review* in 1928, H. H. Kimball of the Weather Bureau published his results of the observations. One of the first things that Rossby did in 1929 was see that C. L. Pekeris went to the Smithsonian and confer with the people there. Pekeris then wrote a very good summary of solar and terrestrial radiation which was published in the MIT Miscellaneous Notes in 1932, rather a delayed publication.

E. Alt in 1929 had published in the *Zeitscehrift Geophysik* in Germany, a good paper on terrestrial radiation and R. Enden in the Bavarian Academy publication in 1913 had published in this. Water vapor absorption was also of interest going back to 1898 in the *Physical Annals*, Rubins and Auskinos published. Also, a man by the name of F.E. Fowler of the Smithsonian Institution, published in 1917 some data based on this previous German paper on the water vapor absorption. Then the following year, C. Hettner in the *Annalen der Physik* in Germany (1918) published essentially what we now know as the water vapor absorption spectrum. At the same time, G. C. Simpson published the very important paper in which he placed the terrestrial radiation in the spectrum showing the absorption by carbon dioxide with water vapor and showing the radiation window and setting out the 8-1/2 to 11 micron area window. This was summarized a little bit also by C. G. Abbott. Meanwhile, Abbott was starting to publish important talks and small papers on the effect of sunspots on the temperature of the earth. He concluded that the temperature went up during sunspot activity and that this was a very striking thing. He employed H. Helm Clayton to work with Charles F. Brooks at the Blue Hill Observatory to work out the solar terrestrial relationships. Nothing very much came of that. But Dr. Charles F. Brooks at Clark University, who was sort of a leader of meteorology at Blue Hill Observatory and the American Meteorological Society, had Clayton there and somehow or other (I don't quite remember the way in which it happened) but Jerome Namias was employed to work with H.H. Clayton on that subject.

So much for radiation, which was about the only really scholarly work that was done by the Americans during this period.

To get to the more general meteorology. Of course, there were papers by Bjerknes and Sölberg that I mentioned before. In 1929, W.J. Humphreys published the first edition or revised edition, at least, of his books <u>Physics of the Air</u>. W.J. Humphreys was at the

Weather Bureau at that time. Hurd Willett, who later went to MIT, was his protege' and Willett published a paper in 1931, sponsored by the National Academy of Science of the United States, which had suddenly realized that there was something to meteorology. The title of that Bulletin was called *Dynamic Meteorology* -some 40 pages, a very well produced thing. By that time he had come under the influence of Rossby and it was a very modern statement of the new meteorology.

Another interesting thing that occurred during the 20's was the book by Sir Napier Shaw on the Manual of Meteorology which introduced the tephigram and other adiabatic charts and energy diagrams. The Hertz-Neuhoff diagram was the basic adiabatic chart before World War I. Then, A. Refsdal in the Geophysical Institute of Bergen, published a paper and called it the emagram, which for the first time gave the dry adiabatic lines with P to the .288 factor which were straight lines on the adiabatic chart. Then in 1932, Rossby at MIT published the Thermodynamic Diagram which was called the Rossby Diagram, which became very important particularly in the identification of air masses. Meanwhile, observations were improving the sounding balloons, of course, by Tesserenc de Bort and later at Blue Hill; Sverdrup in 1929-30; and some soundings in India and Batavia. They were mostly the sounding balloons with recording instruments. Of course, at this time, the U.S. Weather Bureau had kite stations using box kites. The stations were in Ellendale, ND; Omaha, NB (Omaha also used some sounding balloons); DueWest, SC; Broken Arrow, OK; and Groesbeck, TX. Note that there was nothing in the western states at the time except that Dean Blake, U.S. Weather Bureau, then worked with the Navy who was carrying on airplane soundings in San Diego. That was the first demonstration of the California temperature inversion. I made great use of that in my paper when I was a student at Berkeley. I gave Dean Blake credit for that.

In 1930, Bergeron published in the *Meteorlogische Zeitschrift* in Germany about air masses. In 1933, Hurd Willett's famous paper on the air masses of North America using the Weather Bureau kite stations showing the different characters of it. In the general circulation of the atmosphere the idea of solenoids had been put forth by V. Bjerknes. The solenoidal circulation, which is the thermal circulation of the atmosphere, was quite a predominant idea at that time. This was, of course, in the early 20s.

Also in the late 20's, Cleveland Abbe published a book called *The Mechanics of the Earth's Atmosphere* in which he reproduced a number of papers, including the work by the Austrian/German meteorologist Margolis, which showed that in a discontinuity the relationship of the slope of the discontinuity was a function of temperature and wind shear, wrote a very famous paper which was reproduced by Abbe. This book of Abbe's received very little attention at the time. I think it was published around 1927-8. Margolis had published a paper in 1903 on the inversions and so on.

Getting on. In the early 30's, a Belgian meteorologist by name of Jaumotte invented a balloon sonde which had a moving smoked glass plate and a moving pen which inscribed on it. The glass plate was moved by pressure and the pen was moved by temperature, scratched temperature on this plate. In the early 30's, a series of ascents was made through Europe using the Jaumotte instrument which went well into the stratosphere. Although the stratosphere had been discovered many years earlier by Tesserenc de Bort, this for the first time presented a synoptic picture, because these were sent up in serial ascents from a number of places, and this way for the first time the synoptic picture of the stratosphere was obtained. The data from that were analyzed in detail by Eric Palmen, the Finnish meteorologist, published in 1933, and for the first time it showed the slope of the tropopause and the relationship between the stratosphere and what happens down below.

On the theoretical side.

RB: Excuse me Dr. Byers. Maybe before we start the theoretical side, it is time for me to change the tape.

SIDE NO. TWO of the taped interview with Horace R. Byers, Anaheim, California, 7 February 1990.

RB: Dr. Byers, you were just going to recall some of the theoretical developments in this period of the 1930's.

HB: I might mention the Ekman spiral which was first written about in 1905 which became important in writings in dynamic meteorology at that time including Willett's paper published by the National Academy of Sciences.

In the early 30's, an important thing happened, namely the development of aviation. I think from that period right on through World War II, aviation was a principal thing that stimulated meteorology. During the interval between my Master degree and my Doctorate at MIT, I accepted a job with TWA instructing their meteorologists who had been trained in the old Signal Corps in World War I, instructing their pilots on essentially synoptic meteorology and the application of the Bergen School to meteorology, and airline meteorology became a real force in meteorology.

The establishment of a meteorology course at Cal Tech by Irving Krick in cooperation with R. A. Milliken happened in the early 30's. Actually, Beno Guttenberg, a seismologist and geophysicist, was Krick's mentor at that time. With his interest in meteorology Milliken took advantage of meteorology and particularly Irving Krick's fascination with public recognition.

When the Navy airship, <u>The Akron</u>, was destroyed in a mild squall line at Lakehurst when it was coming in for a landing, with Milliken's encouragement, Krick did a hindcast on that and said that "it could have been predicted." It was very critical of the Navy for having failed in this. This resulted in quite a stir in the Navy, in the Weather Bureau and in Congress. The CalTech people made a lot out of this. With Milliken's prestige, he required or demanded that there be an investigation of the Weather Bureau, and reform in the Weather Bureau and all of the meteorological services.. As a result of this, Milliken, through the National Academy of Sciences of which he was a member, had formed a Committee on the Weather Bureau and Meteorology in the United States. The committee (the name may be in historical records) was referred to as the Milliken Committee. This committee said in its report that things were in a primitive state in the United States and particularly the upper air was not being properly explored. The older kite stations were still in existence. Arrangements were made with the Army Air Corps and the Navy to make airplane ascents.

The MIT flew a young German by the name of Carl Lange who had already carried on a series of airplane ascents beginning in 1930 and carried on for several years at Boston, but those ascents were not telegraphed to the Weather Bureau. Really the Weather Bureau wasn't interested in them. It was more of a demonstration to show what could be done and it was a very successful series of flights. On that pattern, the Air Force or the Army Air Corps airplanes took up aerometer aerographs mounted on wing struts of the airplanes. They had stations at Cheyenne, Cleveland and later Detroit, Montgomery, AL, I believe, and the Navy at Pensacola and San Diego and Seattle, Anacostia, DC; there might have been some others that I don't quite remember, maybe at Kelly Field in Texas. Anyway, quite a network of airplane observations. This was during the middle 30's. This greatly improved the aerological studies to be included in the daily meteorological signals in the Weather Bureau. This made it possible to have fairly good studies of the atmosphere. The flights went up to about 5 km height which was the ceiling in general of the airplanes.

Cloud physics, I might say something about. In the first place, of course, fog was very much in importance during the 1920's and 30's. Willett wrote a paper on fogs studied at a Hadley Field in New Jersey, published in 1928 and 30. G.I. Taylor in 1917 had made his historic fog studies on the Grand Banks off Newfoundland which were commissioned by the British Board of Trade after the sinking of the Titanic. He published this in 1917 showing the relation between wind speed, turbulence, and fog. He had balloon soundings off the deck of a coast-guard type ship of the British and had tethered balloons and showed how the temperature inversion was a strong influence in the formation of the fog. That the wind speed caused the fog to lift the base, to be lifted off the ocean due to some adiabatic process. This was also very useful to me in studying the California coastal fogs.

In 1936, in the *Monthly Weather Review*, Robert G. Stone, who worked as Secretary to the AMS for a number of years, published a paper on distribution of fog as defined by the Weather Bureau showing that, particularly in the eastern United States, the valleys of West Virginia had more dense fog than anyplace else, and that they occurred mainly in the month of September where there was strong radiation and still the high water vapor content of summer. About that time I published a paper on the fogs of California or the weather phenomena in California which was published in the MIT Series with the assistance of Lieutenant W. M. Lockhart of the Navy.

In those days, dust storms were very important and there were quite a few publications on that in the period of the mid 30's.

Turbulence studies had been made since 1905 by Ekman. And in 1917 W. Schmidt invented the term of Austausch and showed how that developed under different forms of stability. In 1908, Ekerblom, a Swede from University of Uppsala, had established some instruments on the Eiffel Tower in Paris and showed the relationship between day and night, the temperature gradient and stability of the atmosphere. Sverdrup in his north polar expedition of 1936 studied the turbulence over the ice fields.

In the area of general meteorology, L.F. Richardson, in the *Proceedings of the Royal Society in 1920*, had published on turbulence. Rossby, actually when he came to the Weather Bureau under the Scandinavian/American fellowship in 1925-26, when he first came to this country, was supposed to study Norwegian methods applied to American meteorology. He was pretty much thwarted in that, so he turned to a study of turbulence and turned out several papers in 1926. This he continued on until at MIT he continued to publish on turbulence with Raymond Montgomery, for example.

RB: Why was he thwarted in the study of application of the Norwegian methods to the United States?

HB: Presumably, in his application for a Scandinavian/American fellowship he stated that as the purpose. But he was sidetracked by the Weather Bureau; they weren't particularly interested in it. He tried to construct a rotating tank experiment at the Weather Bureau with the help of the people in the instrument division shops. This didn't show very much except patterns began to show in the rotating tank. But this he was able to realize more fully later on, as you know, with his association with Dave Fultz at the University of Chicago during the 40's.

RB: You don't think it was the lack of quality observations from the vast continent of North America that played the role in that decision?

HB: Some kite observations were used. A Weather Bureau Supplement was published by Rossby and Whiteman called. "Application of polar front theory to a series of North American weather maps." This drew some attention. I believe the date of publication was somewhat around 1929 or 30, a delayed publication. Rossby started out in the so-called map room or forecast room at the Weather Bureau, but he soon was sort of ousted and went to his own department. Reichelderfer was in the Navy at that time and he and Rossby were associated together informally. With the formation of the Daniel Guggenheim Fund, they decided to have a Committee on Meteorology. The Weather Bureau sent this young Rossby to work with them and he was later made Chairman of the Committee on Meteorology, and that was how he met with people like E. P. Warner and others who were associated with him in the Aeronautical Engineering Department at MIT. That's what was happening in those days. As a matter of fact, maybe I shouldn't say this, because it brings up what might be libelous statements. This history was published actually in the Rossby Memorial Volume, in which I had an article called "Rossby the Organizer." I pointed out in there that by 1927 Rossby was more or less persona non grata in the Weather Bureau. I was told that a letter went out to the Weather Bureau stations when he went to San Francisco that this fellow was going around and you'd better watch out for him. First thing Rossby did when he arrived in San Francisco was go to the Weather Bureau office and E. H. Bowie was the official in charge. He was a well-informed person. He had seniority over most of the people in the central office of the Weather Bureau. He welcomed Rossby with open arms.

Rossby went out with Air Corps pilots and established these stations. I don't know if you would be interested or not. I have an interesting paper which was sent to me by Charlie Bates in which he described the relationships between the Weather Bureau and Rossby and Reichelderfer and so on, giving pretty much some of the things which I have just said. This was published in December 1989. He talks about changes in the Weather Bureau which came about also as a result of the Milliken Committee. Incidentally, Karl T. Compton, President of MIT, was also on that committee. That's about the only name I remember. Anyway, I would like to write a letter to the editor to make a correction that the Weather Bureau was transferred to the Department of Commerce in the period that Bates was writing about. As a matter of fact, it was under the Department of Agriculture and Henry Wallace was the Cabinet Member and Secretary of Agriculture at the time. Through people at the Department of Agriculture, they were greatly impressed with Rossby. They had talked about the possibility of Rossby as Chief of the Weather Bureau. But he had just been declared a U.S. citizen, so they didn't think that was proper. Meanwhile, the wheels were set in action also to transfer the Weather Bureau to the Department of Commerce. But Reichelderfer was actually appointed by the Secretary of Agriculture, I think it became the Department of Commerce almost a year/months after Reichelderfer became chief.

In the organizational field, I should say that after I got my degree at MIT, as a result of the recommendation of the Milliken/Compton/Secretary of Agriculture Committee, they set up the air-mass analysis section in Washington. I was hired to be in charge of that section. I had with me: Harry Wexler; a fellow by the name of Steven Lichtblau, also of MIT; Charles H. Pearce came on later; and Gardner Emmons came on somewhat later and worked with us. Namias, in a paper in the *Bulletin,* talks about those early days and describes how we had map discussions with the forecasters every morning and the forecasters to quote Namias "came in there loaded for bear." (Laugh) This is one of the items I included in some papers so I wanted you to read it to get an idea of some of the things. If you want me to, I can quote some of the things in it.

RB: That's up to you.

HB: To have a recording of it or something like that?

RB: Perhaps, if it is just repeating what is there, you might not want to do that. If you have things with which you take issue or new developments.

HB: For example, in the *Bulletin of the American Meteorological Society*, it goes into the history of it starting with the "History of Atmospheric Sciences" by Ed Lorenz. He shows some of the original ideas I talked about, he has photographs of the Norwegian Center with Bjerknes and Bergeron and somewhere off the end table there was Rossby. A picture of Meisinger; he made the famous balloon, manned balloon flights to try to explore the upper atmosphere. Willett, myself, Rossby, and Wexler; and, of course, later there is a picture of the map room in the Weather Bureau in the old days and so on.

Another article entitled "The history of polar front and air mass concepts in the United States: An eye witness account."

I think if UCAR is going to try to do something about the history, they should refer to these articles. There are papers from the *Symposium on the Impact of the Bergen School of Meteorology on the Development of U.S. Meteorology*, which was held in San Jose. I also presented a paper at that time.

I don't know how far we want to go in this early history.

RB: Let's pick up the trend of your trail--from the Guggenheim Fellow at MIT to the map room at the Weather Bureau--what happened after that?

HB: The Chief of the Weather Bureau at the time that I went there was Charles F. Marvin. He retired and Willis R. Gregg became Chief of the Weather Bureau in the Department of Agriculture. He died very shortly after he served in that office and that was when the question of the appointment of Reichelderfer came up. Of course, meanwhile, the Milliken/Compton Committee existed. I don't know if Marvin was still Chief or if it was entirely with Gregg. Gregg was a much more forward looking Chief than Marvin. Anyway, we worked under him. In 1938, something like that, whenever Reichelderfer became Chief, Rossby became Assistant Chief of the Weather Bureau. He served in that capacity for a couple years, about three years, then went to the University of Chicago. While I was in the Weather Bureau they decided that the terrible lack of adequate education of the Weather Bureau officials needed to be corrected, so they started the In-Service Training Program. After starting that program for a year or so in Washington, we urged Gregg and the Department of Agriculture to set up some system for sending outstanding employees to the institutions which existed at that time. Specifically to MIT and to New York University. New York University, as you probably know, started by Athelstan Spilhaus, one of Rossby's protegés'.

In 1939, the Nazis invaded Poland in September of that year, I believe it was. This happened while there was a joint meeting of the American and British Meteorological Societies in Toronto. J. Bjerknes was there and presented a paper at that meeting in Toronto. With the outbreak of the war, he came down to Washington and actually stayed with me and Frances for a few days. He was stranded, in other words, because Norway was essentially blockaded. I think he tried to go back. Anyway, through manipulations with the Committee and Rossby, the UCLA Department was established with Bjerknes. The end of 1939 and beginning 1940, they decided I should go to Chicago at the Weather Bureau there and we would set up a class at the top floor of the old Post Office Bldg, the Federal Bldg., in downtown Chicago. They brought in meteorologists from various stations for a three-month course. We conducted courses during the winter of 1940. Meanwhile, while I was there, as a result of some informal discussions over beer or something with Rossby, we thought it might be a good idea if we had a weather or meteorological course at a middle western university. I got to thinking about that toward the end the Weather Bureau in-service classes. I one day called up the University of

Chicago and said, "Well, I don't know who the chairman of the Physics Department is, but I would like to talk to whoever he is." I talked to him and he happened to also be the Dean of the Physical Sciences, Dean Henry Gale. He agreed that Victor Starr who had gone along with me to Chicago and worked with me in that training program should be involved. Starr also originated in the Weather Bureau, you know, we brought him in because he appeared to be so smart. Anyway, he went with me to Chicago and I called up Dean Gale, and he said "Well, we have a Physics symposium, why don't you come and talk to us.? I'm interested in meteorology. I worked in the Signal Corps during World War I and had something to do with the development of the rate of ascent of pilot balloons." We arranged for a symposium with Victor Starr and myself. I gave a brief talk on the thermodynamics of the atmosphere and the idea that the atmosphere in general moved along isentropic surfaces. The isentropic era was then the fashion in the Weather Bureau; isentropic charts which had been developed by Rossby and Namias. I talked about that showing the thermodynamic relationship for potential temperature and energy. Victor Starr gave a talk about energy in the atmosphere in relation to the general circulation. Dean Gale was impressed so he arranged for me to meet with Vice President for Academic Affairs, Emery T. Filbey. We had a luncheon at the Quadrangle Club and he said they might be interested in doing something about meteorology. Gale said we could put it in the Physics Department. They asked me how much money it would take to get the thing started. I said something like $50,000; it just came off the top of my head, I wasn't prepared for anything like that. But a couple of weeks later I was called again for a luncheon meeting at the Quadrangle Club and Vice President Filbey had the promise of this money. This was to come from an anonymous source, who I later found out was Sewel Avery, Chief of the Montgomery Ward Co. This was 1940, and he said he thought this might be important in the war effort. (Laugh). Well, you know what happened.

I would recommend, if you are interested, that you read this paper about the formation of the Department of Meteorology at the University of Chicago which was published with the title "Founding of the Institute of Meteorology in Chicago." This was at a meeting where I was to speak and Jule Charney was to talk about Rossby's work and the theoretical meteorology, but, darn it, Jule never produced a paper on it, so it was just published with out that. I mentioned in there what I just told you about the founding.

I don't know if UCAR is going to produce this (interview), if we should be referring to these papers. Maybe when you submit this to them you might mention it. . . .

RB: Right, what Dr. Byers is referring to was in the *Bulletin of the AMS*, November 1976.

We are coming down to the end of side two. I suggest we put in a new tape and move right on to more recent times. Particularly how you sense the current trends in meteorology. What do think is going on as you see it.

HB: Well, coming to this meeting and previous meetings of the Society, I feel completely overwhelmed by what is going on. Of course, before I retired, it was obvious the computer, the satellites, the radar, all those things were causing a complete change in meteorology. This was going on and it is interesting that one of the things that has been talked about a great deal at this time and is going to make a great thing for a number of years to come is to take observations of the question of the warming trend under the greenhouse gases. That needs to be straightened out. I have been critical of some of the papers that have been given and published by people who have made computer models which I consider trash in/trash out because they have neglected particularly the areas of cloudiness. Of course, that's already been pointed out by other people.

I might say that, going back in history also, when I was in Washington, that we considered in discussions with a Department of Agriculture Committee that I was on, the whole question of the carbon dioxide and its influence on the outgoing radiation. We had studied this paper by Callender, who published the first real indication of what the carbon dioxide did in affecting temperature and outgoing radiation. This is going to be a big thing.

RB: Let's stop at this point.

SIDE THREE (SIDE ONE OF TAPE TWO) of our interview with Horace Byers which took place on 7 February 1990 at the Annual Meeting of the American Meteorological Society in Anaheim.

RB: Dr. Byers, we reviewed some of the early days, particularly your involvement in it and had just established the school at the University of Chicago. Of course, that's where I (Braham) entered the University of Chicago scene. What were your recollections of that period? That must have been a hectic period.

HB: It was hectic, but I can hardly remember what went on. Rossby established himself there as really the world leader in meteorology. Not only theoretical meteorology, but in organization of meteorology in the United States and in the world. Both Rossby and I participated actively in the *International Union of Geodesy and Geophysics*, that is, in the Association of Meteorology and Atmospheric Physics. In another words, it became known as the Rossby school. Of course, it's in this paper here in the ***Bulletin*** about the establishment of the Department of Meteorology. It is very well known that we had some very brilliant young people in the course, largely through the Army/Air Force's Cadet program, and a similar Navy program on a somewhat smaller scale.

Also, the Civil Aeronautics Administration sent some people who had gone through the civil flight training program. If you recall, CAA got stirred up with the coming of the war and the fact they felt one of their missions should be to train more civilians in the flying, particularly college students. The field was dominated by Air Corps with the pilot training. The airlines got nearly all their pilots from Air Force schools. I think in total we had 700 students trained in military programs in which Rossby took a leading role.

There was a University Meteorological Committee formed with Rossby, myself and Joe Kaplan of UCLA, he was head of the Physics Department there and was more or less running things for Bjerknes. We also got a young fellow from the President of the University of New Hampshire. He contacted schools particularly for the pre-meteorology courses, which were established subsequently. As far as the University of Chicago was concerned, Victor Starr went with me and then subsequently Harry Wexler, who had been associated with us at the Weather Bureau., and Rossby, of course. I ran the show for the first year, and he didn't show up until 1941, still continuing as Assistant Chief of the Weather Bureau. We, at first, received grants from the Bankhead Jones Fund and this was under the Department of Agriculture, and we had some statisticians who were employed as research associates on that program. Dr. Horace Norton who had been working at the University of Minnesota, Glenn Brier, who you know became an important fixture in the field of statistical meteorology. Then we got George Platzman. He was in one of our early courses and we kept him on. He was a brilliant young man in theoretical meteorology. He was deferred from the military because of his importance in this program. Some of the early key people, I don't remember exactly what the staff consisted of: We had Michael Ference who worked with us on instruments; Earl Barrett was an instructor in instruments; Mike Ference also helped us organize the hydrodynamics laboratory which eventually Dave Fultz took over.

My recollection is that in synoptic meteorology, we had Vincent Oliver who had been helping me in the in-service training program downtown in Chicago when we first started, so I think Victor Starr and Vincent Oliver were the first ones besides myself to come there.

The Air Force's program came on and we had students' where first classes were rather small and then became larger. Because of our facilities, some of the people from UCLA were transferred to Chicago to be with us. I remember in the early days when I went in the Department of Physics in Chicago, I noticed a young lady, who, it seemed like every day, was sitting on these steps inside studying Physics with somebody else. Later that person was introduced as Joanne Gerould. Joanne decided that she would like to be in meteorology because it seemed more exciting than Physics at that time. She received one of the Weather Bureau scholarships. I guess it was Weather Bureau and CAA jointly. So, she came on and went through the course fairly early, becoming, as you know, an outstanding person, now married to our former student Robert Simpson, and she was elected President of the AMS.

RB: Let's move to immediately following the war. There was a strong research effort there on, shall we say global circulation, large scale circulation. In the laboratory and in the analysis of data. What led you to focus on what is a much smaller scale phenomenon of thunderstorm? How is it that thunderstorm came into being in an environment that had stressed the larger scale.?

HB: This was my personal interest--the thunderstorm. While I was at the Weather Bureau in Washington, I thought, somebody ought to do something about these very important smaller scale phenomena. Victor Starr was doing marvelous things in the general circulation with us at the Weather Bureau. In the first place, my old friend, Warren Thornthwaite, from my undergraduate days at UC-Berkeley, was in the Soil Conservation Service. The Soil Conservation Service had a research group in the Muskingum Basin in Ohio. He set up a network of stations there in the Muskingum Basin. He had previously done this in Oklahoma. We got together and were looking at some of his data and it seemed obvious that this was a wonderful thing for tracing the movements of thunderstorms, particularly nonfrontal thunderstorms. Everybody knew about thunderstorms on cold fronts. I started working with those charts and published a Miscellaneous Report of the University of Chicago Institute of Meteorology called "Nonfrontal thunderstorms" in which I used the Muskingum Basin data from my old friend Warren Thornthwaite. Then the history of the Thunderstorm Project, as we later knew it, was from the stimulation of aircraft accidents in thunderstorms. Particularly, C. E. Buell of American Airlines was interested in this. If my memory is correct, it was American Airlines where one of the accidents occurred in Tennessee or Kentucky in a thunderstorm. This particular accident was one in which a United States Senior Senator was killed (maybe you remember his name, I don't). This resulted in a Senate investigation of safety in airplanes and the question of thunderstorms and how they could be studied.

Buell, with the stimulation of American Airlines, came to me and Rossby and said that the time was right when we should set up a thunderstorm investigation project. In the more or less spontaneous way that Rossby had, he said "Horace, you organize it and do it." I was a little aghast, it seemed then that the powers-that-be in the Senate and various places saw to it that money was appropriated to the Weather Bureau to set up a research project on thunderstorms. I think the Congress appropriated a sum of $125,000 to the Weather Bureau, which seemed like quite a bit of money in 1945, or whenever. I don't think they had any inkling of the magnitude this would take. I was serving on the Meteorological Committee of the NACA and I was familiar with the work of the Gust Loads Division of NACA in which they had studied from a more or less engineering structural thing, the accelerations that airplanes get. I thought, that's great, we know now how turbulent things can be, at least in cumulus, (they were afraid to fly in thunderstorms). What we really need is, of course, to study the updrafts and downdrafts and that sort of thing. My friends in the Air Force, particularly Ben G. Holzman, Will Kellogg, and Howard Orville of the Navy, and myself got together to form a committee with various University people on it, including J. Bjerknes, I remember him particularly. And, also, I had been interested in thunderstorm electricity through some early associations beginning in 1939 or earlier with E. J. Workman at the University of New Mexico, later the New Mexico Institute of Mining and Technology and New Mexico Tech. Also, I had met in Washington Ross Gunn, who had been working at a Center on Lightning Strikes to Airplanes at the University of Minnesota. All those things sort of got together and with the help of the Air Force's people, we finally decided that maybe things had reached a stage where flights could be made through thunderstorms.

The suggestion was made that the Instrument Flight School at Bryan, Texas, would be a good place to get pilots who had apparently, on an informal basis at least, flown through thunderstorms and were excellent at keeping their bearings in adverse conditions of instrument flight. The Air Force arranged that these people be assigned to help us and we knew Florida was the place that had the most thunderstorms, and that would be a good place to start.

Meanwhile, with the cooperation of the Weather Bureau and the Navy, we fell heir to a lot of instruments. The reason we got all this stuff was because just in the midst of our preparation, World War II ended in August 1945 when Japan surrendered. That was a day that all this materiel in the Navy, Army, Signal Corps and so on we could get. By this time, radar had been developed and we could get some of the better long-range scanning radar and some vertical scanning radar. As the plans developed, with the help of a number of people. Ferguson Hall was at Chicago at that time working with Earl Barrett and so on. George Benton and his wife who had already tried to use accelerometers in balloons.

Anyway, that's how it all came together and this project was organized. I can't remember all the meetings we had and all the discussion we had. Then we built a crew to work with us there. Ferguson Hall was my principal helper there. He left and you (Braham) were in charge of briefing and debriefing, analysis, and that sort of thing. So, we got going. Roscoe Braham succeeded Fred White. And there was Laurence Dye of the Weather Bureau. Ferguson Hall was just in it at the very beginning. I don't know if he was actually operational or not. We had Harry Moses and he handled the surface observation system.

RB: We operated for one season in Florida. You went to Florida because of the high frequency of thunderstorms?

HB: Yes, also a happenstance that helped us in that decision, that underlined and emphasized Florida, was that the Orlando Air Force Base had a beautiful radar. Also, that was a good operational base. I don't remember just what units they had at Orlando Air Force Base, it was a very good choice. We had the radar just outside Orlando there scanning the horizon.

RB: You may recall, they never did tell us the designation of that. In the final report, we do not give the designation of that radar, what type it was. We only describe in general terms as high power and that sort of thing. It was regarded as classified, I guess, at the time we wrote the report in 1949.

HB: I remember in 1944, I published the third edition for a text book. I had a discussion for instrumentation for sounding the atmosphere and I referred to some work by a physicist who had made some radio soundings, which were really a precursor of radar. Incidentally, I had been in on some radar studies that were made out in the open in Puerto Rico in 1943 or something when we established the Institute of Meteorology down there. Anyway, I had in this book manuscript something about *radar*. And the publisher of McGraw Hill wrote back to me and said the word *radar* is prohibited, we cannot use it. Apparently the word had gone out to all publishing houses, don't mention *radar*. This was in the 1944 edition of my book, so you can see how much under secrecy radar was.

RB: So now you had one operation in Florida, the specific site chosen dictated by existence of the radar facility and air base, but the general region chosen because of the high frequency of thunderstorms. Then to move somewhat farther north in Wilmington, again it was the existence of radar at the air base a substantial bearing on the precise location.

HB: One of the disappointments was that they were getting a wonderful radar, a better one than the one in Florida. It was supposed to be ready, but there were all kinds of delays, it was almost July before we got it.

RB: Have you ever considered or wondered what the thunderstorm project might have been like or what the developments might have been had you chosen to operate the second season say in Nebraska or Kansas somewhere where the severe squall line thunderstorm might have occurred or what some of our operations might have been? The turbulence is really quite a bit stronger and the vertical motion much stronger.

HB: Of course, in those areas you have a higher tornado frequency and we might have been able to play around with tornadoes. Of course, hail was much more prevalent there and the nighttime thunderstorms in Nebraska. We thought of that but we figured in the first place, in addition to the lower frequency of thunderstorms there and so many of them occurred at night, which made it more difficult, and there were tornadoes and hail and all that sort of stuff, we didn't give it that much consideration. The fact that they had so many thunderstorms in Florida and so on in Ohio maybe, it was quite obvious that that was the place to start. As far as Ohio was concerned, we did want to get more of the middle latitude thunderstorms. That was the best possible place. There was this Wilmington Air Force Base which was used to train Afro-American pilots during the war which is out of existence now, very little going on. We thought of the Plains States. I wanted to stay out of it. I imagine today something of that sort is being done. Of course, the group in Norman, Oklahoma, has done a lot with tornadoes. I don't know what kind of flight work is done.

RB: You mentioned Ross Gunn, the group in Minneapolis, Jack Workman and your interest in the atmospheric electricity aspects. Yet that aspect of the thunderstorm was not included in the Thunderstorm Project. We had on the wings these field mills you know and started out with that. I don't know if we carried it on through. We had, of course, point receiver on the ground and Harry Moses operated that and he found out that when that was operated in connection with the other data, we found out that there were no significant lightning strikes until the temperature of -20 or something was reached in the clouds. They've grown to the -20° isotherm. That was, for general cloud physics, important.

Later on we tried to put in a network of field meters around Champaign in cooperation with the Water Survey group. There we found that the low clouds, the fog and the stratus which came in whenever the rain began, sort of interferred with it because you get the effects of the low clouds. They had much better success in New Mexico where they didn't have such high moisture in those low clouds. It's a very difficult job, and it was really beyond us to develop instrumentation from an airplane which could be used to really significantly give us results. We looked at the data and decided we can't do much with it. Maybe things are better now, I don't know.

RB: The airplane itself was our sensor at times. The turbulence and vertical motions and I guess the airplane as a sensor is not very good for lightning studies. You have the equipment on the airplane and the equipment we had available at the Thunderstorm Project by today's standards is rather primitive.

HB: Ross Gunn had pointed out that you get autogenous discharges from the airplane and people flying over the "hump" in the CBI theater used to get it all the time. Flying through the clouds there, the autogenous discharges are banging all the time they are flying through there on the end of the Himalayas.

RB: You had a group from the Soaring Society on the airplane with us and they, on several occasions, entered the thunderstorms with sail planes. Do you recall how that element came about? It seemed like such an unlikely thing. The names I remember are Rasmusson. . .

HB: Stu Parkin? Wasn't he one of them? I had been interested in soaring when I was at MIT. We used to go to Elmira and I did it even afterwards when I was in the Weather Bureau. They were interested in sail planes and we helped these fellows set long distance and altitude records from Elmira, NY, by giving them advice on winds and turbulent wind and updrafts and that sort of thing. The New York Academy of Sciences had a meeting in which I described that we were going to have a Thunderstorm Project. They invited me to give this paper. I had things more or less set up. These soaring types were all centered around New York. They came up to me and said "We'd like to participate in that." I think that's how it started.

RB: A little after the Thunderstorm Project, activities at Chicago and elsewhere. Why at the close of the Thunderstorm Project there was the development of the activities in weather modification. We know a lot about that. In particular, I would like to ask you to reminisce on the arguments between the Weather Bureau, or I should say discussions between the Weather Bureau, and Irving Langmuir, and Jack Workman, and subjects like the seven-day periodicity, the role of the American Meteorological Society meetings took in New York. When much of that early material was first discussed and published so to speak.

HB: I went into that rather thoroughly when W. N. Hess in Boulder was assigned the job of this thick book on *Weather and Climate Modification*, published by John Wiley. I was asked to write the first chapter that was on the history of weather modification and I can't say anything better really than what I have in there, in which I trace all of Langmuir's activities and the work of the periodic seeding in New Mexico and the strange statistics that

Langmuir used. Through the General Electric Laboratories, they issued the published works of Langmuir and I wrote the section dealing with Langmuir's work in weather modification. I said to them, I can't do this because it would be too critical and make General Electric look like a bunch of fools. They said go ahead and tell it like it was. So I did that.

RB: There is one story you haven't told to my knowledge, and it has to do with your knowledge of the writing of text books. Your first text was called *Synoptic and Aeronautical Meteorology,"* something roughly that title. Later was revised *General Meteorology.*

HB: This title *Synoptic and Aeronautical Meteorology* was a reflection of the fact that synoptic meteorology and forecasting was really in that period in 1937 directed at airlines and aviation. The book really was started when I was working for TWA in the early 30's between my Masters and Doctorate studies at MIT. They thought it would be a good idea to put this in some kind of a brochure or syllabus for training their pilots and meteorologists and so on and the discussion went on. I thought the U.S. needs a book on the subject. "If you will support me in this I will write it." You know, if you look at that first edition, it was copyrighted by TWA. Did you know that?

RB: No.

HB: If you will notice on the first page, it says Copyrighted by TWA. I don't know why. Anyway, they helped in it, after I left the TWA job, I got McGraw Hill to publish it for TWA.

RB: It must make you feel rather proud, as it was pointed out last night, that your text is a standard in the early days of meteorological education.

HB: When you write a textbook like that, its something like getting married. You have to do it in revised form. You hate to have to teach print that is out of date. So there were four editions, the last one in 1974. Each one, I think, a little bit better than the first.

Incidentally, getting back to TWA, an interesting fact that doesn't have anything to do with this maybe. But I was between the Masters Degree and Doctors Degree at MIT. Rossby arranged for me to work at Scripps Institution of Oceanography and there I wrote a paper on the air masses of the North Pacific and I also did some airplane ascents there. We had a fellow at the old Glendale Airport whom we engaged to fly. He was a private pilot for a business of some kind. He flew in an old Curtis airplane which carried an aerograph. I was walking into the hangar there one day, and who should I meet but a fellow by the name of Bill Clover, who is the nephew of E.H. Bowie of the San Francisco Forecast Office. He was the chief meteorologist at the time of TWA and it was his idea that I come and teach the new methods to their meteorologists, and incidentally their pilots. So I went there and worked for TWA at Kansas City, Newark in the wintertime, and finishing at Glendale. I wanted to go back to MIT for my doctorate, but this was kind of hard to finance. They paid me a good salary and I was in pretty good shape, but meanwhile, TWA and Eastern airlines were acquired by General Motors. One day, when I was at TWA in Los Angeles in the summer of 1934, I received a telegram from the Vice President of MIT saying that I had been awarded the Alfred P. Sloane Fellowship in Meteorology in the Department of Aeronautical Sciences, which was the most lucrative Scholarship that MIT ever had. Until just quite recently, I couldn't figure out why, of all people, of all the wonderful students at MIT in engineering and what not, I had got this. I was playing golf with a retired engineer from General Motors just last year and I said I had a Scholarship from Alfred P. Sloane who was chief executive officer of General Motors. I said I never could figure out why. He said, I know they had this for employees of General Motors, they had a Fellowship Program for employees of General Motors. Bygosh, I told him about the fact that I was with TWA when General Motors was the holding company for it as General Aviation. He said, "That's why you got the damn scholarship, you didn't know the first-hand information. That was obvious." Then it finally dawned on me.

RB: We are coming to end of side number three. I don't know if there is anything you particularly want to talk about and we will put in another tape, otherwise we will call it quits.

HB: I have a lot of things to talk to you about personally on occasion--about meteorology or elaborating on some other things.

RB: We certainly thank you for this brief interlude this morning.

HB: I'm afraid I took too much time.

RB: Not at all.

HB: Over two hours.

RB: This concludes **SIDE THREE (TAPE NUMBER TWO)** of the interview with Horace R. Byers.