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TAPE RECORDED INTERVIEW PROJECT

Interview with C. C. (Carl Christian) Wallén
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Interviewer: Gordon Cartwright

Cartwright: [This is an interview by] Dr. Gordon Cartwright of Dr. C. C. Wallén, a consultant with both UNEP and WMO, on environmental and climate questions. The interview is being held in the main headquarters of the World Meteorological Organization in Geneva [Switzerland] on November 21, 1995.

The idea of such an interview came about during a visit with Dr. Will Kellogg in Boulder back in August. I spoke to Dr. Kellogg of the many conversations I had had with Dr. Wallén in Geneva, and suggested to him that it would be very useful if this background that Dr. Wallén has on the early days of the Bergen school and other developments in meteorology in the European area. Dr. Kellogg agreed and wrote both to me and Dr. Wallén, asking us to go ahead with such an interview.

It would seem appropriate to begin with Dr. Wallén describing his own personal background. There is a separate interview by the WMO giving many details of this, but I think it would be appropriate if he could just highlight this background in the beginning of this tape. Now, Dr. Wallén, could you do this and give us some background on your career?

Wallén: Thankyou, Gordon. It's a great pleasure for me to participate with you in this interview, and the basic reason I think that this has come about is really the fact that I have lived with the science and applications of meteorology all my life. I was born in Stockholm in 1917. My father, who was at that time Director of the Hydrological Bureau in Sweden, had through his own efforts been able to merge this Hydrological Bureau with the Meteorological Institute, to become what we now know as the "Swedish Meteorological and Hydrological Institute."

My father, Axel Wallén, was its first Director-General. My father suddenly died in 1935, and this was of course a blow to the family, and I had no clear idea of what to do under the circumstances. But I was helped about, so that I could go to the University and I studied as the first subject, geography, under the then-quite well-known professor, Hans Ahlmann, at the Stockholm University. I graduated in 1941 with geography, but also with mathematics, physics and later on, I was fortunate to get Professor Tor Bergeron as my supervisor and friend. With him, I took meteorology, as he was the person who was, at that time, charged with examining the subject of meteorology at Stockholm University.

After the war, I went for a year to Chicago, which was the mecca for all meteorologists at that time, the reason of course being that there was Professor Carl-Gustaf Rossby, who at that time was managing his well-known Chicago school, which was the second important school and educational development during this century in the subject of meteorology. I studied with him for a year as a student-at-large, and then returned to Sweden and got my Doctor's degree in 1949. I then became assistant professor at the Stockholm University in 1950.

My first international activity was in Mexico, where I was for a year in 1954, on a technical cooperation assignment for UNESCO. After that, I returned to the Swedish Meteorological and Hydrological Institute and became Assistant Director in 1955. And later, in 1963, Deputy Director-General, under Dr. Alf Nyberg, a well-known figure within WMO. I also carried out various assignments for WMO, including a special-fund project in Peru and an expert assignment in agroclimatology in FAO. In 1968, I accepted a post in the WMO Secretariat in Geneva; while my daily duties covered the wide range of scientific fields, my main interests were climatology, applications of meteorology, and the socioeconomic aspects of climate change. I came to be involved in the environmental aspects of meteorology, and played some role in the organization of the UN Conference on the Environment in Stockholm, 1972.

From 1976 to 1980, I was Deputy Director of the Global Environmental Monitoring System within the United Nations Environment Programme, and was its main liaison officer with WMO. I have also had a number of assignments as Executive Editor of the famous Swedish journal, **Tellus**, 1947-53, and Editor of **Geografiska Annaler**, another famous Swedish journal from 1950-65. I have also been a resident of several Swedish societies, and Vice-President of the WMO Commission for Climatology, from 1960-68. At present I am chairman of SAC in UNEP, i.e., the scientific advisory committee in UNEP on climate questions.

Many technical and scientific papers, dealing with climate change, rainfall, acid rain, agriculture, agroclimatology and environment have been published over my lifetime.

I think this may be sufficient as a background for understanding why I have an overall view of the development of meteorology throughout this century.

Cartwright: You can see from these remarks about his career that Dr. Wallén is an ideal person to describe the developments of meteorology as seen from the European side and particularly the developments that took place in Bergen about the time of World War I. Since we are looking at the development of meteorology at that time, and between that period and World War II, perhaps you could describe the formation of the so-called Bergen School, which played such an important role in the whole development of meteorology most of this century.

Wallén: The father of the Bergen School was the famous hydrodynamicist and later on, meteorologist, Vilhelm Bjerknes, a Norwegian born in Oslo in 1862. His father was also a professor of hydrodynamics in Oslo, and obviously, he had important influences from his father in his further development of his important ideas in hydrodynamics. Bjerknes, after having studied in Oslo, came to the newly-established Stockholm University. It was not called a university at that time; it was called "high school"--*hoch-shule*, to indicate it was not a state enterprise. He was professor from about 1895 to the early years of the 1900's. The thing that really became important from the point of further developments in meteorology and particularly synoptic meteorology, was the invention of or rather the formulation by Vilhelm Bjerknes of the so-called "circulation theory," which was formulated in 1897 and the first article of which was published in 1898. He was able to show there that within the mid-latitude cyclones the distribution of the cold and warm air tends to become such that in connection with the cyclogenesis it comes to "an internal fight" between them and a circulation takes place. From this point of view, it became basic for the further development of the cyclone theory.

From his studies on this circulation theorem, he later on in the year 1904 to 1906, became further and more deeply interested in its application to meteorology. He started to write a number of articles about the possibility of applying the circulation theorem in meteorology. He got even further interested in synoptic meteorology and the more practical aspects of hydrodynamics in general. In 1910-1913, he was for a few years professor in Leipzig, where he started to draw the first synoptic maps and to apply his theoretical ideas to the more practical development of a model of how he understood the temperate latitude cyclones developed.

When Vilhelm Bjerknes came to Leipzig in 1913, he had with him, from Norway, as assistant, later on a famous meteorologist and oceanographer, Harald Ulrich Sverdrup, who helped him further in development of the ideas of how hydrodynamics and the circulation theorem could be applied to the practical meteorology.

Unfortunately, the situation in Leipzig became very difficult when the First World War broke out, and of course in large parts, not to say all of Germany, the food situation gradually became disastrous. Indeed, it led to the fact that Harald Ulrich Sverdrup in a letter home told that he had to finish working because he was so weak from not having eaten for several days that he just couldn't continue. Fortunately, in May, 1918, having finished his assignment, Vilhelm Bjerknes and his assistant, Sverdrup, moved on back to Norway. In 1918, Vilhelm Bjerknes was invited by the then professor in oceanography in Bergen, Björn Holland Hansen, to establish an institute for development of meteorology at the Geofysical

Institute in Bergen. Then, we can say that the real Bergen School started.

Cartwright: This is indeed a remarkable narrative of how this famous institute, which had such a profound influence on the rest of the meteorological world, came about. It is completely new information to me, and I hope that Dr. Wallén can continue to elaborate on how this school developed further. He knew many of the participants personally, either at the time or later on, and his account of his their participation in this school, I think, will be of extreme interest to meteorologists in most parts of the U.S.

Dr. Wallén, would you continue then?

Wallén: Well, as I said, the real Bergen School started then in 1918. The first, should we say, not students, rather scientists, who got involved in the developing of the future ideas, based themselves on the first attempts to look into the application of the circulation theorem to practical meteorology that had been developed in Leipzig by Vilhelm Bjerknes.

The first picture of the mid-latitude cyclone, and the first paper on the cyclone model was in fact published by Vilhelm Bjerknes' son, Jac Bjerknes, as early as 1918. This means that quite a bit of preliminary work on this model must have been carried out during the Leipzig period rather than in Bergen itself.

In addition to Jac Bjerknes, who was probably the "pillar" of the school together with his father during the first year, there was at that time one more theoretically-oriented person, namely the Norwegian scientist, and mathematician Halvør Solberg, who worked with Vilhelm Bjerknes and his son, Jac, during the years 1918-19. In 1919, a third person who became one of the pioneers of this school joined the group, namely the later-on professor and teacher of modern synoptic meteorology, Tor Bergeron. He came from Sweden, where he had studied at Stockholm University for several years.

The interesting reason that carried the Bergen School away from the more theoretical basis to the practical applications and the development of modern methodology in synoptic meteorology was the fact that in the years after the war, the food situation in Norway was in reality quite serious. Both the farmers dealing with agriculture in the southern part of Norway and the fishermen along the coast of the Atlantic, were in great difficulties. So the government felt keen on having better weather forecasting for the country. We know, of course, that Norway is situated in an ideal location to see the mid-latitude cyclones moving in from the West. For the above two reasons, it became important to establish a special network of observations over Southern Norway, which could be used to develop better forecasting of the weather. Vilhelm Bjerknes realized at this time very clearly that this would have to become his new school's really important task.

Cartwright: This is a very interesting commentary by Dr. Wallén. I have been very fortunate to obtain a text, a teletype--telescript text of Professor Sverre Petterssen's autobiography, **Of Storms and Men**. Professor Petterssen was born in the northern part of Norway and understood very keenly the dependence of the farmers and fishermen on improved weather information. In fact, he became part of the further development of that application when he came later to Bergen, and became one of the important members of the Bergen School, a story which will evolve from what Dr. Wallén has to say.

Wallén: This understanding from Bjerknes' side led to the establishment in Southern Norway of a very dense network of observation stations. This was indeed one of the most dense networks that has ever been established over any area. This in its turn had the excellent consequence that it was possible to follow in detail a cyclone's development when it moved across Southern Norway. This must have contributed much to the continually improved understanding of the cyclone model and the family of cyclones. Indeed, it also gradually led the meteorologists of the Bergen School in the Geofisikol Institute to give forecasts to the farmers and fishermen in Norway, which became gradually more and more appreciated.

I'd like to put in here that in the early period of the Bergen School there was a visitor who later on would become very famous in the field of meteorology, namely Carl-Gustaf Rossby, who, after studies in Stockholm, joined the group in 1919 for about two years time. He seems to have contributed to the development of the school basically by having good ideas regarding how to apply theoretical hydrodynamics to the future development of the practical side of the school. He was not known to be interested himself in the drawing of maps or the studies of synoptic meteorology, although he learned his profession quite well.

The other person, who instead showed extremely great interest in synoptic meteorology--which means to study the observations on the synoptic chart and to analyze the chart and to gradually understand more and more the cyclone developments--was Tor Bergeron. Indeed, I think he probably was the one most interested in developing the forecasting technique that developed from the Bergen School. And his contribution to this was quite big. He found in 1919 the so-called "occlusion" model, where the cold front in the cyclone moves close to and joins the warm front, which led to the further understanding of the development of the cyclone family. One understood gradually that the cyclone development from a wave to the "occlusion" happened to each one of the cyclones of a family. Later on, it was easy to conclude that cyclones really moved in families on the polar front--the front between the colder air in the North and the warmer air in the South.

Bergeron went further in studying these things by developing the ideas of the air masses and the importance of the differences between cold air and warm air for the further understanding of the building of cyclones and fronts between air masses around the globe. He was probably the most interested in synoptic meteorology of the whole group of people who studied in the Bergen School. Indeed, in 1923, he published together with Jac Bjerknes and Halvør Solberg the first complete publication on the cyclone model and the life cycles of cyclones. This is also the first publication on the behavior of the cyclones in the temperate latitudes.

Tor Bergeron continued to work in Bergen during the whole 1920's, and to add to his famous, but very difficult to read doctoral thesis, which was published in German and was called *"Über die drei-demensional verknüpfende Wetteranalyse"* (translated, "On the Three-Dimensionally Coupled Weather Analysis,") which was defended in Oslo University in 1928, when he got his doctor's degree. The book is really considered to be a bible of the Bergen School, and practically all of what had developed from 1918 to 1928 exists in that book, although as I said, it is not easy reading.

Cartwright: Yes, C. C., this is a indeed a fascinating insight into how these famous theories and accounts were developed. Many, many American meteorologists met Professor Bergeron during his stay in the United States and learned to regard him as one of the fathers of the synoptic model. Would you continue now with further development of the school?

Wallén: Bergeron, after having received his doctors' degree, started what was just as important as the development of the ideas in the Bergen School itself, namely the teaching of them to meteorological services around the world. So he became for a number of years a very well-known teacher of the Bergen School ideas, and he ended up after several other visits in other places in Moscow in 1931-32...

His lectures in Moscow became particularly famous because they were summarized by a Russian meteorologist, whose name was Chromov, in a book called **Lehr Buch der Synoptische Wetteranalîise**. Which became later on translated into German by the Czech meteorologist, Dr. Gustav Swoboda, who was Chief of the International Meteorological Organization Secretariat from 1938-1951 and Secretary-General of WMO from 1951-55.

To end up with what Bergeron did in the Bergen School context, he moved in 1934 to his native country, Sweden, and the Swedish Meteorological and Hydrological Institute, where he had been invited by my father to gradually introduce the new Bergen School ideas to the meteorologists of the Institute.

Among other persons who joined the Bergen School during various periods of its existence, and later on became famous in meteorology was Erik Palmén, from Finland, who studied in Bergen during the late 1920's. You will hear more about him at a later stage when we discuss the development of the Chicago School.

Sverre Petterssen, who was mentioned earlier, joined the Bergen group in 1928 and became indeed, together with Tor Bergeron, the one who helped more than others to spread the new ideas around the world. Sverre Petterssen's importance to the Bergen School is therefore his publishing of a number of textbooks where he outlined in a very understandable and teachable way the fundaments of the Bergen School methodology. These books have been extremely important for students in meteorology around the world over the years, particularly from the 1940's to the 1960's.

As far as the United States is concerned, there were some difficulties to introduce the new ideas and the new technology in the meteorological services, both in the civilian and in the military. But there were a couple of persons who were trying hard from about 1928 and onwards to have this done. The two persons were very close to each other, and close friends as they had taken as one of their common tasks to insist on having these ideas introduced in the U.S. during the 1930's. These two were the Swede, Carl-Gustaf Rossby, who had, as I mentioned, been in the U.S. since 1925, and the American, Dr. Francis Reichelderfer, who became "hooked" on the 'Bergen' School when he visited Norway in the early 1920's and later in 1933.

The development of the Bergen School ideas in the U.S. picked up real momentum at the time when Dr. Reichelderfer became Director of the U.S. Weather Bureau in 1938 and had Carl-Gustaf Rossby as his Deputy Director. Another person who then became involved in developing these ideas in the U.S. was Professor Hurd Willett, who had been sent to Bergen in 1929 by Dr. Rossby, then Professor at the Massachusetts Institute of Technology, where he had established the first department of meteorology in the United States, in 1928. I cannot avoid mentioning another person who was extremely material to developing ideas from the Bergen School in the U.S., and who has also written several reviews which I have been using for this interview about the development of the school in the U.S. This is Dr. Jerome Namias, who, as far as I know, did not visit Bergen in those days, but became very interested in the new methods when they were introduced in the U.S. and then furthered them over several years to come.

It may be interesting to recall that other events in the meteorological and oceanographical field took place in the 1920's which have become famous. The first thing to mention is that Harald Ulrich Sverdrup, the assistant to Vilhelm Bjerknes in the Leipzig period, entered into oceanography and indeed became the leader of a famous oceanographic expedition with the Norwegian ship, "Maud," which followed the currents over the North Pole and in the Arctic during the years 1924-26. On this expedition, he had with him a young Swedish meteorologist who carried out the meteorological work during the expedition. It was the later-on famous young man, Finn Malmgren, who disappeared so tragically during the

"Nobile" expedition with the zeppelin, "Italia," over the North Pole in 1928. That expedition that was all the way through a tragedy, and was led by the Italian General Umberto Nobile. I cannot help mentioning that Malmgren in the 1920's was the one my father had hoped would become his successor at the Swedish Meteorological Service.

The second thing that happened during the 1920's which had a fundamental impact on the further development of meteorology took place in the U.K. A great thinker in psychology as well as in meteorology, Dr. F. Richardson already before the First World War had played with some basic ideas in meteorology, which he did not publish until 1922; the first ideas about the possibility of numerical forecasting of weather. He played indeed for many years with the idea that it would be possible mathematically from the hydrodynamic basic equations to forecast the weather 24 hours later if you had taken simultaneous observations at a certain time, which were to be included in the hydrodynamic equations. This idea he later discussed thoroughly in a book published in 1922. He had to agree in this book that it would take too much work and time to reach a result with the means available at that time. Indeed I think he found that to make a forecast for 24 hours with this method would take about three weeks. It should take us all into the era of the mathematical computers during the second half of the century before it was possible to completely appreciate the fundamental ideas of Richardson. They nevertheless were always with us meteorologists, while we waited for the invention to come that would make these ideas possible to realize. More about this later.

Cartwright: C.C., this brings up a very vivid personal experience I had many years later, when I visited with Wendell Mordy at his home in Stockholm, while he was attending the famous Stockholm Institute under Rossby. He had the good fortune to be able to live in the home of one of Sweden's well-known sculptors. He took me into the large atrium where there was a heroic statue of the meteorologist, Finn Malmgren, who lost his life in the Italia expedition. This statue was to be placed at a suitable point in Sweden, and I never did see it in its final version. But it's a remarkable coincidence that I had the good fortune to spend a few days with the Mordys in this beautiful Swedish home.

Wallén: This reminds me about something that I would like to tell about my teacher and friend, Tor Bergeron, or rather about his wife, with whom I also became a very good friend in the later years, in Sweden. When Tor Bergeron had finished his lecturing in Moscow, he had met a lady whom he obviously had fallen in love with, and he had decided to take her with him back to Sweden. However, this was not an easy thing in those days, to get a person out of the Soviet Union. He obviously had to try to steal her out of the Soviet Union. He had no problem taking her with him on the train from Moscow to Leningrad. But obviously she could not have any baggage with her. She was a companion who would get off at the next station, having no baggage. However, the real problem came, of course, when he wanted to take her over the border from Leningrad to Helsinki. But as this lady had nothing with her other than a fur coat, which was on, he succeeded in cheating the customs officers at the border by hiding her momentarily on the train. After the border was passed, she was free to come with him not only to Helsinki, but also to Stockholm, where she became his wife. A very lovely lady, whom I admired greatly for her sweetness, but particularly for her piano playing. She was a great pianist, and it should be remembered that one of the main interests of Tor Bergeron was music. He was a great singer himself.

Cartwright: This will end the recording of Side A, Tape 1. We will pick up again on Side B with the transition period between the developments in Europe and the Bergen School, and how the famous Chicago School under Rossby came about.

END OF TAPE 1, SIDE 1

Interview with C. C. Wallén

TAPE 1, SIDE 2

Cartwright: This is November 22, 1995 and the tape interview will continue with Dr. Wallén taking on from here.

Wallén: <u>The transition period between the Bergen School and the Chicago School</u>

One can say that the Bergen School existed in principle throughout the 1930's because indeed grants from the Carnegie Institution, which had been given to Vilhelm Bjerknes for his development of the ideas of the Bergen School, had started in 1906, and didn't finish until the year 1941. However, the 1930's as I pointed out in the earlier part of this tape, was basically devoted to, should we say, trying to transfer the ideas of the Bergen School to various countries and to various meteorological services around the world. As I pointed out in my earlier part, Carl-Gustaf Rossby became one of the persons who would transfer the Bergen School ideas to the U.S. in cooperation with various other persons who were mentioned in that early part. He had, as I pointed out, been studying himself in Bergen between 1918 and 1920, and he was then employed in the Swedish Meteorological Service throughout some years in the early 1920's, before he left for the United States in 1926, on a Swedish-America Foundation grant. In the U.S. he devoted himself first in various ways to developing meteorological services for aviation with support from the Guggenheim Foundation. In his work in this period he met a number of important persons who later on would help him to transfer the Bergen methodology to the operational services within the United States.

The main person in this regard was, as earlier mentioned, Dr. F. Reichelderfer, who became the Director of the Weather Bureau in 1938, with Rossby as the second-in-command. The detailed story of Rossby's activities from the time that he came to the United States until about the date when he became Deputy Director of the Weather Bureau, is well accounted-for in various articles in the American Meteorological Society **Bulletin**, and in articles by Jerome Namias and need not be repeated here.

Rossby, in addition to trying to convince various operational services within the United States to adopt the Bergen School methodology, made a main contribution to the development of education in meteorology in the United States by the formation of the first meteorological department in the U.S., which was established in 1928 in the Massachusetts Institute of Technology, also with support from the Guggenheim Foundation.

In addition to furthering education in meteorology in various ways, Rossby concentrated in the 1930's in his science on application of various hydrodynamic problems to the circulation in the atmosphere, and also to the circulation in the ocean in order to try to understand better how the ocean influences atmospheric circulation. He did those studies basically together with an American scientist named Raymond Montgomery.

Secondly, he interested himself in the first upper air maps which became available in the late 1930's, after the invention of the radiosonde. He worked for some time with the so-called isentropic maps, and later concentrated on the 700 and 500 millibar upper air maps. He came to become just about as important as Vilhelm Bjerknes had become for the development of the synoptic surface map. Therefore, if we have said earlier that Vilhelm Bjerknes was the father of the modern surface observation map in meteorology, Rossby became the father of the upper air maps and thereby the father of what has been called the science of aerology. In 1939, he found the waves on the upper air maps that have been named after him, called the Rossby waves. They are long waves in the upper air circulation, and he made the first theoretical analysis of their movement around the globe in the above-mentioned publication.

In all his work from this time, a simplified approach to the theoretical analysis, in order to find the essential of a geophysical process is typical of his work.

In transferring the emphasis of the meteorological analysis of what happens in the atmosphere from the surface, which had been the area of the Bergen School, to the upper air, I think Carl Rossby made a very particular contribution. He was not of course alone in this. There were several others who were interested in what this emphasis on the upper air could give in addition to surface maps analysis. The person who was particularly interested and had carried out research upper air research in Europe was Eric Palmén, who was mentioned earlier, as one of the persons who had been studying in Bergen during the 1920's. Eric Palmén would become one of Rossby's closest collaborators during the 1940's.

In 1942, Rossby made his next important contribution to the development of education in the United States. He was called in in 1942 to train those large numbers of meteorologists who were required for taking of observations, making analyses, and helping in various ways both in the Air Force, the Navy and the Army of the United States during the Second World War, which for the U.S. had started in late 1941. The courses for the number of students that were necessary for this purpose was of the order of magnitude of 1,000 students per year, and a school had to be established for teaching them. It was established in Chicago, in the first half of 1942, and Rossby became the leader of these courses.

He also developed a new department of meteorology in Chicago with Horace Byers as his deputy and in close coordination with these military courses. This department started with a number of young students and scientists dealing with various research problems that Rossby was particularly interested in. Although the main interest during these first years, when the war was still going on, of course was the training of the military students, the first attempts to develop certain new basic ideas, especially in aerology, were taking place in the meteorological department. However, it was not until the war was really over when what has later been called the "Chicago School," in the Department of Meteorology of the University of Chicago, became an important research institution.

Perhaps the most important of all years when this Chicago School was operating was the year 1946-47, when there was a great migration to Chicago from all around the world; where many meteorologists came to Chicago to see and understand the new ideas that were being developed by Rossby and his collaborators.

Cartwright: Dr. Wallén, before we proceed with your discussion of the School and its developments, I would like to interject a question. I was working in the Weather Service during the twenties and thirties. And when the Bjerknes methods were first brought forward, there was, as you have pointed out, considerable difficulty in gaining their acceptance on the part of the bench forecasters of that period. Most of the forecasters were using persistence analysis of the pressure patterns, and knowledge of how these would vary with time based on experience. In a sense, it was a kind of weather typing that was later developed to a high degree by Krick and pushed on to the scene very actively by his group. Could you indicate whether this was true in other parts of the world to your knowledge, or was this typical of the United States?

Wallén: This is an interesting question. I think there are many different reasons for this problem, which was not typical of the U.S. Basically, one reason was that most of those people everywhere who had become weather forecasters had over the years got their personal ideas of how to analyze and how to interpret the maps. Each one of them seems to have had their own methodology and they would foresee with intuitive feeling what would happen. Indeed, the forecast was based either on their intuition or on their ideas about movements of various isobaric patterns, as you have pointed out yourself. But there was also a fundamental difference between those who, at an early stage, understood and accepted that the Bergen School was based much more on hydrodynamics than the isobaric geometry ideas that for so long had dominated the profession. They were therefore those who became interested in the Bergen School while many others stayed with their own ideas. These pioneers were not so many; however, there were, of course, the Norwegians themselves, influenced by V. Bjerknes, and in addition some few other scientists with a good theoretical background in meteorology who gradually accepted them. However, among the forecasters, I would agree that many years went by before they accepted the new ideas as an operational methodology for weather forecasting.

Cartwright: Dr. Wallén, I remember before the war there was a lot of discussion about the major development of meteorologists in Germany, presumably in preparation for their war efforts. Can you comment on the contributions of German scientists to the elaboration or further development of the Bergen School?

Wallén: Well, indeed, I was just going to comment on the basic differences between two existing approaches. In the theoretical meteorology which existed during the early part of this century and interestingly enough at about the same time as the first ideas of the Bergen School methodology were developed by Vilhelm Bjerknes and as a consequence of his formulation of the circulation theorem, there was in Vienna a German school tha, like the Bergen School, in general was based on Margules' ideas about hydrodynamics. This school further developed old ideas from Helmholtz' time, which were to a certain extent in conflict with the Bjerknes ideas. At least, they can be said not to have accepted the basic fundaments of the circulation theorem. The approach was slightly different, and for that basic reason, the scientists or practical meteorologists who were taught in Vienna had great difficulties accepting the ideas of Vilhelm Bjerknes. And I think this conflict indeed existed at least until 1925 when, as far as I know, a book called **Dynamic Meteorology** was published by a well-known Vienna meteorologist whose name was Exner, who summarized the ideas of the Vienna School. That is still an excellent textbook in hydrodynamics but it does not follow the same development line that Vilhelm Bjerknes had.

That touches then upon how the situation was in Germany. As far as I can understand, German meteorology in general was quite well-developed. The fact that the University of Leipzig called in Vilhelm Bjerknes to work in the department of meteorology was a sign of the openness of the German meteorology in general. Although the German meteorologists were closer to the Vienna School at that time than to Bjerknes' ideas, they were open enough to new ideas to take this, at that time, rather unusual step.

In general, I would like to make further the comment that German meteorology was a big and important scientific field in those days. It was much broader in its approach to all aspects of meteorology than in any other part of the world, as far as I can see. From Germany came the first interest in many areas of meteorology like the area of climate and health as well as the first ideas about microclimate, which were developed theoretically by Wilhelm Schmidt and later presented by Rudolf Geiger in **Das Klima der bodennahen Luftschicht**, published in 1927 for the first time. These approaches indicated that a broad spectrum of meteorology was well-covered in Germany in these days. Geiger's book was in the 1930's translated into English, and was then the bible for those who developed microclimatology in the U.S., particularly Warren Thornthwaite.

Cartwright: Dr. Wallén, you mentioned earlier the fact that Professor Bergeron spent some time in the Soviet Union giving lectures on the Bergen methods. Later, you said that these had been transcribed by Chromov, and published in German. Have you any information about how well the Russians were able to use the Bergen methods and whether they advanced the general principles to any significant extent?

Wallén: This is again an interesting question. The only answer I can give to that is that I think that they applied the Bergen School methodology definitely and very thoroughly, by following the book by Chromov, which was later translated by Swoboda into German. It was later used also in practically all other countries for the students to learn the Bergen School methodology. The importance of that textbook must be considered very great because all over Europe at least and in many parts of the world otherwise, it has been used for teaching. I have, however, no particular knowledge about what the Russians have made in developing further synoptic meteorology. The Bergen School methodology dominated synoptic meteorology all over the world until the Chicago School developments entered into an era where numerical forecasting became dominant. These developments also took place in the Soviet Union and in Russia.

Cartwright: ...This is all quite interesting to me. I had later some experience with the Russians during the IGY in the Antarctic, and I found their analysis techniques very similar to those which were used in the West. At the end of your comment, you mentioned the development of numerical weather prediction. I wonder if that had started at the time of the Chicago School when Rossby was developing many of his very important fundamental theories about the circulation of the atmosphere.

Wallén: Well, I will come back to that later in my discussion about the developments of meteorology, and particularly synoptic meteorology, in connection with and after the Chicago School. As we are now dealing with the transition period, basically between the Bergen School and the Chicago School, I would like again to make a reference to Tor Bergeron, whom you mentioned a while ago. In addition to his outstanding contributions to the Bergen School methodology, he was quite a broad-minded meteorologist in the sense that he made contributions also in other areas. A famous contribution by Bergeron is the so-called Bergeron-Findeisen precipitation theory, which was, I think, published by Bergeron in an IUGG publication at the Lisbon meeting of the IUGG in 1933.

This theory shows the broad understanding of the physics of the atmosphere that Tor Bergeron had. In addition to his special skill in analyzing a synoptic map, he understood what was going on in the atmosphere in such a way that he could, from a surface observation, conclude what is happening in the upper air. I was his assistant forecaster many times when he taught me how to understand from the surface observations what happened in the upper air. That was what he called, "indirect aerology."

As far as the Bergeron-Findeisen precipitation theory is concerned, it was as I said, published by Bergeron in 1933 in a publication which was based on on the ideas of Findeisen, which go back to about the early 1920's. But the whole theory, which includes the idea that in order that precipitation would take place in the temperate latitudes, it is necessary that the condensation nuclei reach the ice level so that they become ice drops, which fall back into the lower layers, where they become rain. This basic idea has become of fundamental importance in the application to weather modification that was attempted in later years during this century. In 1947, in the US, Schaeffer and Langmuir did the experiments which confirmed the Bergeron ideas on this matter, and later on, tried to use them for weather modification, however with little success.

The Chicago School

I think by having added these ideas that Gordon Cartwright has brought up, we have finished the transition period from the Bergen School into the Chicago School. I already mentioned that the beginning of the Chicago School in the years 1942-45 was of course concentrated on the war efforts and to train the necessary military meteorologists that were required. But, as I said, immediately after the war was over, there started a migration of scientists in meteorology who wanted to come to Rossby's institution in order to be familiar with what was going on and for the important research that took place. The research was indeed concentrated on the upper air circulation, and on the connection between the upper air circulation and the surface circulation on one side and on the other, how the upper air circulation is related to the general circulation of the atmosphere. This migration included myself; I studied in Chicago during 1946-47.

In the year 1946-47, which showed, I think, the height of the Chicago School, there were many famous meteorologists in Chicago, who contributed to the development of the understanding of the relations between the upper air circulation, both with the surface circulation and the general circulation of the atmosphere. The closest collaborator of Carl Rossby in those days was obviously Erik Palmén, who was well-known to be the dominant person to understand at that time the new ideas of the upper air circulation. He contributed very much to what was going on in Chicago in 1946-47. One of the persons who had studied under him in the early forties was Alf Nyberg, from Stockholm, who was in Chicago, and, among others, contributed to the famous paper of the Chicago School that was published by all the collaborators in Chicago 1946-47, which had the title, "On the General Circulation of the Atmosphere in the Middle Latitudes."

Before I come to special events during this year, I would like to mention a few others who were there either to study in Chicago, to lecture to help Rossby in his training of students or to give special scientific lectures in their field. Among the students of Rossby was Herbert Riehl, who later became a specialist in tropical meteorology and also contributed to the relations between the upper air circulation and the general circulation of the atmosphere. Another of his students was George Cressman, who was a lecturer and instructor in the basics of aerology, then later became Director of the National Weather Service in the US. There was also Victor Starr, who later on became famous in his contribution to the general circulation of the atmosphere, particularly after he became professor at MIT. Other persons who became famous were Rolando Garcia, who from his year as a student in Chicago, moved further on to Los Angeles, where he studied with Jac Bjerknes and Jörgen Holmboe. Later on, he became not only professor of meteorology at the University of Buenos Aires, but also later Minister of Education in the government of Argentina. Other ones to mention were Tu Chen Yeh, who was one of the Chinese students who later on became famous as Chief of Research within the Academia Sinica, the Chinese Academy of Science. Among the students was Chester Newton, who later on became famous for his collaboration with Erik Palmén on the mid-latitude circulation. Also among the students who appeared in the spring was Navy Lieutenant Commander Dan Rex, of whom we shall talk later.

Among those who contributed to the development of numerical forecasting there was among several others, Jule Charney, who was one of those who became more famous in that connection. The visit of Jule Charney was, indeed, a special one, typical for Rossby. Jule Charney was on his way from Los Angeles, where he had just published his doctor's thesis, to Norway on a fellowship, to study with the successors of the people of the Bergen School. Among them were several theoreticians, who also concentrated on the subject of trying to understand the upper air circulation in relation to the surface circulation, and the general circulation. Charney got a cable from Rossby before he left Los Angeles, asking him to stay over in Chicago for a few days. In fact, he came and he got off the train, when Rossby said, "Well, OK, please go and see Erik Palmén because he has a big apartment and you can stay there for a while." He went, and he moved in, together with his wife, Eleanor. Then he and Rossby started to discuss Charney's thesis, and Charney started to participate both in our map discussions every afternoon and in the more secret conferences on numerical forecasting that had started. There were, at that time, map discussions to acquaint ourselves with the development of the weather over the United States, which was very useful for all of us from the point of view of practical understanding. Charney therefore stayed on and on and on, as Rossby didn't want him to leave. He stayed for more than half a year before he continued to Norway.

In addition to those that I mentioned, I can tell about those who visited Chicago for a few weeks or a few days during that year. There was Professor Hans Ahlmann from Sweden, who had been my professor of geography; there was Halvar Solberg from Norway, who was one of the famous ones from the early days of the Bergen School; I remember he gave a lecture which was very difficult to understand because it was extremely theoretical. Tor Bergeron was also visiting Chicago during that year.

An event which was very interesting and became well-known from the year 1946-47 and which I had the pleasure of attending was the first, should we call it, theoretical confirmation of the existence of what later on has been called the "jetstream" in the upper air. I think that the jetstream itself had been experienced by the war pilots over the Pacific a few years earlier. They did not understand what happened to them, but their airplanes were standing practically still in the upper air when they met these kinds of headwinds. But at this stage, there had still been no clear confirmation of the existence of the jetstream. One had seen the packing of the isobars on the upper air maps in certain areas where one could say that this is clear indication of a very hard wind. But this had never been really analyzed in detail. As far as I can remember--it's close to 50 years ago now but, I think it was the 7th of February, 1947, that in the map discussion where we all were gathered together, with Rossby as the chairman, Erik Palmén for the first time presented a clear-cut, detailed analysis of a cross-section through a jetstream. Of course, by this analysis it was basically confirmed for the first time, how it was structured, what it was, how strong the winds were, etc. And this in my understanding was the birth of the "jetstream." That it was the birth of the jetstream, nobody knew at the time, because nobody had a name for it. So in the discussions that took place after the presentation, Rossby asked, "What should we call this?" Then, after a little while, he said, "The jetstream." That has then become the name of this phenomenon in the upper atmosphere.

END OF TAPE 1, SIDE 2

Interview of C. C. Wallén

TAPE 2, SIDE 1

[Transcriptionist's original note: This tape has a great deal of static-like noise, making it difficult to hear much of the interview.]

** **January, 1997**: Dr. Wallén has re-recorded this section that was so difficult to hear. He has requested that this section be inserted at the beginning of Tape 2, Side 1. [D. Rabson--transcriptionist.]

NEW TEXT:

Wallén: <u>The Chicago School and the development of numerical forecasting</u>

I think the year 1946-47, at the height of the Chicago School, laid the groundwork for two further developments in meteorology, namely, the very important new numerical forecasting of weather and secondly, the developments at the International Meteorological Institute in Stockholm, created by Rossby in 1948--particularly the part on atmospheric chemistry. We shall follow these developments one after the other.

It is not surprising that the intense studies of the relationship between temperate latitude cyclones, the upper air circulation and the general circulation of the atmosphere, that Rossby and his collaborators pursued from the late 1930's to 1947, mainly in Chicago, led to the birth of the ideas of numerical forecasting. Indeed, also here, Vilhelm Bjerknes was the first one to propose that integration of the hydrodynamic ground equations provided with data for a certain moment, could furnish a basis for weather forecasting. He did this in an article in 1904. However, it was not until the famous British meteorologist, L. F. Richardson, in 1922, developed this idea further in a classic book, <u>Weather Prediction and Numerical Process</u>, that one realized clearly the difficulties with such an approach due to the enormous quantities of mathematical calculations which were involved. Therefore, the idea was dormant until after World War II or, rather, in connection with World War II when the first creation of numerical computers took place in the United States. Then the idea came up again.

During the Second World War, when the first computers appeared that would make this multiple integration possible, it was the great mathematician, John von Neumann, who had a vision about the further developments of such computers. In 1943-1945, he was a consultant in the preparation of the first American computer for the military, the so-called "ENIAC." However, he had already in these days also a vision for the future of a much more rapid computer to be built, for both civilian and military use, by the experts then gathered in Princeton. Sometime in August, 1946, he called a meeting and approached Rossby about the possibility of civilian weather forecasting, using first the ENIAC, and, as he hoped later, the so-called "JOHNIAC," as his vision at that time was called by his friends.

In the famous meeting in 1946, the participants included Rossby, Wexler, Haurwitz, Montgomery, Starr, Namias, Rex and Elliott. This first meeting stimulated a theoretical discussion over several years of how to approach the issue of numerical forecasting in which Rossby, Charney, Fjörtoft and Eliassen, as well as von Neumann himself, participated. In this discussion, also, the shadow of L. F. Richardson, who was the first one to tackle the idea, played an important role. The theoretical discussion has become a very hot issue because it is unclear why the first experiments, which took place on ENIAC in the U.S.A. and on BESK later on in Sweden, came to be based on the integration of the simple barotropic vorticity equation. It is rather clear that the discussions between von Neumann, Rossby and Charney started with the assumption that integration of the fundamental hydrodynamic equations by the new computer had to be the leading approach.

However, rather soon it became clear--I think it was in 1947 or 1948--that some sort of approximation had to be undertaken and the possibility of approximating by integrating the quasi-barotropic equations was favored, especially by Charney. At this point, in particular, the Norwegian and English meteorologists maintained that no success could be expected, more or less, before baroclinic models were developed for integration.

The most intense part of the discussion took place between Rossby, Charney, and von Neumann while Fjörtoft and Eliassen contributed by further research. It seems from studies in the history of numerical forecasting carried out by Mr. Anders Persson in the European Medium Range Forecasting Center in Bracknell, England, that a certain convergence of ideas gradually centered on the possibility of starting with using integration of the simple barotropic vorticity equation. It seems likely that Rossby, from having earlier been convinced that the right approach was the integration of the hydrodynamic preliminary equations, started to feel that his pet equation--the barotropic vorticity equation--would do for the first experiments. Charney seems to have ended up with the same conviction after having attempted various approaches with a more complicated quasi-barotropic atmosphere.

So it happened, presumably through a joint agreement between Rossby and Charney, that the first experiments with ENIAC in 1950 were done with the barotropic vorticity equation and proved very successful, more successful than expected. When the first operational calculations took place in 1954 on BESK in Sweden, the same basic approach was attempted and again proved to be more successful than expected.

I should explain that both the ENIAC, the first American electronic computer, and the BESK, the first Swedish computer, were made without semi-conductors--not in existence at that time--but by electronic bulbs and they therefore became rather enormous devices that took up a whole room, and they were rather difficult to handle. The calculations on the ENIAC in America took place in the early 1950's, and were laboratory experiments on the barotropic vorticity equation. The BESK calculations which took place in 1954 were the first operational ones that ever took place. I think I should mention the fact that the BESK operations at that time were made by the Air Force Military Service in Sweden and not by the Swedish Meteorological Institute, basically due to the fact that at that time it was easier to get funding from the Air Force for this purpose than from the civilian Meteorological Service.

The unexpected success of this crude approach to the issue made it possible to apply it during a considerable time while scientists in the U.S.A., Sweden, Norway and the United Kingdom continued to develop further the baroclinic approach which finally became used. In all developed countries it has been in existence since 1965. Although Charney claims to have been the one to have decided to start with the crude barotropic approach in the first experiments in 1950, it is likely, as I said earlier, that Rossby, after having been skeptical about the whole project for some time, decided in 1947-48 to support this very simple approach for the start of the experiments.

To him, the barotropic vorticity equation that he had dealt with for many years in the development of the Rossby waves in the upper air must have been an obvious approach. It is also interesting that he was in favor that the simple approach be maintained, even in the first operational attempts that took place in Sweden in 1954. He was different from the others obviously in maintaining very ______________ that everything should be done to try to exhaust the barotropic vorticity equations' possibilities before one went further into more complicated baroclinic models. It's very interesting that the whole numerical forecasting was based on the barotropic vorticity equation for many years before the more complicated models were ready to be used in 1965.

The operations in Sweden that took place from 1954 on the BESK machine were taken care of by two students of Rossby's, Bert Bolin and Bo Döös. Both of them were in charge of these operations and were the ones that could make good applied use of the successful results for weather forecasting, which continued for several years in Sweden on the simple approach. It should be clear here that the International Meteorological Institute, where Rossby himself was the leader since he moved to Sweden in 1948, included, of course, Bert Bolin and Bo Döös, who were theoretical research workers who developed the basic course for the operations which took place first in the military service, but later on was moved over to the State Meteorological Institute in Sweden, which took over the running of the operations from, I would think, 1956. This continuation of the whole issue of numerical forecasting in Sweden is interesting to compare with that in the United States, where it was operated through the National Weather Service by George Cressman, with the theoretical background developed basically by Norman Phillips at MIT for several years after 1955. He was the one who gradually developed the baroclinic models that became used from 1965.

In Sweden, we also decided to go further on to the baroclinic atmosphere and models for that purpose from about 1965.

The Princeton group, who has always been the basis in the United States for these developments, has, since John von Neumann's time gradually become part of the Weather Bureau/Weather Service that would continue the development of numerical forecasting, under the leadership of Joe Smagorinsky. He took over in the late 1960's and was in fact a very important leader of this group in Princeton until the early 1980's, when he retired.

In Sweden, the Swedish Meteorological Service, as I said, took over the running of the models for numerical forecasting from 1956. Bo Döös was in charge of these activities until 1970, at the same time that he was professor at the meteorological department of Stockholm University. After him, in Sweden, Lennart Bengtsson was in charge until the creation of the European Centre for Medium-Range Weather Forecasting in 1974, where he later moved and ultimately became director.

Cartwright: C.C., you referred earlier to Bert Bolin. We know Bert very well because of his long chairmanship of the IPCC. Did he continue to have an active and important role in all these developments at the same time that Bo Döös was involved?

Wallén: Yes, in the sense that he was after Rossby passed away professor at the University of Stockholm in meteorology, and also the head of the International Meteorological Institute. And of course, Bo Döös was working with him. About the further development of numerical forecasting after 1974 we could mention here that Aksel Wiin-Nielsen, also a student of Rossby's in the 1950's, was the first director of the Centre in Bracknell, and later became the Secretary-General of the World Meteorological Organization in 1979. Then, Lennart Bengstsson took over the Centre in Bracknell and became director. He later became basically interested in climatology, as the models developed for the general circulation of the atmosphere for numerical forecasting proved to be useful in studies of climate change. Lennart Bengtsson, after he left the Medium-Range Centre in Bracknell, moved to Hamburg, where he is now professor of climatology at the Max Planck Institut.

Let me now talk a little about the second issue that I think was a development out of the Chicago School, when Rossby came to Sweden in 1947 after his many years in the United States. The second issue deals with the formation and development of the International Meteorological Institute in Stockholm. In this Institute, Rossby, as a second research issue after numerical forecasting, developed what has been called atmospheric chemistry--which was an absolutely new field for research. In reality, he may have had the idea from the U.S., but basically he developed the field from certain articles that were published in the first issue of the journal, **Tellus**, which was another thing that he created, at that time a new journal for meteorology issued by the Swedish Geophysical Society. I was the executive editor of **Tellus** for four or five years, and helped him as Editor during these years, until Bert Bolin took over in 1954.

I should like to mention somewhat more about atmospheric chemistry because it became later an important new issue. As I said, his idea was based to a large extent on articles by Anders Ångström and Harald Egnér. Ångström was at the time the director at the Swedish Meteorological Institute and Egnér was professor at the Agricultural College in Ultuna, outside of Uppsala. They had written a number of articles in **Tellus** about "Precipitation Chemistry," then a new subject. They were interested to what extent the various kinds of wheat, corn and other cereals cultivated at Ultuna were fed by chemical consitutents in the rain. They were also studying in these articles to what extent soils and what was in the soil was fed by the chemistry coming from the atmosphere through the rain. Of course their ______________________ was basically related to the applications it could have for agriculture. But Rossby became interested in this issue more from the research side, and started therefore to study this problem scientifically. I do not know whether it was late or early in the development that he asked Professor Egnér in Ultuna if he had somebody to come to Rossby's institute to help him develop these new ideas that he had considered important for the atmosphere. This way, he got a very good agricultural student who later became a meteorologist and even later on, a hydrologist, Dr. Erik Eriksson.

He got him over to Meteorology from the Agricultural College, and he developed into something quite remarkable, both in meteorology and hydrology.

I think another aspect of this issue is interesting to note: Rossby and his collaborators in Stockholm--he had many from various parts of the world-- but he also had a number of Swedes. With them, he wanted to get more closely into a related subject, which later on has proven to be of enormous importance to the development of meteorology; he wanted to study the content of CO_2in the atmosphere and its variations, completely scientifically. However, at that time he made no direct reference to what later has become so important, namely its impact on possible climate change. I think it is interesting to note that he made a number of observations for monitoring CO_2 in the atmosphere during this period. He also started the first experiments or measurements of CO_2 from SAS airplanes, which were continued by Bert Bolin. Together with Harry Wexler in the United States, he also created the firstb real observatory to study this question, the Mauna Loa Observatory in Hawaii in 1957.

This all has great interest because later on, at the end of the century, it has became so important for the development of climatology.

Cartwright: This concludes Dr. Wallén's additional comments, which we feel are so important to the overall appreciation of his background in this very important field. [Comments to transcriptionist.] This is Cartwright concluding this interview on October 29 [1996].

TEXT FROM ORIGINAL TAPE:

Cartwright: This is the beginning of Tape [2] of the interview with Dr. C. C. Wallén, being held at WMO headquarters in Geneva by Gordon Cartwright. It is now December 5 [1995], and the interview will continue...with Dr. Wallén continuing the discussion of the various connections he visualizes between events of the old Bergen School and the developments which followed through the decades later. I'll ask Dr. Wallén to take over from here.

Wallén: In this historic review, we should mention that the development of observations and methodology for forecasting weather reached its highest point in the early 1960's, when the U.N. General Assembly approved the WMO Programme for establishing a Global Network of Observation Stations. It was called the World Weather Watch and the research program called the Global Atmospheric Research Program (GARP) was at the time also endorsed by the General Assembly. This was a big event; as I remember, it was initiated basically by Harry Wexler, and it was he who during the Kennedy administration carried this resolution through the U.N.

About the same time, in the mid-1960's, the numerical forecasting methodology had reached the operational stage in most developed countries. A basic reason also for the U.N. approval of the WMO Programme seems to have been the fact that the United States had launched its first satellites in 1960, and the satellites used for weather forecasting were coming into operation at this time. They have played a fundamental role in the development of modern weather forecasting.

In this decade--1960--instruments like the weather radar and various types of automatic stations also started to be used operationally. Of course, the development of the weather radar took place gradually during the years after the War, from 1945 to 1960, but the available instruments, weather radar instruments, seems to have become available around 1965. They have played a particular role in the development of what we now call "now"-casting, i.e., forecasting for the next six hours, especially used in forecasting for aviation.

Another subject area of great importance for the future development was studies of atmospheric pollution, particularly in cities, where it first became a problem. The theoretical development of dispersion of pollution--that means then, dispersion immediately from industries and cars, is basically connected, especially with two British scientists, Sir Graham Sutton and Dr. James Pasquale. In addition to these two, in the more practical sense, the German scientist Professor Kris Yunger was the first one to establish a network of stations to observe chemistry and precipitation. This was established in the Eastern United States and was running in the 1950's and 1960's. I am sure that the instigation of this network came from Carl Rossby's studies of atmospheric chemistry in Stockholm during the early 1

{This section is extremely broken up by static. (Transcriptionist)}

In the same decade of the 1960's, a very important development took place in the theoretical field when the theoretician, Edward Lorenz, made his first theoretical studies of the non-linearity and chaotic type of various disturbances in the atmosphere, both at the small-scale as the turbulence or at the large-scale as the climatology. His studies have been of fundamental importance to the mathematical treatment of these processes so that the approach launched by Lorenz gradually developed into a new kind of mathematics, the so-called "chaotic mathematics." We have not seen the final applications of this kind of mathematics yet, but it will obviously be very interesting to see to what extent it can help us in understanding better turbulence and similar phenomena in the meteorological field. 950's.

Another subject area of great importance for the future development was the studies of atmospheric pollution which became common in the 1950's and 1960's, particularly in cities, where pollution first became a problem. The mathematical development of the dispersion of pollution, i.e., pollution from industries and cars, is basically connected with two British scientists, Sir Graham Sutton and Dr. James Pasquale. In addition to these two, and in the more practical sense, the German scientist, Professor Kris Yunge, was the first to establish a network of stations to observe the chemistry of precipitation. This was established in the Eastern United States and was running in the 1950's and 1960's. I am sure that the instigation of this network came from Carl Rossby's studies of atmospheric chemistry in Stockholm during the early 1950's.

At the national level, the air pollution problem became more and more severe, and it led to development of urban models in many countries. These were developed in order to make a possible mathematical study of how to locate industries in new planning of cities so as to mitigate their having an impact on the population of the city. The whole science of modeling cities developed during this period.

Also in WMO, one started to understand that more efficient studies in this field at the global level would have to be undertaken. The atmospheric science working group on atmospheric chemistry in March 1969 therefore decided, under the leadership of Bob McCormack from the United States, to launch a network of stations whereby a small number, ten to fifteen, baseline stations would regularly observe CO_2, and a larger number--150 regional stations--there would be continued measurements of precipitation chemistry and the turbidity of the atmosphere.

The idea of launching such a network was approved by the Executive Committee in 1969, and by the WMO Congress in 1971. This network later was named BAPMON--Background Air Pollution Monitoring Network, is still in operation, within the context of the GAW Programme of WMO.

In the early 1970's, the whole approach of meteorology to its applications took a new turn. The pollution problem, both in the atmosphere, the oceans, in fresh waters and on land had now started to become a global problem. The then-Swedish ambassador to the United Nations, Mr. Åström, on his government's instigation, invited in 1968 the world's governments and the United Nations to organize a Conference on the Human Environment in Stockholm in the summer of 1972. The U.N. accepted this invitation and started preparations in 1969. Dr. Davies, then Secretary-General of WMO, was charged with the chairmanship of the preparing work within the ACC of the U.N., and in 1970, Mr. Maurice Strong, from Canada, was made the Secretary-General for the Conference. The Conference was held in June, 1972, in Stockholm and became a great success, as the first one in a long line of similar U.N. global conferences. The Conference and the document emanating from it is still the best overall compendium on the environmental problem.

[At this point, the tape is blank.]

As to meteorology, the report from the Stockholm Conference does not only refer to various pollution problems, but also foresees the possibility of a climate change due to changes in the chemical composition of the atmosphere. It also mentions the problem of the ozone layer, because it was considered at that time that the supersonic aircraft ["SST"], of which the first one had been constructed--might in the future cause problems for the ozone layer, due to the chemicals that are exhausted from those kinds of aircraft. In fact during the 60's, there had been quite a bit of theoretical studies made on the problem of the ozone layer, basically by Dr. Paul Crutzen, who had had his earlier education at the Stockholm Institute of Meteorology and with Carl Rossby. Crutzen was a Dutch scientist who had joined the Rossby group of atmospheric chemical scientists in the late 50's. In the 1950's and 1960's, he laid the foundation for the understanding of what problems could be the causes for a diminishing of the ozone layer. Only a few years after the Stockholm Conference, in 1974, a famous article was published by Roland and Molina, an American chemist and his Mexican cooperative, being themselves interested in studying the problems of the ozone layer, concluded from various theoretical studies, that the chemicals that might be of danger to the ozone layer were not so much the ones coming from the supersonic aircraft, but rather those which at that time were emitted by anthropogenic means frequently into the atmosphere, namely the so-called chlorofluorocarbons, the CFC's. They are called in the industry, the freons. The freons were used in the industry basically in refrigerators and devices for extinguishing fires. But they had all sorts of applications and therefore were emitted by all developed countries into the atmosphere.

The article by Roland and Molina showed that these in the long run could become a real danger to the ozone layer. This is the article with its theoretical __________ by Crutzen that in this year of 1995 has been awarded the Nobel Prize for 1995, as the forecast of 1974 has proven completely correct in the sense that today, twenty years later, there exists a gradual diminishing of the ozone layer. It is now accepted that the chlorofluorocarbons are the cause of this danger to the ozone layer.

UNEP, the body that was created after the Stockholm Conference to look after the environment within the U.N. system, took immediate action on the article by Roland and Molina in 1974, and called a conference in Washington in 1977, decided to create a committee of experts which followed the development of the ozone layer for fifteen years until it was proven that diminishing took place. Various conventions and protocols on the protection of the ozone layer were finally adopted by the world's governments. Today the emission of the chlorofluorocarbons in the atmosphere is forbidden, according to an agreement of all countries of the world, in a protocol, agreed to in Montreal in 1989; this protocol is attached to the Vienna Convention on the Ozone Layer, which was approved in 1984.

WMO has in addition to what UNEP did, over the last few years increased considerably its monitoring observations of the ozone layer and plays nowadays a very important role in finding out how the diminishing of the ozone layer is taking place.

It is not surprising that the development of meteorology took a new turn with this strong impact from the Conference on the Environment. In addition to those problems which I have mentioned above, the Conference also referred to another one that has been of great importance and a disturbing problem in large parts of the world, namely the desertification problem. This has finally in 1992 led to a the world's governments adopting a convention among countries to follow and try to mitigate the desertification that is going on in various regions of the world.

WMO started already in the 1970's to concentrate more than before on climate problems. An Executive Committee panel on Climate Change was established in 1975, with Dr. Bill Gibbs from Australia as Chairman, and CCL, under the chairmanship of Helmut Landsberg, from 1973, re-oriented its effort towards environmental problems related to climate. About the same time, in 1974, the Global Atmospheric Research Program had a meeting in Stockholm in order to agree on which problems related to climate that should be of main interest to this program in the next few years. In fact the meeting discussed the fundamental question to change the classical approach to climate studies from the statistical one towards a more physically-oriented one. In fact in the Stockholm Conference on the Physics of Climate in 1974, the numerical forecasting modelers who had worked for about ten years or more on modeling the general circulation of the atmosphere were now interested in trying to apply similar mathematical approaches to the global circulation of the atmosphere and to other aspects of understanding of the future climate. It would then be possible to clarify what could be expected to happen on the globe, if the increase of the carbon dioxide from human emissions from burning fossil fuel would continue without change.

The Global Atmospheric Research Program, when it had been accepted by the U.N. in 1962, included a proposal for a program divided into two parts: one on the experiment to improve the weather forecasting on the basis of increased observations around the globe. This experiment, proposed for about ten years by Bo Döös in WMO, had in 1974, reached a stage where it could be expected to take place within the next five years. For that reason, Dr. Bert Bolin, who was in charge of the Global Atmospheric Research Program, thought that it was timely to start with the second part of the GARP program, namely the climate part. This was the basic reason why the Conference in Stockholm in 1974 was called and the physical foundations of climate were established.

The climate change continued to be of great interest during the latter part of the 1970's. Indeed this was a period when one didn't exactly know if climates over the globe were going to be colder or if it was going to be warmer. There were journalists who published books and papers on the coming of a new ice age at the same time as papers came out which showed that the future would become warmer. In fact, I think that the latter idea has prevailed, and we are now seeing the continual increase of the CO_2 in the atmosphere causing a problem that could mean a warming of the mean temperatures of the globe with between 2°- 3½° over the next century. The WMO committee on climate change took note of all these developments and issued every other year or so, from 1975, a statement about the situation on the problem of climate change.

The committee also was the instigator of an idea that gradually became an appeal to all of WMO and all its governments, namely that a Conference on World Climate should be called and organized by WMO.

So the First World Climate Conference was called to meet in Geneva in the headquarters of WMO in 1979. Indeed, this conference became a great success and as a consequence of this conference, the so-called World Climate Program was launched, which since 1980-81, has occupied a special department of WMO where the coordination of all efforts regarding climate, both the scientific ones in the application and monitoring aspects of it, have taken place.

It's interesting to note that this pre-research effort within the Global Atmospheric Research Program, from 1962-1979, and later on, the so-called World Climate Research Program: WCRP, the research part of the World Climate Program; in both cases work was not a single WMO effort. In this case, the scientific community, i.e., ICSU, joined the WMO to look after the carrying out of the research activities as WMO alone as a coordinating secretariat was not able to carry out the research; it had to be done by the scientific community. Hence WMO took the initiative, already at the GARP early stage in the 60's, to join with ICSU for the implementation of the activities that were headed both in the GARP and later on in the World Climate Research Program.

This initiative of joining between the administrative, coordinating functions of WMO among all the governmental meteorological services around the world on the one side, and the scientific community in ICSU on the other, has proven to be an extremely successful cooperation that indeed has become a model for other activities of a similar nature.

Before we go further into the development of the climate change issue and the assessments that have taken place over the years in that area, I should like to mention in more detail about how the classical climatology gradually developed into this new, should we say, more physically-based climatology. As early as in 1963, UNESCO took an initiative to call a conference in UNESCO, in Paris, to discuss the general problem of climate change. A problem that had been of great importance to myself during my early work at the University of Stockholm. With Professor Ahlmann, I had become involved in his scientific work on the "climate improvement" that took place from the early part of this century up to the end of the 1940's in the Northern Hemisphere. This climate change was the basis for the discussions that took place in the conference in 1963 in Paris. This shows that already at that time, with the classical treatment of climate in the more statistical way, we were concerned with the climate change problem. Indeed, it is interesting to note that the studies of the climate change from the early part of the century to the mid-century, never were successful in the sense that they clarified the causes of that change. I think there is one theory about the causes for that climatic change that is more relevant than all others, namely the one launched by Hubert Lamb, the famous climatologist of this century, who has made a fundamental contribution to historic climatology in general. He has maintained that the reason why the climate warmed in the Northern Hemisphere in the period from 1900-1950 was basically that the volcanic activity in the world was at a minimum during that period, i.e., lower than both before and after this period. It seems to me that this is probably the most likely explanation for this period.

In addition to Hubert Lamb's contribution to classical climatology, which is collected in his big books on this matter which were published in the 1970's, there has been at least one person who has played a fundamental role in the development of climatology in this century, namely Helmut Landsberg. As I mentioned, he was the ________________ CCL, in 1973, who turned the activities of that commission on climatology basically in the direction of the environment from that time on. He was originally from Frankfurt in Germany. I think he left Germany already in the late 30's, and came to the USA where he continued his studies. At the time, when I saw him for the first time, he was an advisor at the Pentagon on climate around the world. I came to see him on the advice of Carl Rossby, who said, "You must go and see Helmut Landsberg because he has written an important statistical study of the climate on the Hawaiian Islands that you may need for your studies of the precipitation there." So I met him for the first time at the Pentagon in 1947 and it was the only time I ever visited the Pentagon. Indeed, he had a bit of a German style, so that anybody who remembered him from his lifetime may agree that he looked a bit like the famous film star from the beginning of this century, Erich von Stroheim. [*Do I include this sentence: "Indeed this German formality made it clear that during our final meeting [we] did not find each other on a personal basis"??*--transcriber]

He contributed in climatology to the understanding of the need for applications of climatology to various aspects in the modern society. His main contribution to the science of climatology--indeed he added contributions to all aspects of climatology--but he thoroughly involved himself particularly in studies of urban climatology.

He became president of the Commission for Climatology in 1969, and then carried on to 1977 and this period, as I indicated earlier, became a very important period for the Commission for Climatology and should we say also, for the understanding of the use worldwide of modern telecommunications in climatology. Indeed, this had started already during the 60's when the use of namely the ones with punch cards, were applied to collect and to organize the climatological data that were available in various meteorological services around the world. It became quite normal that this change from handwork to computer work in the 1960's took place in all the developed countries. In developing countries and in developed countries in a new sense, the introduction of newer telecommunications and computers took place in the 1970's. All this, of course, was based on the old concept of climate, which was mostly treated by statistical means. I know that the Anglo-Saxons, particularly the English, at that time considered climate only as "clerk work" in the meteorological service, which meant the adding up of thirty values and divide by thirty. They didn't accept that climatology was anything else. In fact, if you go back historically to the early part of the 19th century, Alexander von Humboldt, the German who was the creator of modern climatology, had a completely different approach to it. He felt that the climate was built up by impact from both the atmosphere, the land and the ocean, and as such, it includes understanding of the whole global climate system. We now understand the global climate in this sense and the observation system for climate, which is now being created, is based on this modern thinking.

Now, I've always been surprised by the differences in approach to climatology in the old Russia, in the latter part of the last century, represented by Professor ?______. The approach in Germany, represented by other famous climatologists of those days, particularly Helmut Landsberg as well as the similar approach in France, represented by a geographers like de Martonne, for example. In the British approach, there was absolutely no understanding for this way of understanding climatology. This has brought about a conflict in the area of climatology which has been noticeable throughout the century. When we come to the next fifteen years, which we will discuss later in this interview, we will see that the change towards the physical approach to climatology has changed this situation, so that we are now together in understanding the science of climatology.

END OF TAPE 2, SIDE 1

Interview with C. C. Wallén

TAPE 2, SIDE 2

Cartwright: ...December, 1995, and we are beginning the tape interview with Dr. C. C. Wallén on Tape #3 [marked Tape #2]. This recording is being made in Sir Arthur Davis Hall, the largest conference hall in the WMO headquarters.

C. C., I understand you have been collecting your thoughts and you are prepared now to continue the interview that we started a couple of weeks ago. Would you begin now?

Wallén: Thank you, Gordon. I would like here first to come back for awhile to those problems that I discussed in the last tape. That is, the transition, from pure meteorology into climatology becoming a main subject within meteorology of the last decades. I referred to, in the last tape, the conference in Stockholm in 1974 that I think is basically the turning point from, should we say, the great interest that was shown in the earlier part of the century in synoptic meteorology to rather a coordination of synoptic meteorology and climatology, in the sense that climatology in its new form differed from its classical form and was based on a similar thinking as synoptic meteorology, namely the physics and the hydrodynamics of the atmosphere. This has led to a modern modeling approach of the atmosphere and, in particular, to general circulation of the atmosphere, which has been a basis for the further developments in climatology over the years from that time.

As I said, I remember that one of the reasons we jokingly said for this transfer was the fact that the modelers of numerical forecasting had more or less exhausted this field, so that they had to find another area where they could continue their modeling activities. And they choose for that purpose the general circulation of the atmosphere and climatology. This, of course, has a joke, but on the other hand, there is some truth in it; I'm sure some of those modelers would agree to that.

The possibility of simulating climate and the general circulation of the atmosphere by running a model several times had been investigated by the Princeton group on numerical forecasting under Smagorinsky during the late 1960's. Dr. Manabe was perhaps the person who pursued this idea with most success, and at the conference in 1974, it was agreed that this approach should be continued to the great satisfaction of the modelers, who got a field of general circulation modeling for the climate purpose as a new field of research. Numerical forecasting started to become, should we say, so operational that it was not any longer necessary to continue research for all people who were interested in it.

By the time of the first World Climate Conference in 1979, the time was right for meteorology to enter seriously the field of climatology as an environmentally oriented part of meteorology. At that time, it became clear that climate was a natural resource that needs to be sustained and therefore the climate system of the globe needs better understanding. The main conclusion of the conference was the creation of the World Climate Program, in which WMO and UNEP jointly participate in its implementation from 1980.

It could be added here that ICSU was involved, as it has always been, in the WCRP (World Climate Research Program), the part of the World Climate Program that deals with research. Parallel with the need for a better understanding of climate in general, the problem of a possible climate change due to the ongoing increase of CO_2 in the atmosphere, grew more and more urgent. The first assessment of the scientific knowledge about the potential for a climate change, due to the increase of carbon dioxide in the atmosphere caused by the burning of fossil fuels, was made in a joint expert meeting called by WMO, UNEP and ICSU in Latenburg outside of Vienna in 1980. This meeting was chaired by Bert Bolin, who has been in charge of assessment for the world in this field ever since. At that time, it was concluded that although the uncertainty was great, models so far indicated that by the mid-next century, the mean temperature of the globe could have increased by 2°- 5° Celsius.

After a rather slow start, the World Climate Program got off the ground, and WMO started research as well as collection of climatological data and on climatological applications. In UNEP, studies started on the impact of climate on socioeconomic structures, after an agreement of the Governing Council of UNEP to a suggestion from WMO that UNEP take over this particular part of the World Climate Program, because they were considered more competent in studies of the impact on socioeconomics than WMO.

On the problem of climate change, UNEP in 1982 took a further initiative by hiring the Stockholm International Meteorological Institute under Bert Bolin to undertake by 1988 a more thorough assessment of the scientific basis for a possible climate change due to anthropogenic activities.

WMO soon thereafter joined this exercise as a co-sponsor. This assessment went on in parallel with the development of the World Climate Program under Dr. Tom Potter from the United States during the years 1983-85. I'd like to add here that under Tom Potter, the Climate Program made very definite progress and developed into something that was now clearly stabilized as an undertaking by WMO. It started to become well-known around the world, not only as a program, but also with results both in the research area and particularly in the application area. In this area, it led to the development of a system for the developing countries to be able to gather and archive their data in on computer. It has been useful in the sense that data has been possible to use for applications in countries where it earlier was not looked after. This communication system has come to be known under the abbreviation, CLICOM.

In 1985, the first thorough assessment of the climate change problem was published and led to an expert conference, which was held in Villach, Austria, organized by UNEP, WMO and ICSU. About sixty experts from around the world were present, and the conference was chaired by Dr. Jim Bruce from Canada. The conference concluded that in the year 2050 the mean temperature of the world could have risen by 1½° - 4½° Celsius, due not only to an increase in the carbon dioxide in the atmosphere, but also to the increase of other greenhouse gases like the CFC's, methane, and nitric oxide. This last finding of this assessment was reasonably new. It was new that these other greenhouse gases were also increasing in the atmosphere. In some cases they increased even more actively than the CO_2, and therefore they may have a rather important role in the possible increase of temperature.

It was also concluded that although the uncertainty was big, such a temperature increase of an order of magnitude of perhaps 3° could lead to serious interruptions of socio-economic circumstances in large parts of the world, if this would really materialize by the year 2050.

In order to study these socioeconomic consequences a little bit in more detail, UNEP and WMO and ICSU in 1987 called for a new expert conference which was held under the auspices of the Rockefeller Foundation in Bellagio, Italy, which was attended for the first time by experts in socioeconomics and governmental decision makers. The conference in Bellagio on the impact of a possible worldwide climatic change on the socioeconomics of the world's structures concluded that sooner or later the situation might call for a creation of a more political and governmental body to continue the assessment procedure as the scientific progress of the problem went on. Although there was at this time no overall agreement when this should happen, WMO in Congress and EC (Executive Council) in 1987 decided that a joint governmental body should be formed by WMO and UNEP, which should continue to undertake regular assessments of the scientific progress on the climate change issue. UNEP's Governing Council in the same year agreed with the WMO proposal, and so the Intergovernmental Panel on Climate Change was formed in 1988. It is usually called IPCC. In this panel, all governments were invited to participate.

The first meeting of the panel was held in Geneva in late 1988, and Professor Bert Bolin was elected chairman of IPCC. As an indication of how serious the problem of possible climate change only within a few years has become, it is worth mentioning that two important big conferences in the world were held during these years. One took place in Toronto, Canada, in 1988, and one in Holland in 1989. Both focused on the serious consequences of a global climate change, and drew great interest from mass media.

In 1990, IPCC delivered its first assessment at a meeting in Sweden, in the small town of Sundsvall. This assessment mainly confirmed the dangers inherent in a continuous increase in emissions of greenhouse gases and called for global reductions of these emissions. The value for an expected increase of global mean temperature was still 1.5° - 4.5° Celsius.

In the following two years, the IPCC continued to follow the progress of scientific development and in 1992, added additional assessment in which an important contribution was the impact of the aerosols in the atmosphere, which reduced the likely values of warming of the mean temperature to something between 2½° - 3½°. It's an interesting issue, the one with the aerosols, because as earlier in the beginning of the 1970's, when the issue for the first time was considered, the question about the aerosols was very much discussed, and various scientists argued that they must have an impact on the warming, which would then go in the contrary direction from the impact of carbon dioxide.

Talking about aerosols, we are mainly concerned with the aerosols that are emitted into the atmosphere through the burning of fossil fuels, which are mostly from sulfur or nitrogen. And it is now clear that at least over the European continent, and also over the U.S./North America, they had become a problem which is known as the problem of acid deposition, which has influence on both freshwaters and vegetation.

This new additional assessment that was made by IPCC during 1992 was quite useful for being presented in the big United Nations Conference on the Environment and Development that took place in Rio de Janeiro in 1992. This conference was a conference that was called by the UN, partially to celebrate the 20 years after Stockholm 1972, since the UN Conference twenty years earlier. This conference had in fact been more carefully prepared than the Stockholm Conference in its time because at this time one knew better how to prepare oneself. The same person who was Secretary-General of the Stockholm conference, Mr. Maurice Strong, who was also the first Executive Director of the United Nations Environment Programme, was asked to become Secretary-General of the conference in Rio de Janeiro also. He set up a preparatory secretariat, in Geneva, which worked for about two years in preparation of the conference. The conference indeed became a success in the sense that the whole area of the environment was looked upon from future economic aspects. The difference between this and the Stockholm Conference was indeed that a basic problem was how the developing world would be able to look after the environment in their territories at the same time as they would achieve the economic development they wanted. In an earlier context, which became useful for this conference also, a special expression had been created for dealing with this subject of how to meet the two requirements, looking after the environment and achieving development. The expression for this way of approaching the future has been called "sustainable development," an expression that had been created by a commission on environment and development that worked during the 80's under the chairmanship of the Norwegian prime minister, Dr. GroHarlem Brundtland. ??

So the result of the Rio conference was basically the sustainable development of the future of the world. The conclusions were gathered together in something that has been called the Agenda 21, meaning an Agenda for the 21st century. It aims at looking at all aspects of the environment, from the sustainable development point of view.

In order to look after the future of sustainable development of the world, the Rio conference created in the United Nations a Commission for Sustainable Development under the Economic and Social Council of the United Nations, which today works in cooperation with the United Nations Environment Programme. It has the capacity of being a watchdog for sustainable development.

Another achievement that is particularly relevant to this interview was the approval in the Rio conference of the Governmental framework, Convention for Climate Change. This convention had been prepared very carefully before the Rio conference. It has not yet any protocols which ask for particular actions, but it is a framework convention which basically tells countries and governments to coordinate their activities toward the aim of meeting the _______________ problems of climate change. It is the intention that gradually protocols will be added to this convention. It has a request of governments already at this stage as it asks the governments, if possible, to mitigate or reduce their emissions of fossil fuels down to the level of 1990. In fact, the achievement on this point has not been too impressive over the years since 1990. There are some countries which have really decided to reduce emissions down to that level, but among the big countries, no real decisions on this point have so far been taken.

As far as IPCC is concerned, it is now coming close to its next assessment, which will be published in the same days as we are making this interview. This will be the 1995 assessment of the situation with the global climate change issue. It has not been approved by a meeting of the IPCC yet, but it will certainly be and the main results of the 1995 assessment is now that the aerosol problem has been more thoroughly studied and also confirmed. The latest values for a likely temperature increase to be expected until the mid-next century is now estimated to be by, let's say, 1½° - 3½°.

An interior secretariat for the convention has existed for several years and has now become a permanent secretariat. It was earlier located here in Geneva. However, the parties of the convention met in the early part of this year, in Berlin, and decided that the future location of the permanent secretariat for the convention will be in Bonn, Germany. This was perhaps a blow to the city of Geneva and perhaps also to a certain extent to WMO; where one had hoped very much to get the secretariat. Which could _______ the World Climate Program. The reason why this did not happen was basically because Germany was extremely keen on getting the U.N. body and therefore lobbied very strongly in the meeting of the parties to have the secretariat in Germany. So it became located in Bonn. I think it will certainly be supported very strongly by Germany, when it comes over there. This could have great implications to the future of the whole of climatology because it's unlikely that the Convention will develop a scientific and technical arm within the secretariat which will work in support of the convention over the coming years. In that way it could become involved in the scientific aspect of the global climate change issue.

There is another recent development in addition to what I have mentioned, which I would like to mention here, namely, the recent development in the area of the World Climate Program of the WMO. We recall that the World Climate Program of the WMO was created after the first World Climate Conference, which was held at WMO in 1979. At the Second World Climate Conference, it was decided that the climate program should deal with four areas: one on the World Climate Research Program, a second area on Data and Monitoring needed for climate development, the third area being the Applications of Climate Data, and the fourth area being the Impact of climate and climate variability, on socioeconomic structures of various regions of the world.

Over the last five years, however, there has been a recent development of the WCP. The WMO Congress in 1991 decided in view of decisions from the second World Climate Conference, which had very clearly that climate is an area where many of the UN agencies are involved in various ways and therefore a really global climate program should indeed involve those other agencies that have their special aspects on climate, and are carrying out programs in these areas. Therefore, the WMO Congress in 1991 decided that the matter of the whole problem of expanding the World Climate Program to the other agencies, and to a global approach should be discussed in an inter-governmental meeting, which took place in Geneva in 1993. In that meeting, it became clear that many environmental aspects of climate in general had to be taken into consideration, and needed to become __________________ of a future World Climate Program.

Therefore, the conclusion was that the World Climate Program should be expanded through a new inter-agency effort that would look at it from the point of view of integrating all of these activities that go on related to climate within various agencies. This has led to the fact that in early 1995, the so-called Climate Agenda has been approved by a coordinating committee chaired by WMO and involving the other agencies, namely, UNEP, FAO, UNESCO, IOC and WHO.

So the Climate Agenda has been approved by the coordinating committee in

WMO in the early part of this year. It has now been submitted for approval by the WMO Congress, by the IOC steering body, by FAO and the other organizations. It has been approved by at least three or four of these and it will now go into further development and at a certain moment, I hope it will be really going into a more implementation stage.

[At this point, there is another lapse in the recording of the tape.]

END OF TAPE 2, SIDE 2

Interview with C. C. Wallén

TAPE 3, SIDE 1

Cartwright: This is the beginning of the third tape in the interview with Dr. C. C. Wallén in Geneva...It is now December 19, 1995, and Dr. Wallén expects that his concluding remarks will be on this side of the tape.

Wallén: Thankyou, Gordon, yes, I would like to make some concluding remarks to this interview. The amazing thing is that this interview was intended to be basically a discussion of the relationship between USA and Scandinavian meteorologists and the impact of them in the U.S. It deals with this in the beginning, but it has developed into a review of the historical development of meteorology over the past century. I should like to emphasize the linkages that I have mentioned throughout this interview, and which indeed show a logical development over the century, taking into account not only what has been the special impact of what has happened in meteorological sciences, but also the impact of external developments in the world's political aspects.

Let's try now to summarize these developments and we will see how they are linked together over the century.

[The tape becomes unclear due to static at this point].

Obviously, time was ripe at the beginning of the century to develop the more scientifically based methodology than existed at that time for weather forecasting; in particular, with the special development of aviation that took place in the first half of the century. The Bergen School under Vilhelm Bjerknes and a number of other very famous meteorologists and personalities provided the Bergen School technology, which became used all over the world as quite distinct from the earlier used isobaric geometry technique.

The transition from the Bergen School to the Chicago School followed logically upon the invention of the radiosonde and brought about a new era of upper air meteorology, called "aerology." The Chicago School provided not only the scientific basis for understanding the upper air circulation, but contributed much to improved knowledge of how the surface cyclones and the upper air circulation are linked with each other and with the general circulation of the atmosphere.

This improved understanding of the dynamics of the atmospheric circulation, led together with the invention of the modern electronic computer to a new methodology for weather forecasting, called numerical weather forecasting, which indeed started to develop around 1950, and in fact was used in most developed countries from around 1970.

A second branch of the Chicago School was developed in the International Meteorological Institute in Stockholm when Carl Rossby came back to Sweden around 1950. He started, in addition to his contribution to ____________ forecasting, a new era in looking at the chemical composition of the atmosphere. The first developments in this area were caused by research interests of Carl Rossby. But in a few years, ten or so, the atmospheric chemistry became very important from a practical point of view of as we found it necessary to understand how man may incur a change in the composition of the atmosphere, and thereby introducing the possibility of a climate change. Through the birth of the concern about the human environment in general, which took place in Stockholm in 1972, meteorology came to deal with such practical problems as air pollution in the atmosphere, the depletion of the ozone layer, and the possible long-term changes in climate due to increasing emission of greenhouse gases by man. In all these three cases, it is the change of the composition of the atmosphere due to anthropogenic influences that are the basic causes. So, in this way, atmospheric chemistry came to become an important branch of meteorology.

The change of emphasis from weather to the environment and climate is therefore to be seen as a logical development, directed both by the impact of those persons who saw the need for this change and due also to the impact of the changes in the world's political and scientific approach to the environmental field. Coming to the end of this large-scale development, I wish to emphasize that my having tried to find out how these logical developments over the century have come about does not in any way diminish the importance of those contributions to meteorology and its applications which have been provided by the many single research workers and applied scientists who have throughout the century carried the main burden of the development of meteorology and its applications.

Let me finally end with pointing out that this interview in no way attempts to be an objective historical account. On the contrary, this is the way I have personally understood the evolution from what I have seen over a long life in meteorology. I must therefore apologize for a number of inconsistencies and probable misunderstandings that may appear within this interview.

Thankyou.

Cartwright: Yes, indeed... [Tape is quite staticky at this point.]

I have found this extremely educational and interesting
intimately involved in many of these developments...

When we first started, it was from a focus, as you pointed out, on the individuals who led to the development and expansion of the early ...

I think the additional comments you have made...in a very good relationship with

the overall development of meteorology...
I regret to say that this interview is now concluded, and I will send the tape to...

Thankyou, again...

ADDENDUM:

Cartwright: It is now the 10th of January (1995) and in reviewing and re-recording the tapes, we discovered a gap of about four minutes. Working together, we have not been able to establish exactly what has caused that gap or what the contents of the gap should have been. Dr. Wallén recalls that it was a discussion of the developments following the Stockholm Conference for Atmospheric Monitoring of the Chemistry of the Atmosphere, and he would like to add a few comments about how that came about, and these can be interspersed or introduced into the tape at some appropriate point after the transcript has been made. So I'll ask Dr. Wallén to, as far as he recalls those comments, to add them here at the end of the tape.

Wallén: Yes, I think that, in discussing the content of the report of the Stockholm Conference, I, in this gap, referred to the part of the report that deals with the air pollution problem. In this context, I also wanted to point out that WMO had been aware of these air pollution problems already and started to make monitors ________________________.

The report of the Stockholm Conference in 1972 refers to the need for monitoring of the atmosphere at certain baseline stations. A number of ten baseline stations are being set up around the globe, and in addition, there would be about 150 regional stations to follow the development of the background air pollution. This is all mentioned in the report and should we say, also requested to be done by WMO. Indeed, the interesting thing is that already two years, three years earlier than the Conference, WMO had indeed established a network for this purpose which has been called the Background Air Pollution Monitoring network, abbreviated BAPMON, and indeed it had exactly the basic ideas that are referred to in the book; they are already in the basic definitions in this network.

C. C. Wallén
Addendum to Interview

Cartwright: This is October 6, 1996, and we are continuing on this side of the tape the final remarks of Dr. C. C. Wallén in connection with his AMS interview being held in Geneva at the WMO headquarters by Mr. [Gordon] Cartwright.

As was noted at the beginning of the other side of this tape, this particular recording is being made to replace a part on the earlier interview that turned out to blank or un-usable. Dr. Wallén is now filling out those parts that he feels were missing from that part of the tape.

I will now ask Dr. Wallén to continue with his recording.

Wallén: Thankyou, Gordon. I would like to fill in on this part of the tape a little more about Rossby in Stockholm, and his last days before he died.

When Carl-Gustaf Rossby in 1947 moved to Stockholm, he became official head of the Department of Meteorology at the Stockholm University, and also adviser to the Swedish Meteorological and Hydrological Institute. He was still professor at Chicago University, and visiting professor at Woods Hole. As if these many appointments were not enough, he started an international meteorological institute in Stockholm, where he could house his many international colleagues, who visited Stockholm, and those who worked on research for the many projects he still operated from the United States. In addition, several of his new students, joining him in Sweden, worked at the same time as students in the Department of Meteorology at the University, and at the International Meteorological Institute in Stockholm.

Carl-Gustaf Rossby was well-known for his ability to convince authorities for the need of further development of meteorology, and for funding. He seems to have been able to use this ability in convincing the then-young and enthusiastic prime minister of Sweden, Carl-Ger Langer, to give the necessary funding for the IMI. During the few years that the IMI operated under Rossby, from 1948 to 1957, at least sixteen of his students from the USA and Sweden spent long or short periods in Stockholm. His first student who got the doctors' degree in Sweden in 1950 was the Navy commander, Dan Rex, who had already joined Rossby in Chicago in 1947. Among the others who visited Stockholm in these years were, from Germany, Professor Sherhart, Professor Av_____, and Professor Flöhn, [as well as] Professor Delfant. From England came Dr. Eady, Dr. Mason. From the USA came Dr. [Harry] Wexler, Dr. [Jerome] Namias, Dr. [Wendell] Morty, and many more. More and more than anybody else, Professor Palmén came from Finland.

An important activity of the International Meteorological Institute was the editorial work of **Tellus**, a new journal that Rossby had started with the agreement of the Swedish Geophysical Society in Stockholm. Rossby asked me to become executive director of this new journal, which I was from 1948 to 1953, when Bert Bolin took over.

In these years, Rossby was very often in the U.S., as he was still, as I said, professor at Chicago, and sitting professor in Woods Hole. With this kind of workload, together with the Swedish one, he certainly worked himself out much too early. He had an heart attack as early as 1949, was told to take it much more easily, regarding working, drinking and smoking. He tried for a couple of months, but then all went back to normal. By 1955-56, we all noted that he became more tired and showed a certain tendency to slow down. He was ill for a period in 1956, and took it more easily for some time. What was not commonly known was that at this time he started to feel that his responsibilities in Stockholm were finished, and he started to look forward to other duties. It was found in one of his last letters, that was addressed to UNESCO, and revealed that he had for some time discussed with UNESCO a project to start a training institute in meteorology for developing countries in Beirut. So his planning capability was unchanged.

However, he had already in his youth through a period of rheumatic fever had his heart weakened, and with the extraordinary pace of life that he had lived for more than thirty years, it is not surprising that his heart was not capable of continuing to work. He died very unexpectedly in his office on the afternoon of 19 August 1957, an irreparable loss to meteorological science and applications.

To me, it was a great shock. I got to know about it when I returned to Stockholm in the evening of the same day after having had a short vacation in Spain.

Carl Rossby was a person who was greatly admired by both his friends and colleagues all over the world. As John Lewis has so well pointed-out in his article in the **AMS Bulletin**, (September 1992), Carl Rossby was or became a typical example of a mentor to his many students and research workers, who he brought up during his active years, from 1939-1957. The study indicates--and here I would like to quote [from] the article: "That Rossby expected an unusually high degree of independence on the part of his protégés, but he was exceptional in his ability to engage the protégés on an intellectual basis and to scientifically excite them on issues of importance to him. In this way, he became an extremely highly-regarded teacher who also was extraordinary in his generosity to his friends and colleagues. One can definitely hear talk about a person who became a beloved teacher and mentor, which Rossby's name became connected with the Chicago School, and will always be a witness to his work.

I would like to quote a few words from Professor Tor Bergeron, which will illustrate the kind of person he was and how he planned his great achievements from the years 1939-1957.

> To the Bergen School, the polar front was still the central concept to which all its other entities were more or less attached. Intellectual at Stockholm in 1935, Rossby, on the other hand, positively declared that he offered another basic system for the middle latitude weather processes, where the upper westerly air flow was the fundamental entity and where one did not need the polar front. The rest of his system was at that time in many respects quite different from what it became some five years later. That the nucleus of his later truly Lagrangian study of the jetstream and its long waves was already there. Vilhelm Bjerknes, in his famous program of 1904, had outlined the goal of our science, which L. F. Richardson had attempted in 1910-1922--strictly to apply these principles to one trial case, but that failed. Only Rossby in 1938-40 would achieve this next much longed-for breakthrough thanks to his extraordinary combination of a good mathematical-mechanical education, an intense desire to do better than his forerunners, and his ingenious faculty of ruthless simplifying a problem until it becomes solvable, his practical sense in the world of thought. One may even say that the very fact that Rossby had failed to play a role in the Bergen School and in the first significant meteorological advance of this century goaded him into making the utmost of his great endowments and to found a new school built on methods that use his special gifts, thereby initiating a second great meteorological advance, which finally found its applications in numerical forecasting.

A note in this quotation from Tor Bergeron's article that while Carl-Gustaf Rossby was in Stockholm in 1935, the grade-A intellectual that he refers to in his article, I think I know that Rossby had been called home to Sweden to possibly become the director after my father, who had died earlier in 1935, and the directorship of the meteorological service in Sweden was therefore open. During his summer stay in Stockholm, he, however, found out that there was someone else who would succeed my father and he left, went back to the United States, where he was, at that time, a professor at MIT. I can not help noting here that it was, although I find it regrettable that he didn't become at the time director of the Swedish Meteorological Service, I must note that it was very, very good for the meteorological science that he did not, because if he had at that time come back to Sweden, he would never have had the opportunity to build the Chicago School with all its achievements for the development of meteorology.

My personal feelings for Carl were very deep indeed. When I came to the U.S. to study with him, he was clearly skeptical about my specialization in climatology, which he wanted to learn about, but did not know himself very well. When the fellowship that I had been promised at the University of Chicago by the Sweden-America Foundation did not materialize, Carl told me to come over to the States anyway and promised to help me out. He gave me a half-time job to study the precipitation conditions for pineapple cultivators in Hawaii, which helped me to survive in Chicago for a whole academic year. This showed exactly how should we say "informal" and "spontaneous" he was. He always worked out something when there were some difficulties around.

Another example of his generosity of--this was a small thing--was when I asked if he could loan me $30 for my trip back to Sweden after the end of that year, because I had exhausted my money completely, and I needed $30. I calculated that, because I had my ticket to go back on the ship so I needed only for extra things, $30. He said, "$30? What amount is that?! I write you a check for $50." He did, immediately, wrote me a check for $50. "But, for God's sake, don't cash this check before the end of the month because there will be nothing in my account before they put in my salary!"

It shows exactly how generous, how spontaneously generous, Carl was to his friends. It shows also how much difficulty he had with getting his salary to last until the end of the year, which I'm sure his wife and his children happened to note on several occasions.

Cartwright: This is the end of the interview, the additional tape by Dr. Wallén, which is intended to fill in some of the material that was lost in the noise and difficulty with one of the earlier tapes.

END OF INTERVIEW