

Interview with Erik Eriksson

Dr Taba recounts:

The international symposium, Rossby 100, was held in Stockholm, Sweden, 8-12 June 1998, to commemorate the 100th anniversary of the birth of Prof. Carl-Gustaf Rossby. It was organized by the Department of Meteorology, Stockholm University (MISU) and was sponsored by the International Meteorological Institute in Stockholm (IMI), the Sweden Geophysical Society and the Werner-Gren and Henrik Granholms foundations. Some 200 scientists and researchers took part. In his opening remarks, Hilding Sundquist, chairman of the organizing committee, said that although he had not had the privilege of meeting Rossby, he was acquainted with him through his work. Those who had known Rossby had painted for him the portrait of an enthusiastic, stimulating and generous scientist and human being. His engagement spanned a wide range of interests and problems.



Erik Eriksson

Regular readers will have noticed that the WMO Bulletin interviewees often speak of Rossby. As I have already mentioned in my autobiographical sketch which appears in the two volumes containing the Bulletin interviews, I was one of the persons closest to Rossby and the last to see him alive. I was happy therefore to have the opportunity to attend the Symposium and see so many friends and colleagues of the Rossby period. My main reason to be in Stockholm, however, was to interview Prof. Erik Eriksson, who also worked closely with Rossby. Indeed, in the early 1950s, when Rossby became interested in the study of large-scale atmospheric processes with chemistry as a tracer and the possibility of understanding the role of chemical substances in rain and snow formation, he found in Erik Eriksson the man to conduct this work. Since, to my knowledge, no interview has ever appeared with Rossby and for the benefit of those who wish to know him better, Rossby's life is briefly described in the following paragraphs.

Carl-Gustaf Rossby was born in 1898. In the spring of 1917 he passed his matriculation examination in Latin but turned to mathematical sciences. He acquired his bachelor's degree with astronomy, mathematics and mechanics as the main subjects in one year—a phenomenal achievement, since it usually took at least three years. On 20 June 1919, not yet of age and with no knowledge of meteorology, he joined the circle of the famous Vilhelm Bjerknes in Bergen, Norway. His first days there revealed that this 20-year old had an amazing faculty of persuasion and organization and far-reaching ideas. Being largely of a practical-technical nature, the work at Bergen did not satisfy Rossby's searching spirit. Bjerknes, however, was the ideal teacher of theoretical hydrodynamics, a field that later proved to take hold of Rossby's mind and interest.

In the summer of 1920, he went to Leipzig in Germany, and from 1922 and 1925 worked at the Swedish Meteorological and Hydrological Institute (SMHI) in Stockholm as a junior meteorologist. During those years, Rossby took part in some unusual adventures. These included a voyage in 1923 on the oceanographic vessel Conrad Holmboe through pack-ice off east Greenland and a summer cruise around the British Isles on the training ship Chapman in 1924.

His biggest move was a trip to the USA in 1926 on a one-year fellowship from the Swedish-American Foundation. One year stretched to eventually more than 20, during which he established important departments of meteorology at Chicago University and the Massachusetts Institute of Technology.

In 1946, Rossby was invited to Stockholm to advise on promoting meteorological research and training. In 1947, he accepted a personal chair in meteorology at the University of Stockholm and soon afterwards, the MISU came into existence, celebrating its 50th anniversary last year. Fundamental progress was made in the fields of numerical modelling and this became the central research activity at MISU. Another of Rossby's achievements during this time was the creation of the MI. Rossby initiated studies in several other related disciplines such as cloud physics, oceanography and atmospheric chemistry.

Readers will learn more about this latter field from this interview. Norman Phillips¹ has already given an interesting account of Rossby's life with emphasis on his early years when he was the leading meteorologist in the USA, and Bert Bolin² gave an account of Rossby's vision of meteorology and oceanography during his last years in Stockholm. Bolin's paper was presented at the opening session of the Rossby-100 Symposium. In his words: "few have had a more decisive influence on the development of meteorology in the world during the 20th century than Carl-Gustaf Rossby". During his years in Stockholm, Rossby attracted a multiplicity of visitors, including well-known meteorologists such as E. Palmén³, T. Bergeron, S. Pettersen, R. Fjortoft⁴, A. Eliassen⁵, J. Namias⁶, J. Mason⁷, H. Lettau, J. Malcus (Simpson)⁸ and K. Hinkelmann⁹ and brilliant young scientists such as N. Phillips and J. Charney. The US Navy and Air Force sent many meteorologists to Rossby for further training and research. Some of the many names which come to mind are A. Bedient, D. Rex, C. Palmer, E. Jess and N. Medrud.

During 1956, Rossby felt that he had stayed too long in Stockholm and that it was time to do something different. In 1957, he took me with him to Geneva to visit the WMO Secretariat. He and Arthur Davies¹⁰, Secretary-General, agreed to establish an international meteorological institute in Beirut, Lebanon. It was during a discussion with me in his office

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about this institute that Rossby died in 1957. His death was a great loss to his family, friends and colleagues and to the meteorological world as a whole.

In the following pages, Erik Eriksson gives an account of his life and of his acquaintance with Rossby. I met Erik at MISU in 1954 and we have remained in contact ever since. Erik is an extremely calm and kind person. He started his career as a farmer and reached the level of professor in all subjects relevant to nature and agriculture. It was an immense pleasure for me to see Erik once again to conduct this interview.

H.T. — Please start by telling readers where you were born and something about your family.

E.E. — I was born on 9 December 1917 in a small village, Bredkalen, in the northern part of Jamtland, 12 km from Strömsund. The village is situated on a hillside with a magnificent view of the mountains and lakes to the east and north. My parents owned a small farm with cows, pigs, horses, goats, sheep and chickens. It also comprised wetlands and a spruce forest. We were four children, of whom I was the only boy.

H.T. — What form did your early studies take?

E.E. — I attended primary school from the age of six to 12, herding our stock of cows and goats during the summer. After that, I worked on the farm in summer and in the woods in winter. In 1936/1937, however, I attended a six-month course at a farming school, preparing for my future profession as a farmer. I introduced some new practices to the farm and managed quite well but wanted to know more. One of the teachers urged me to take up studies at a special school with all the high school courses crammed into two years. In 1942, I was ready to enter academic territory and was accepted as a student at the Royal Agricultural College of Sweden. Because of military service in Sweden and World War II, I could not begin my studies until 1943. My life as a farmer ended there.

H.T. — Tell us something about the Royal Agricultural College of Sweden and your studies there.

E.E. — The Royal College of Agriculture of Sweden is located 5 km south of Uppsala and is often referred to as Ultuna. It was created by upgrading the former Institute of Agriculture to include pertinent academic studies. Some years ago, the College of Agriculture was combined with the School of Forestry and the School of Veterinary Sciences, which justified the present name of Swedish Agricultural University (SLU). It consequently has three faculties. Part of the Forest Faculty is found at Umeå University in the north. My studies followed a four-year course | specializing in soils and plants and my ambition was to enter the extension service as a senior advisor to farmers. My studies included agricultural chemistry, soil science, botany, some physics and mathematics, statistics, soil management, plant husbandry and hydrotechnics. There was a meteorological station and observations were taken at 07h00 and 19h00, usually carried out by a student with the status of assistant, who also helped the physics lecturer during laboratory exercises. I was offered this post for a year, together with free lodging at a house nearby.

H.T. — In 1946 you became research assistant at the Institute of Soil Science. How did that come about?

E.E. — In December 1945, I married my wife, Therese. At the same time, I won a scholarship for three months in Helsinki, Therese's home city, to study the effect of the weather on winter wheat (Finland had accumulated considerable experience in the field). My wife and I enjoyed the stay, despite an unusually cold winter and scarcity of good food. Everything was rationed and the monthly meat ration was consumed in three meals. Back in Uppsala, I had to earn my living. I was offered a part-time assistant's job by Prof. Hans Egner in the Agricultural Chemistry Department. I devised a method for analysing ammonia in rain, which was later used in the atmospheric chemistry network. Egner ran out of funds but his colleague, Prof. Sante Mattson at the Institute of Soil Science (pedology), offered me a position as his assistant. The salary was fair and my economic problems evaporated, at least for the moment. I finished my courses for the grade of agronomist but became so absorbed in my new job that I forgot all about the extension service which had been my original aim!



Erik Eriksson on the occasion of his confirmation in 1930

H.T. — What did your duties consist of?

E.E. — My position was partly administrative, dealing with salesmen in glassware and laboratory instruments and keeping an eye on funds. Administration was simple, however, and did not take up much time. A major part of the job was teaching and training students. Mattson liked lecturing and took most of that work. Laboratory training was done with the help of an assistant, Svante Oden, whose name will appear again later. He was a good instructor, who could explain complicated matters in a simple way and was an unlimited source of astute inventions which were useful in research. We became good friends.

H.T. — What were your research activities?

E.E. — As to research, I selected optical properties of organic matter in soil, particularly in the ultra-violet region. I continued this line for a year then realized that nothing would come of it. Organic matter in soils has always been a stumbling block for soil scientists. I shifted to polarography for studies of complexes, a work which ended with two publications. I continued with studies of interaction between water and clays, particularly of the montmorillonite type, and this also resulted in a couple of publications. Then I constructed an automatic instrument for differential thermal analysis of clays intended as a tool for identifying clay minerals. The instrument worked well but I became so fascinated

by the theory of heat flow in such instruments that it resulted in three papers filled with formulas of almost every conceivable kind but, presumably, hardly ever read!

H.T. — You then left your family in Sweden to go to Scotland?

E.E. — All this work resulted in a degree and an increase in salary. In 1950, I was granted a British Council scholarship to study for one year at the Macaulay Institute for Soil Research in Aberdeen, Scotland. I could not take my family since this was against the rules. There was a social idea behind the grant; I should also learn something about the British way of life and the presence of family members might distract attention from how well the British Post Office worked! That apart, it was a fruitful time. I spent a month together with a good friend from Australia who had a Morris Minor car, travelling through England, Scotland and Wales.

H.T. — What did you do when you returned to Sweden?

E.E. — I resumed cooperation with Prof. Egner on the development of equipment for rainwater chemistry studies and helped develop a device for collecting chemical substances from rainwater. New materials, exchange resins, appeared on the market and proved effective for removing dissolved ions from atmospheric waters. They are widely used nowadays for technical purposes, e.g. to make de-ionized water which has replaced ordinary distilled water. If a cation exchanger was followed by an anion exchanger, the device attached to a funnel collecting rainwater, would remove and store all cations in the cation exchanger and anions in the anion exchanger, so there was no need to transport rainwater.

H.T. — What was the upshot of this work?

E.E. — This idea was tested in a project on a national scale, in which Dr Anders Angström¹¹ participated. Every month, a parcel of the collection device was sent out to the stations in the national network. On their return one month later, the cation exchanger was separated from the anion exchanger. The ionic contents were removed and analysed for ammonium or nitrate. The meteorological aspects of the results were published by Angstrom and a co-worker in *Tellus*. This was probably the first atmospheric chemistry network in the world. The collection technique using ion exchangers is not appropriate in the climate of Sweden. During winter, a number of collection devices were damaged by frost and could not be analysed. Since sulphate was also of agricultural interest, the arrangement of cation exchanger followed by an anion exchanger did not work well. The effective part of the cation exchanger was sulphuric acid groups within the resin but the bond holding these groups was not strong enough so a slight loss occurred as sulphuric acid and this was absorbed by the anion exchanger. Most of the sulphate obtained on analysis was not of atmospheric origin but came from the cation exchanger. This realization led Prof. Egner to develop cabinets with electric heaters and temperature control, housing part of the collecting funnel and the sample bottle. Air sampling was also provided by a washing bottle, a small pump and a gas meter. These devices worked well. Egner managed to obtain enough funds to run a few such stations in the southern half of Sweden. The cabinets were also used by the agronomist Olle Johansson in a study of the effect of sulphuric acid emissions from a plant for retrieving oil from shale.

H.T. — In 1953 you were employed by Rossby at MISU. How did that come about?

E.E. — In 1953, Rossby was acquainting himself with the state of atmospheric sciences in Uppsala when he visited Prof. Egner in Ultuna. This was the time of rainmaking; H. Kohler, professor of meteorology at the University of Uppsala, was well known for his work on condensation and nucleation processes in the atmosphere, part of which bordered on geochemistry. The atmospheric chemistry work at Ultuna was also known by chemists. The study of atmospheric large-scale transport processes with chemistry as a tracer might have entered Rossby's thoughts or, alternatively, the possibility of understanding the role of chemical substances in rain and snow formation and even to manipulate this process. On the whole, however, the range of possibilities at that time must have been somewhat hazy; the only way out was to study atmospheric chemistry and learn from Nature itself. Rossby introduced an atmospheric chemistry section at MISU and, on Egner's recommendation, asked me to organize and run it. I hesitated, because I understood that this would most likely end my career in soil science. However, I accepted and took two years leave from Ultuna. Farmer, extension service consultant, soil chemist, what next?

H.T. — Together with Egner, you set up the European Atmospheric Chemistry Network. Could you please describe the various stages of your work?

E.E. — I started my work at MISU with a series of lectures on the geochemistry of the atmosphere. Most of them were prepared on the train between Stockholm and Uppsala. This work gave me a fascinating view of the history of atmospheric research and the role of the atmosphere in long-term processes. I probably learned more from these lectures than the audience did! The next step was to set up a small laboratory and arrange for rainwater sampling. The site was the roof of the building where we were housed and we used a stainless steel funnel 1 x 1 m². Usually, the sampling interval was 24 hours but, when a well-defined warm front passed, I would sample at hourly intervals. A meteorologist at nearby SMHI analysed the meteorological situation on interesting occasions and provided a base for interpreting the chemical results. It could be seen from the results that the environment of MISU was highly polluted. I prepared a paper on the data available but *Tellus* turned it down. The only interesting result was obtained during the passage of a warm front with rain of varying intensity. The data indicated that the deposition rate of sodium was virtually constant, independent of rainfall rate. But the sampling programme was closed. Rossby was also aware of the difficulty of sampling in this environment. I suggested a long-term atmospheric chemistry programme using a network of sampling stations of the Egner type with a view to expanding it to neighbouring countries. At my suggestion, Rossby invited Egner to MISU to help set it up. We wanted to expand the Swedish network into the entire north-western part of Europe. Rossby knew almost all the decision-makers in meteorology and they were invited to attend, or send representatives to, a small workshop at which station sites were selected, sampling equipment was demonstrated, procedures were discussed, analytical methods agreed on and analytical centres recommended. A year later, another small workshop was arranged at MISU to review experiences and suggest improvements. The data were published in *Tellus*. The European Atmospheric Chemistry Network (EACN) was thus operational.

H.T. — In 1955, you went to Hawaii to take part in Project Shower. What was the idea behind this project?

E.E. — Project Shower research was the result of differing opinions on the rain-formation process. The Findeisen-Bergeron mechanism required cloud temperatures far below zero to produce the ice nuclei needed to activate supercooled cloud droplets. In the trade-wind belts, the clouds seldom reached that high because of stable air and yet, there was no doubt rain at times, particularly on islands. Bergeron clung to his theory that the warm rains were due to drizzle. Later it was found that sea-salt particles were usually very large compared to other hygroscopic particles in the atmosphere and would therefore grow to such a size that scavenging of smaller cloud droplets occurred, thus enabling raindrops to form. The only way to solve this was to make observations where warm rain appeared. The large Hawaiian island was the place for this. The US Office of Naval Research and Air Force and the Cloud Physics Group in Sydney were interested in this subject. In 1954, a field research group was set up in Hilo on the north-east coast. Most of the fieldwork concerned meteorological conditions such as wind profile and measurements of temperature and moisture from Australian DC3s.

H. T. — What were your duties under the project?

E.E. — I had to arrange for rainwater sampling along Saddle Road and Kulani Road and occasionally at the camp near the shore—pineapple cans were used for sampling and rainfall measurements! Some of the chemical analyses were made at the Crop Log Laboratory a few miles north of Hilo, situated at a dreamlike place with rustling palms by a deep blue sea. Our headquarters was a club with a swimming pool—there was no beach in Hilo. Food was provided ready-made and was excellent; beer arrived in kegs by air from a brewery in Honolulu three times a week. At one time, at an altitude of 610 m (2 000 ft), 0.6 mm min⁻¹ was observed, really no drizzle. The data were published in *Tellus* 9 (4). Prof. H. Byers of Chicago was the editor but he found to his dismay that practically no attempt was made by the participants to analyse their data as there was no provision in the budget for this.



Erik Eriksson (far left) at a "Lucia party" at MISU in 1963

When the project was over, therefore, everybody rushed home to continue their usual work—but we had great fun!

H.T. — You then had some interesting experiences during an expedition to the Arctic, I believe?

E.E. — In 1956, Rossby asked me to participate in a Soviet expedition to the Arctic Basin on board the icebreaker *Ob*. We left in the middle of August and after a few days, arrived in the Russian coal mining town of Barentsburg. We then went west to Greenland and cruised along the mostly ice-free coast, admiring the gigantic stranded icebergs and near-shore ice floes on which walrus were lying. Further north, Arctic ice appeared and the ship turned east towards Spitsbergen. The Sun never set but was perpetually at a low angle, creating incredible, constantly shifting hues in the sea and ice. The sea was covered by large slicks of decaying organic matter.

The ship then turned northwards into an icefloe which was impossible to penetrate. Fifteen of us were transported by helicopter in two trips to a cupola glacier in the north-east. A large tent was set up, dinner was prepared, vodka was consumed and everybody went happily to bed. The following morning, 10 people went back to the boat and the helicopter was supposed to come back for the rest of us. We waited and waited but no helicopter came. Another night went by. The next day, an airplane approached, and dropped a message telling us to wait: the helicopter was lying on the bottom of a lake on an island west of us, although the crew had managed to escape and reach the ship. An airplane with skis was to come as soon as possible and pick us up. This took some time—two weeks to be precise. However, an airplane came fairly regularly and dropped food, vodka and firewood. The firewood was for when our transport aircraft landed. We set two rows of fires, using the vodka to light them. Everything went well and we boarded the plane for the flight to the closest airfield, Nagorskaia, on Franz Joseph Land. After a couple of days, the ship arrived and the last leg of our trip brought us to Gothenburg, from where we took a train to Stockholm.

H.T. —Tell us about your participation in the UNESCO Arid Zone Conference on Arid Climate in Canberra, Australia in 1956.

E.E. — In 1995, Rossby was asked by UNESCO to present a paper on arid zone climatology for the Conference. As he was particularly busy, he asked me to prepare a draft and finally asked me to go to the meeting and present the paper in his place. The paper was, to a great extent, a plea for a better understanding of the origin of saline soils. I still maintain that, in most cases, the salinity originates from airborne sea salts coupled with a small and sometimes highly variable discharge of water. In a closed basin, a continuous accumulation of salts occurs, as exemplified by Lake Eyre in the Great Artesian Basin of Australia. The Conference papers were edited by an old friend, C. C. Wallén¹².

H.T. — Did you have the opportunity to visit the country at all?

E.E. —After the conference, excursions were arranged in New South Wales: Broken Hill on the desert border, Merbein in the River Murray Valley and much more. The Australian breakfast fascinated me; the bacon was replaced by lamb cutlets. Merbein had the longest bar in the world, about 100 m long. It was usually empty until 17h00, when it became crowded, and closed at 18h00. A visit to the Meteorological Research Centre outside Melbourne showed how advanced their turbulence research was, particularly instrumentation and electromechanical devices for data analysis. Computers as we know them did not yet exist. I also visited the Soil Chemistry Department at the university, led by Prof. G. Leeper, who was well known for his merciless criticism of papers on soil chemistry! Finally, I made a trip to Adelaide, where soil-water investigations were being carried out using the new neutron sonde. Afterwards, I returned, westwards, to Sweden.

H.T. — During the years 1957 and 1958 you visited New Haven, Chicago University and Woods Hole Oceanographic

Institute. How long did you stay in each place and what did you do?

E.E. — My family and I spent about a year (1957/1958) at three places in the USA. The first stop was New Haven, where I worked at Prof. Evelyn G. Hutchinson's Institute of Zoology for two months. Hutchinson is famous in water sciences for his book *A Treatise in Limnology*. He also published reviews of scientific papers covering almost the entire natural science field. Another book of his, *The Itinerant Ivory Tower*, is a collection of essays of rare quality. My work was to obtain rainwater samples from a few representative places for analysis of stable sulphur isotopes, ^{32}S and ^{34}S . The ratios between these in sulphate in the sea and in fossil fuels differ considerably. The ratios in rainwater should therefore tell about the origin of sulphur in the atmosphere. I sent the bottles and received them back after a month and then separated the sulphate by precipitation with barium. They were eventually analysed at the Karolinska Sjuk-huset in Stockholm, which had a mass spectrometer. The result was that the isotope ratios did not differ much from that of biologic samples. Ostlund at the Radiocarbon Laboratory published the data later. While awaiting the samples to be collected, I assembled the chemistry data from the Albatross expedition, using them in a study of the possible atmospheric carbon dioxide circulation between the cold and warm areas of the sea. The first paper on this appeared in the Rossby memorial volume. The model I used was simple; one box for warm surface water, one for cold surface water and one for deep water. On the other hand, it probably matched the amount of information on the chemistry of the sea available at that time.

The next stop was Chicago; I stayed seven months in Prof. H. Byer's Department of Meteorology at the university and worked mostly on a concentration method for tritium in natural waters. The method was in principle a thermal diffusion one. I constructed a set-up in a laboratory nearby and ran a couple of water samples with some success. I wrote a report giving theory, construction and results. Later, back in Sweden, Ostlund constructed a similar one. It did not work and the idea was forgotten. The stay in Chicago was, however, fruitful in many ways although I was shocked during my stay to learn of Rossby's death.

The final two months were spent in Woods Hole with Al Woodcock and his colleague Blanchard, working on the immense amount of data on sea salts in the atmosphere available. I used some of them later in my thesis. The stay in Woods Hole was a pleasant period both for me and my family—as long as we kept away from the poison ivy!

H.T. — Back in Stockholm in 1959, you prepared your thesis and passed your doctorate. What was the subject of your thesis and how did the ceremony go?

E.E. — The subject of my thesis was the global circulation of oceanic constituents, global implying exchange between ocean and continents. I submitted two papers, including the one which appeared in the Rossby memorial volume. The second paper concentrated on deposition patterns of sea-salt components, mainly chloride, sodium, sulphate, calcium, magnesium and potassium. The faculty opponent was a well-known bio-geochemist, Prof. L. N. H. Cooper from England. Unfortunately for him, his copy of my thesis, carried on his bicycle, was stolen—together with the bicycle, presumably—the day before he left for Sweden. He remembered enough to be able to do his job during the examination. Admittedly, the questions were few but intelligent, but this may have been his style—who knows? All went well and I gave the traditional party for friends and opponents and Mrs Rossby came, too.

H.T. — In 1960, the International Atomic Energy Agency (IAEA) hired you as a consultant to establish a global network on the distribution of environmental isotopes in precipitation. Could you please expand on this?

E.E. — Hydrogen bomb testing started in 1954 and spread tritium (^3H) into the atmosphere. This increased the tritium concentration in rain in the USA by a factor of 20. In 1957, W. F. Libby and F. Begeman published a paper on continental water balance and groundwater inventory derived from studies of tritium in precipitation and river water. Large tests were to come and the IAEA saw an opportunity to aid a global programme for monitoring tritium in rain and snow. I was invited to participate in an expert group on the peaceful use of isotopes. With reference to hydro-logical applications, I suggested setting up a global network of stations for collecting rainwater samples and we marked crosses on a world map of where collections should be made on a routine basis. Studies of stable isotopes of water on natural waters were made in a few laboratories and the inclusion of these for analysis was evident. Somewhat later, the term "environmental isotopes" was suggested as a suitable label. As to stations, I suggested meteorological stations, considering that the participation of WMO in the programme would be a guarantee for the future.

H.T. — What was the extent of the collaboration between WMO and IAEA?

E.E. — IAEA was a young organization and had a special international position insofar as it reported to the Secretary-General of the UN and not to the Economic and Social Council as the other agencies did. IAEA had cooperated with FAO supporting agricultural research using isotope techniques. Cooperation with other agencies was established as appropriate. Mr Bryan Payne of the IAEA Secretariat and I went to Geneva to discuss the proposed global programme with the Secretary-General of WMO. WMO undertook to recommend the national Meteorological Service in the countries where we wanted a collection site to cooperate. All costs incurred were to be born by the IAEA.

H.T. — You then joined the IAEA Secretariat as a senior research officer. What were your main duties?

E.E. — I joined the IAEA in February 1961 for a two-year period. My main duty was to arrange meetings reviewing the use of isotopes in hydrology. Groundwater storage and groundwater flow paths are typical practical subjects. Another duty was to suggest and help in experimental field work. Such an experiment was carried out in Pelopon-nese on the Tripoli plain to discover connections between sinkholes on the plain and springs near the coast. In Cambodia, a French team carried out an experiment to determine bottom transport of sand. In Africa, on Kilimanjaro's eastern slope, there is a large lake without any visible in-or outflow. Using tritiated water as tracer, it was found that the water was replaced about every two years. A third duty was to prepare for international symposia on the application of isotopes. This comprised selecting venues, reviewing contributions, inviting key-note speakers, etc. A great deal of work and travel was involved.



Erik Eriksson using a portable conductivity meter during a study of acidification and fish life in the mountains of Jämtland. When the balance was correct, the signal in his ear stopped.

H.T. —What happened to the global network of isotopes?

E.E. — The IAEA/WMO network for monitoring environmental isotopes started with 151 stations, reaching a maximum of 220 stations in 1963/ 1964. A revision of the network was made in 1977, when some stations were discontinued, and the number oscillated around 80 in the 1980s. From 1990, 15 additional stations were added, mainly to improve coverage. At present, there are some 60 stations in the IAEA/WMO network.

H.T. — It was in 1963 when you returned to MISU. What sort of activities did you take up?

E.E. — I had asked Mr Oden, my colleague in Ultuna, to run the atmospheric chemistry programme at MISU during my absence. He started to assemble EACN data on concentrations in precipitation and of yearly depositions on maps. In particular, he was struck by the strong increase of sulphur deposition and concomitant decrease in pH.

He also collected the mounting information on acidification of lakes, particularly in the southwest, and the effect on lake fisheries, and prepared a paper, which he had published by *Dagens Nyheter*. The effect was a focus on environmental degradation by acid rain, not only in Sweden but also in a number of other European countries and in the eastern part of the USA and Canada. Oden was invited to deliver his message in many places; it was particularly well accepted by the "greens" and by politicians who realized its potential power. In Europe, a programme was created for the purpose of forecasting the atmospheric transport of sulphur dioxide. There were two major information sources for this; the release of sulphur dioxide from surface sources and information on the meteorological state. In a fairly simple model of atmospheric motion in the region and some empirical knowledge of the rate of removal of sulphur from the atmosphere, the distribution of deposition rates could be calculated. Verification of the model was made by studies at a particular network of sulphur dioxide in air and rate of deposition. The model permitted studies of effects of decreasing output. Personally, I did not contribute much to this but MISU was heavily involved in the development work.

H.T. — You were a member of the national committee for planning the International Hydrological Decade programme. Could you expand on this?

E.E. — The International Hydrological Decade (IHD) programme was conceived at a UNESCO arid zone meeting in Turkey. The reason from the hydrological point of view was the scarce and often poor quality of data on the water balance in many areas of the world. Various national IHD committees were formed and I became a member of the Swedish IHD committee, working out research programmes in line with an internationally accepted design. WMO was responsible for the operational part of the programme. The Nordic countries cooperated closely in the IHD, which gave them considerable strength during discussions in Paris on the design of the programme.

H.T. — In 1970, you were appointed Professor of Hydrology at the University of Uppsala. How long did you stay?

E.E. — The appointment to this post, the first in hydrology at the university, meant that my work at MISU ended. My new duties became education and research in hydrology. I kept this position for 12 years until I retired in 1982. The educational programme also included postgraduate studies. A number of my former students are now in the hydrology section of the SMHI. We also carried out a number of research projects under the IHD.

H.T. —After your retirement, you continued research and consulting work. Was this on a national or an international basis?

E.E. — After retirement, I continued with some hydrology research projects but I was also involved in international projects as a consultant. I visited Botswana twice on hydrogeological problems and made a few trips to Viet Nam as a consultant for properties and management of acid sulphate soils in a project of the Mekong Committee. A number of staff members of the Southern Institute for Water Resources Research in Ho Chi Minh City spent some time in Uppsala, where I instructed them in data handling, modelling and reporting.

H.T. — Do you maintain contact with MISU these days?

E.E. — My contacts with MISU at present are sporadic. MISU is only 80 km away and the area where MISU is housed is pleasant but I dislike going to Stockholm.

H.T. — Erik, I cannot tell you how pleased I am to have seen you again. I know that you derive a great deal of satisfaction these days from your farm—you have returned to your origins. I wish you many more happy years.

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 - ² Interviewed WMO Bulletin 37 (4) [[back](#)]
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 - ⁸ Interviewed WMO Bulletin 35 (1) [[back](#)]
 - ⁹ Interviewed WMO Bulletin 34 (4) [[back](#)]
 - ¹⁰ Interviewed WMO Bulletin 40 (1) [[back](#)]
 - ¹¹ Interviewed WMO Bulletin 31 (2) [[back](#)]
 - ¹² Interviewed WMO Bulletin 42 (1) [[back](#)]
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