THE *BULLETIN* INTERVIEWS

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> *Have you ever noticed that the Nobel Prize in physics has never been awarded for a discovery in meteorology? Today there is at least one active meteorologist whose contribution to science is so profound and pervasive that it is difficult to comprehend why he has not been selected a Nobel laureate. His name is Edward Lorenz, and he is the father of chaos theory.*
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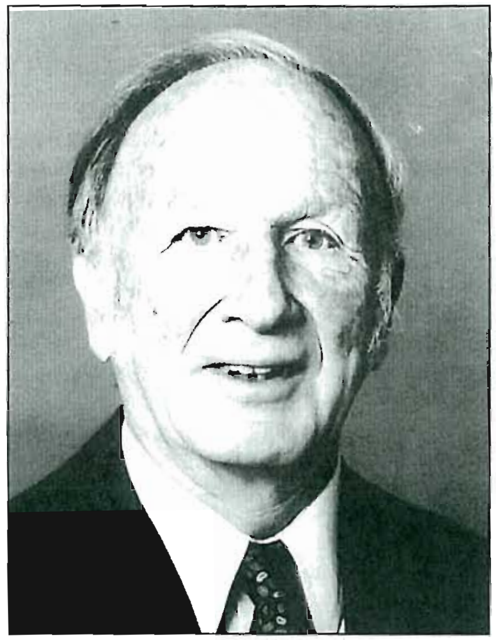
> *For centuries scientists put on blinkers, ignoring quirky and unpredictable phenomena in order to ferret out a tenuous sense of order in nature. This is the triumph of science, but it made us deny that chaos lurks everywhere. Ed Lorenz not only opened our eyes to the ever-present chaos in nature but also found its governing principles. He crowned 20th century meteorology with a discovery that irreversibly changed our view of the world.*

The above quotation from an article entitled "Chaos rules" written by Prof. Stanley David Gedzelman serves as a good starting point for this interview with a man whose fame extends beyond meteorology to other sciences such as biology, physics, astronomy, chemistry and geology. Indeed, Ed Lorenz helped scientists from these fields to understand unpredictable behaviour in their work.

Edward N. Lorenz was born in West Hartford, Connecticut, on 23 May 1917. From an early age, he was fascinated with numbers. Before the age of two, he could read all the numbers on the houses when his mother took him for walks. A few years later, he could recite the numbers from one to 1 000 that were perfect squares and he spent many hours with his father, solving mathematical puzzles. At the age of seven, he became fond of maps, especially enlargements of sections of other maps.

About this time also, he acquired a love for astronomy that he has never lost. He became interested in card and board games of all sorts. His mother taught him to play chess and he later became captain of the high-school team. Ed was smaller than most of the other boys of his age and a year younger than most of his classmates; it was perhaps for this reason that he never became adept at team sports. He enjoyed hiking, however, and, to this day, mountains and music are his greatest leisure-time interests.



Edward N. Lorenz
Photo: courtesy of AMS

When he entered Dartmouth College, Ed had already made up his mind to major in mathematics. In the autumn of 1938, he entered graduate school at Harvard University and worked under the guidance of George Birkhoff, a top American mathematician. When the USA entered World War II, he enrolled in the regular graduate course in meteorology at the Massachusetts Institute of Technology (MIT) and quickly realized that the subject most naturally related to his mathematical background was dynamic meteorology. In due course, Ed was sent to the tropics as a forecaster.

After the War, Lorenz decided to continue in meteorology at MIT. With his interest in dynamic meteorology and forecasting, he proceeded to write a doctoral thesis which proposed a method of applying dynamic equations to predicting the motions of storms. Not only did he obtain his doctorate without difficulty, he even managed, during the next few weeks, to acquire a wife, in the person of Jane Loban.

He made the acquaintance of Victor Starr and the two men remained close for some 25 years.

One of Ed Lorenz's significant findings was to unravel the chain of events through which a small percentage of solar energy reaching the Earth is converted into the kinetic energy of atmospheric motions, thereby replacing the energy which is dissipated by friction. His main findings were published in a paper in Tellus *in 1955 entitled "Available potential energy and the maintenance of the general circulation", which became a landmark in meteorology.*

Ed Lorenz's next important discovery was the concept of "chaos". It grew out of the fundamental recognition that the weather outside the tropics was extremely erratic. At the beginning of the 20th century, meteorologists made little use of the laws of motions describing the evolution of weather. The analogue methods used by forecasters were not based on fundamental dynamical principles. Lorenz saw clearly that much of the unpredictability was inherent in the atmosphere. So he took an independent course. Instead of working to improve forecast accuracy, he chose to search for what it was about the atmosphere that made accurate forecasting impossible. He proceeded to dismantle forecasting equations in order to find the culprit behind variability and unpredictability. His conclusion was that advection of quantities such as momentum and energy by the wind was the main cause for non-periodicity. In the course of the computations, Lorenz encountered some consequences of rounding off numbers before re-entering them into the computer. The sensitivity of chaotic phenomena to initial conditions was clearly brought out. Lorenz has published his findings in a book entitled The Essence of Chaos *(University of Washington Press, 1993).*

Lorenz's 1967 monograph "The nature and theory of the general circulation of the atmosphere" *was recognized as a classic. In 1969, the American Meteorological Society awarded him its highest honour, the Carl-Gustaf Rossby Research Medal, for his fundamental work in dynamic meteorology. Thus, the meteorological community honoured Lorenz for his work which has become known as "chaos".*

Ed Lorenz has certainly always been ahead of his time with his ideas about the atmosphere. He continues to work. He says

> *The sciences that have sometimes been called less exact, like some Earth sciences and life sciences, are sciences just as surely as the so-called exact sciences, like some branches of physics and chemistry. They simply require additional care in their practice, because they are more likely to involve chaotic processes. I have been awed by the precision that physicists sometimes attain, but I have never regretted my decision to follow a less exact science.*

In his concluding remarks, Prof. Gedzelman says: "Everywhere you look, and not just in the atmosphere or ocean, Lorenz's work applies. For these qualities his work surely merits the Nobel Prize in physics".

This interview took place at MIT in October 1995. Dr Taba, who had not seen Prof. Lorenz for a long time, says that he had hardly changed at all, either physically or intellectually.

H.T. — **You were born in 1917 in West Hartford, Connecticut, USA. Could you please start by describing what sort of place it was then and is now?**

E.N.L. — West Hartford is a suburb of Hartford, the capital of Connecticut. When I was born, it was a town of about 8 000 people. I lived there until I went to college. By that time, the town had grown to about 25 000. I went there last spring to visit a new science museum. It was on a main street that hadn't even existed when I left. The population now is about 60 000.

H.T. — **Could you say something about your parents?**

E.N.L. — My father was a mechanical engineer, who was born and grew up in Hartford. My mother did social work. They met at a holiday resort in New Hampshire, where we still go in the summer. We were a close family and my parents influenced me a great deal.

H.T. — **What sort of childhood did you have?**

E.N.L. — It was certainly a happy childhood at home but I was much smaller than most of the other children at school and I had some hard times. I wasn't particularly athletic; I joined in the baseball games but wasn't always too welcome. My mother was a good chess player—she had once beaten the champion of MIT—and she taught me how to play. I was eventually able to beat both my father and my mother and became captain of the high-school chess team.

H.T. — **Did you foresee that you might become a scientist one day?**

E.N.L. — As a small child, I enjoyed playing with numbers. At the age of seven, I came across an atlas with an illustrated astronomical page and immediately became interested in astronomy. The following year, we had a total eclipse of the sun in Hartford, which was a great event for me. I was always interested in the weather but it never occurred to me that I would actually study or work in meteorology.

H.T. — **What were your interests at school?**

E.N.L. — I had various hobbies, such as stamp collecting, but my main interest was chess. As far as the curriculum was concerned, I simply liked some courses better than others but I did no more than what was expected of me in the classroom.

H.T. — **When did you go to college and what did you study?**

E.N.L. — I entered Dartmouth College in 1934 and graduated in 1938, having majored in mathematics. A special mathematics course was arranged for me during my senior year, because there were so few mathematics majors. At first, there were 700 students in our class and then 500, but only seven of us were mathematics majors. I studied some calculus and advanced algebra and differential equations.

H.T. — **You entered graduate school at Harvard University in 1938. Could you tell us about the curriculum you chose? Who were your instructors?**

E.N.L. — I joined the Mathematics Department and took a wide variety of courses in almost every branch, in preparation for a doctorate. My advisor during the latter years was Prof. George Birkhoff, an eminent mathematician and the first of his calibre to have been trained in the USA, rather than Europe. It was fascinating to work with him, because he would give a course on something that he was working on and we would see new results appearing right there on the blackboard. Other outstanding people in the Department were Saunders MacLane and Marshall Stone. Prof. James Van Vleck also gave mathematics courses, although he was primarily a physicist; indeed, he later won the Nobel Prize for physics. I eventually realized that I was more interested in algebra than mathematical physics.

H.T. — **You had to interrupt your university studies because of World War II. When did you go to MIT?**

E.N.L. — I had the choice of being drafted immediately or signing up for a special course. I had already received notification of a new meteorological course at MIT to train army forecasters and, being interested in the weather, I decided that that was the best thing for me to do during the War. It was a regular Master's degree course, except that it was squeezed into eight months. We had classes in the morning and synoptic laboratory in the afternoon. Although it was not immediately applicable to weather forecasting, I particularly appreciated the course in dynamic meteorology and my mathematical training proved most useful. I attended the course at MIT from March to November 1942 as an aviation cadet in what was then the Army Air Corps (now the Air Force). Participants in the next course tripled in number and five of us were chosen to teach them in the laboratory. The final course was even larger and 15 of us were chosen to stay on until June 1944.

H.T. — **Who were the teachers and leaders in meteorology before World War II?**

E.N.L. — There were a number of distinguished meteorologists at different universities around the country. At MIT, there were Hurd Willett, Henry Houghton and Bernhard Haurwitz, just to mention a few. At Chicago, there was Carl-Gustaf Rossby. At the University of California at Los Angeles (UCLA), there were Jacob Bjerknes, Jörgen Holmboe and Morris Neiburger. Some outstanding meteorologists, such as Harry Wexler and Francis Reichelderfer[1], were at the US Weather Bureau.

H.T. — **You served in the US Army Air Corps as an operational weather forecaster from 1944 until 1946. What can you tell us about this period?**

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1 Interviewed *WMO Bulletin* **37** (3) (*Ed.*)

E.N.L. — At the end of the last training course at MIT, I received orders to go overseas. After a two-month training course in tropical meteorology in Hawaii, we flew to Saipan in October 1944 and set up a Weather Central. Our mission was to forecast for flights going to Japan and other nearby destinations. My principal job was to forecast upper-level winds and temperatures. One problem we had was that, although we had data from Siberia and near Saipan, not many observations were taken in between, except those taken by the pilots—who, obviously, didn't have much time! What they often did, therefore, was to make a copy of our forecast and give that to us as their observation. While this made for a good forecasting record, it didn't help us make the next forecast! In the spring of 1945, we moved to a new Weather Central in Okinawa. I became head of the upper-air section and we continued in the same way as before until a few months after the War.

One of my tent mates in Saipan was Reid Bryson, who had taken the same course at the University of Chicago as I had taken at MIT. He is well known now and is at the University of Wisconsin. One of his specialities is tracing early weather from archaeological evidence.

H.T. — **What happened after World War II?**

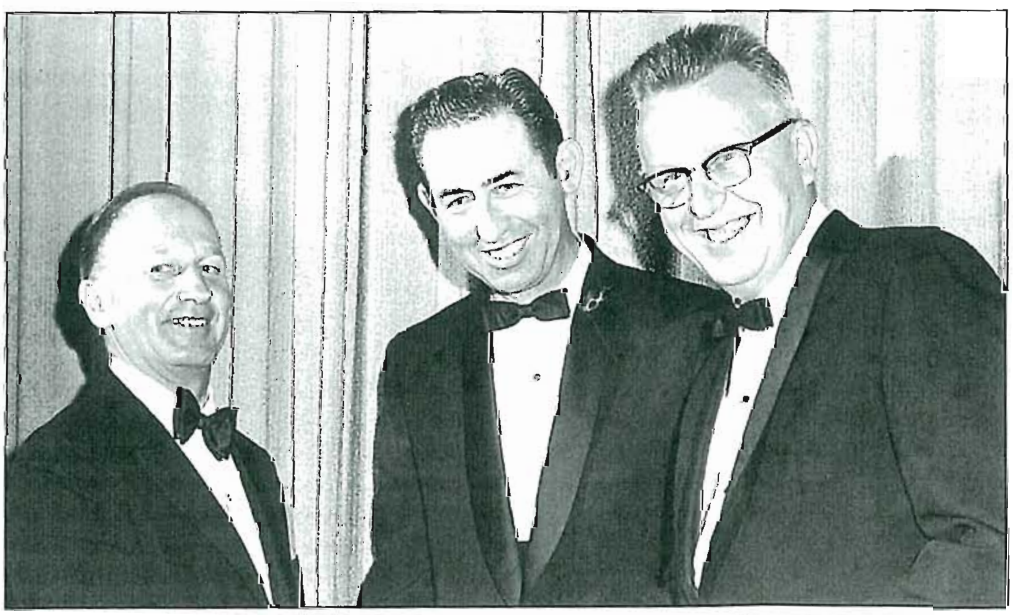
E.N.L. — In the spring of 1946, I had to decide between mathematics and meteorology. After discussions with Prof. Henry Houghton, who was head of the Meteorology Department at MIT, I decided I had more to offer meteorology. I obtained my doctorate in 1948.

H.T. — **What was the topic of your thesis?**

E.N.L. — My thesis was related to numerical weather forecasting and proceeded by expressing the variables as a power series in time. I was able to find ways of getting the first few terms of the series and evaluating them for a short time. There were no computers and I preferred to do the necessary calculations by hand rather than use a desk calculator. The time taken for writing up the results and deciding what to do was much more than the actual computing time.

H.T. — **Were your results encouraging?**

E.N.L. — What I did was use an initial state in which a cyclone was present. I specified the form of the cyclone and attempted to see whether the equations would tell how the cyclone was going to behave. The results were fairly reasonable for the cyclone motion for a period of six hours.



During the 48th annual meeting of the American Meteorological Society (AMS) in San Francisco, California, USA, in 1968 *(from left to right)*: Edward Lorenz, Chair of AMS Awards Committee; Louis Battan, President of AMS; and Verner Suomi, President elect of AMS (who, on this occasion, also received the Carl-Gustaf Rossby Research Medal)

Photo: V.M. Hanks, Jr., courtesy of AMS

H.T. — **Did this have any forecast value?**

E.N.L. — It would have had value for a short-range rain forecast but it would not have been as good as a good synoptic forecast. The same was true of the first years in numerical weather forecasting. Even though the early attempts to apply the equations were encouraging, numerical forecasts in the early 1950s had yielded nothing spectacular. It was a considerable time before they could compete with synoptic forecasts.

H.T. — **You decided to stay on at MIT. What did you do?**

E.N.L. — Prof. Houghton offered me a job in the Meteorology Department as a research scientist on a project to study the general circulation of the atmosphere, headed by Victor Starr. I published a few papers but it was a few years before I managed to achieve results that I considered worthwhile.

H.T. — **I believe it was about this time that you got married?**

E.N.L. — Jane and I met in the Department, where she was also working. We were married in 1948, just after I received my doctorate. We honeymooned in Mexico and then settled down in the Cambridge area, close to the MIT campus, and I could walk to work.

H.T. — **You were close to Victor Starr. What can you tell us about him?**

E.N.L. — As a weather forecaster in Florida, Victor Starr wrote an article which came to the attention of Harry Wexler, who was in charge of research at the US Weather Bureau in Washington and immediately recognized its worth. Victor joined him in Washington and soon afterwards went to the University of Chicago, where he obtained his doctorate.

Victor Starr came to MIT in 1947. I did my thesis with Prof. Austin, whose main field was synoptic meteorology. If Victor had come a year earlier, I would probably have done my thesis with him. He was a wonderful person to work with; we had discussions about meteorology almost every day and worked closely together for more than 25 years, until he died in 1976.

H.T. — **What were his particular qualities?**

E.N.L. — He was unique in that he had ideas about the general circulation that he was able to express clearly. I knew how to forecast cyclones but did not know why they existed in the shape they did or why they moved the way they did. It was Victor who first provided explanations in words that enabled me to understand the behaviour of the atmosphere. His reputation grew and people came from all over the world to work with him.

H.T. — **Meteorology advanced rapidly in the 1950s. Numerical weather prediction was becoming a reality. With your mathematical background and your interest in dynamic meteorology, how did you view future prospects?**

E.N.L. — Victor Starr and I were not completely sure that numerical weather forecasting would work. We visited Jule Charney and colleagues at the Institute for Advanced Study in Princeton to learn more about what some people said would revolutionize meteorology. It was the mid-1950s, however, before I was sure that this was the way of the future.

H.T. — **I believe you made an unforgettable visit to the University of Chicago in the early days of your career?**

E.N.L. — My visit to Dave Fultz at the University of Chicago to watch his experiments with a rotating dishpan was a memorable one. Some people were sceptical, claiming that the resemblance between the dishpan and the atmosphere was superficial. They did not agree that there were some basic principles that applied to all heated rotating fluids, be it the dishpan or the atmosphere. Dave and I decided to take some measurements. The dishpan was filled with a shallow layer of water and rotated while one applied heat at the outer edge and sometimes cooling at the centre. When the pan rotated slowly, water rose at the outer edge, sinking near the centre; in other words, a Hadley mode dominated. With faster rotation, the flow became more complex. At first, a few symmetrical stable waves developed. As the dishpan rotated faster, the number of waves increased and they became unstable and irregular, growing and shrinking, as in the atmosphere. We inserted a thermometer in the water and watched the rise and fall of the temperature associated with the meandering waves and the jet stream. The passage of the jet stream was accompanied by definite rises and falls in temperature—at times some 6°C— depending on which side of the jet stream the thermometer was. We calculated a flow of about 88 cm s^{-1} at the top level, assuming a zero flow at the bottom, a geostrophic condition and the observed temperature gradient. The observed flow at the top was about 90 cm s^{-1}, which was just about geostrophic. It was a wonderful experience to see this happen and realize that some of the general circulation properties that we had been studying for so long were duplicable.

H.T. — **When and why did you go to Lowell Observatory in Flagstaff, Arizona?**

E.N.L. — The Lowell Observatory was running a project to investigate the general circulation of the atmospheres of the planets. Seymour Hess and Ralph Shapiro were two meteorologists working with the astronomers and they invited me to visit during the summer of 1951. One outstanding astronomer there was V. M. Slipher, who discovered the red shift[2]. I was particularly interested in the atmosphere of Jupiter. I learned how to use the telescope, took some spectrographs of Jupiter and drew a few conclusions as to how deep its atmosphere might be. It was fascinating work but, as I was there for only three months, I could not become too greatly involved.

H.T. — **A few years later, you started to study the conversion of solar energy into the kinetic energy of the atmospheric motion and the concept of available potential energy. Could you expand on this?**

E.N.L. — The strength of the cyclones, anticyclones and other systems which form weather patterns are often measured in terms of their kinetic energy. Intensifying and weakening weather systems are those which gain or lose that energy. When such gains or losses occur, a knowledge of the source or sink of the kinetic energy is important. It is equally important to note that only a small portion of the Sun's energy reaching the Earth is converted into kinetic energy of the winds. The general circulation of the atmosphere is characterized by a conversion of available potential energy generated by low-latitude heating and high-latitude cooling. The available potential energy of the atmosphere is the difference between the total potential energy and the minimum total potential energy which could result from an adiabatic redistribution of the mass. The large increases in available potential energy are usually accompanied by increases in kinetic energy and involve non-adiabatic effects.

H.T. — **How did you become interested in this problem?**

E.N.L. — Victor Starr said there ought to be some sort of available potential energy but we agreed that neither of us knew how to formulate it. A year later, the idea suddenly came to me that it would be possible to look at the excess energy over the minimum amount of energy that could be present with the same statistical distribution of potential temperature. If you rearrange the atmosphere under the conservation of potential temperature, you might have less energy, and any excess over the minimum could conceivably be available for conversion into kinetic energy.

One night I suddenly woke up and started playing around with equations in my mind. After an hour I had virtually all the equations for available potential energy worked out. I was excited. I soon realized that it ought to be possible to partition this available potential energy into zonal and eddy energy, just as we had already partitioned kinetic energy. Since the transport of angular momentum towards latitudes of different angular velocity is what converted the kinetic energy from one form to another, it ought to be the transport of heat towards latitudes of different temperatures which would convert available potential energy the same way.

H.T. — **Did you often have such good ideas in the middle of the night?**

E.N.L. — That was a memorable occasion and I often thought that maybe one night I would wake up again with some other good ideas but it never happened. Any good ideas I had afterwards did not come that way!

H.T. — **The next stage of your career took you to Los Angeles?**

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2 The displacement of nebular spectral lines towards the red end of the spectrum. Interpreted as a Doppler effect, this leads to Hubble's law that velocity is proportional to distance and velocities up to 80 per cent of the velocity of light have been measured. (*Ed.*)

Edward Lorenz with his wife Jane, after receiving an honorary Doctorate of Science degree from McGill University, Montreal, Canada, in 1983

Photo: courtesy of AMS

E.N.L. — In 1953, I visited UCLA and talked with Bjerknes and Holmboe and learned that Neiburger would be on leave the following year. I accepted an invitation to go as visiting associate professor in his place for one year and I gave the first course there in numerical forecasting. I also met Arnt Eliassen from Norway, who visited for several months while I was there. We became firm friends; in fact, I saw him last week, when he came to Boston.

H.T. — **How did you come to replace Tom Malone[3] in the faculty at MIT?**

E.N.L. — At UCLA, I received a letter from Henry Houghton saying that Tom Malone was leaving MIT to set up a Weather Service in Hartford and asking whether I would be interested in taking over from him. Although I had a research position which I liked, I decided it would be better in the long run if I became a faculty member. I was invited to give a seminar, as is usual when being considered for an appointment, and was offered the position, which I took up in 1955.

H.T. — **This was also your first encounter with statistical weather forecasting?**

E.N.L. — As well as taking over his faculty position, I took over a statistical forecasting project, which Tom had headed. Statistical forecasting was considered by some as being diametrically opposed to numerical weather forecasting. It was entirely new to me. I had to learn about the linear methods used and saw that much could be done. At the same time, I remained a member of Victor Starr's project on the general circulation. It was partly because I was interested in both that I was eventually able to persuade people that the two types of forecasting could complement one another.

H.T. — **At what stage of your work did you start using computers?**

E.N.L. — After about a year on the statistical forecasting project, I realized that I needed a computer and Bob White[4] suggested I acquire one for my own office. The Royal McBee LGP-30 we chose sat in my office for many years. It was slow by today's standards but fast compared with a desk calculator. Subsequently, my work revolved more and more around the computer and small models.

H.T. — **The study of "chaos" became your principal interest in place of the general**

[3] Interviewed *WMO Bulletin* **41** (4) (*Ed.*)

[4] Interviewed *WMO Bulletin* **30** (1) (*Ed.*)

circulation. Could you explain chaos for us in simple terms?

E.N.L. — Chaos means something that looks random but is not random. The theory of chaos deals with sensitive dependence on initial conditions in non-linear dynamical systems, which is responsible for the apparent randomness. The Earth's atmosphere, plus its surroundings, is chaotic. There is evidence suggestive of chaos everywhere, starting with observations, practical experiments and numerical models, which depend heavily on initial conditions. The absence of periodicity is perhaps the easiest way to recognize that a system is chaotic. Most of our ideas about chaos in the atmosphere have come from working with models. Sophisticated models such as the large general circulation models used in operational weather forecasting centres are chaotic in the sense that small differences and perturbations eventually become extremely large.

H.T. — **How and why did you become interested in chaos?**

E.N.L. — Some statistical forecasters claimed there was mathematical proof that linear regression was inherently capable of performing as well as any other procedure, including numerical weather prediction. I was sceptical and proposed to test the idea by using a model to generate an artificial set of weather data, after which I would determine whether a linear formula could produce the data. If the artificial sequences turned out to be periodic, repeating their previous values at regular intervals, linear regression would produce perfect forecasts. For the test, therefore, I needed a model whose solutions would vary irregularly from one time to the next, just as the atmosphere appears to do. I started testing one model after another and finally arrived at one that consisted of 12 equations. The 12 variables represented gross features of the weather, such as the speed of the global westerly winds. After being given 12 numbers to represent the weather pattern at the starting time, the computer would advance the weather in six-hour time-steps, each step requiring 10 seconds of computation. After every fourth step—or every simulated day— the computer would print out the new values of the 12 variables, requiring a further 10 seconds. After a few hours, a large array of numbers would be produced and I would look at one of the 12 columns and see how the numbers were varying. There was no sign of periodicity. At times, I would print out more solutions, sometimes with new starting conditions. It became evident that the general behaviour was non-periodic. When I applied the linear regression method to the simulated weather, I found that it produced only mediocre results.

H.T. — **In which context, and when, was the term chaos used to express this phenomenon?**

E.N.L. — At one point, I wanted to examine a solution in greater detail, so I stopped the computer and typed in the 12 numbers from a row that the computer had printed earlier. I started the computer again and went out for a cup of coffee. When I returned, about an hour later, the computer had generated about two months of data and I found that the new solution did not agree with the original one. At first, I suspected trouble with the computer but, when I compared the new solution, step by step, with the older one, I found that the solutions were the same at first and then differed by one unit in the last decimal place; the differences became larger and larger, doubling in magnitude in about four simulated days until, after 60 days, the solutions were unrecognizably different.

The computer was carrying its numbers to about six decimal places but, in order to have 12 numbers together on one line, I had instructed it to round off the printed values to three places. The numbers I typed in were therefore not the original numbers but rounded off approximations. The model evidently had the property that small differences between solutions would proceed to amplify until they became as large as differences between randomly selected solutions.

This was exciting; if the real atmosphere behaved in the same manner as the model, long-range weather prediction would be impossible, since most real weather elements are certainly not measured accurately to three decimal places. Over the following months, I became convinced that the lack of periodicity and the growth of the small differences were somehow related and I was eventually able to prove that, under fairly general conditions, either type of behaviour implied the other. Phenomena that behave in this manner are now collectively referred to as chaos. That discovery was the most exciting event in my career.

H.T. — **Could you give a meteorological example of the phenomenon?**

E.N.L. — Consider a hypothetical experiment involving the development of convective clouds—the kind where the air is continually overturning. The clouds cannot be reproduced easily in the laboratory and the experiment may have to be taken to a suitable location, where one waits for the right sort of weather to arrive. Suppose that, throughout the vicinity of the chosen location, the weather conditions of two different early mornings appear to be identical. As noon approaches, small innocuous clouds may appear one day, while towering clouds and showers appear on another. This difference may occur because atmospheric convection is inherently chaotic, so that undetectable small differences at sunrise can amplify many times over. Even if the clouds could have been simulated in the laboratory, successive experiments might also have diverged after beginning with undetectable small differences.

H.T. — **You were continuously associated with the MIT Meteorology Department from 1946 until 1981. It would be valuable for readers if you could say what your reminiscences are about this period, in particular about your colleagues.**

E.N.L. — I had a number of notable colleagues, some of whom I have mentioned earlier, such as Hurd Willett, who was one of the best forecasters I knew and a good teacher. I've already spoken at some length about Victor Starr. Henry Houghton, who was head of our Department for 29 years, was a capable administrator as well as a pleasant colleague. He brought good people into the Department, such as Jule Charney and Norman Phillips[5]. Charney was an outstanding person. I never wrote any papers with him but we talked a great deal in his office, during which time he would write equations on the board. He was a wonderful person to know. He had a great deal of influence on meteorologists, not just within the Department but all over the country and the world. I couldn't begin to count the number of people he helped get started in their careers.

Phillips received a faculty appointment soon after he arrived. He had some wonderful ideas and was a hardworking person. He was head of the Department for several years after Henry Houghton retired. Erik Mollo-Christensen, was an oceanographer and experimentalist at MIT for a few years before going to NASA in Washington.

During that period also, principally in the late 1950s, the Department doubled in size and eventually became one of the larger ones in the country and, we thought, the best.

H.T. — **Your main scientific achievements could be said to be distributed in three main areas: atmospheric circulation, atmospheric predictability, and chaotic dynamical systems. Do these three topics follow each other logically or can they be considered as independent?**

E.N.L. — The atmosphere is a chaotic dynamical system and atmospheric predictability is one aspect of the chaos. I became interested in atmospheric predictability a long time before I became interested in chaotic systems in general. They are closely related. The atmospheric circulation could be said to be a property of the dynamical system that consists of the atmosphere but it is not closely related to the chaotic nature of the atmosphere as a dynamical system. I suspect that much of what we consider important about the general circulation would be more or less the same even if the atmosphere were not chaotic. If it were simply a dynamical system, the regular periodic variations might look somewhat like the variations that we actually see.

H.T. — **In 1967 you were invited to give the IMO lecture at Fifth World Meteorological Congress. Would you like to say something about it?**

E.N.L. — I received the invitation to prepare a lecture and a monograph well in advance. The monograph took about a year to prepare. I had never been to Geneva before and I enjoyed going there and giving the lecture. I particularly remember attending some of the meetings during Congress and seeing how things operated. I hadn't seen much of this type of activity before and it was quite an eye-opener for me. I was especially impressed by Bob White, who showed himself to be a real leader. It also gave me an opportunity to go up to Chamonix and do some skiing in the Alps.

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5 Interviewed *WMO Bulletin* **44** (3) (*Ed.*)

H.T. — **Do you still ski?**

E.N.L. — I go to the mountains whenever I can. I had to give up downhill skiing some years ago and now do some cross-country skiing. There is good cross-country skiing in the Boston area within a 10–15 minute drive and downhill skiing within a two-hour drive—if there is enough snow.

H.T. — **You have always been interested in outdoor activities. Have these complemented your scientific interests?**

E.N.L. — I see no close relation between my outdoor activities and my meteorological work. I have never studied mountain meteorology to any extent, whereas a number of meteorologists have devoted a good portion of their lives to this field. Having said that, however, I have always loved the mountains and one cannot be in the mountains and not be aware of the weather. I suspect that the mountains increased my interest in the weather in the first place.

H.T. — **You are still going to MIT. What are you doing there now?**

E.N.L. — Most of my work today involves either chaos or the predictability of the atmosphere and I spend much time devising simple chaotic models for various purposes. I have written some review papers on the general circulation but so much has happened in that field in the last 25 years that I no longer feel I am up to date in the subject.

My early work on the general circulation and chaos was largely funded by the Air Force. Since 1980, and even after my retirement, the National Science Foundation, has continued to support me.

H.T. — **Could you tell us something about your family.**

E.N.L. — Jane and I have three grown-up children. Our eldest daughter, Nancy, is a lawyer. She and her husband live in Boston and have two children: Nicky, who is 10, and Sarah, who is six. We see them regularly and babysit for them. It is wonderful having them around. Our son, Ned, is an economist. He taught for a while at Notre Dame University in Indiana but he married a French girl a few years ago and they now live in Paris, where he teaches. He studied at Cambridge University in England and spent a good deal of that time in France because he was interested in comparing labour problems in France and England. Our youngest daughter, Cheryl, is a psychologist. We have always been a close family and like doing things together, such as going to the mountains and skiing. Jane is an artist and a pilot: she had a pilot's license before she was able to drive a car! I've been up with her occasionally but not often!

January 1995, Dallas, Texas, USA — Edward Lorenz receives the 1995 Louis Battan Award from Warren Washington, President of AMS, at the 75th AMS annual meeting.

Photo: courtesy of AMS

H.T. — **If you had to start your life all over again is there anything you would do differently?**

E.N.L. — That is a hard question to answer. In view of my interest in chaos, however, knowing that one little thing could completely change what happens later, I find it unlikely that I would do the same things; it is not even certain that I would become a meteorologist. The one thing I can be sure of is that I would marry the same girl—if I met her! Otherwise, I feel sure I would do something academic, probably involving mathematics. I got into meteorology more or less accidentally. If I had done what I had planned to do before World War II, I would probably have taught mathematics.

H.T. — **Thank you, Prof. Lorenz. I am sure readers will find this interview as enlightening and rewarding as I have done.**