BIOGRAPHICAL MEMOIRS OF FELLOWS OF THE ROYAL SOCIETY

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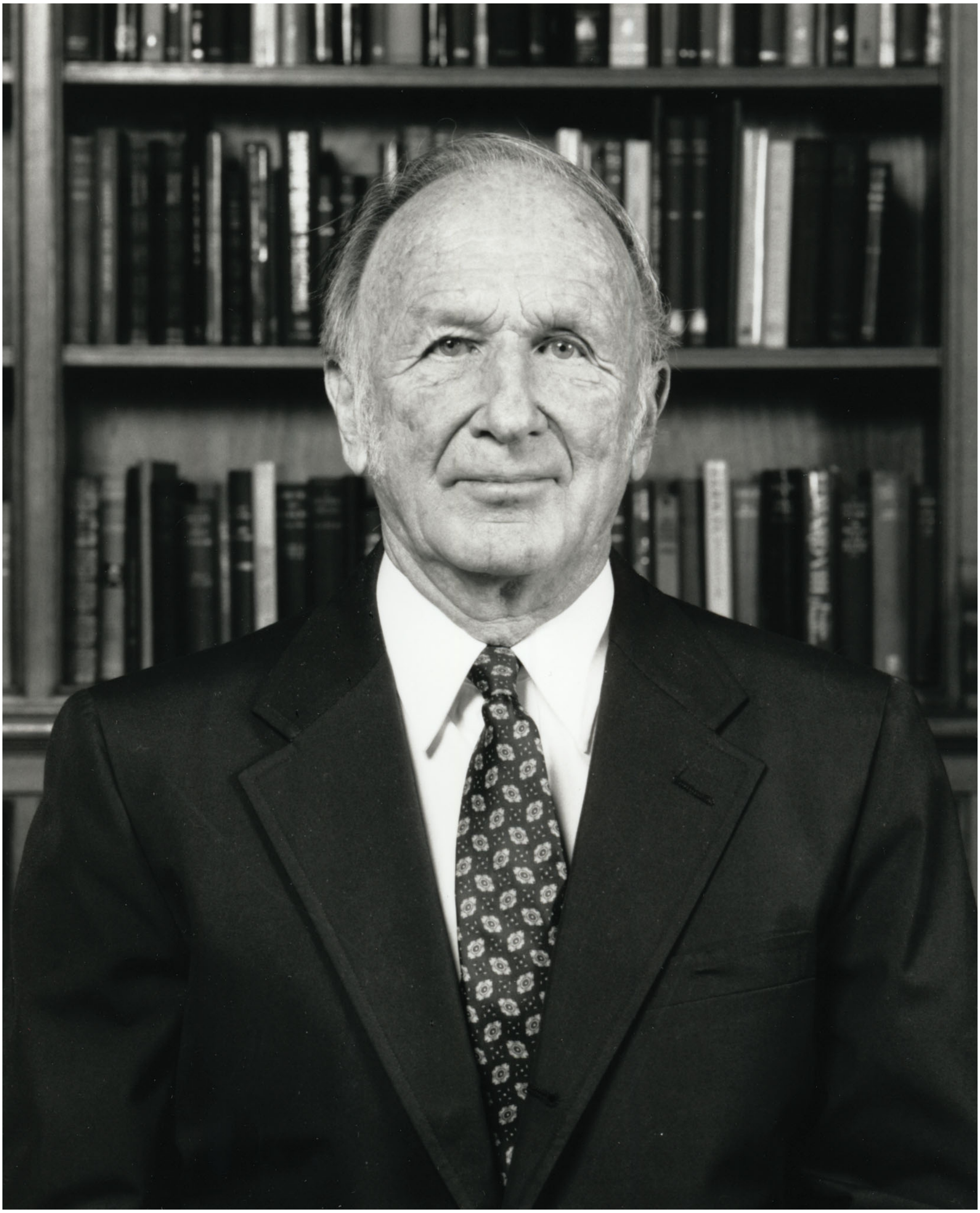

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Elected ForMemRS 1990

By T. N. Palmer

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Ed Lorenz was a pioneer of chaos theory; he provided the first realization of a strange attractor based on a mathematical model of just three coupled differential equations. In addition, Ed made many seminal contributions to theoretical meteorology, not only in studies of the predictability of weather and climate, but also in advancing our basic understanding of the dynamics and thermodynamics of climate.

Early background

Edward Norton Lorenz was born in May 1917 in a suburb of Hartford, the capital of Connecticut, USA. Ed's father was a mechanical engineer and his mother was involved in social work; they met at a holiday resort in New Hampshire, which Ed frequented as an adult.

Ed had a happy childhood at home. From an early age he excelled at chess, which he learned from his mother, who in turn had once beaten the Massachusetts Institute of Technology (MIT) chess champion. It was not long before Ed could beat both his mother and father, and he became captain of the high-school chess team. Apart from chess, Ed also had quite an interest in astronomy, and when eight years old he experienced a total solar eclipse from his home in Connecticut. This led to a lifelong interest in astronomy as a hobby; one summer he managed to spend some time at the Lowell Observatory, taking photographs of Jupiter.

Ed also showed an interest in weather, and liked looking through weather records. However, he never read any meteorological books at that time, and his interest in weather was very much secondary to his interest in astronomy and mathematics.

Although not athletic in the sense of excelling in baseball or football, Ed became passionate about hiking and skiing, activities about which he remained enthusiastic throughout his life. Indeed, these outdoor activities influenced his choice of Dartmouth College (one of the

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Ivy League colleges) as a place to study mathematics after high school. Having finished his degree at Dartmouth in 1938, Ed began the graduate programme in mathematics at Harvard and took a wide variety of courses in preparation for starting a doctorate. His advisor during this period was Professor George Birkhoff. Birkhoff was an eminent mathematician in many different fields, but was particularly well known for his work (and 1927 book) on dynamical systems, proving Poincaré's 'last geometric theorem', a special case of the gravitational three-body problem. This connection is interesting because Poincaré himself was, through the three-body problem, the first to realize one of the key characteristics of chaos theory: sensitive dependence on initial conditions. It is therefore natural to wonder whether, through Birkhoff, some of this may have rubbed off onto the young Lorenz, at least subliminally (Ed implies in his popular book (11)* that he had not been aware of Poincaré's work when he began his own research). In any case, Ed's master's thesis was not on dynamical systems theory but on a topic in Riemannian geometry. As described in detail below, Lorenz's approach to chaos was quite different from that of these earlier investigators. Because of this, Ed's work led to the discovery of one of the most beguiling of geometries that nature has to offer: the fractal geometry of the strange attractor.

World War II saw an interruption to Ed's research activities, and Ed had to decide how best to use his talents during the war years. Early in this period he was sent a circular about a new course at MIT to train weather forecasters for the army. Given his childhood interest in weather, he signed up for this course as an aviation cadet in the then Army Air Corps (now the Air Force). Five from this course, including Ed, were chosen to stay on as teachers for the next course. During this period Ed received a master's degree from the Meteorology Department at MIT. In 1944, Ed was posted to Saipan and later Okinawa in the western Pacific Ocean, where his principal job was to forecast winds and temperatures in the upper troposphere.

At the end of the war, Ed had to decide whether to return to mathematics, or indeed to switch to meteorology. Birkhoff had died in 1944, and after discussions with the then head of the MIT Meteorology Department, Ed decided to make the switch, and so he began a doctorate in meteorology in 1946. He obtained his doctorate from MIT in 1948, deriving a method for forecasting cyclone motion using power series in time, derived from the underlying equations of motion. Ed was largely self-motivated and self-propelled in seeing this work through to a conclusion. Although this aspect of Ed's work has not had lasting impact, his thesis work clearly showed him to be an innovative thinker, capable of independent research. Hence, after his doctorate, the head of the Meteorology Department offered Ed a job as research scientist, and there began the work that was to make him famous: first on the theory of climate, and then on the dynamics of chaos.

In fact, even if Ed had not discovered his famous model of chaos, he would still be exceptionally highly regarded in the meteorological community for his pioneering work on climate theory. However, given that most readers of this memoir will not themselves be meteorologists and may be more interested to learn about the path that led Ed to his seminal 1963 paper, I shall treat Ed's work in climate theory separately from his work on chaotic dynamics, describing the latter first in the two sections below.

* Numbers in this form refer to the bibliography at the end of the text.

The 1963 prototype model of chaos

In 1953 Ed visited the University of California at Los Angeles for a year. During that period he learned of a faculty position that had become vacant at MIT and was asked whether he was interested in applying. It is said that Jule Charney, a leading dynamical meteorologist of the time, also from MIT, had much to do with Ed's being recommended for the position. In any case, Ed was interested, and in 1956 was duly offered the position. As part of his new duties, Ed took over a project on statistical–empirical weather forecasting that the retiring professor had led. Statistical–empirical forecasting is a very different discipline from the type of numerical forecasting in which Ed had specialized for his doctorate. In numerical forecasting, one tries to solve the basic partial differential equations of weather (based on the Navier–Stokes equations), whereas in statistical–empirical forecasting one tries to find empirical relationships in data sets of the weather that allow one to predict future weather patterns. In the 1950s, numerical models of the governing equations were in their infancy, and most practical forecast models were statistical in nature.

Ed believed that numerical and statistical forecasting could complement one another, and embraced this project with enthusiasm. However, in taking on this project, Ed found that several of the statistical forecasters believed that the linear regression methods they had developed would perform as well as any numerical forecast scheme ever could, and therefore that there was no need for the latter. They even claimed they had a mathematical proof of this, based on work of Norbert Wiener. However, Ed was inherently sceptical and looked for methods that could at least test their claim. Because he had already started to work with relatively simple nonlinear models of weather, he hit on the idea of running one of these models on a small computer, thereby generating a lot of output data from which the skill of the statistical forecast methods could be studied. Fortunately, at about this time (now 1958) Ed was offered the possibility of acquiring a 'personal computer' for his office, virtually unheard of in those days.

For the test to be realistic, Ed needed to ensure that the simple nonlinear model agreed with the real atmosphere in at least one key respect, namely that its solutions were aperiodic; if the solutions were periodic, then statistical techniques would trivially be capable of forecasting the system, but that situation was not of realistic interest.

Ed focused attention on a 12-component model, obtained by Fourier truncation of the underlying partial differential equations of weather. The 12-component model represented gross features of weather such as the speed of the global westerly winds.

After analysing output from this model for a variety of initial conditions, Ed convinced himself that the general behaviour of this model was indeed aperiodic. When he applied the statistical regression forecast technique to the simulated weather data, the results were only mediocre, confirming his intuition.

At this point occurs one of the famous stories of Ed's research career. He decided he wanted to examine one of the solutions of the model in greater detail. He typed in the 12 numbers that the computer had earlier printed out, set the computer running again, and went out for a coffee. On returning an hour later he was surprised to find that the new computer output did not agree at all with the old output. Ed realized that the reason lay in the fact that the restart had been made with truncated output from the model.

Over the coming months, Ed began to be convinced that the lack of periodicity and the growth of small differences were related, and was eventually able to prove this under fairly general conditions. Ed had discovered chaos.

At this point in the development of the subject of chaos, it is necessary to rewind the clock a few years to when Ed began to ask why certain artificially produced patterns of isobars looked reasonably plausible as weather disturbances, whereas others were not. It was not obvious from the equations of motion alone. That is to say, Ed began to wonder whether, in the state space of the atmosphere, the realistic weather patterns lay on some sort of surface, not an energy surface because weather systems are dissipative, but a surface nevertheless that might possibly be described by some analytic expression. If one started the equations with 'unrealistic' initial states, they perhaps would tend towards the 'realistic' solutions on the surface. Ed gave up on these ideas until he started to work with the 12-component model. Then he started to think about whether such surfaces might exist in the state space of the 12-component model.

However, small as the dimension of the state space of the 12-component model was compared with the dimension of the state space of the real atmosphere, Ed found it difficult to analyse this 12-dimensional space, and so he became curious to know whether still simpler systems might exist where these possible surfaces might be analysed. While Ed was visiting a colleague, Barry Saltzman from Hartford, Barry showed him some work he had been doing with a model of thermal convection with just seven components. Most of the studied solutions of this model were periodic; however, one set of solutions refused to settle down to periodic behaviour. Ed noticed that, for this particular solution, four of the seven variables went to zero and stayed at zero. He realized that this implied there must exist a three-component subset of Saltzman's model that had chaotic solutions. This three-component model became the celebrated Lorenz (2) model,

$$\mathrm{d}X/\mathrm{d}t=\sigma(Y-X),$$

$$\mathrm{d}Y/\mathrm{d}t=X(r-Z)-Y,$$

$$\mathrm{d}Z/\mathrm{d}t=XY-bZ,$$

about which a whole book has been written (Sparrow 1982). It is fair to say that this model is too idealized and truncated to be considered a uniformly valid representation of fluid convection, but what a wonderful tool it proved to be to understand mathematical and computational chaos!

With this three-component model, Ed started to think again about the question of whether the climatic states lay on some surface in state space. For about a year he thought that the model produced two merging surfaces and that solutions evolved aperiodically on this surface. He began to write his famous paper (2). However, doing so made him realize that his intuition about this surface was wrong, because it implied that a solution coming in from one side of the surface would have to meet a solution coming in from the other side of the surface, and this was impossible in a deterministic system. He wrote:

> We see that each surface is really a pair of surfaces, so that, where they appear to merge, there are really four surfaces. Continuing this process for another circuit, we see that there are really eight surfaces etc and we finally conclude that there is an infinite complex of surfaces, each extremely close to one or the other of two merging surfaces.

Ed finally realized that the surface he was seeking was not a surface at all, but what we now call a strange attractor: an attractor with fractional dimension—a type of geometry that probes the very foundations of our mathematical understanding of number.

In writing up this work, Ed thought that the existence of this fractal attractor was a mathematical curiosity and that the really important point about the paper was the link he had shown between aperiodicity and lack of predictability. However, it was the demonstration of the existence of a fractal attractor from a relatively simple set of differential equations that, when they finally took note of Ed's work, excited mathematicians such as Jim Yorke, who had been working on the theory of dynamical systems and suspected that systems based on differential equations should exhibit fractal attractor sets, but had not found examples. As is well known, it took several years and a certain amount of serendipity for the mathematicians to take note of Ed's analysis of the fractal attractor of his three-component model—mathematicians do not tend to read papers in *Journal of the Atmospheric Sciences*.

This sense of excitement in the mathematical community at Ed's analysis has been perfectly captured by Ian Stewart (FRS 2001), who writes (Stewart 1989) (his italics):

> When I read [Lorenz's] words I get a prickling at the back of my neck and my hair stands on end. *He knew! Thirty four years ago, he knew!* And when I look more closely, I'm even more impressed. In a mere twelve pages Lorenz anticipated several major ideas of nonlinear dynamics before it became fashionable ….

Here we can finally lay to rest the debate about the nature of Ed's contributions in the light of Poincaré's (and Birkhoff's) earlier work on the gravitational three-body problem. I personally believe that it is this link, between the differential equation on the one hand and the fractal attractor on the other, that will be Ed's most enduring and remarkable contribution. I use the word 'remarkable' because Ed's differential equations live in the classical world of continuity and differentiability; Newton himself would have been comfortable with the Lorenz equations as deterministic evolution equations. And yet the geometry that these equations generate in state space is as discontinuous and non-differentiable as one can imagine—one could hardly imagine a geometry more alien to someone like Newton.

Although Ed is generally recognized as the founding father of the theory of chaos in forced dissipative systems, it should be recognized that others were working on this problem completely independently of Ed. For example, in 1962, Doug Allen had published a paper on non-periodic solutions of the Rikitake equations for the geomagnetic dynamo (Allen 1962), and Derek Moore (FRS 1990) and Ed Spiegel wrote about chaos in equations for stellar atmospheres in a paper (Moore & Spiegel 1966) that appeared shortly after Ed's.

The type of forced dissipative chaos that Ed discovered has now been found to have relevance in almost every area of the physical, biological or social sciences (see below). Perhaps one exception is the world of fundamental physics, where it is generally believed that the basic laws of physics can be described by reversible Hamiltonian dynamics. However, the final word has yet to be spoken on this, and there is a chance that one of the most beguiling geometries that mathematics has to offer, the fractal, may yet have a role in providing some deeper understanding of these laws of physics.

The 'real' butterfly effect: a million-dollar problem with trillion-dollar implications!

Ed's work was rightly celebrated in James Gleick's popular book (Gleick 1987), where the phrase 'butterfly effect' was coined. In fact this phrase derives from an earlier title to one of Ed's talks, 'Does the flap of a butterfly's wings in Brazil set off a tornado in Texas', composed not by Ed but by one of Ed's colleagues, Phil Merilees. Phil in turn may have been building on the metaphor used by Ed in his lesser-known 1963 paper (3) suggesting that one flap of a seagull's wings might be enough to alter the course of the weather forever. More importantly, Ed himself knew that his three-component model could not really answer the question of whether seagulls or butterflies really can alter the course of the weather; the model is too severely truncated to describe spatially extended systems.

As a result, after his 1963 paper, Ed set out to study upscale propagation of error in a more realistic spatially extended model. The results were published in a paper (7) entitled 'The predictability of a flow which possesses many scales of motion'. In this paper, Ed obtained a result with which many readers may not be familiar and which implies a form of unpredictability that is much more radical than that associated with low-order chaos.

Ed studied growth of error based on an ensemble-averaged kinetic energy equation derived on the turbulent vorticity equation. He closed the equation by making assumptions about the statistical independence of certain quadratic functions of streamfunction and error. The key finding was that two states of the system differing initially by a small 'observational error' will evolve into two states differing as greatly as randomly chosen states of the system within a finite time interval. Crucially, this time interval cannot be lengthened by reducing the amplitude and scale of the initial error. This is what Ed meant by the butterfly effect—as such I call it here the 'real' butterfly effect.

The reason that this type of unpredictability is more radical than that of low-order chaos is that in a multi-scale system of the type Ed considered, there will always be uncertainty on sufficiently small scales, and these uncertainties will grow and affect the predictability of large weather scales in finite time. By contrast, in a model of low-order chaos, accurate solutions can be made as far ahead as one likes by making the amplitude of initial error sufficiently small. (Of course, from a physical point of view, because error growth in chaotic systems is exponential, the initial error needed for accurate forecasts at long forecast lead time will in practice need to be ridiculously small. Nevertheless, from a mathematical point of view, variations in the solution of such chaotic systems depend continuously on variations in the initial state.)

Does this type of continuous dependence emerge from a rigorous analysis of the three-dimensional Navier–Stokes equations? No one knows. Understanding the behaviour of the Navier–Stokes equations, and whether they exhibit the 'real' butterfly effect, is related to one of the Clay Mathematics Millennium Prize Problems; along with the Riemann hypothesis, it is one of the famous unsolved mathematical problems of the twenty-first century.

The 'real' butterfly effect problem is of practical relevance today, because it addresses questions about how our ability to simulate climate converges with increasing model resolution. As emphasized in recent studies, climate change is a trillion-dollar problem for the world's economies, and accurate predictions of climate change are at a premium, both to inform and motivate national and international policy on cutting greenhouse-gas emissions, and, increasingly, to guide infrastructure investment for society to adapt to inevitable climate change. Never has it been so important to understand the limitations of the 'real' butterfly effect on our ability to predict climate.

Meteorology

It has been said on many occasions that even if Ed had done none of the above work, he would still be considered one of the pioneers in dynamical meteorology. We return to his meteorological career in this section.

From his earliest research days, Ed was interested in the dynamics and thermodynamics of the weather systems, and their role in determining the spatio-temporal averages of wind and temperature that characterize our climate. For several years, Ed was associated with what was known as the General Circulation Project at MIT led by Victor Starr. The work of this group was in turn influenced by the earlier work of Sir Harold Jeffreys FRS, who was the first to identify correctly the role of weather systems in conveying angular momentum from low to high latitudes. Ed was keen to understand further the role of the weather systems in determining the energetics of the atmosphere's 'general circulation'.

The strength of weather systems is often expressed in terms of kinetic energy. When a weather system intensifies, it gains kinetic energy. Where does that energy come from, and how could one formulate this source term quantitatively? At one level the solution to this problem seems obvious: the low-latitude heating of the atmosphere by the Sun and the high-latitude cooling of the atmosphere to space give rise to a potential energy that fuels the weather systems. However, after discussions with Victor Starr, Ed's mentor and long-term close friend, it became obvious to Ed that only a small portion of this potential energy could be converted into kinetic energy of the weather systems.

Soon Ed had solved the problem and, in doing so, introduced the concept of 'available potential energy'. Essentially the available potential energy of the atmosphere is the difference between the total potential energy and the minimum total potential energy that could result from an adiabatic redistribution of mass in the atmosphere.

Under adiabatic conditions, the sum of available potential energy and kinetic energy are conserved. However, Ed found that large increases in kinetic energy were actually accompanied by large increases in available potential energy, indicating the importance of diabatic source terms.

Ed realized it was possible to partition the available potential energy into a longitudinally averaged 'zonal' part and the 'eddy' deviations, just as kinetic energy or angular momentum can be so partitioned. This led to the famous Lorenz energy cycle of the atmospheric general circulation, characterized by a conversion of zonal available potential energy (associated with low-latitude heating and high-latitude cooling) to eddy available potential energy, to eddy kinetic energy and thence to zonal kinetic energy. Although this work was published in 1955 (1), Ed's definitive contribution to the topic of the atmospheric general circulation was his monograph published in 1967 by the World Meteorological Organization on the occasion of Ed's giving the International Meteorological Organization lecture (5).

Ed took a great interest in laboratory simulations of large-scale atmospheric circulations performed by Raymond Hide (FRS 1971), initially at Cambridge University and later a colleague of Ed's at MIT, and Dave Fultz from the University of Chicago. Ed realized that these simulations could be used to illustrate the properties of the nonlinear dynamical systems he was studying on the computer. For example, in his paper (4) on the mechanics of 'vacillation' (a term introduced by Hide), Ed discussed the transition from steady to unsteady vacillation cycles, noting that where aperiodic behaviour is found, steady vacillation cycles exist but are unstable.

As part of his PhD work at Cambridge University on thermal convection in a rotating liquid annulus, Hide discovered a type of non-uniqueness in terms of the number of waves in steady-state flows in the annulus. Although some felt that this non-uniqueness was merely the result of having failed to set up the experiments with sufficient care, Ed realized (6) that there was in fact a subtle explanation based on nonlinear dynamical-systems theory, described as 'intransitivity'. In these situations, the system has more than one attractor, and which attractor is reached can depend sensitively on the initial condition; that is to say, the basins of attraction of the two attractors are fractally intertwined.

Ed was interested in the implications of intransitivity for the practical problem of estimating the climate of the atmosphere. Of particular interest was a system that Ed described as 'almost intransitive': there might be one attractor, but the system might spend a very long time in one part of the attractor before making a transition to another part. If the real climate behaves like an almost intransitive system, we cannot be sure that a given set of observations, no matter over how long a time they are taken, truly describes the climate of the system.

The atmosphere exhibits dynamical modes of variability that can be distinguished by timescale. The Rossby modes, with timescales of days, are relatively slow and are crucial for forecasts of weather over the coming days. The gravity modes, with timescales of hours, are relatively fast and are relatively unimportant for such weather forecasts. An important question, therefore, especially for devising initialization schemes for weather forecasts, is the following: Does there exist an invariant manifold comprising solely the slow modes, in the state space of primitive-equation models of the atmosphere that support both fast and slow modes? This exercised Lorenz during much of the 1980s (10) and turned out to be a remarkably subtle problem: essentially it was found that manifolds that are locally invariant and locally slow do exist, but that manifolds that are globally invariant and globally slow do not.

It has often been speculated that, in regions where the atmosphere is unstable to small perturbations, small man-made influences could conceivably change the course of the weather. In the 1990s, Ed was interested in a variant of this idea: in regions where the atmosphere is especially unstable to small perturbations, extra observations of these regions of the atmosphere would have more impact on improving the initial conditions of a weather forecast than would extra observations of any other regions (12). Because the location of these unstable regions varies with the flow, these extra observations cannot be taken at fixed geographic locations. However, such targeted observations can and have been made in several recent field campaigns, and work establishing the value of targeted observations continues to the present day.

The impact of Ed Lorenz's work

I want to begin this section with what were almost the last words written by Ed to appear in print. The extract below is at the end of a paper written in 2008 for an Award Lecture that he prepared for the University of Rome (given by a colleague because of growing health problems). Ed wrote:

> The recognition of chaos has also led to a key change in the operational forecasting routine. In 1964 I suggested that, since we do not know the present weather pattern precisely, we might, instead of making a single weather forecast, make a large number, originating from slightly

> different initial states, and one of which might happen to be, but more likely would not be, the correct initial state. I suspect that the suggestion had been made earlier, although I am unable to find documentation. At ranges where the separate forecasts have begun to show important disagreements, they may still share certain features, suggesting that these features are likely to occur. When the forecasts disagree completely, we may still estimate the probability of occurrence of a specific feature by counting the number of forecasts in which it appears.
>
> It has been a source of pleasure to me to see that this technique, known as 'ensemble forecasting' has within recent decades become standard practice at some operational weather centers. A typical ensemble of initial states may contain 100 members. To avoid a 100-fold increase in computational effort, the ensemble of forecasts is commonly produced with a similar model but with reduced horizontal resolution. In many instances a 'consensus' forecast proves to outperform the individual ensemble members.
>
> Chaos has been detected in many fields of endeavour, and I have mentioned but a few. I hope, however, that my remarks give some indication of what chaos is about. I thank you.

Let me start by commenting on the last paragraph of this quotation. It is indeed true that chaos has been detected in many fields of endeavour. However, Ed characteristically underestimates the significance of his work in these many fields of endeavour.

In fact, as mentioned above, the advent of chaos theory in the second half of the twentieth century has brought about a revolution in science; hardly any scientific discipline (whether on the physical or biological side) has been untouched by it. In its essence, this revolution has been brought about by a paradigm shift to rival that of quantum theory, or relativity theory. Simply stated, the shift is this: if a system exhibits highly complex or apparently random behaviour, this does not imply that the underlying system dynamics is itself highly complex, or random. The importance of this result cannot be overstated—before Lorenz, the overwhelming view in science was that complicated behaviour of the sort mentioned would imply that the system's underlying dynamics was necessarily complicated, for example that associated with multiple nonlinear interactions between a very large number of degrees of freedom. This view certainly prevailed in meteorology: weather was unpredictable because climate had so many interacting degrees of freedom.

The word 'chaos' was first coined by Jim Yorke to describe the unpredictability of certain types of nonlinear dynamical system, and what we now call 'chaos theory' should be seen as an amalgam of three largely independent strands of research using different classes of dynamical systems: the Hamiltonian systems of Poincaré, Ed's systems of dissipative differential equations, and the finite-difference equations whose use was pioneered by Robert (now Lord) May (FRS 1979) (May 1976) and his colleagues Tien-Yien Li and Jim Yorke (Li & Yorke 1975). The impact that chaos theory has had in changing pre-existing paradigms in science has been discussed at length in numerous multidisciplinary books, both technical and popular (see, for example, Gleick 1987; Stewart 1989), including the popular book that Ed himself wrote (11). A brief summary of some of the areas in which chaos theory has had an impact are worth summarizing; in many cases I have drawn from the collection of articles in Hall (1991). I leave meteorology and climatology to the end.

In fluid flow, the 'classical' view—that as the fluid Reynolds number increased, the number of Fourier components in the fluid would necessarily increase rapidly until at the onset of irregularity all components would be active—was shown to be false.

In ecology, the prevailing view before chaos theory was that the effects regulating population density would, in the absence of other factors, keep a population at some constant level, and that the irregular fluctuations actually seen in many natural populations resulted from unpredictable ups and downs in various environmental influences. Again, this view was overturned by chaos theory.

In chemistry, it was imagined before the advent of chaos theory that the course of a chemical reaction is always predictable. However, it has been shown that some catalytic reactions in both inorganic and organic chemistry can behave in apparently random ways, and this can now be understood in terms of chaos theory. This has been shown to be relevant for understanding the biochemistry of the nervous system.

The study of stability has been transformed by chaos theory, with important practical consequences relating to the design of ships, for example. With gradual changes in parameter, attractors can bifurcate, the attractor splitting or simply disappearing. In studies of how ships capsize, it has been observed how 'integrity measures', to quantify the margin of safety between an attractor and the basin boundary, become eroded when fractal fingers of the basin boundary suddenly intrude deep into the basin of attraction.

In astronomy it has been shown that the orbits of asteroids moving under the gravitational effects of the Sun and Jupiter, at the 3:1 Jovian resonance, could undergo large chaotic variations in orbit. This has led to studies of the predictability of the planets as a whole, and it is now believed that Earth's orbit itself is chaotic.

One of the most remarkable applications of chaos theory is in quantum chaology, a discipline on the borders between quantum theory and classical theory in which questions are asked about how quantum systems become chaotic as they reach the classical limit. This area of work has developed links with pure mathematics—the existence of certain quantum systems with chaotic classical counterparts depends on the truth of the Riemann hypothesis.

In mathematics, the long-standing problem of proving the normality of certain fundamental irrational numbers such as π has been shown to be equivalent to proving that the attractors of certain chaotic dynamical systems have 'uniform' statistical properties in state space. There is renewed hope that some such long-standing problems in number theory can be proved by using chaos theory.

Chaos theory has had an enormous impact in economics. One of the key areas of interest here is the analysis of time series. Can time-series analysis distinguish between the existence of a low-order attractor and high-order noise? Here Takens's embedding theorem has been used extensively to find low-order structure (and hence finite-time predictability) in economic time series.

Chaotic control theory, in which the behaviour of chaotic systems is controlled by externally perturbing the system, ensuring that the state is close to the system's stable manifold, has numerous applications in neurology and cardiology, for example.

Finally, I shall comment on meteorology and climatology. Here I dwell on the first paragraphs of the quote at the beginning of this section.

If one takes a dynamical system written here as $\dot{\boldsymbol{X}}=\boldsymbol{F}(\boldsymbol{X})$ and assumes that the initial state is not perfectly known, then the probability distribution $\rho(t)$, which describes the evolution of error or uncertainty, satisfies a Liouville equation

$$\partial\rho/\partial t+\nabla\cdot(\dot{\boldsymbol{X}}\rho)=0.$$

Figure 1 shows solutions of the Liouville equation made by ensemble integrations of $\dot{\boldsymbol{X}}=\boldsymbol{F}(\boldsymbol{X})$ based on the Lorenz (1963) equations (see above) for some assumed $\rho(0)$ of initial error. Because $\boldsymbol{F}$ is nonlinear, the growth of small perturbations is necessarily dependent on flow. For example, there is little evidence of the chaotic nature of the system if one looks at the

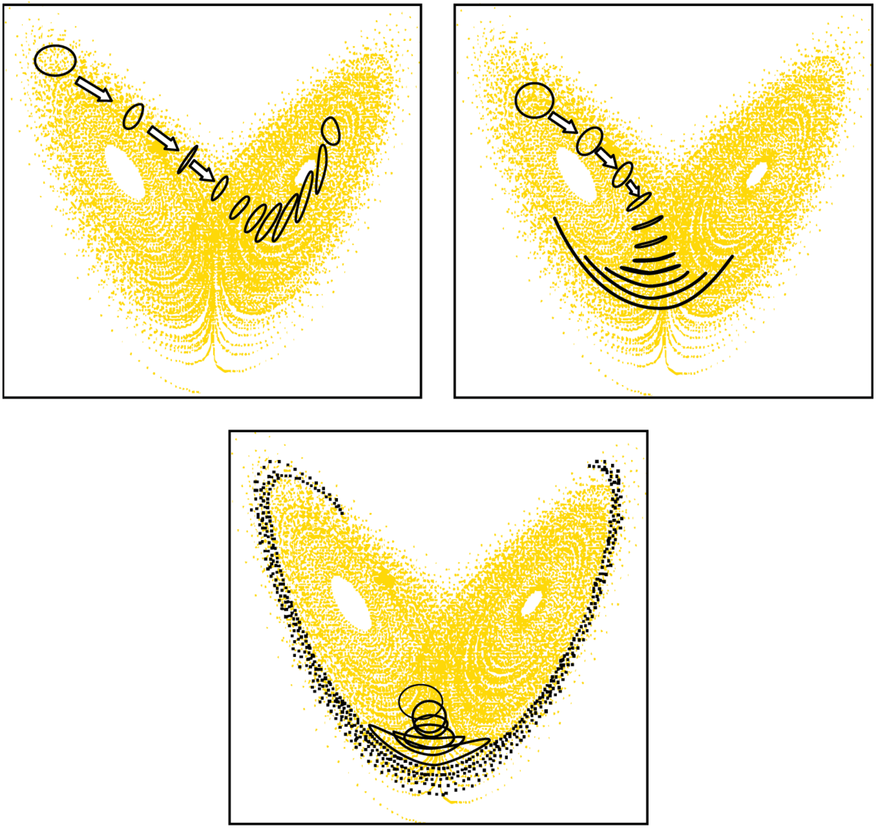

Figure 1. The scientific basis for ensemble forecasting illustrated by a Monte Carlo solution of the Liouville equation for the Lorenz (1963) system (2), showing that predictability is dependent on flow in a nonlinear system. (Online version in colour.)

growth of initial error in the top left panel. By contrast, the growth of error in the bottom panel is explosive. As a result of flow-dependent instabilities, the weather can also be exceptionally unpredictable from time to time.

As Lorenz comments in the quote above, over the past 20 years this ensemble method has been applied in weather prediction to provide flow-dependent estimates of the predictability of the flow, and hence the confidence that a forecast user can have in the prediction. When the flow is particularly chaotic, no definite forecast can be given. In these circumstances, the most reliable type of prediction is one in which outcomes are expressed probabilistically.

This ensemble forecast method is now being applied in predictions of climate change. This is particularly important for regional predictions of climate change, especially for variables such as precipitation or storminess. Such regional predictions are particularly important for climate adaptation studies: should we be investing in better flood defences on the one hand, or in more reservoir capacity on the other?

For predictions of climate change, uncertainty arises not only in the initial conditions but also in the computational representation of the equations of motion. Recently, work has begun to represent such model uncertainty by using stochastic representations of unresolved

Figure 2. Ed with his wife, Jane, in the White Mountains of New Hampshire.

processes. This development was anticipated many years earlier by Ed (8): 'I believe that the ultimate climate models … will be stochastic, i.e. random numbers will appear somewhere in the time derivatives.'

The impact of Ed's work in the field of predictability of weather and climate, both theoretical and practical, is described in Palmer & Hagedorn (2006).

Personal details

Ed met his wife-to-be, Jane Loban, at the MIT department where they were both working, and they were married in 1948 (figure 2). Jane was herself an artist and an aeroplane pilot; indeed, she had a pilot's licence before she could drive a car! They had a very happy marriage.

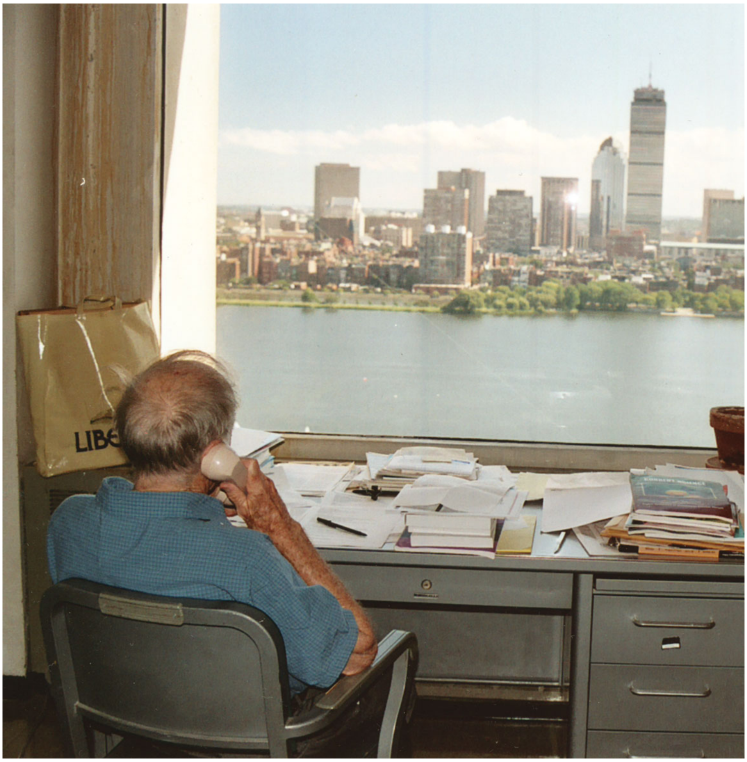

Figure 3. Ed at work in his MIT office. (Online version in colour.)

When asked whether, if he could start his life over again he would do anything differently, Ed replied that although it is not certain he would necessarily become a meteorologist, he would certainly have married the same girl! However, having realized the logical conundrum raised by this comment given the ubiquity and unpredictability of chaos, Ed added the rejoinder: '… if I met her!' They had two daughters and a son. Ed loved hiking in the mountains of New England, and, during summer visits to the National Center for Atmospheric Research, climbed many of the big mountains of the Rockies. He was climbing the New England mountains just months before his death.

Ed spent his career at MIT (figure 3), but with many sabbaticals and visits to institutes around the world. He made several extended visits to my own institute, the European Centre for Medium-Range Weather Forecasts, where he became fascinated with studying the practical levels of skill of operational weather forecasts and comparing them with theoretical estimates (9).

Ed received many honorary degrees and awards. Among the awards, Ed won the Crafoord Prize in 1983, the Kyoto Prize in 1991 and the International Meteorological Organization Prize of the World Meteorological Organization in 2000. Ed was a Fellow of the National Academy of Sciences, and a foreign or honorary member of many academies of science (including the Royal Society).

On top of this, Ed was universally recognized as a great teacher; his lectures were always crystal clear and self-sufficient. In fact Ed was awarded the 'Best Teacher Award' by the MIT

Graduate Student Council for so many consecutive years that it was decided to retire his name from future considerations! Over his career Ed supervised 22 doctoral and 27 master's students.

When Ed died aged 90 years in April 2008, media around the world announced that the 'pioneer of chaos theory' had passed away. Ed, himself a very modest person, would no doubt have been rather bemused, even puzzled, by all this attention.

Awards and honours

1962	Fellow, American Academy of Arts and Sciences
1963	Clarence Leroy Meisinger Award, American Meteorological Society
1965	Fellow, American Meteorological Society
1967	I.M.O. Lectureship, World Meteorological Organization
1969	Carl Gustaf Rossby Research Medal, American Meteorological Society
1973	Symons Memorial Gold Medal, Royal Meteorological Society
1975	Fellow, National Academy of Sciences
1981	Honorary Fellow, Indian Academy of Sciences
	Member, Norwegian Academy of Science and Letters
	Foreign Associate, Academy of Sciences, Lisbon
1983	Holger and Anna-Greta Crafoord Prize, Royal Swedish Academy of Sciences
1984	Honorary Member, Royal Meteorological Society
1988	Foreign Member, USSR Academy of Sciences
1989	Elliott Creson Medal, Franklin Institute
1990	Foreign Member, Royal Society of London
	Honorary Member, American Meteorological Society
1991	Kyoto Prize, Inamori Foundation, Kyoto
1992	Roger Revelle Medal, American Geophysical Union
1995	Louis J. Battan Author's Award, American Meteorological Society
2000	I.M.O. Prize, World Meteorological Organization
2004	Buys Ballot Medal, Royal Netherlands Academy of Arts and Sciences

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