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History and Epistemology of Models: Meteorology (1946–1963) as a Case Study

Amy Dahan Dalmedico

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Abstract

This paper focuses on scientific practices and problems of modeling in the case of meteorology. This domain is especially interesting because of its crucial influence on the conception of mathematical models and its links with numerical instability, computers, and the science of chaos and dynamic systems. All of these questions involve for their solution a considerable mathematical apparatus which was developed by several mathematicians.

An early example is von Neumann's and Charney's Princeton Meteorological Project in the period 1946–53 which ended with daily numerical prediction in less than 2 hours. After this stage, the questions of long-range forecasting and general circulation of the atmosphere became of greater importance. The late 1950s saw the emergence of an alternative: were atmospheric models used mainly for prediction or understanding? This controversial debate in particular occurred during an important colloquium in Tokyo in 1960 which gathered together J. Charney, E. Lorenz, A. Eliassen, and B. Saltzman, among others, and witnessed discussions on statistical methods for predictions and/or maximum simplification of dynamic equations. This phase ended in 1963 with Lorenz's seminal paper on "Deterministic non periodic flows."

Introduction

In the history of the relationship between mathematics and the natural world, one may discern a transition from an era of applied mathematics best associated with the expression "mathematization" (of physical phenomena) to a new era in scientific practice which, albeit appearing in the 1930s, was mainly developed after the Second World War and which is associated with expressions such as "modeling." In the aftermath of the war, important transformations occurred in both the social and the technological realms that fostered the development of a mathematical understanding of the world. On the one hand, as John von Neumann's career aptly symbolizes, the status of the mathematician changed significantly.[1] The image of the mathematician more or less locked into his

[1] These questions will not be developed here. See Dahan Dalmedico [1996].

academic milieu was replaced by that of an expert close to the political power and who was the ‘manager’ of big scientific and technological projects. On the other hand, with the development of increasingly powerful computational technologies, new possibilities and challenges arose which gave rise to innovative modeling practices. Epistemological tensions between statistical and deterministic models were made manifest by these new practices. In particular, the alternative between understanding and operating or predicting became one of the most important aspects of science in the second half of the century, in physics (as discussed by Paul Forman in the case of quantum electrodynamics), but also in applied mathematics.

I have selected the case of meteorology for the present study because, in my opinion, it is particularly emblematic of the shift from mathematization to modeling. Set up in part by von Neumann immediately after the end of World War II, the Princeton Meteorological Project was explicitly intended as a means of transforming the social settings in which research was conducted from an individual to an essentially collective endeavor. Moreover, meteorology was among the first scientific disciplines where the advent of numerical methods transformed the mathematical practice of its scientists. As William Aspray wrote in his biography of von Neumann, the “computer almost transformed meteorology into a mathematical science.”[2] Therefore, meteorology made the tensions, between statistics and dynamics, between prediction and understanding, especially acute. One may even argue that it was in the conflict between predicting (and indeed controlling) the weather and understanding the atmospheric system that these epistemological tensions initially surfaced most forcefully. This crucial role of computers and numerical simulations merits further examination.

I will first briefly sketch what meteorology and meteorological practices were like during the early 20th century, before the computer era. Then, I deal with the beginnings of von Neumann’s Electronic Project and his Meteorological Numerical Project. In these historical accounts, I rely mainly on W. Aspray’s and F. Nebecker’s books.[3] However, I focus on the epistemological aspects of modeling practices in order to better illuminate the drastic changes which occured in the late 1950’s and which I discuss in the remainder of the article.

I. Meteorology before the computer era

Up until the middle of the 20th century scientific interest in the weather involved three kinds of activity:

1) An empirical activity consisting of recording observations, and then trying to infer something from the recorded data. Generally, this *empirical tradition* was mainly interested in the production of an “average weather” and was characterized as a descriptive science – called ‘climatology’ – mainly based on weather statistics.
2) A theoretical activity striving to explain atmospheric phenomena on the basis of general principles. Using the laws of physics as its starting point, this *theoretical*

[2] Aspray [1990], p. 152.
[3] Aspray [1990], Nebeker [1995].

tradition established dynamical meteorology as a scientific discipline. In composing their treatises, however, theorists seldom used the large amount of available meteorological data.

3) A practical activity concerned with weather prediction. Forecasters based their predictions on only a small amount of data and hardly any theory at all. Hence their work was regarded by the two other groups as unscientific. More an art than a science, forecasts were founded on individual experiences, a few elementary statistical relations, and some qualitative physical arguments, rather than on theory or the application of general methods and procedures. The *synoptic tradition*, as it was called, studied some characteristic structures by means of which the atmosphere could be analyzed. Thanks to their "know-how", synopticians were supposedly able to recognize how these structures changed and to detect signals announcing their appearance or disappearance.

Connections among these three separate traditions became stronger and more numerous in the first half of the 20th century. Transformations occurred, which later led to their unification. But, although several attempts were made to introduce objective methods based on theoretical foundations, until 1950 no one was able to match the success of the earlier subjective methods of forecasting. No significant increase in the accuracy of short-range predictions resulted. Unification hinged mainly on the new availability of fast computing machines.

Two men are particularly important during this first period: Vilhelm Bjerknes and Lewis Fry Richardson. In 1903 Bjerknes, a Norwegian physicist who had turned to meteorology, became an advocate for the computational approach to weather forecasting. He wished to bring the full range of observations together with theory in order to predict the weather. He was the first to identify key-concepts such as *baroclinicity* and *barotropicity*.[4] In his 1904 program, Bjerknes wrote: "We forgo any thought of analytical methods of integration and instead, pose the problem of weather prediction in the following practical form: *Based upon the observations that have been made, the initial state of the atmosphere is represented by a number of charts which give the distribution of seven variables from level to level in the atmosphere. With these charts as the starting point, new charts of a similar kind are to be drawn which represent the new state from hour to hour.*"[5] However, no analytic methods for solving the physical equations involved in this procedure were known at that time, and so Bjerknes was forced to rely mainly on graphical methods.

Richardson's 1922 program

The next person to try to carry out Bjerknes's program was the English scientist Lewis Fry Richardson. Perhaps, as Aspray suggested, it was the hope of applying a

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[4] A model is *barotropic* when pressure depends on the location only, not on the local height; it is *baroclinic* when a vertical pressure component is incorporated which takes into account the mixing of the air and the loss of potential energy.

[5] Aspray [1990], p. 126.

numerical method for solving partial differential equations (which he had discovered while working in industry) that led Richardson to turn towards meteorology. Shortly after World War I, he devised an algorithmic scheme for weather prediction based on this method. In 1922, Richardson published his book *Weather Prediction by Numerical Process*, which became well known. It opened with an analogy between astronomy and meteorology, leading up to the following critical remark: "The forecast is based on the supposition that what the atmosphere did then [at a previous time when the weather conditions were similar], it will do again now.... *The past history of the atmosphere is used, so to speak, as a full-scale model of its present self.*" Richardson went on: "But – one may reflect – the *Nautical Almanac*, that marvel of accurate forecasting is not based on the principle that astronomical history repeats itself in the aggregate. It would be safe to say that a particular disposition of stars, planets, and satellites never occurs twice. Why then should we expect a present weather map to be exactly represented in a catalogue of past weather?".[6] Here Richardson implicitly referred to the dominant synoptic point of view of which he strongly disapproved. Following Bjerknes's program, he advocated the use of differential equations and analytic methods.

Richardson set out to formulate seven equations that would completely determine the behavior of the atmosphere given its initial state. The first three equations are essentially Newton's law (force equals mass times acceleration). The fourth equation says that if the mass-density decreases at a place, than matter must have moved away. The fifth says the same for the water content. The sixth says that an addition of heat must either raise the temperature or do work or both. There is also a seventh equation (not written here) which is a combination of Boyle's law (that pressure is inversely proportional to volume) and Charles' law (that volume is directly proportional to absolute temperature) with an allowance made for the presence of water vapor.

In the following, we summarily describe Richardson's procedure, which became canonical much later.[7]

The six equations are:

$$\frac{\partial u}{\partial t} = -u\frac{\partial u}{\partial x} - v\frac{\partial u}{\partial y} - w\frac{\partial u}{\partial z} - fu - \frac{1}{p}\frac{\partial p}{\partial x}$$

$$\frac{\partial v}{\partial t} = -u\frac{\partial v}{\partial x} - v\frac{\partial v}{\partial y} - w\frac{\partial v}{\partial z} - fu - \frac{1}{p}\frac{\partial p}{\partial y}$$

$$\frac{\partial w}{\partial t} = -u\frac{\partial w}{\partial x} - v\frac{\partial w}{\partial y} - w\frac{\partial w}{\partial z} - g - \frac{1}{p}\frac{\partial p}{\partial z}$$

$$\frac{\partial \rho}{\partial t} = -u\frac{\partial \rho}{\partial x} - v\frac{\partial \rho}{\partial y} - w\frac{\partial \rho}{\partial z} - \left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z}\right)$$

$$\frac{\partial p}{\partial t} = -u\frac{\partial p}{\partial x} - v\frac{\partial p}{\partial y} - w\frac{\partial p}{\partial z} - \frac{C_p}{C_v}p\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z}\right)$$

[6] *Ibidem*, p. 127. Emphasis mine.
[7] *Ibidem*, pp. 124–127. See also Nebecker [1995], pp. 65–67.

$$\frac{\partial T}{\partial t} = -u\frac{\partial T}{\partial x} - v\frac{\partial T}{\partial y} - w\frac{\partial T}{\partial z} - \frac{RT}{C_v}\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z}\right)$$

where u, v, w, are components of the fluid velocity in the x, y, z directions; C_p represents the heat coefficient at constant pressure, ρ the local mass density of the fluid, f the Coriolis parameter, p the ambient pressure, g the constant of gravity, and, finally, C_v the specific heat at constant volume. T is the absolute temperature, R is the gas constant related to Avogadro's number.

These equations imply that the time derivatives can be expressed in terms of spatial derivatives.

– Observational data for all independent variables in the equations are collected at an initial time t at each point of a finite grid evenly distributed over a region of the atmosphere.
– The method of finite differences is used as an approximation of spatial derivatives, and the time derivatives are then computed by means of the equations above.
– In this way, the values of the parameters at each point of the grid are extrapolated to a time shortly thereafter, $t + \Delta t$.
– Using the extrapolated data, this procedure is then repeated and values at $t + 2\Delta t$ are evaluated. This process is repeated again and again, as many times as necessary to extend the prediction to a desired time interval.

Unfortunately, for a "forecast" extending over a 6-hour period, Richardson needed six weeks of intensive labor in order to complete the computations! Needless to say, this was discouraging. Richardson imputed the impracticability of his method mainly to the dramatic slowness of the computation.

Numerical instability

Before World War II, only one relevant important advance took place concerning the numerical methods for solving partial differential equations. It was presented in a paper written in 1928 in Göttingen by Richard Courant and his two young assistants Karl Friedrichs and Hans Lewy.[8] Their initial idea was to use a method of numerical approximation (the method of finite differences) in order to demonstrate the existence of solutions of partial differential equations. Elaborating this idea, they discovered a new phenomenon – called *numerical instability* – which would later turn out to be exceptionally important in the development of numerical methods. When one replaces a differential equation by a set of numerical equations however close to the given equation, solutions of these approximating equations may have nothing to do with the solutions of the original equation.[9] The three mathematicians showed that the solutions of the finite-difference equations converged towards the solution of the differential equation only if the grid of the finite-difference equations

[8] Courant, R., Friedrichs, & Lewy, [1928].
[9] Goldstine [1972], p. 288.

satisfied the following condition (later to be called the ‘Courant condition’): the ratio $\Delta t/\Delta x$ of the time step to the space step is less than a number C depending only on the differential equation and the initial conditions. If on the contrary the Courant condition was not met, they showed, errors would increase without limit. The paper contained a rigorous criterion for stability in the cases of hyperbolic and parabolic equations with constant coefficients in one dependent, or two independent, variables. Later, von Neumann generalized this last condition in a heuristic, rather than rigorous, fashion, and although he lectured on it several times he never published the result.[10]

World War II: a “near discontinuous change” in meteorological practice

During the Second World War, forecasts were vital military information and critical for the planning of military operations. The secrecy and reliability of weather predictions could have strategic importance, such as, for example, the choice of the day for Operation Overlord. World War II created a huge demand for meteorology and hence for meteorologists. Major changes that occurred during the war included:[11]

- New subjects and interests emerged; when planes flew at much greater heights, forecasters were asked about clouds, ice formation, fog and high-level winds. As a result of the new abundance of upper-air data, physics became more relevant to forecasters’ work. However, attempts to increase the range of the forecasts to more than two or three days were recognized as hardly realistic.
- About six or seven thousand meteorologists were trained in various laboratories during the war. The total strength of the Air Weather Service reached 19,000 while the Navy’s Aerological Service employed 6,000 more. An urgent need to train a large number of people very rapidly may have indirectly contributed to making meteorology more mathematical, more formalized, and also more theoretical.
- Observational methods and weather data records were standardized.
- The use of punched-card machines, and the automation of data-processing in meteorology increased dramatically.
- The meteorological forecasting practice became a group activity focusing on specific tasks.
- Important research topics changed, and objective forecasting was favored over the synopticians’ subjective methods.

[10] Lectures of von Neumann at Los Alamos Laboratories in February 1947 and letters from von Neumann to the mathematician Werner Leuter (October, 15, 1950, and November, 10, 1950) mentioned by Aspray [1990], p. 286 f. notes 27 and 28. See also von Neumann & Richtmyer [1950], and Goldstine [1972].

[11] Nebeker [1995], pp. 111–132; Saltzman [1967]; Thompson [1983].

Von Neumann's electronic and meteorology projects

In the 1940s, the mathematician John von Neumann initiated a significant rearrangement of interests concerning hydrodynamics, computers, and numerical analysis. Because of his work at Los Alamos on nuclear reactions, he was convinced that hydrodynamics was of crucial importance to physics and mathematics, and he was one of the first to realize that it required radically new developments in computational resources. In 1945, in a memorandum sent to O.Veblen, von Neumann wrote:

> A further experience which has been acquired during this period [World War II] is that many problems which do not prima facie appear to be hydrodynamical necessitate the solution of hydrodynamical questions or lead to calculations of the hydrodynamical type. It should be noted that it is only natural that this should be so since hydrodynamical problems are the prototype for anything involving non-linear partial differential equations, particularly those of the hyperbolic or mixed type, hydrodynamics being a major physical guide in this important field, which is clearly too difficult at present from the purely mathematical point of view.[12]

Hydrodynamics appeared to von Neumann as the prototype for non-linearity. While electronic computers existed merely in his imagination, he insightfully declared in 1945: "Many branches of pure and applied mathematics are in a great need of computing instruments to break the present stalemate created by the failure of the purely analytical approach to non-linear problems."[13] In 1946, a program for the building of an electronic computer – the Electronic Computer Project – was launched at the Institute for Advanced Study at Princeton under his direction.[14] As a large-scale application of the Electronic Project, von Neumann chose numerical meteorology, which he judged strategic. He succeeded in convincing the Navy to support the project.

On August 29 and 30, 1946, von Neumann organized a conference in Princeton in order to acquaint the meteorological community with the electronic computer being built at the Institute and to solicit their advice and support in designing research strategies. A consensus emerged that six areas merited study, the three most interesting of which were: numerical methods for solving important differential equations by means of the computer, tropical hurricane theory, and forecasting by direct numerical methods.[15] During the first two years of the Meteorology Group set up after von Neumann's conference, a series of exploratory investigations was conducted. The main one concerned the system of hydrodynamic equations of atmospheric flow and, because it became apparent that the basic equations were ill-suited to numerical as well as analytical solutions, their systematic simplification for direct integration was undertaken.

Albert Cahn and Philip Thompson simplified the mathematical model of atmospheric flow so that it retained important meteorological phenomena while *filtering out* non-meteorological ones, such as gravity and sound waves. This *filtering process* had already been implicitly practiced by meteorologists when formulating their theories. In the first

[12] Memorandum, 26th March 1945. Von Neumann, Works VI, pp. 357–359.
[13] Works, V, p. 2.
[14] See Aspray [1990], pp. 49–94 and Goldstine [1972], pp. 208–234.
[15] Aspray [1990], p. 17.

two years of the project, Paul Queney and Thompson introduced *filtered models* which they checked against meteorological data.[16] But, as Aspray has noticed, "Jules Charney was the first to make this procedure explicit and recommend it as the most promising approach for numerical meteorology".[17] In 1947, Charney used a metaphor which was to become famous: "This leads us to the next problem, namely, how to filter out the noise. Pardon me, but let us again think metaphorically. *The atmosphere is a transmitter. The computing machine is the receiver*.... Now there are two ways to eliminate noise in the output. The first is to make sure that the input is free from objectionable noises, or the second is to employ a filtering system in the receiver."[18] In fact, when Charney took the lead of the Meteorological Project, this metaphor expressed the basic idea of his methodology of atmospheric modeling.

II. Von Neumann's and Charney's program (1946–53)

With Jules Charney's arrival in the summer of 1948, the Meteorological Project took a new direction: the work of the group became much more unified, goal-oriented, and was summarized in two annual reports. Under Charney's directorship, resources and earlier, parallel investigations were focused through a single approach: "to consider a hierarchy of 'pilot problems' embodying successively more and more of the physical, numerical, and observational aspects of the general forecast problem." The objective of the project was clear: "the development of a method for the numerical integration of the meteorological equations which is suitable for use in conjunction with the electronic computing machine now under construction at the Institute for Advanced Study."[19]

The methodology of "theory pull"

Charney and his colleagues adopted a progressive process through which algorithms could be implemented with small incremental steps. The algorithms consid-

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[16] The basic equations such as the set of equations Richardson used can be said to describe too much since they admit solutions that correspond to types of motion that are not meteorologically significant, especially higher-frequency wave motions such as sound waves, gravity waves, slow inertial waves etc. The Report of Progress of the Meteorology Project for the year 1947 concluded: "... it might simplify matters considerably if those equations were somehow informed that we are interested in certain kind of atmospheric behavior – i.e, the propagation of large-scale disturbances. This is tantamount to constructing a 'mathematical filter' to remove 'noise' and otherwise unwanted regions of the spectrum." The process of filtering consists in separating the meteorologically significant solutions from the insignificant ones.

[17] Aspray [1990], p. 139.

[18] Letter to P. Thompson, February, 12, 1947, quoted by Aspray [1990], p. 300, note 73; emphasis is mine.

[19] Progress Report of the Meteorology Group at the Institute of Advanced Study, July 1, 1948 to June 30, 1949, JCMI. quoted by Aspray [1990], p. 139.

ered also depended on computer feasibility, which itself was continuously evolving. One started from a simplified model whose behavior could be computed; one compared how the predictions of this model fitted observed phenomena; then one altered the model, generally by adding a physical factor whose previous exclusion, the scientists thought, was the cause of important observed distortions. Charney called this approach "theory pull".

In 1949, Charney suggested a "*two-dimensional barotropic*" model[20] as the group's object of study and he studied numerical properties of linearized barotropic equations in order to anticipate the numerical integration of nonlinear equations. Charney was aware that Richardson's algorithm does not satisfy Courant's condition so that its repeated application was computationally unstable and could not give useful results. But he found that by filtering out most atmospheric motions he made it much easier to satisfy the Courant condition, which in this case becomes: the time increment must be less than the time required for a wave impulse to be transmitted from one grid point to another. Based on the principle of conservation of vorticity, Charney's model retained only observed variables while filtering out waves traveling faster than what was allowed by Courant's condition (sound and gravity waves in particular). Thompson later explained:

> Charney's 1947 formulation, later known as the quasi-geostrophic model [motion is said to be quasi-geostrophic when there is an approximate balance between the pressure-gradient force and the Coriolis force], *simultaneously skirted two major difficulties*: first, it imposed much less stringent conditions for computational stability, and second, it did not demand that the horizontal divergence or accelerations be computed as small differences between large and compensating terms, each of which is subject to sizable percentage errors. These features alone evaded the two fundamental difficulties inherent in the practical application of Richardson's method.[21]

The integration was done on the ENIAC computer, and numerical stability was studied according to the methods introduced in Courant, Friedrichs, and Lewy's paper.

In 1950, Charney, Ragnar Fjötoft, and von Neumann published their first report, which contained an analysis of the equations they used and a summary of predictions over a period of 24 hours, for four chosen days in 1940. Earlier ENIAC computations and other studies indicated that the atmosphere behaved barotropically over long time periods. The above model therefore performed satisfactorily during these periods. Based on vorticity conservation, however, it was unable to take into account the formation, intensification, or dissipation of storms. Further attempts were made to improve the model in this direction.

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[20] As explained in note 4, a model is *barotropic* when pressure is considered constant and *baroclinic* when a vertical component is incorporated which takes into account its variation. In his 1949 model, Charney assumed (1) a uniform density for the atmosphere, (2) purely horizontal motion, (3) the coincidence of constant density and constant pressure surfaces (barotropy), (4) an approximate balance of the pressure gradients produced and the Coriolis force (quasi-geostrophy), (5) the constancy over time of the vorticity of a fluid element around its vertical axis (vorticity conservation), (6) parallel streams of wind at all levels, and (7) the thermodynamic closure of the system. See Aspray [1990], p. 301.

[21] Thompson [1983], p. 760.

In 1951, another meteorologist on the team, Norman Phillips, introduced a *baroclinic model*, formed by two barotropic, incompressible, and homogeneous levels of different densities, which provided almost the same features as a fully three-dimensional model. Although the model predicted cloudiness and precipitation with reasonable accuracy, the improvements were not judged satisfactory by von Neumann and Charney, because this two-layer model had not predicted the rapid development of the Thanksgiving weekend storm. Charney then used a *three-level model* which correlated better with predictions. In 1953, additional baroclinic forecasts were made with three-, five- and even seven-layer, models to try to take into account certain kinetic and dynamical effects such as, among others, the horizontal-vertical vorticity conversion.

On August 5, 1952, von Neumann called a meeting at Princeton, which gathered members of the Weather Bureau, the Navy, and the Air Force. His goal was to assess the operational value of numerical meteorological prediction. In order to reach 24-hour forecasts over the whole territory of the United States, he advocated the use of a general baroclinic model which he thought could be valid over periods up to 36 hours. He argued that altogether the total time required for collecting and preparing data, and computing, treating, and printing results should not exceed 12 hours. Less than one year later this aim was achieved. The group's report stipulated that "the trend in the application to short-range prediction of the quasi-geostrophic model equations had become so predictable that this phase of the work had ceased to be – for the Project – a matter of major scientific interest."[22] Henceforth, kept busy by his other responsibilities (in particular as a member of the Atomic Energy Commission), John von Neumann no longer participated in the Meteorology Group.

The computer: an inductive machine for physical models in meteorology?

After his first important successes, Charney wrote in 1952 that: "The philosophy guiding the approach to this problem [of numerical prediction] has been to construct a *hierarchy of atmospheric models of increasing complexity*, the features of each successive model being determined by an analysis of the shortcomings of the previous model."[23]

The "theory pull" methodology led to a series of "*physical models*" of the atmosphere, in the sense that they were based on simplified *physical* hypotheses and yet they were meant to copy the atmospheric behavior. Thompson's early filtered models, Charney's two-dimensional barotropic model, Phillips' baroclinic model, Charney's three-level model, additional baroclinic models etc, belonged to this type of physical models; they were mainly developed for predicting the weather. In the first stage of the computer era, the trend had been to adjust oversimplified physical models of atmospheric behavior by making them more complicated. As a *giant calculator*, the computer made possible the treatment of increasingly involved equations that provided better descriptions of the

[22] *Meteorology Project*, Quaterly Progress Report, July 1, 1953 to March 31, 1954 (Jule Charney Papers, MIT Archives).

[23] *Meteorology Project Report* quoted in Aspray [1990], p. 302.

atmosphere and therefore gave hope for more precise predictions. But meteorology was also highly constrained – and this is still the case – by electronic computers. Their performance capacity, the size of their memories, the restrictions on arithmetic operations, the selection of numerical methods convenient for electronic computations, and their limited dependability constituted unavoidable factors that transformed meteorology into an engineering science.[24]

In a lecture addressed to the National Academy of Sciences in 1955, Charney analyzed the role played by the computer in his discipline:

> The advent of the large-scale electronic computer has given a profound stimulus to the science of meteorology. For the first time, the meteorologist possesses a mathematical apparatus capable of dealing with the large number of parameters required for determining the state of the atmosphere and of solving the nonlinear equations governing its motion. Earlier, for want of such an apparatus, both the forecaster and the investigator were forced to content themselves with such highly oversimplified descriptions or models of the atmosphere that forecasting remained largely a matter of extrapolation and memory, and dynamical meteorology a field in which belief in a theory was often more a matter of faith than of experience. Needless to say, the practicing meteorologist could ignore the results of theory with good conscience.[25]

Charney went even further. He envisioned new and promising roles for the computer:

> The radical alteration that is taking place in this state of affairs is due not merely to the ability of the machine to solve known equations with known initial and boundary conditions but even more to its ability to serve as an inductive device. The laws of motion of the atmosphere, especially as they concern the dynamics of the largest motions and their energy-exchange processes, are very imperfectly known. The machine, by reducing the mathematical difficulties involved in carrying a physical argument to its logical conclusion, makes possible the making and testing of physical hypotheses in a field where controlled experiment is still visionary and model experiment difficult, and so permits a wider use of inductive methods.[26]

Therefore, Charney suggested that the computer could be used as an *inductive machine* which could test selected physical hypotheses. A few years later, this same idea would lead to a very different kind of model in meteorology: the so-called "*laboratory models*". These differed from the physical models in that they didn't copy atmospheric behavior in order to predict but instead generated theoretical situations in order to understand specific atmospheric phenomena. We will study this point in the third part of this paper.

E.N. Lorenz reconstructed, in 1960, the entire procedure used for obtaining the equations of numerical weather prediction from the fundamental laws governing the atmosphere. He looked at the system of these equations from both a physical point of view (gauging the significance of different physical hypotheses and that of their simpli-

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[24] See Phillips [1960].
[25] Charney, J. [1955] in Aspray [1990], p. 152.
[26] Charney, J., *Ibidem*, p. 153.

fication) and from a mathematical point of view (studying the dependency of variables, the separation of the equations in different groups, the role of differential operators, etc). He distinguished the following steps:[27]

– For a dry atmosphere, the physical laws determine the well-known set of six equations (Richardson's equations cited above).
– The equation of vertical motion – the third equation of Richardson's system – is first discarded, and replaced by the so-called "hydrostatic equation". Slightly modified with the help of the time derivative of the hydrostatic equation, this system is generally called the *primitive equation*.
– The new system is then expressed in terms of two independent variables: pressure and height. Horizontal wind components are expressed in terms of vorticity and divergence (a differential operator applied to the movement).
– This last equation is then discarded and replaced by the *equation of balance* obtained by dropping out all terms containing a divergence from the previous equation. Moreover, it is often more convenient to omit certain additional terms from the equation of balance, reducing it to the *geostrophic equation*.
– The equation of balance and the geostrophic equation are often called *filtering approximations*, since they eliminate the occurrence of certain waves that can occur in systems governed by the primitive equations, but are often considered irrelevant.
– Finally the vertical dimensions can be replaced by a small number of layers. Each function depending on time and on the three spatial dimensions is then replaced by several functions of time and two spatial dimensions.

It is important to emphasize that Lorenz' *logical reconstruction* has nothing to do with the *historical development* of the simplified equations (for example, the filtered equations appear here as a last step, whereas, historically, filtering was the first issue addressed). The historical development was determined by interactions among numerical results, observed and measured physical results, the availability of computational machines, and the availability and rapidity of computing methods. In fact, then, the development of the practice of modeling followed a very different route than might be assumed on the basis of logic.

III. The debate about models (from the late 1950s to 1963)

In the late 1950s, it became obvious that the so-called "filtered equations",[28] which made numerical weather predictions possible with the first computers, would no longer be able to provide predictions of as high a quality as was now required for long-range forecasting. Consequently, the general attitude was either to return to the primitive hydrodynamic equations, given the advent of more powerful computers, or to extend filtering

[27] E.N. Lorenz [1960a], p. 366. We summarize the big lines of this reconstruction to give an idea of the relationships between the different systems of equations (without entering into details) and to introduce their names.

[28] As we already explained, the filtering process had been practiced by meteorologists for a long time; after 1947 and Charney's metaphor, the expression 'filtered equations' became canonical.

principles; this second way led to so-called balance equations.[29] Both ways concerned what we have called above "physical models" of the atmosphere. But another attitude began to emerge, which raised the very question of predictability and introduced the use of "laboratory models." This last expression will be elucidated below.

Statistics and predictability

In the 1950s, statistical methods prevailed in many areas of science. In particular they were seen as appropriate tools for understanding disorderly phenomena. Although some well-known results of Henri Poincaré and George D. Birkhoff proved that the opposition between order and disorder was not so radical as had been thought, [30] statistical methods appeared to be the only adequate ones for studying such phenomena. This was the case not only in physics, mechanics (with the ergodicity concept), fluid mechanics (with Kolmogorov's statistical theory of turbulence elaborated in 1941), astrophysics (where Neymann, Scott, and Shana[31] studied the distribution of galaxies by statistical methods), dynamics of epidemics (Bartlett's work[32]), but also in biology and population genetics. A series of annual symposia at Berkeley on mathematical statistics provides a good picture of the spectrum of research from very different fields in which mathematical statistics was the primary method of investigation.

In meteorology, two trends in fact coexisted: the first privileged statistical methods, the second, deterministic dynamics. Meteorologists debated the attendant merits of both. Von Neumann's initial approach had been to use fast computers to solve dynamical equations of atmospheric movement. The race of the computer against time that he launched – to provide predictions of the weather 24 hours in advance in less than 24 hours – had succeeded. It seemed his optimism for accurate predictive analysis had paid off. In a certain sense, even if he was absolutely convinced that long-term forecasting was a meteorological illusion, his goal remained the mastery and control of instabilities, approximations, and errors in initial data and observations. These could – and should – be overcome by computing methods. Furthermore, von Neumann held on to the dream according to which the knowledge of the effects of a disturbance on general meteorological features would enable humankind to *control* the weather. Norbert Wiener, on the other hand, was critical of this approach. He was much more sensitive to randomness and believed it to be fundamentally impossible to gain this kind of weather control. Instead, he stressed that models needed to take into account from the outset the fact that information and knowledge would be always incomplete. Wiener had always been very

[29] This is explained in J.G. Charney [1962].

[30] Let us mention the difficulties raised by the interpretation of the Fermi-Pasta-Ulam numerical experiment. Ergodicity allowed the use of statistical methods, but the mixing between order and disorder revealed by this experiment resisted any mathematical formulation. At the International Congress of Mathematicians of Amsterdam in 1954, Kolmogorov's announcement of his result on dynamical systems – later known as the Kolmogorov-Arnold-Möser (KAM) theorem – would drastically change the picture. Fermi, Pasta & Ulam [1947] in Ulam [1974].

[31] *Proceedings* [1955], vol III, p. 75.

[32] Idem, vol IV, p. 81.

fond of formulating scientific problems in terms of time-varying statistical processes. He gave a mathematical theory of the Brownian motion, considered as a special prototype of these processes. He strongly advocated the use of statistical methods for all problems of prediction[33]; for meteorology, that meant formulating the problem of prediction in terms of statistical properties of past weather and not in terms of deterministic dynamics.

At the third Berkeley Symposium on mathematical statistics, Wiener cited Shakespeare in support of sensitivity to initial conditions:

> for want a nail, the shoe was lost.
> for want a shoe, the horse was lost.
> for want a horse, the rider was lost.
> for want a rider, the battle was lost.
> for want a battle, the kingdom was lost.[34]

Wiener suggested leaving exact computations to the exact sciences and adopting statistical methods in all other areas of science. He underlined "the very real possibility of the self amplification of small details in the weather map," but he did not know the origin of these instabilities nor their actual frequency any better than von Neumann. What opposed the two mathematicians, as Steve Heims has pointed out, was more a question of conviction, of metaphysics, or, let us say, of conception of world (Weltanschaung).[35] In particular, time and error play a different role in their respective works.

Like the majority of meteorologists, Edward Lorenz had for several years used statistical methods as he was involved in an experiment performed by the Statistical Forecasting Project at MIT. Lorenz thought that in meteorology: "philosophically, statistical prevision is more like synoptic meteorology than dynamical prediction, because it is founded more on observations of what happened in the past than on physical principles." The most common method in statistical forecasting, linear regression, required very long mathematical procedures in order to estimate the best values for its constants. But linear regression methods often failed to yield good weather forecasts; this happenned either because the weather was basically unpredictable from currently available initial data, or because linear methods were inadequate. During the MIT experiment, meteorologists generated a series of "weather maps" through the numerical integration of a set of nonlinear differential equations (i.e. by dynamical prediction), then, they attempted to *reproduce* these maps by means of linear regressions. On this basis, in 1960, Lorenz had reached the conviction that the statistical method was invalid. He presented his results during a Symposium held in Tokyo, which became a turning point for the community, and is particularly instructive for us[36].

Those who favored linear regression methods could draw on a theorem of Wiener's showing that if a statistically stationary system was 'deterministic', in the sense that it could be predicted exactly from the knowledge of its own present and past by some for-

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[33] During World War II, Wiener worked also on a statistical theory of prediction; he presented it at the International Congress of Mathematicians in 1950 at Harvard.

[34] Wiener [1956], p. 248.

[35] see Heims, [1980] pp. 116–162.

[36] Lorenz [1960b].

mula, then it could also be predicted exactly from its own present and past by some *linear formula*. The linear formula might involve an infinite quantity of past data and many terms might be required for a good approximation. From the meteorologists' standpoint, the important question was whether there was a nearly perfect linear formula involving data solely from a recent past. In the Statistical Forecasting Project at MIT, this question was tackled by investigating, using linear regression methods, the predictive quality of a numerical solution previously obtained from a set of deterministic equations. Since the investigators were ultimately interested in the weather, they chose, as their deterministic equations, one of the simplest models used in numerical weather prediction.

Lorenz and his colleagues took the geostrophic form of the two-layer baroclinic model (Charney's model).[37] They appended linear terms [representing heating, friction at the surface separating layers, etc]. They then simplified the equations so that the model was reduced to one of the more conventional two-layer models. The machine used by the Project was a small electronic computer (Royal McBee) that could solve 12 equations by an iterative process at the rate of one time step every 10 seconds. Retaining only the large scale features, the time-step was a six-hour increment during which the solution was computationally stable. Lorenz and his colleagues generated nearly six years of data in 24 hours of machine time. The results showed that one-day predictions were nearly perfect, two-day predictions acceptable, and four-day predictions terrible. Lorenz estimated that, applied to the real atmosphere, this linear regression would collapse in half a day. They observed large fluctuations with irregularities and random features in the graph.

Hence, this simple model of 12 equations used to test the linear regression method exhibited a *nonperiodic solution* roughly mimicking the evolution of the atmosphere. As a consequence of his work, Lorenz was convinced of the existence of deterministic systems governed by equations whose nonlinearity resembled that of the atmosphere, but which were not perfectly nor even approximately predictable by simple linear formulae. This failure was not a fatal blow to statistical methods: Lorenz merely concluded that better results than those obtained by purely statistical linear predictions would eventually have to be reached by some *nonlinear statistical* procedures or by the methods of dynamical weather prediction.

The Tokyo symposium: a turning point

Let us now focus on the very rich discussions which took place during the final panel session of the Tokyo symposium. They concerned mainly two issues, statistical methods and sensitivity of predictions to variations in inital data.[38]

First, the debate about statistical methods evolved. In a sense, Lorenz's work was a setback for statistical methods. The hope for long-range predictions on the basis of physical principles rested on the possibility that some aspects of the process remained predictable and that these aspects should be statistical properties. This seemed analogous

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[37] Lorenz [1960a].

[38] The discussion appeared in the *Proceedings* [1962], pp. 639–655.

to statistical mechanics (which led to the laws of thermodynamics without a detailed knowledge of the forces between molecules). In this case, *climatology* would be analogous to the equilibrium state of the system in statistical mechanics.[39] But for long-range weather predictions, one was not interested in climatology, but rather, as Arnt Eliassen (University of Oslo) pointed out, in *deviations* from general climatic patterns. There, statistics apparently did not work.

However, in another way, a spectacular comeback of statistics was in store. Eliassen mentioned as a particularly interesting field of research the possibility of *computing the climatology of an atmosphere*, thereby opening up the climatologists' general program which was mainly developed after the 1980s. Remarkably, he outlined this program as a very pure mathematical problem: "given a planet with specified properties, distribution of oceans and continents, elevations, insolations and so on; determine the distribution of climate." It should stressed that Eliassen here suggested not only new methods of mathematical modeling, but also a new *philosophy of modeling*, expressing an ideal about what science could supply. According to this conception, the role of mathematical modeling was considerably expanded. Eliassen further added:

> This should in principle be possible, from weather forecasting techniques by making forecasting for a long period of time and *making statistics*. This may become of importance for determining the climate of previous geological periods where the surface features of the earth were different, from what they are now, and it may also be of importance for determining changes in climate caused by various external and internal changes of the system, changes within the atmosphere or of the earth's surface or of the solar radiation. Since mankind is all the time changing the properties of our planet, there are, of course, already artificially produced changes of climate, and one is even thinking of producing such changes deliberately . It is vitally important that we shall be able to predict the effects before we try change the properties of the planet.[40]

Triggering a second subject of debate at the Tokyo symposium, George Platzmann asked Lorenz the reason why he assumed that the sensitivity of the prediction to a slight variation in initial data was in some way connected with the small number of parameters in the model. Lorenz answered that he was unable to provide a really satisfying answer and that it was a "matter of feeling".[41] In reading the proceedings, we may perceive the emergence of great perplexity: was there a hunch of an *unpredictability principle* in the forecasting problem? All meteorologists present at the meeting considered the question of possible limitations of a fundamental nature to weather prediction as "*philosophical*". Although they tried focusing debates on the practical aspects of the problem, these

[39] Climatology is the study of climate-systems. In climatology, one is mainly interested in the general circulation of atmosphere considered in statistical terms, and in time-scales much longer than in meteorology.

[40] *Ibidem*, p. 646.

[41] We know that Lorenz worked hard to arrive at this result: it is not at all by pure chance, as many popular books and papers would have it, that he discovered chaotic behavior. Many popular accounts greatly exaggerate, according to me, an anecdote according to which, Lorenz left his computer to get a cup of coffee and found chaotic solutions flashing on the computer screen, when he returned.

deeper questions constantly reemerged. Eliassen brought up the issue of baroclinic and barotropic instabilities and their importance. He noticed that these obstacles to numerical predictability were difficult to assess. It was impossible to compute the effect of a disturbance on the atmosphere since this latter was always disturbed by external noises. Eliassen declared: "The significance of this instability is not clear to me because we do not see in the atmosphere, disturbances developing from a very small initial disturbance on the straight current, but we rather see a fully disturbed field all the time and non-linear interactions between various components."[42] Starting from his 12-equation model, Lorenz had already tried to investigate this issue: among 40 different "errors" applied to his aperiodic solution – meaning that he ran the computation with 40 small deviations from the initial conditions – 39 very rapidly diverged. If this model in some sense represented the atmosphere, long-term predictability would definitively be an utopian illusion.

The crucial question was to assess to what extent the model represented atmospheric evolution. "Lorenz-like systems", as Charney then nicknamed them, had very few dimensions and therefore could only very roughly copy reality. Many people hoped that by adding a large number of degrees of freedom, one would get stability in the system and hence long-term predictability. At the time, Charney was still very optimistic[43]: "there is no reason why numerical methods should not be capable of predicting the life cycle of a single system," he declared, it is our models which have "fatal defects".

With the aim of overcoming these defects, Charney listed three sets of problems:

1) As to the purely mathematical computing techniques: the problem that the time intervals were much too small for the time scale of the phenomena. This problem was linked with the economy of machine computation, but Charney intuited that it would probably remain with meteorologists for all times since problems would grow as fast as the power of machines – an evolution which in fact happened later. So Charney suggested to look for economical integration methods which would use much longer time intervals.[44]
2) As to the *physical models* adopted and their mathematical expression: geostrophic equations, the so-called "filtering equations," or the balance equations, and others: the different kinds of physical simplifying hypotheses.
3) As to problems having to do with the application of the computer as a tool of analysis in the solution of physical problems, for instance typhoons: What were the physical mechanisms of the process, and what caused the growth of a small-amplitude depression into a typhoon? Charney once more suggested that, instead of feeding observed atmospheric phenomena directly into the machine, computers could be used to study simpler analogies, or component parts, of the phenomena just as one did in a laboratory by studying individual physical factors involved in complicated atmospheric motions. Likewise, machines and *laboratory models* would have to be combined.

[42] *Ibidem*, p. 645.
[43] *Proceedings*, [1962], p. 648.
[44] Technically speaking, this means converting the marching problem in the so-called 'jury problem' in Richardson's sense, and is related to Courant's condition in Courant-Friedrichs-Levy's theory. *Proceedings*, [1962], p. 640.

Finally, Charney raised the issue of computer use for this purpose: are the truncation errors and the methods of computation responsible for failures? Always, the same crucial question emerged: should one impute prediction difficulties to computers and computation methods, to models, or, more fundamentally, to the atmosphere itself?

Obviously, the answer to these questions was very difficult to find. Meteorologists held on to their old dream; they were scarcely ready to give up their ambitions.

Laboratory models for understanding

Lorenz's program focused precisely on the third difficulty listed and underlined by Charney, and on his suggestion of testing laboratory models. In the same year as the Tokyo Symposium, he published a paper in which he explained that the use of dynamic equations to further our *understanding* of atmospheric phenomena, justified their simplification *beyond the point* where they were expected to yield acceptable weather predictions.[45] There were two kinds of such simplifications. First, one might omit or modify certain terms; by doing so, one neglected or altered some physical processes believed to be of secondary importance. A second type of simplification was demanded by the impossibility of solving certain partial differential equations exactly. Thus, by finite-difference methods or otherwise, one converted each partial differential equation into a specific finite number of ordinary differential equations (several hundreds of them is typical in short-range dynamic forecasting). Also, simplifications of the initial conditions were necessary because of the system of observations in use. But Lorenz insisted that "if our interest is confined to furthering our understanding of the atmosphere, we may simplify the equations and initial conditions to the point where good predictions can no longer be expected."[46] The independent variables which Lorenz retained thus corresponded to features of the largest scale.

As an illustration, let us present how Lorenz arrives at the so-called "minimum hydrodynamic equations".

First he simplified the dynamic equations governing the atmosphere by reducing them to the familiar vorticity equation

$$\frac{\partial}{\partial t}\nabla^2\psi = -k\nabla\psi \times \nabla(\nabla^2\psi) \ , \tag{1}$$

where t is time, ψ is a stream function for two-dimensional horizontal flow, ∇ is a horizontal differential operator, $\nabla^2 = \nabla \cdot \nabla$ is the horizontal Laplacian operator, $\times$ is the internal product of vectors and k is a unit vertical vector. Equation (1) is equivalent to the barotropic vorticity equation, which also governs the motion of a general two-dimensional homogeneous incompressible nonviscous fluid; it states that the vorticity of each material parcel of fluid is conserved.

He applied Eq. (1) to flow in a plane region, in which ψ is doubly periodic at all time, i.e.

[45] Lorenz, E.N. [1960c], pp. 243–245.

[46] *Ibidem*, p. 245.

$$\psi(x + 2\pi/k, y + 2\pi/l, t) = \psi(x, y, t) \ , \tag{2}$$

where the x and y axes point eastward and northward, respectively, and k and l are specified constants. In this way he distorted the geometry of the spherical earth, but he retained the important property that the effective area is finite but unbounded. He also neglected the horizontal variations of the Coriolis parameter, although Eq. (1) is still consistent with a constant Coriolis parameter.

Taking the Fourier transform of (1), and after some further substitutions introducing the terms K, H, and M, Lorenz found the equations

$$\frac{dC_M}{dt} = -\sum_H \frac{K \cdot H \times M}{H \cdot H} C_H C_{M-H} \tag{3}$$

which is actually the infinite set of ordinary differential equations which he sought to determine. The coefficients C_M are the dependent variables; as Lorenz explains, a further simplification is the omission of reference to all but a finite number of variables C_M corresponding to a specified set of values of m and n. If these values are small, only large-scale features are retained. The summation in (3) becomes a finite sum, but the equations are otherwise unaltered.

Equation (3) may be regarded as describing a nonlinear interaction between the components whose coefficents are C_H and C_{M-H}, to alter a third coefficient C_M. Lorenz wanted to seek the *maximum simplification* of (3) which still describes this process. He noticed (but we don't give here his proof) that clearly at least three terms with different eigenvalues must be retained.

Finally, the governing equations, obtained either from (3), (or directly by some substitutions into (1)), are written in the form:

$$\frac{dA}{dt} = \left(\frac{1}{k^2} - \frac{1}{k^2 + l^2}\right) klFG \ , \tag{4}$$

$$\frac{dF}{dt} = \left(\frac{1}{l^2} - \frac{1}{k^2 + l^2}\right) klAG \ , \tag{5}$$

$$\frac{dG}{dt} = -\frac{1}{2}\left(\frac{1}{l^2} - \frac{1}{k^2}\right) klAF \ , \tag{6}$$

A, F, G appear in the Fourier's series of ψ and the coefficients of FG, AG and AF are actually determined by the ratio k/l. The mean kinetic energy and the mean square of the vorticity,

$$E = \frac{1}{4}\left(\frac{A^2}{l^2} + \frac{F^2}{k^2} + \frac{2G^2}{k^2 + l^2}\right) \ , \tag{7}$$

and

$$V = \frac{1}{2}\left(A^2 + F^2 + 2G^2\right) \ ; \tag{8}$$

are readily seen to be conserved under Eqs. (4)–(6).

According to Lorenz, these equations presumably contain the minimum number of degrees of freedom required to picture nonlinear barotropic phenomena. Lorenz called them the “minimum hydrodynamic equations”, or simply the minimum equations.[47]

Lorenz was aware that, when partial differential equations were numerically integrated by means of finite differences in space and time, the phenomenon of *computational instability* might arise. In order to avoid such instability, the time interval Δt should not to be too large compared with the space intervals Δx and Δy: when using orthogonal functions, the corresponding condition for computational stability was that Δt should not be too large a fraction of the oscillation period of the most rapidly oscillating variable. The simplified equations were realistic enough to provide a qualitative description of some of the important physical phenomena in the atmosphere; they even led to plausible hypotheses concerning phenomena as yet not explained. According to Lorenz, the degree of simplification, which one was permitted to use depended upon the particular phenomena one wished to investigate.

Lorenz developed different examples of “maximum simplification” of the system of equations in order to investigate the following problems:

- The behavior of a jet under barotropic flow or its splitting into two streams.
- Simple baroclinic flow. In this case, he said, one of the two-layer numerical prediction models could be used instead of the barotropic vorticity equation. The maximum allowable simplification would then retain three degrees of freedom for the flow in each layer, hence a total of six dependent variables. With such a system, the instability of zonal baroclinic flow could, among other phenomena, be studied.
- Forced baroclinic flow such as that which characterized the general circulation of the atmosphere.

All these simplifications might appear as rather crude approximations, Lorenz claimed, but they should clarify our understanding of the phenomena and lead to plausible hypotheses which might then be tested by means of careful observational studies and more refined systems of dynamic equations. By judiciously omitting certain terms in the dynamical equations and by comparing the result of the prediction with reality, one could estimate the cost of these omissions and thereby decide which terms were important and which were not. We have here an explicit and complete description of what is, in Lorenz’s mind, the right practice of modeling. A few years later, this conception would be largely considered as the canonical conception of modeling with computer simulations. Remarkably, that practice of modeling didn’t emerge spontaneously or immediately. On the contrary, it was object of much trial and error and many discussions.

At the same time, other meteorologists such as N.A. Phillips (also from MIT) went on with their Sisyphean work.[48] Also published in 1960, Phillips’s paper illustrated the methods in use at the time. He discussed the advantages of primitive equations over the geostrophic model where wind is independent of the vertical coordinate; he also mentioned a method for reducing initial noise. Phillips’s and the whole community’s efforts were not in vain, contributing, for instance, to the improvement of the quality

[47] Lorenz [1960c], p. 247.
[48] Phillips, [1960].

of numerical integration. The two approaches which may be associated with Phillips's and Lorenz's names respectivly, that of physical models and that of laboratory models, proved useful.

In 1962, Charney gave a very good summary of the alternative: when faced with nonlinear problems, scientists will "*choose either a precise model in order to predict or an extreme simplification in order to understand*".[49] In his paper, Charney himself chose the second alternative: he modeled whirlwinds with points, like Fermi, Pasta, and Ulam had done at Los Alamos in 1947 when they modeled a rope by 64 punctual masses.[50] Very simple and nonlinear, his system reproduced several whirlwinds around a circle and showed their extreme instability. But Charney attributed the instability of the model to repeated truncation errors which would play the role of a disruptive force. Charney still thought that a better computation would overcome that difficulty. This was very far from the point of view of Lorenz who had intuited that a small error could be self-amplified by the system even if one had exact computations at one's disposal.

Encountering chaos

Barry Saltzman, from the Travelers Weather Center at Hartford, Connecticut, understood the importance of Lorenz's ideas. Instead of global atmospheric circulation, Saltzman studied the convective motion of a fluid heated from below, a very frequent atmospheric phenomenon which for example may locally occur over warm grounds. For this, he developed in 1961 a simple model involving 7 variables.[51] He then made two observations which were to make his model famous. First, he noticed that in simulations 4 of the 7 variables quickly became very small, and solutions flattened on the 3 others. Second, aside from regular solutions, he pointed out one that was *aperiodic*. After having paid a visit to Saltzman, Lorenz became convinced that these 3 variables were keeping each other going and that a system involving only 3 variables might well exhibit the same behavior.

Let us enter in details. Rayleigh [1916] studied the flow occurring in a layer of fluid of uniform depth H, when the temperature difference between the upper and lower surfaces is maintained at a constant value ΔT. Such a system possesses a steady-state solution in which there is no motion, and the temperature varies linearly with depth. If this solution is unstable, convection should develop. In the case where all motions are parallel to the x–z plane, and no variations in the direction of the y-axis occur, the governing equations had been written by Saltzman in the following form:[52]

$$\frac{\partial}{\partial t}\nabla^2\psi = -\frac{\partial(\psi, \nabla^2\psi)}{\partial(x, z)} + \upsilon\nabla^4\psi + g\alpha\frac{\partial\theta}{\partial x} \ , \tag{9}$$

$$\frac{\partial}{\partial t}\theta = -\frac{\partial(\psi, \theta)}{\partial(x, z)} + \frac{\Delta T}{H}\frac{\partial\psi}{\partial x} + k\nabla^2\theta \ . \tag{10}$$

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[49] Charney, [1962b], p. 289.
[50] Fermi, Pasta & Ulam [1947], in Ulam [1974].
[51] Saltzman [1962].
[52] Lorenz, [1963], p. 135. We refer here to Lorenz quoting Saltzman.

Here ψ is a stream function for the two-dimensional motion, θ is the deviation of the temperature from that occuring in the state of no convection, and the constants g, α, υ and k denote, respectively, the acceleration of gravity, the coefficient of thermal expansion, the kinematic viscosity, and the thermal conductivity. The problem is most tractable when both the upper and lower boundaries are taken to be free, in which case ψ and $\nabla^2\psi$ vanish at both boundaries.

Rayleigh [1916] had found that fields of motion of the form

$$\psi = \psi_0 \sin(\pi a H^{-1}x)\sin(\pi H^{-1}z) \ \ , \tag{11}$$

$$\theta = \theta_0 \cos(\pi a H^{-1}x)\sin(\pi H^{-1}z) \ \ , \tag{12}$$

would develop if the quantity

$$R_a = g\alpha H^3 \Delta T \upsilon^{-1} k^{-1} \ \ , \tag{13}$$

now called *the Rayleigh number*, exceeded a critical value

$$R_c = \pi^4 a^{-2}(1+a^2)^3 \ \ . \tag{14}$$

The minimum value of R_a, namely 27 $\pi^4/4$, occurs when $a^2 = \frac{1}{2}$.

Saltzman [1962] derived a set of ordinary differential equations by expanding ψ and θ in double Fourier series in x and z, with functions of t alone for coefficents, and substituting these series into (9) and (10). He arranged the right-hand sides of the resulting equations in double Fourier series form, by replacing products of trigonometric functions of x (or z) by sums of trigonometric functions, and then adequated coefficients of similar functions of x and z. He then reduced the resulting infinite system to a finite system by omitting reference to all but a specified finite set of functions of t, in a manner already proposed by Edward Lorenz himself.[53] Saltzman then obtained time dependent solutions by numerical integration. In certain cases all but three of the dependent variables eventually tended to zero, and these three variables underwent irregular, apparently nonperiodic fluctuations.

Lorenz [1963] notices that these same solutions would have been obtained if the series had at the start been truncated to include a total of three terms. Accordingly, he sets

$$a(1+a^2)^{-1}k^{-1}\psi = X\sqrt{2}\sin(\pi a H^{-1}x)\sin(\pi H^{-1}z) \ \ , \tag{15}$$

$$\pi R_c^{-1} R_a \Delta T^{-1}\theta = Y\sqrt{2}\cos(\pi a H^{-1}x)\sin(\pi H^{-1}z) - Z\sin(2\pi H^{-1}z) \tag{16}$$

where X, Y, and Z are functions of time alone.

By substituting expressions (15) and (16) into (9) and (10), and omitting trigonometric terms other than those occuring in (15) and (16), Lorenz obtained the equations:

$$X^{\bullet} = \sigma X + \sigma Y \ \ , \tag{17}$$

[53] Lorenz [1960c], pp. 243–254.

$$Y^{\bullet} = XZ + \gamma X - Y \quad , \tag{18}$$

$$Z^{\bullet} = XY - bZ \quad . \tag{19}$$

Here a dot denotes a derivative with respect to the dimensionless time $\tau = \pi^2 H^{-2}(1 + a^2)kt$, while $\sigma = k^{-1}\upsilon$ is the *Prandtl number*, $r = R_c^{-1}R_o$, and $b = 4(1 + a^2)^{-1}$. Equations (17), (18), and (19) are the convection equations whose solutions Lorenz studied.

In these equations X is proportional to the intensity of the convective motion, while Y is proportional to the temperature difference between the ascending and descending currents, equal signs of X and Y denoting that warm fluid is rising and cold fluid is descending. The variable Z is proportional to the deviation of the vertical temperature profile from linearity, a positive value indicating that the strongest gradients occur near the boundaries.

Lorenz emphasizes that Eqs. (17)–(19) may give realistic results when the Rayleigh number is slightly supercritical, but, in view of the extreme truncation, their solutions cannot be expected to resemble those of Salzman's equations (9) and (10) when strong convection occurs. Lorenz coded this system of 3 equations involving 3 variables for machine computing. Just as Saltzman had discovered with his model, Lorenz observed the lack of periodicity for some solutions.[54]

Before he was hired as a meteorologist during World War II, Lorenz had been passionate about mathematics and had even contemplated a career in this field. With Saltzman's system, he was allowed to go back to his first love. In order to interpret his results, Lorenz dove into Poincaré's and Birkhoff's works and wrote a first version of an article about which nothing unfortunately is known today. An unknown referee directed him, not surprisingly, to Solomon Lefschetz's books and, luckily for him, to the recent translation of Niemytskii and Stepanov's book which covered the qualitative theory of dynamical systems developed by Alexandr Andronov's Soviet school.[55] There, Lorenz realized, was a set of elementary theoretical tools for the analysis of his results. In his seminal paper published in 1963, Lorenz proved, after a brief introduction linking his problem to meteorology, that almost all solutions to his system of 3 equations (which he studied in phase space) were unstable.[56] If periodic solutions, of which there was a countable infinity, were unstable, then quasiperiodic solutions could not exist. In his system, irregular unstable trajectories were the general case. Hence a fundamental problem emerged: if nonperiodic solutions were unstable, two neighboring trajectories would diverge very quickly. Otherwise, the "*attractor*", which according to Lorenz was the portion of phase space to which solutions tended, would have to be confined in a three-dimensional bowl; forcing two trajectories to come back very close to one another. Together with the requirement that two solutions cannot intersect, these constraints led Lorenz to imagine a very special structure for his attractor: an infinitely folded surface, the "butterfly" that

[54] See Lorenz [1993], pp. 136–160.

[55] About the development of dynamical systems in the 1950s and 1960s and the diffusion of Soviet results see Dahan Dalmedico [1994] and [1997].

[56] Lorenz [1963] pp. 130–141.

has now become very familiar. Representing the attractor in terms of a two-dimensional projection, Lorenz used Poincaré's first-return map in order to study his attractor better.

The conclusions drawn by Lorenz in his paper were twofold: (1) From a theoretical standpoint, he proved that very complicated behaviors could arise from very simple systems – simplicity could generate complexity, in contradiction to the old, well-established conviction that simple causes should give rise to simple effects. And (2) he also exhibited the property of sensitivity to initial conditions and thus opened a window on the understanding of turbulence. At the meteorological level (provided his model had anything to do with the atmosphere), hopes for long-range predictions were doomed. The old debate about whether a larger number of degrees of freedom would stabilize the system, or not, was put to rest. In this perspective, all that remained for Charney to work on was to try discovering the limits for effective prediction.

The aftermath of Lorenz's work goes beyond the scope of this paper; I shall not deal with the mathematical apprehension of these results; how mathematicians grasped them, how different communities (meteorologists, mathematicians, but also physicists, specialists of fluid mechanics, and population biologists) came to meet each other around a few similar issues. Let me simply mention that, published in the *Journal of Atmospheric Sciences*, Lorenz's paper remained unnoticed by mathematicians until 1972. At least three times, Lorenz's results failed to attract the mathematical community's attention when they might have.[57] First, one of the referees asked to evaluate Lorenz's paper was none other than Stan Ulam. Having carefully studied with Stein at Los Alamos, the iteration of nonlinear functions and having been involved in the famous Pasta-Fermi-Ulam experiment, he should have been, one may think, sensitive to these issues. But, too busy at the time, he entrusted the job to a colleague. Secondly, meeting Ulam around 1965, Lorenz mentioned his research without showing him his attractor; this again was not enough to catch the mathematician's attention. Finally, at a meeting on "Statistical Methods and Turbulence" in 1971[58], Ruelle gave a short talk about a mathematical explanation for the onset of turbulence (regarded as a bifurcation problem) and briefly expounded what became known later as his strange attractor theory. Participating in the same session at this symposium, Lorenz remarkably was the only one, apart from Ruelle himself, who shunned stochastic processes and statistical methods and was rather more interested in the prediction of what he called "models" of turbulence.[59] A modest, shy man who belonged to a scientific community different from Ruelle's, Lorenz preferred to wait until he could check his own results before mentioning that his earlier work of 1963 perfectly corresponded to what he had just heard from the mathematical physicist. Once more, an opportunity was missed to call other scientists' attention to his work. But this time, he would not have to wait for long before fame would catch up with him.

[57] Personal Interview with the author (Boston, Mai 1992).

[58] Ruelle [1971].

[59] Lorenz [1971], p. 195, where he wrote: "Ensembles of solutions of simplified or ortherwise modified forms of the Navier-Stokes equations will not qualify as turbulence; we shall instead regard them as models of turbulence".

Concluding remarks

Returning to the epistemological issues pertaining to the question of models and modeling raised at the beginning of this paper, we realize that contemporary modeling practice takes place between two poles: one is operational and linked to prediction, the other is turned towards cognition, explanation, and understanding. Although examined here for the specific case of meteorology, this duality is a very general feature. The first pole played the major role in 1946–55, the second one became dominant around 1960; nevertheless, they coexisted and intermingled throughout both periods. Moreover, although protagonists often spoke simply of the *representativeness of a model*, it is clear that the confrontation does not rest upon a reality supposed as being already there and a model which would represent it. It is not reality which is represented but already a subtle and complex *reconstruction*, both theoretical and empirical, of this reality. The reconstruction takes place on several levels: (1) a system of hypotheses, theories, and concepts, which are both fundamental principles and simplifying assumptions; (2) a system of observations and measurements; and (3) objectives and norms of prediction, efficiency, and uses. None of these levels is obvious and transparent. In the case we followed here, the first level mobilized general laws of physics (laws of motion, conservation of energy, the Navier-Stokes equations, and other hydrodynamic concepts), but also a whole set of assumptions allowing for the reduction of the system of equations into simplified forms (geostrophic equation or balance equations, for instance). On the second level, a statistically and cartographically complex device of observations, measurements, and interpolations intervened. The third level was linked with what meteorologists are presently confronting: the fact that modeling of the atmosphere depends on whether one makes one or two-day weather forecasts, studies the evolution of a typhoon or a jet-stream, or investigates climate change.

In conclusion, this story underlines the crucial importance of numerical simulations: by isolating different factors and evaluating their respective roles, computer and numerical experiments as well as laboratory models gave rise to a better exploration and understanding of “reality”. We have also noted the role of numerical methods and the difficulties related to different kinds of algorithms for the integration or discretization of equations at the different points of the lattice (in particular, the determination of time- and space-steps satisfying the Courant condition). In testing models for predicting atmospheric phenomena, failures may have their origin either in each of the levels mentioned above, or in computing methods and truncation errors, or in the phenomenon itself *regardless of the way in which it has been modeled*. But, confronted with unpredictable behavior and instability, it is almost impossible to affirm whether or not they arise from the *ontology* of the phenomenon. Even after Lorenz’s results, a few clung to hopes for bigger models and the question was not definitively settled.

In the field of meteorology, the introduction of the mathematical theory of dynamical systems came after that of modeling practices. One may remark that this process was rather the opposite to what happened with Ruelle and his followers, who delved into the turbulence problem by means of qualitative dynamics, only to turn later to simulations as a direct result of their acceptance of Lorenz’s system. Modeling practices, however, were hardly foreign to fluid mechanicists, but because of the field’s much greater simplicity compared with atmospheric motions, the question of the (linear) stability of dynamical equations in pure hydrodynamics was solvable by means of a combination of numerical and analytic methods. While in the latter case, modeling practices remained subservient to the analytical problems, meteorologists tackled the very issue of stability by means of computer simulations. Conversely, it was the wider understanding of chaotic dynamics which gradually led meteorologists to adopt an approach inspired by dynamical systems theory.

From the early computing machines to giant calculators, the computer was employed in a variety of ways in fluid geophysics. Identifying four types of computer usage between 1945 and 1972, Charney used the terms synthetic, experimental, heuristic, and data-analytic:

> According to this classification, a computer's operation is *synthetic* when it is used to predict or stimulate large-scale phenomena. *Experimental* use occurs when the computer simulates an individual phenomenon, in isolation from its environment, so that the user may infer its physical causes. The computer is used *heuristically* to build highly simplified models as a means to discover new relationships in the atmosphere. Finally the computer can be used to *reduce and analyze* observational data.[60]

Concerning the Meteorological Project, one may state that the work on general circulation was *synthetic*, whereas the use of simple baroclinic models to determine the physical basis of cyclogenesis was *experimental*. The arguments above imply that the *heuristic* use of computers took off only in the early 1960s, largely on the basis of Saltzman's and Lorenz's work on maximum simplification models.

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References

Aspray, W. [1988] "The Emergence of Princeton as a World Center for Mathematical Research", 1896–1939, in *History and Philosophy of Modern Mathematics*, William Aspray and Philip Kitcher, (ed.), Minnesota Studies in the Philosophy of Science, vol. 11, Minneapolis: University of Minnesota, 346–66.

——[1990] *John von Neumann and the origins of modern computing*, Cambridge (MA), MIT Press.

Bigelow, J. [1980] "Computer development at the Institute for Advanced Study", in *A History of computing in the Twentieth Century*, N. Metropolis, J. Howlett and G.C. Rota, (eds.), Academic Press, New York, 291–310.

Birkhoff, G. [1977] "Applied mathematics and its future", in Thomson, R.W., (ed.), *Science and technology in America*, NBS, publ. 465.

——[1980] Computing developments 1935–1955, as seen from Cambridge, U.S.A., in "A History of Computing in the Twentieth Century", N. Metropolis, J. Howlett and G.C. Rota (eds.), Academic Press, New York, 21–30.

Charney, J. [1955] "Numerical Methods in Dynamical Meteorology", *Proceedings of the National Academy of Science* 41, 11 (November), 798–802; quoted in Aspray [1990], 152–153.

[60] Charney [1972], p. 121, quoted in Aspray [1990], p. 153.

——[1960] "Numerical prediction and the general circulation", in *Dynamics of Climate*, R.L. Pfeffer (ed.), Pergamon, New York, 12–17.

——[1962a] "Integration of the Primitive and Balance Equations", *Proceedings International Symposium on Numerical Weather Prediction in Tokyo, November 1960*, pub. in 1962, 131–152.

——[1962b] "Numerical experiment in atmospheric hydrodynamics", *Proceedings of Symposium in Applied Mathematics* XV, p. 289.

——[1972] "Impacts of Computers on Meteorology", *Comput. Phil. Commun.*, 3, Suppl., 117–126.

Charney, J., Fjörtoft, R. and von Neumann, J. [1950] "Numerical integration of the barotropic vorticity equation", *Tellus*, 2, 237–254.

Courant, R., Friedrichs, K. and Lewy, H. [1928] "Über die partiellen Differenzengleichungen der Mathematischen Physik", *Mathematische Annalen*, 100, 32–74.

Cvitanovic, P. [1984] *Universality in Chaos: a Reprint Selection*, Bristol, Adam Hilger.

Dahan Dalmedico, A. [1994] "La renaissance des systèmes dynamiques aux États-Unis après la deuxième guerre mondiale: l'action de Solomon Lefschetz", *Supplemento ai Rendiconti del Circolo matematico di Palermo*, II, 34, 133–166.

——[1997] "Le difficile héritage de Henri Poincaré en systèmes dynamiques", in Greffe, J., Heinzmann, G., & Lorenz, K., (eds.), *Henri Poincaré, science et philosophie*, Berlin, Akademie Verlag & Paris, Blanchard, 13–33.

——[1996] "L'essor des mathématiques appliquées aux Etats-Unis: l'impact de la seconde guerre mondiale", *Revue d'Histoire des Mathématiques*, 2 (1996), 149–213.

——[1997] "Mathematics in the 20th century", in Krige, J. & Pestre, D., (eds.), *Science in the 20th century*, London, Harwood Academic Publishers, 651–667.

——[1999] "Pur versus appliqué? Un point de vue d'historien sur une 'guerre d'images' ", *La Gazette des mathématiciens*, Publication de la Société Mathématique de France, n° 80, 31–46.

——[forthcoming] "Chaos, Disorder and Mixing: A new 'Fin de siècle' Image of Science?", to appear in *Growing Explanations*, N. Wise (ed.), Princeton University Press.

——[in press] "An Image Conflict in Mathematics after World War II", in Bottazzini, U. & Dahan Dalmedico, A. (eds), *Changing Images in Mathematics. From the French Revolution to the new Millenium*, Harwood. Acad. Publ., London.

Fermi, E., Pasta, J. and Ulam, S. [1947], *Studies of non-linear problems* (Los Alamos Document), published in Ulam [1974], *Sets, Numbers and Universes, Selected Papers*, (Cambridge, MIT-Press).

Goldstine, H.H. [1972] *The Computer from Pascal to von Neumann*, Princeton University Press, Princeton, NJ.

——[1977] *A history of Numerical Analysis from the 16th through the 19th Century*, Springer-Verlag, New York.

Heims, S.J. [1980] *John von Neumann and Norbert Wiener. From mathematics to the technologies of life and death*, Cambridge (MA), MIT-Press.

Lorenz, E.N. [1960a] "Energy and Numerical Prediction", *Tellus* n° 12, 364–373.

——[1960b] "The Statistical Prediction of Solutions of Dynamic Equations", *Proceedings International Symposium on Numerical Weather Prediction* in Tokyo, 629–635.

——[1960c] "Maximum Simplification of the Dynamic Equations", *Tellus* 12, n° 3, 243–254.

——[1963] "Deterministic non periodic flow", *Journal of Atmospheric Sciences*, 20, n° 2, 130–141.

——[1970] "The nature of the global circulation of the atmosphere: A present view", The Global Circulation of the Atmosphere, G.A. Corby (ed.), *Meterol. Soc.*, London, 2–23.

——[1993] *The Essence of Chaos*, Seattle, University of Washington Press.

Metropolis, N., Howlett, J. and Rota, G.C. (eds.), *A History of computing in the Twentieth Century*, Academic Press, New York, 291–310.

Nebeker, F. [1995] *Calculating the Weather. Meteorology in the 20th Century*, San Diego, Academic Press.

Nemytskii, V.V. and Stepanov, V.V. [1960] *Qualitative theory of differential equations*, Princeton, Princeton Univ. Press.

Neumann, J. von. and Richtmyer, R.D. [1950] "A method for the numerical calculation of hydrodynamic shocks", *Journal of Applied Physics*, 21, 232–237; von Neumann *Works* VI, 380–385.

Phillips, N.A. [1951] "A simple three-dimensional model for the study of large-scale extratropical flox patterns", *J. Meteorol. Soc.*, 8, 381–394

_____[1960] "Numerical Weather Prediction" in *Advances in Computers*, New York Academic Press.

_____[1960] "On the Problem of initial fluid data for the primitive equations" (1959) *Tellus* 12, n° 2.

Platzman, G.W. [1952] "Some remarks on high-speed automatic computers and their use in meteorology", *Tellus*, 4, 168–178.

_____[1967] "A retrospective view of Richardson's book on weather prediction", *Bull. Am. Meteorol. Soc.*, 48, 514–550.

_____[1979] "The ENIAC computations of 1950 – gateway to numerical weather prediction", *Bull. Am. Meteorol. Soc.*, 60, 302–312.

Proceedings Third Berkeley Symposium of Mathematical Statistics, [1955], vol I, II, III, IV.

Proceedings International Symposium on Numerical Weather Prediction in Tokyo, November 1960, [1962] ed. Sigekata Siono (1962). Tokyo: Meteorological Society of Japan.

Rayleigh, Lord [1916] "On convective currents is a horizontal layer of fluid when the higher temperature is on the under side", *Phil. Mag.*, 32, 529–546.

Ruelle, D. [1980] "Strange Attractors", *Math. Intelligencer*, 2, n° 126, 37–48.

Saltzman, B. [1962] "Finite amplitude free convection as an initial value problem", *Journal of Atmospheric Sciences* 19, 329–341.

_____[1967] "Meteorology: A brief history", in *Encyclopedia of Atmospheric Sciences and Astrogeology*, R.W. Fairbridge (ed.), Reinhold, New York, 583–591.

Smagorinsky, J. [1983] "The beginnings of numerical weather prediction and general circulation modeling: Early recollections", *Adv. Geophys.*, 25, 3–37.

Thompson, P. [1983] "A history of Numerical Weather Prediction in the United States", *Bulletin of The American Meteorological Society*, 64, 7, 755–769.

Wiener, N. [1956] "Nonlinear Prediction and Dynamics", *Proc. 3rd Berkeley Sympos. Math. Stat. and Prob.*, 247–252.

Centre Alexandre Koyré
CNRS
57 Rue Cuvier (Pavillon Chevreuie)
75005 Paris
France
dahan@paris7.jussieu.fr

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